GRATITUDE AND PROSOCIALITY: A BEHAVIOURAL ECONOMICS AND PSYCHOMETRIC PERSPECTIVE

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Abstracts

We feel *gratitude*—a *positive* emotion upon receiving an *undeserve*d benefit which is attributable to the givers' *benevolent* intent (Watkins, 2007, 2014). Meanwhile, *indebtedness* symbolises an *unpleasant* mental state which is also triggered by benefit receipts (Greenberg, 1980). Theories and empirical evidence in the literature have highlighted how *gratitude* and *indebtedness* each relates to *prosociality* (or *sanctioning*), and importantly, how *via different routes* these two constructs will elicit cooperativeness. Nonetheless, there is still a gap in the literature on how *gratitude* and *indebtedness* will contribute to prosociality and sanctioning in economic exchanges (Leung, 2011).

Thus via three economic games (i.e. Experiments 1 to 3, presented in Chapters 2 to 5) I endeavour to thoroughly examine how *gratitude* (and *indebtedness*) would relate to *prosociality* or *sanctioning* in a *Behavioural Economics* context. In so doing I intend to combine *Psychometrics* and *Experimental Economics* in the examination of the *gratitude* (and *indebtedness*)-*prosociality* association. Additionally, via meta-analysing (i.e. Chapter 2) over three decades of research on the *gratitude-prosociality* link I intend to offer i) a comprehensive quantitative synthesis of the findings and, ii) a systematic exploration of moderators, which are both absent in the literature.

The present thesis also features a series of extensive follow-up analyses on an interesting economic observation from Experiment 1— i.e. the *cheap-rider* problem (Cornes & Sandler, 1984). While Experiment 2 entails a more focused scrutiny (via a *one-shot* game) over the *occurrences* and *motives* behind cheap-riding, Experiment 3 builds on that by testing how cheap-riding may be used to enforce normative fairness in an *iterated* exchange context.

Results of the meta-analysis revealed a *moderate positive* link between *gratitude* and *prosociality*. The moderator analyses showed that this link is stronger when, a) *state* rather than *trait* gratitude was measured, b) *direct* instead of *indirect* or *non*-reciprocal outcomes was examined, and c) *benefit-triggered* instead of *generalized* gratitude (Lambert et al., 2009) was examined.

Meanwhile, results of Experiment 1 built upon the above by showing how the *gratitude-reciprocity* link will be subject to helper intent attribution, and how the injunctive fairness norm (Elster, 2006) could influence this attribution and thereby shaped recipients' feeling of *gratitude* (or *indebtedness*) throughout the episode, and ultimately his/her urge to *directly* reciprocate. Additionally, a noticeable degree of *cheap-riding* was observed when *unfairly treated* participants were granted an avenue to sanction their helpers.

The data of Experiment 2 revealed a pattern of *cheap-riding* that corresponded not only to that of Experiment 1 but also to the reality. Crucially, the analyses of the *motives* behind repayment allowed the disentanglement of the *psychology* between that of the *cheap-riders*, *cooperators*, and *free-riders*.

Lastly, analyses of Experiment 3 revealed three main findings. They included, a) people's preference for an 'optimal' platform for cheap-riding to better serve its *norm-enforcing* function, although its actual efficacy in promoting mutual compliance to normative fairness is still questionable; b) how the Relative Rank Model of Gratitude (Wood, Brown, & Maltby, 2011) will supersede the injunctive fairness norm in guiding the recipients' benefit appraisals, experienced *gratitude*, and eventual *direct reciprocal* acts toward the helpers; and c) how *gratitude* and *indebtedness* were both predictive of more *trustworthiness* and *generosity* in an *iterated*, variant of Trust Game.

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Declarations

I, Lawrence Ka-yin Ma, declare that this thesis and the work presented in it are the results of my own research. This thesis was wholly written while I was registered as a PhD Psychology student at the University of Nottingham

List of Publication, Submitted Manuscript, and Conference Proceedings

Chapter 2

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Chapter 4

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List of Contents

List of TABLES

Chapter 2 (Tables 2.1 to 2.9)

Chapter 3 (Tables 3.1 to 3.13)

Chapter 4 (Tables 4.1 to 4.18)

Chapter 5 (Tables 5.1 to 5.19)

List of FIGURES

Chapter 2 (Figures 2.1 and 2.2)

Chapter 3 (Figures 3.1 to 3.5)

Chapter 4 (Figures 4.1 to 4.13)

Chapter 5 (Figures 5.1 to 5.11)

Chapter 6 (Figures 6.1 and 6.2)

List of APPENDICES

Chapter 1

General Introduction

1.0. Overall Objective of the Current Thesis

The current thesis concerns the detailed role of *gratitude*—an emotion that arises when people receive an undeserved benefit (Bertocci & Millard, 1963) attributable to someone else's benign intent (Emmons, 2004; Watkins, 2007)—in relation to *prosociality* and *sanctioning* in a behavioural economics (i.e. BE hereafter) context. In particular, I examined—via an iterated and two one-shot variant of Trust Game—how *gratitude*, as a transitory *state* or as an enduring *disposition*, will facilitate people's *reciprocal* (as opposed to *sanctioning*) decisions in situations under which pursuit of self-gain will clash with maximization of collective benefits (Allison, Beggan, & Midgley, 1996).

Another objective of the current thesis is to dissociate the mechanisms through which *gratitude* and *indebtedness*—an unpleasant mental state triggered by benefit receipts (Goei & Boster, 2005; Greenberg, 1980)—prompts *positive* and *negative* reciprocity (Fehr & Falk, 1999) in a BE context. I also examined the importance of *intention* and *injunctive norm* on fostering gratitude or indebtedness, and how these may eventually contribute to one's economic decision making. Furthermore, a meta-analytic review over the link between *gratitude* and *prosociality* was included. Via this review I aimed to provide an overall estimate on how strongly gratitude relates to prosociality, and also what other issues might mould the strength of this association.

Leung (2011) argued that there is a paucity of empirical works which specifically investigated how *gratitude* engenders cooperation in *economic exchanges*. Indeed, despite a plethora of psychological research (e.g. Froh et al, 2014; Halali, Kogut, & Ritov, 2016; Krumrei-Mancuso, 2016) on the *gratitude-prosociality* link, only a few had outright examined prosociality in an economic

exchange (e.g. Desteno, Bartlett, Baumann, Williams, & Dickens, 2010; Exline & Hill, 2012; Rubin, 2012). Even worse, *indebtedness* is an overlooked topic in the BE literature despite its theoretical connectedness with *direct reciprocity* (Greenberg & Shapiro, 1971; Watkins, Scheer, Ovnicek, & Kolts, 2006) and *sanctioning* (e.g. Fisher, Nadler, & Whitcher-Alagna, 1983; Greenberg & Westcott, 1983; Hatfield & Sprecher, 1983). To date, there is still a *total absence* of published articles which directly scrutinised the effect of indebtedness—as an aversive psychological *state* or a *disposition* (Mathews & Green, 2010; Mathews & Shook, 2013)—in economic exchanges. Thus, one intended major contribution of the current thesis is to combine *psychometrics* and *Behavioural Economics* in scrutinising the role of *gratitude* and *indebtedness* in nurturing (or undermining) cooperation and sanctioning.

Meanwhile, an *injunctive norm* represents a cluster of expectations on what a typical person *should* do in a given situation to warrant social approval (Burger et al., 2010; Cialdini, Kallgren, & Reno, 1990; Perkins, 2003). Bicchieri (2006) argued that when evaluating someone else's intent people will base their judgment upon whether the injunctive norm was upheld. Thus, injunctive norm forms the basis of fairness judgment and this notion has been evidenced in the social psychology, social cooperation, and social learning literature (e.g. Hoffman, McCabe, & Smith, 1996; Irving & Simpson, 2013; Kazemi, Eek, & Gärling, 2016). Nevertheless, it still remains unaddressed as of how such a norm-based judgment would tamper with people's experience of *gratitude*, or *indebtedness*, and crucially, how these might contribute to one's *actual* decisions to *reciprocate* or to *sanction* in an economic interaction.

Moreover, in spite of the host of *empirical* (e.g. Tsang, 2006a, 2007) and *theoretical* evidence (e.g. Algoe, Haidt, & Gable, 2008; McCullough, Kilpatrick, Emmons, & Larson, 2001; McCullough & Tsang, 2004; Roberts, 2004) for *gratitude-prosociality* link, there is still a lack of a comprehensive quantitative review of these findings. Given the overarching aim of this thesis concerns the analyses of how *gratitude* will enhance *prosociality* in a specific context (i.e. a BE setting), it is of utmost importance to examine if there is a reliable and consistent association between *gratitude* and *prosociality* for starters, and also what *methodological* or *theoretical* factors may account for any inconsistencies of the effect estimates across the sampled studies.

Taken together, I believe that the current thesis should bridge a gap in the literature by illustrating 1) how *gratitude* and *indebtedness*, would—potentially in a different fashion (See Schaumberg and Flynn (2009) for a review)—influence one's intention or actual *cooperation* or *sanctioning* in a BE context; 2) how the *injunctive norm*, and its effect on fairness perception, will affect one's gratitude or indebtedness and consequently guide that individual's decision to cooperate or sanction; and lastly, 3) how *gratitude* relates to prosociality and what other factors could have moderated this link.

Below (i.e. Section 1.1) I discuss the theoretical build-ups of the present thesis. That includes a range of theories and models of cooperation and altruism, so as the role of *positive* and *negative* emotions in these cooperation (or sanctioning) models. Section 1.1 also entails a review of the evidence in the Social Psychology and Experimental Economics literature that pertains to the *gratitude-prosociality* association. The present chapter is at last wrapped up by

a few basic predictions and hypotheses, which are then followed by a preview of Chapters 2 to 5 which present the meta-analysis and Experiments 1 to 3.

1.1. Cooperation and Altruism Revisited

1.1.1. Sanctioning and Cooperation

The effect of sanctioning on cooperation maintenance is well documented in the experimental economics literature (See Fehr and Rockenbach (2004) for a review). Particularly, it is argued that to enforce the norm of cooperation it is imperative that one should penalise norm violators for victimising a third party (Bendor & Swistak, 2001), i.e., altruistic 'third-party' punishment (Landman & Hess, 2016; Nelissen & Zeelenberg, 2009).

Indeed, data of Fehr and Fischbacher's (2004) third-party punishment game illustrated that 55% of the spectators would punish, in spite of a cost to themselves, people for refusing to apportion a fair share of endowment to their co-players. Also, nearly 80% of participants will anticipate the third-parties (i.e. the spectators) to intervene via sanctioning their partners if they were not playing fair. These implied that there should be a consensus of zero tolerance toward norm violators, and crucially, an expectation of these perpetrators to get their comeuppances even if punishing them may not be utterly cost-free.

Additionally, Gürerk, Irlenbusch, and Rockenbach (2004, 2006) demonstrated, via their iterated public goods game, that there was a shift in preference from a *sanctioning-free* (SFI hereafter) to a *sanctioning* institution (SI hereafter) as the game proceeded. Participants will decide before the start of each trial whether they preferred the *SFI*—in which players could not penalise one another, or the *SI*— in which sanctioning non-cooperation was feasible.

Indeed, not only had their data revealed a near extinction of preferences for SFI in the later experimental phases, Gürerk et al. (2006) also showed that the contributions were significantly larger among players who picked the SI. Thus, the results of Gürerk et al. (2004, 2006) again pointed to the fact that people do subscribe to the idea that sanctioning (or the threat of which) is instrumental in safeguarding mutual cooperation, and importantly, the data further corroborated the potency of sanctioning in *nurturing cooperation*.

Nevertheless, a few questions remain unanswered: Does sanctioning alone already suffice to guarantee mutual cooperation? What other factors may as well influence the efficacy of sanctioning in preventing defection?

1.1.2. Intent Attribution, Reciprocity Model, and Emotion

1.1.2.1. Sanctioning No Panacea to Maintenance of Cooperation

To address the above questions Fehr and associates (Fehr & Rockenbach, 2003; Fehr & List, 2004) examined the downside of *unfair, unreasonable* sanctioning on cooperation maintenance via a series of modified Trust Games (i.e. TG hereafter). In these modified TGs Fehr and associates had granted a portion of the *investors* the option to fine their *trustees* for back-transfers (BTs) that fell short of the investors' 'desired BTs' (i.e. the 'Incentive Condition'). Investors could hand-pick their own 'desired BTs' and were under no pressure to impose any fine if they deemed unnecessary. As such, the experimenters examined if a *selfishly*-driven sanctioning arrangement, in which an *investor* demanded an unreasonably high back-transfer while threatened to sanction, would rather *encourage* (but not *undermine*) non-cooperation.

The data confirmed this hypothesis by showing that unfair sanctioning overall haunted a trustee's compliance. Specifically, *unfairly* treated trustees

would rather pay the penalty charges than complying with their investors' selfish demands. By contrast, trustees whose investors chose *not* resort to the punishment threat—even when they could—demonstrated more *trustworthiness* (in the form of proportionately higher BTs), than trustees i) who were *threatened* to not under-repay; and those ii) whose trustors could not penalise. These showed that when one decides whether to cooperate he/she does not just reflexively respond to the sanctioning parameters, he/she also ponders over whether the arrangement comprises *moral legitimacy* or it reflects malicious intent on behalf of the enforcers (Fehr & Rockenbach, 2003). In short, a lack of perceived negativity (e.g. unfairness and animosity), or the presence of perceived kindness (e.g. trust and respect), is also instrumental in the preservation of mutual cooperation (McCabe, Rigdon & Smith, 2003).

1.1.2.2. Intent Perception and Reciprocities

Fehr and colleagues' observations are indeed consistent with the Theory of Reciprocity (Falk & Fischbacher, 2006) which asserts that reciprocation (or retaliation) will depend on a recipient's evaluation of helper's intent. Charness and Levine (2007) even argued that intent perception could override actual distributive outcomes in predicting positive or negative reciprocity, as their data showed that when individuals decided whether to remunerate or to sanction they tend to prioritise the evaluation of the intentionality of the outcomes.

Fehr and Falk (1999) argued that reciprocity comprises both cooperation and sanctioning. While *Positive Reciprocity* arises from an urge to 'reward actions perceived as generous, kind or fair (pp.109)', *Negative Reciprocity* is driven by a desire to sanction behaviours deemed 'hostile or unfair (pp.109).' Additionally, Greenberg and Westcott (1983) suggested that emotion may

mediate the link between intent attribution and reciprocation. They argued that a *prosocial* gesture which communicates good intentions—on the benefactor's behalf—should elicit *positive* emotions such as *gratitude* (Komter, 2004; Nadler, 2012), and these positive emotions should be conducive to reciprocation (Fredrickson, 2004a; McCullough et al., 2001; Tsang, 2007). Nowak and Roch (2007) highlighted the role of *positive* affects—while emphasising gratitude—in triggering and sustaining both the *direct* and *indirect* reciprocation. This resonates with Fredrickson's (2004a) Broaden-And-Build Theory in which gratitude could propel a beneficiary to act 'prosocially oneself , either towards one's benefactor, towards others, or both. (pp.150)'

By contrast, a maliciously-intentioned act of ingratiation should elicit negative emotions such as *indebtedness* and *annoyance* in the recipients (Greenberg & Shapiro, 1971; Hatfield & Sprecher, 1983; Lazarus & Lazarus, 1996), and these negative emotions have been well-documented to foster non-cooperation or even sanctioning (Ma et al., 2012; Xiao & Houser, 2005).

Taken together, the above underlines the role of emotion in transforming intent attributions to the subsequent behavioural retaliation and reciprocation. Section 1.1.3 further highlights how cooperation (or prosociality), thanks to its evolutionary origin, would depend upon emotions.

1.1.3. Cooperation as an Intuition

Rand, Greene, and Nowak (2012) had referred to the Dual-process framework (Dijksterhuis & Nordgren, 2006; Kahneman, 2011)—which theorises one's social decision making as an outcome of interaction between *Intuition* (System 1) and *Reflection* (System 2)—to account for the observed pattern of cooperation in various one-shot economic games (such as the PGs).

Their results identified a causal, direct link between *processing speed* and *cooperation* which suggests that one's 'automatic first response (pp.429)' is cooperation, and that selfishness only creeps into one's mind once that person can afford to be deliberative. As a result, Rand et al. (2012) argued that humans are predisposed to prosociality whereas rationality results from self-control.

Rand et al. (2012) further theorised that human beings develop the *cooperative instinct* in daily lives under which cooperation is advantageous; as the key interactions are repeated (Fudenberg & Maskin, 1986) and that cooperation are critical in reputation management (Pfeiffer, Tran, Krumme, & Rand, 2012; Rockenbach & Milinski, 2006; Milinski, 2016). These participants, when cornered into acting fast, might therefore just act on their altruistic 'defaults' even in a context in which cooperation is anything but advantageous (e.g. one-off exchanges with anonymous co-players in a laboratory). However, as the time pressure dissipates (which implies more room for *reflection*), people can adapt to the context and therefore make a more calculated decision as they suppress their *cooperative heuristics* (Rand et al., 2014; Jordan et al., 2014).

Meanwhile, it is well documented (e.g. Kahneman, 2003, 2011) that the System 1 is characterised by automaticity, and more importantly, *susceptibility to emotions*. System 2, on the other hand, epitomises consciousness and emotionlessness. Taken together, the intuitive nature of prosociality should by default render cooperation an emotionally dependent concept. Indeed, the current thesis will particularly examine how emotions *gratitude* and *indebtedness* will relate to prosociality in the realm of economic exchanges.

1.2. Emotion, Altruism and Sanctioning Revisited

Section 1.1.2 discussed the importance of intent attributions and emotions from the recipients' perspective in shaping the two-part exchange—i.e. initial favour followed by reciprocation or retaliation—between a benefactor and a recipient. Indeed, the emotional reactions of recipients used to be a scarcely researched topic in the Altruism literature until the 80s (See Fisher, Nadler, & DePaulo, 1983; Gross & McMullen, 1983 for a review).

Fisher (1983) attributed such an oversight to the oversimplified construal of a helper-recipient relationship. That is, all helping relationships were presumed to function *uniformly* and *mechanistically* insofar as the helpers deliver the needed resources, with little to none regards to recipients' reactions. As such, researchers were prone to take a *helper-centred* perspective when analysing a helper-recipient relationship (Fisher, 1983). In the following, I discuss three *helper-centred* models that theorised the different roles of a helper's *positive* and *negative* emotions will play in inciting prosociality.

1.2.1. Empathy-Altruism Hypothesis (i.e. EAH hereafter)

Batson and associates (e.g. Batson et al., 1989; Batson, Bolen, Cross, & Neuringerbenefiel, 1986; Batson & Weeks, 1996) theorised that *empathic concern* is the source of altruistic motivation. According to EAH, after witnessing the suffering of a needy person an empathic spectator would identify with the former and also vicariously experience that suffering (Batson, 1991). Consequently, to relieve that sufferer of his/her misery—via helping—will become the top priority for the prospective helper because he/she feels for the former (Batson, 1990, 2010; Batson, Eklund, Chermok, Hoyt, & Ortiz, 2007).

Nonetheless, it is important to note that this empathy-driven helping should have no association with self-gains such as avoidance of disapproval or

desire for social acclaim (Batson et al., 1988), considering that the helper's primary concern is to benefit the person toward whom empathy was felt (Batson et al., 1986; Batson et al., 1989). Indeed, the EAH epitomises what altruism comprises by definition: an *unselfish* or *unconditional* concern for others (Fehr & Schmidt, 2006), and an intention to enhance others' welfare without expecting any extrinsic rewards (Bar-Tal, 1986). The EAH had received extensive empirical support (e.g. Batson et al., 1988; Batson & Weeks, 1996; Fultz, Batson, Fortenbach, McCarthy, & Varney, 1986; Schroeder, Dovidio, Sibicky, Matthews, & Allen, 1988) despite the presence of a rival model—i.e. *Negative State Relief Model* (i.e. NSRM hereafter) which argued that negative mood management is what truly underlies helping (Cialdini et al., 1987; Cialdini & Fultz, 1990). Section 1.2.2 offers an overview of the NRSM.

1.2.2. Negative State Relief Model (NSRM)

Despite acknowledging the importance of perspective-taking in inspiring helping, Cialdini and colleagues (e.g. Cialdini, Brown, Lewis, Luce, & Neuberg, 1997; Cialdini, Darby, & Vincent, 1973;) asserted that there is no such thing as a 'selfless' helping. That is, people, having witnessed the victim's suffering, are motivated to help by an *egoistic objective* to eradicate their own negative state. This *Mood Management* perspective of helping has been, for instance, evidenced in Cialdini et al.'s (1973) observed decline in helping among people who were given an alternative means to salvage their mood.

Cialdini and colleagues (Cialdini et al. 1997; Goldstein & Cialdini, 2007; Maner et al. 2002) further questioned the EAH by stating that helping someone with whom one has a sense of *merged identity* hardly qualifies a 'selfless' demeanour. A helper who empathises with recipient prospective

recipient is expected to have cultivated a sense of 'oneness' between the pair (Maner et al., 2002). Therefore, it is illogical to claim whatever attempts to help as genuinely *selfless* as the helper considers him/herself and the victim as a *single entity*. In particular, Maner et al. (2002) revealed that once non-altruistic factors such as the helper's *negative state* and sense of *merged identity* with the victim had been controlled for, *empathic concern* was no longer correlated with helping. Nonetheless, the paths of 1) perceived merged identity and helping, and 2) negative state and helping both remained statistically significant, thereby further endorsing the NRSM and meanwhile dismissing the EAH.

1.2.3. Impure Altruism and Warm-Glow Giving—the Integrative Model

While EAH and NSRM constitute the polar opposites in the 'Egoism-Altruism debate' (Batson, Ahmad, Lisner, & Tsang, 2002; Batson & Shaw, 1991), Andreoni's (1989, 1990, 1993) concept of *Impure Altruism* appeared to fall somewhere between these two extremes. An *impure altruist* (i.e. IA hereafter), according to Andreoni (1989, 1990), derives a joy of giving (Abel & Warshawsky, 1987; Ribar & Wilhelm, 2002) from 1) the fact that his/her altruistic act had contributed to the betterment of others, and crucially, 2) the sheer act of giving. Thus, apart from the benevolent concerns for the needy, *impure altruists* acquire a sense of 'warm-glow' (i.e. an internal satisfaction) (Beccheti & Degli Antoni, 2010; Harbaugh, 1998) via their prosocial acts.

By contrast, a *pure altruist* (i.e. PA hereafter) helps purely out of their concern for others' welfare (Becker, 1974; Rose-Ackerman, 1997). This is indeed consistent with what EAH theorises as the primary motivator of helping (See Section 1.2.1). As such, PAs should be apathetic to the *sources* of help as long as the needy are taken care of, as PAs do not help out of the 'warm-glow'

feeling. To verify whether *prosociality*, especially in the form of charitable donations, is accountable by *pure* or *impure* altruism researchers examined a phenomenon termed 'crowding-out' (e.g. Kingma, 1989; Payne 1998, 2001).

Crowding-out occurs when a donor reduces his/her contribution to a charity knowing that every dollar he/she donates will be matched with an *equivalent* withdrawal by an external agency (Abrams & Schmitz, 1984). Since PAs, by definition, should only be concerned about the *total* contribution, one should expect a *complete* crowding out whereby a dollar increase in external funding should induce exactly a dollar's drop in the *private* donation (Konow, 2010). On the contrary, an *incomplete* crowding-out can be anticipated from the *impure altruists* as their very act of generosity already brings them warm-glow (Andreoni, 1990; Ferguson & Flynn, 2016). Thus, *impure altruists* may not be that concerned about the efficacy of their donations unlike the *pure altruists*

Findings from both lab and field experiments (e.g. Khanna, Posnett, & Sandler, 1995; Manzoor & Straub, 2005; Okten & Weisbrod, 2000) overall favoured the IA, which constitutes an integrated perspective of *egoism* and *altruism*, in explaining charitable giving. In fact, the notion of 'Warm-Glow' giving has gathered extensive support from various economic experiments (e.g. Carpenter, Connolly, & Myers, 2008; Palfrey & Prisbrey, 1997), particularly the *Charity Dictator Game* in which a dictator was deprived of any altruistic motivation to make any donations (See Crumpler & Grossman, 2008).

All in all, the EAH, NSRM and the IA models had acknowledged the role of *positive* (i.e. empathy for EAH; 'warm-glow' for IA) and *negative* emotions (i.e. negative state relief in NSRM) in cultivating prosociality, despite the fact that each model had theorised a unique mechanism through which a

specific emotion will foster prosociality. However, as mentioned previously all these models had adopted a *helper-centred* perspective in examining prosociality (Fisher, 1983). As a result, little attention was placed on 1) how a recipient may react emotionally to that help, for instance, a wounded self-esteem (Nadler, Peri, & Chemerinski, 1985; Nadler & Mayseless, 1983;) or *gratitude* (Lazarus & Lazarus, 1996; McCullough et al., 2001); and 2) how such a reaction (e.g. to demonise the helper (Hatfield & Sprecher, 1983) or to gratefully reciprocate (Ma, Tunney, & Ferguson, 2014)) will contribute to any future, ongoing helper-recipient interactions (Fisher, 1983; Fisher et al., 1983).

Therefore, one principal objective of the current thesis is to examine the effect of *recipients'* positive and negative emotions on their cooperative or sanctioning behaviours. Particularly, present thesis primarily focuses on how *gratitude*, i.e. a typical positive *benefit-triggered* affect (Lambert, Graham, & Fincham, 2009; Lambert & Fincham, 2011), would facilitate the recipient's reciprocal or sanctioning decision in a behavioural economics context. Meanwhile, the thesis also sheds light on how *indebtedness*, a *negatively*-valenced affective *state* triggered via a favour acceptance (Goei & Boster, 2005; Peebles, 2010), contributes to people's economic decisions. The current thesis entails also the scrutiny over how *dispositional gratitude* (Adler & Fagley, 2005; McCullough, Emmons, & Tsang, 2002; Watkins, Woodward, Stone, & Kolts, 2003) and *indebtedness* (Mathews & Shook, 2013; Solom, Watkins, McCurrach, & Scheibe, 2016; Watkins, 2014) might shape people's prosociality or sanctioning. Below (i.e. Section 1.3) I review the evidence on the associations of *gratitude* and *indebtedness* to prosociality or sanctioning.

1.3. Gratitude, Indebtedness and Prosociality

1.3.1. What Gratitude Constitutes?

Roman philosopher Marcus Cicero (106 BC – 43 BC) once proclaimed that 'Gratitude is not only the greatest of virtues, but the parent of all the others.' Clearly, *gratitude* has always been held in high regard (Fitzgerald, 1998) whereby the expression of such, when it is due, draws approval whereas a failure to acknowledge (or appreciate) others' benevolence is egregious (Amato, 1982; Emmons & Shelton, 2002; McCullough et al., 2001).

In addition to an *emotional* acknowledgement of someone else's generosity, gratitude is also conceptualised as a *trait* that encapsulates a 'life orientation toward noticing and appreciating the positives in life (pp.891)' (Wood, Froh, & Geraghty, 2010). McCullough et al. (2002) contended that such a worldview is what motivates a *grateful* person to not take any positives for granted and therefore recognises other people's contribution to his/her well-being. Consequently, these people are more capable of reaping the benefits, e.g. subjective happiness, from their positive life encounters (Weiner, 1985). This resonates with the notion whereby gratitude constitutes an essential part to a quality life (Peterson & Seligman, 2004; Watkins et al., 2003; Watkins, 2007).

1.3.2. *Emotional* Gratitude and Prosociality

In addition to the betterment of one's emotional well-being, McCullough and colleagues (McCullough et al., 2001; McCullough, Kimeldorf, & Cohen, 2008) argued that gratitude as an *emotion* is integral to one's social functioning. They theorised that *gratitude*, thanks to its three inbuilt *moral* functions, promotes and perpetuates prosocial interactions between a giver and a recipient (Watkins, 2007). The below delves into these three functions and how each of them relates to the *facilitation* and *maintenance* of prosociality.

1.3.2.1. Gratitude the *Moral Barometer*

First, the *Moral Barometer* hypothesis argues that gratitude would function as a *benefit detector* (McCullough et al., 2001; McCullough et al., 2008) as it informs a recipient if someone had benefited him/her, and crucially, whether that benefit was *worth feeling gratitude* for. People only experience gratitude when they sense that someone was being benevolent to them and that generosity should, in no circumstances, originate from any selfish or deceitful intent on behalf of the benefactors (Heider, 1958; Simmons, 1979).

As such, the experience of gratitude entails *social cognition* (See Steindl-Rast, 2004; Wood, Maltby, Stewart, Linley, & Joseph, 2008). In particular, McCullough et al. (2001) outlined four social-cognitive factors recipients typically attend to when evaluating whether a benefit is worthy of their gratitude. They included, 1) whether that benefit was *valuable*; 2) whether the givers incurred a *material cost* amid the conferment of that benefit; 3) whether that benefit was delivered *intentionally* (or by *chance*); and 4) whether that benefit was rendered because of the benefactor's *role-based* obligations.

1.3.2.2. Gratitude as a *Moral Motive*

Meanwhile, the *moral motive* hypothesis stipulates that *gratitude* also serves to motivate recipients to repay their benefactors via '*contributing* to their welfare in the future (pp.252)' and to also *refrain from* acting '*destructively* toward the benefactor (pp.252)' (McCullough et al., 2001). That is, in addition to celebrating an *undeserved* although *welcomed* kindness (Steindl-Rast, 2004), *gratitude* also prompts a beneficiary to return this kindness (Komter, 2004).

Nonetheless, such a return of generosity should by not be likened to as an act of debt settlement. Gratitude is characterised by (or at least closely linked

to) a feeling of *warmth* (Wood, Joseph, & Maltby, 2008), *compassion* (Horberg, Oveis, & Keltner, 2011), and key among those, a desire for *social proximity* to one's benefactor (Mayer, Salovey, Gomberg-Kaufman, & Blainey, 1991). Thus, a grateful recipient's endeavour to reciprocate should be deemed more of a stepping stone to establishing social closeness with the helper (Fitzgerald, 1998). This gratitude-driven reciprocity perspective resonates with the *Impure Altruism* model (See Andreoni, 1989, 1990) in that a prosocial gesture (i.e. reciprocation) is driven by both *altruistic* (i.e. to contribute to benefactor's welfare) and *egoistic* (i.e. to acquire social proximity to helper) motives.

1.3.2.3. Gratitude as a *Moral Reinforcer*

McCullough et al. (2001) stated that gratitude functions also as a *moral reinforcer* as its expression symbolises recognition for the helpers' initial kindness. Such a recognition will encourage the same helper to continue to be generous in the future (Grant & Gino, 2010) and hence strengthens the existing *helper-recipient* rapport (Fitzgerald, 1998). Indeed, the robust association between the frequency of expressing gratitude and the relationship quality in intimate relationships, which is well-documented in the literature (e.g. Algoe, Gable, & Maisel, 2010; Chang, Li, Teng, Berki, & Chen, 2013; Gordon, Impett, Kogan, Oveis, & Keltner, 2012; Kubacka, Finkenauer, Rusbult, & Keijser, 2011), best attests to the hypothesis of gratitude as a moral reinforcer.

To conclude, the above three *moral functions* of gratitude outlined the mechanism through which a recipient develops an urge to reciprocate rather than to 'bite the hand that feeds'. The above hypotheses also underlined the conceptual discrepancy between the *gratitude-motivated* reciprocity with the act of debt settlement from a *rapport-building* perspective. In other words,

gratitude inspires a 'want to (pp. 111)' form of reciprocity (Schaumberg & Flynn, 2009) and in so doing prompts a recipient to establish a social affiliation with the benefactor (Bartlett, Condon, Cruz, Baumann & Desteno, 2012).

1.3.3. *Dispositional* Gratitude and its Indirect Link to Prosociality

While the above underscored how *gratitude* as a transitory *state* could motivate and sustain prosociality, the Social-Cognitive Model of Gratitude (i.e. SCMG hereafter; Wood et al., 2008) outlined how gratitude as a *trait* may indirectly prompt prosociality via a particular chain of *attributional* and *emotional* processing. In particular, the SCMG argued that what makes a *dispositionally* grateful recipient more capable of *experiencing* gratitude—following a benefit receipt—is the manner he/she appraises the benefit.

Wood et al. (2008) theorised that a *grateful* recipient is more likely to interpret the benefit he/she received as i) propelled by the giver's *benevolence*; ii) *costly* to the giver; and ii) valuable to oneself. Such an overall favourable appraisal thus elicits more intense experience of *gratitude* (Wood et al., 2008), with this heightened feeling of *gratitude* being well-documented to predict *reciprocity* or *prosociality* from the recipient (Tsang, 2007, Tsang et al., 2012). Overall, the SCMG hinted a possible *indirect* role of *dispositional* gratitude in fostering positive reciprocity by somewhat 'distorting' the perception of the benefice and the ensuing feeling of gratefulness toward one's benefactor.

1.3.4. Gratitude and Prosociality in the Behavioural Economics Literature

Interestingly, despite the strong theoretical link between *gratitude* and *cooperation* (or sanctioning) *gratitude* still remains an under-researched domain in the experimental economics literature (Leung, 2011). Nonetheless, there is no shortage of evidence in the BE literature for the link between perceived

kindness (and fairness) and decisions to *sanction* (or *reciprocate*) (e.g. Camerer, 2003; Charness & Rabin, 2002; Güney & Newell, 2013). Indeed, the link between *intent* attribution (i.e. kindness and fairness) and *gratitude* is also well documented in the Social Psychology literature (Emmons, 2004; Pruyser, 1976). Taken together, it is conceivable that the kindness-driven reciprocity may be attributable to gratitude. However, to date there is only one *published* article which examined how the direct manipulation of *state gratitude* will contribute to cooperative economic decision-making (i.e. Desteno et al., 2010). As such, one primary contribution of the present thesis entails the combination of Experimental Economics and Psychometrics in the analyses of the role of gratitude in nurturing prosociality. The below two sub-sections (i.e. Sub-sections 1.3.4.1 and 1.3.4.2.) entail a brief review of Desteno et al's (2010) work and a discussion of how that paper relates to the current investigation.

1.3.4.1. Brief Review of Desteno et al.'s (2010) Paper

Desteno et al. (2010) employed the 'Give-Some Dilemma Game (i.e. GSDG hereafter)' in which a player decides whether to be *selfish* (i.e. to retain all the endowment) or *trustful* (i.e. to entrust their endowment to a co-player and runs the risk of being taken advantage of) (Nelissen, Dijker, & deVries, 2007; Parise, Kiesler, Sproull, & Waters, 1999). Prior to the GSDG, a portion of participants (i.e. the '*Gratitude* condition') were induced to feel *gratitude* toward the confederates following a benefit receipt (See Bartlett & Desteno, 2006). Desteno et al. (2010) also manipulated the partner identities in the GSDG: some participants were partnered with strangers (i.e. '*Stranger*' condition) while others played alongside the confederates who helped them during the *gratitude* induction phase (i.e. '*Benefactor*' condition).

Gratitude was overall demonstrated to predict trustfulness in the GSDG. Interestingly, Desteno et al. (2010) showed no evidence that whether a participant played with his/her *benefactor* or a *stranger* had tampered with their prosociality (or lack thereof) in the GSDG. In other words, *gratitude-inspired* reciprocities are not confined to *direct* reciprocation (Nowak & Roch, 2007; Nowak & Sigmund, 2005), as it also entails a 'pay-it-forward' variant via which a recipient passes on the benevolence of the helper to a *third party* (Chang, Lin, & Chen, 2012; Gray, Ward, & Norton, 2014; Schaumberg & Flynn, 2009).

Desteno et al's (2010) findings resonated with Nowak and Roch's (2007) argument in relation to the critical role of *gratitude* in inciting (and also perpetuating) both *direct* and *indirect* reciprocations. The data also gave credence to Fredrickson's (2004a) *Broaden-and-Build* perspective on how gratitude nurtures indirect and direct reciprocities. In particular, Fredrickson (2004b) theorised that *gratitude* could add to the *creativity* of a recipient thanks to its 'perspective-broadening' property as a typical *positive affect*. This *increased* creativity thus enables that recipient to not religiously stick with the traditional 'tit-for-tat' restitution of the helpers' kindness, but to adopt an alternative approach such as directing such a generosity to an 'innocent' other.

1.3.4.2. What was lacking in Desteno et al. (2010)?

While Desteno et al. (2010) had pioneered the scrutiny of *gratitude* in the realm of economic exchanges; there are certain glaring methodological issues which may have undermined the validity in their analyses. Examining the method through which Desteno et al. (2010) induced gratitude, that is, to subject the participants to a bogus computer failure and then the confederates came to their 'rescue', it is conceivable that these 'victims' may feel *indebted*

apart from *grateful* toward their 'saviours'. Given the supposedly 'breakdown' took place when the participants were working on their cover task, the participants may have held themselves responsible for that mishap and therefore felt guilty for having bothered their helpers for starters (Naito & Sakata, 2010).

Indeed, Greenberg and Shapiro (1971) argued that *indebtedness* is characterised by a sentiment of *guilt* which originates the receipt of an undeserved kindness. Therefore, as highlighted in the Negative-State Relief Model (Cialdini et al., 1973; Cialdini et al., 1987) these *indebted* participants (in Desteno et al (2010)) may behave prosocially later on with the aim to salvage their moods. However, neither had Desteno et al. (2010) even explicitly mentioned nor implied, let alone addressed this alternative explanation throughout their paper. As a result, I believe this should leave the door open for the present thesis to a) explore how *indebtedness* could affect people's cooperation or sanctioning in an economic game; and to b) discern the mechanisms through which *indebtedness* and *gratitude* prompt sanctioning or reciprocities. Below (i.e. Sub-sections 1.3.5 to 1.3.7) I review *indebtedness* as a *psychological* concept and how it relates to the scope of the current thesis.

1.3.5. What Indebtedness Constitutes?

German philosopher Friedrich Nietzsche once described that 'Great indebtedness does not make men grateful, but vengeful; and if a little charity is not forgotten, it turns into a gnawing worm'. Indeed, indebtedness represents an unpleasant *mental* and *physiological* state as triggered by an urge to repay a benefactor (Greenberg, 1980; Greenberg & Frisch, 1972). Unlike gratitude, *state* indebtedness is often accompanied by negative feelings such as perceived *inferiority* to the helper (Homans, 1961) and *inequity* (Adams, 1963, 1965).

Meanwhile, *indebtedness* as a *disposition*, that is, the 'creditor personality' (Greenberg & Westcott, 1983), is characterised by, a) a religious adherence to the reciprocity principles (Gouldner, 1960); b) intense discomfort being a receiver in an exchange; c) an urge to immediately settle any outstanding debts; and interestingly, d) a tendency to keep his/her beneficiaries indebted via shunning their endeavours to repay (Greenberg & Westcott, 1983).

1.3.6. Indebtedness, Reciprocity and Sanctioning

1.3.6.1. *State* Indebtedness, Reciprocity and Sanctioning

Schaumberg and Flynn (2009) noted that despite the differences in motivation, *indebtedness*, just like *gratitude*, also prompts a return of benefactor's kindness. However, this motivation to make restitutions stems from the urge to escape from the unpleasant feeling of owing (Gergen, Ellsworth, Maslach, & Seipel, 1975) or to eradicate the perceived inequity (i.e. being 'over-benefited', See Hatfield & Sprecher, 1983). Greenberg, Block and Silverman (1971) even added that an *indebted* person would normally feel apprehension at the prospects of a lack of avenue to make restitutions.

Therefore, contrary to what *Empathy-Altruism* Hypothesis (Batson et al., 1981) and the *Impure Altruism* model (Andreoni, 1989, 1990) suggest, indebtedness-inspired reciprocity may not stem from any altruistic motives. In fact, such a viewpoint has been recapitulated by Greenberg and Westcott (1983) who argued that to assure the window of reciprocation is open an *indebted* person may go as far to sabotage a third party's efforts to help the initial helper. Thus, unlike *gratitude*-inspired reciprocity, *indebtedness*-inspired reciprocity leans more towards the *Negative State Relief Model* (Cialdini et al., 1987) whereby a person engages in prosociality for the sole purpose of *mood*

management (i.e. to escape from the psychological shackle of being in debt). All in all, *indebtedness* as an emotion should, resembling gratitude, also possess a capacity to incite reciprocity in an economic exchange, although it is well documented that the two constructs will operate on two distinct mechanisms in necessitating reciprocity (Schaumberg & Flynn, 2009; Watkins et al., 2006).

Nevertheless, Greenberg (1980) argued that other than reciprocation *Cognitive Restructuring* also constitutes a 'major mode of indebtedness reduction (pp.95)'. *Cognitive Restructuring* (i.e. CR) entails a re-assessment and hence a restructuring of the cognition that dictates the initial experience of *indebtedness* (Greenberg & Westcott, 1983). Specifically, a recipient would re-consider factors such as 1) whether the helper was that benevolent as originally interpreted, or 2) whether the benefit was intentional or just a fluke. These restructuring efforts will downplay a recipient's feeling of obligation and more importantly, his/her perceived necessity to reciprocate (Greenberg & Westcott, 1983). Nonetheless, when applying *CR* the recipient also runs the risk of distorting the reality (Folkman & Lazarus, 1991), and thereby earns disapproval from others. This is especially the case when there are witnesses, particularly the benefactor, to the exchange episode (Greenberg & Westcott, 1983).

As such, a recipient would only prefer *Cognitive Restructuring* when certain parameters are met (Greenberg, 1980). They include, first, the lack of *witnesses* to the initial helping episode(s), second, a perceived lack of opportunity to reciprocate; and finally, recipients expecting minimal future exchanges with the benefactors (or the witnesses). Indeed, all these parameters constitute a typical economic game context. That is, a *one-shot, double-blind* game which disincentivises reputation building (Güney & Newell, 2013) while

preserves players' anonymity (e.g. Eckel & Grossman, 1996a). Building upon this logic, it should be no surprise that players in such economic games may resort to *Cognitive Restructuring* to mitigate their experienced indebtedness.

1.3.6.2. *Dispositional* Indebtedness and Economic Decision-making

Indebtedness as a *disposition* could also influence people's sanctioning or reciprocal decisions in an economic exchange. As discussed above, a *dispositionally* indebted individual has 'low threshold' toward the concepts of owing and being owed, coupled with a distaste to be on the recipient side of an exchange (Greenberg & Westcott, 1983). These attributes should account for a 'trait creditor's' reluctance to accept favours, and their urge to immediately *reciprocate* following their futile endeavours to 'say no' to the favours. Meanwhile, another hallmark of a trait creditor entails the preference to be *under-benefited* in an interaction (Hatfield & Sprecher, 1983). *Dispositionally* indebted individuals fancy putting others in their debt and would even strive to sabotage their debtors' efforts to repay (Greenberg & Westcott, 1983).

Taken together, one should expect the following observations from a *dispositionally* indebted participant in an economic game. They include, a) benefit rejections, b) reciprocity (or a lack of defaulting), and c) generous giving (should the participant assumes the role of a prospective helper).

1.3.7. Indebtedness in the Behavioural Economics Literature

Despite the above inferences and theoretical evidence pertaining to the link between *indebtedness* and *reciprocity*, indebtedness is still an unexplored domain in the behavioural economics literature. To date, I am yet to locate any published, empirical study on how *indebtedness* contributes to people's sanctioning or reciprocal decisions in the realm of economic exchanges.

Meanwhile, there is a strong evidence base for the role of negative emotion in inciting retaliations in economic games. For instance, Xiao and Houser (2005) revealed, via the ultimatum game (i.e. UGs hereafter), how recipients may exercise costly retaliation to vent out their fury sparked by their unfair treatment. Yamagishi et al. (2009) further theorised that the refusal to cooperate, after being subjected to an unfair treatment in the UGs (i.e. unequal split in endowment), symbolises an emotional reaction to other's exploitations.

As discussed in Section 1.3.6.1, an *indebted* recipient may resort to *Cognitive Restructuring* to get over their feeling of indebtedness (Greenberg, 1980). It is, therefore, theoretically intriguing to examine whether these restructuring efforts would not only downplay a recipient's desire to reciprocate, but to also lead to sanctioning when opportunity beckons. For instance, an *indebted* recipient, following a secondary assessment of the whole episode of benefit conferment, may infer more cynicism and ill intent from the helper's initial ingratiating gesture (e.g. 'He/she is helping me because he/she has a 'benevolent' image to maintain. God knows what he/she would have done otherwise had there been no one else watching!'). This perceived cynical intent may well induce sanctions or retaliations (Falk & Fischbacher, 2006). Taken together, via the current thesis I intend to also examine whether this Cognitive Restructuring (Greenberg, 1980; Greenberg & Westcott, 1983) route does encapsulate how the construct *indebtedness* will relate to reciprocity or sanctioning in economic exchanges—which remains a gap in the literature.

1.4. Basic Predictions or Hypotheses

First, I hypothesised that *gratitude* should predict more *cooperation* (in the form of *positive reciprocity*) and less sanctioning in the series of economic

games. This prediction indeed echoes the *Moral Motive* functional hypothesis of *gratitude* (McCullough et al, 2001) as discussed above (i.e. Section 1.3.2.2). Second, with reference to the Social Cognitive Model of Gratitude (Wood et al., 2008) I hypothesise an *indirect* effect of *dispositional* gratitude on one's sanctioning or reciprocal decisions. Specifically, I predict the path between *trait* gratitude and reciprocation to be *serially* mediated by 1) benefit appraisal (e.g. perceived value in the benefit and helpers' intent etc.) and 2) *state* gratitude.

While indebtedness could invoke reciprocation out of a drive-reduction motive (Greenberg & Shapiro, 1971), it may also prompt a recipient to re-assess the whole episode of benefit receipt (Greenberg & Westcott, 1983). Greenberg (1980) argued that such a re-assessment may result in denigration of the benefactors which ultimately prompts sanctioning. Since the three economic games (details to be discussed in Chapters 3 to 5) at present all entail the 'ingredients' for *Cognitive Restructuring* (Greenberg & Westcott, 1983), namely, a) a double-blind design, b) a lack of *repeated* interactions; and c) the lack of witnesses to the whole sequence of benefit exchanges, there is no reason to not expect individuals to favour re-construing their entire episode of being benefited over *reciprocation* as to get over their emotional indebtedness.

Additionally, I predicted *dispositional* indebtedness to prompt benefit rejections (if possible), as dispositionally indebted people typically loathe being in a position to receive (Greenberg & Westcott, 1983). I anticipate *dispositional* indebtedness to also prompt benevolent or even 'over-generous' economic decisions, such as surrendering resources *unconditionally* to an unacquainted co-player (Gergen et al., 1975), since a 'trait creditor' will relish the feeling of someone in their debt (Mathews & Green, 2010; Mathews & Shook, 2013).

1.5. An Overview of the Empirical Chapters (i.e. Chapters 2 to 5)

In this thesis an aggregate of three economic games, which involved 390 participants, were conducted to primarily examine how *gratitude* relate to *cooperation* or *sanctioning* in an economic exchange context. Various sets of parameters (e.g. cost of helping, repeated or iterated design etc.) were manipulated throughout the three experiments. Additionally, a meta-analysis was conducted to examine the overall strength of the *gratitude-prosociality* link. The meta-analysis and the three experimental studies are presented in four separate but related empirical chapters (i.e. Chapters 2 to 5). The following sub-sections (i.e. 1.5.1 to 1.5.4) present an overview of each empirical chapter.

1.5.1. Chapter 2: The Meta-analysis on Gratitude and Prosociality

Chapter 2 presents the findings of the meta-analytic review. Via meta-analysing over a hundred studies—conducted in the past three decades—I intended to achieve two objectives. First, I aimed to provide an estimate on how strong the *gratitude-prosociality* association is based on the existing empirical studies. Since the overarching aim of the current thesis concerns the analysis over how gratitude relates to prosociality in a specific context, it is, therefore, crucial to ascertain that there is at least a reliable association the two constructs.

Second, I scrutinised what *methodological* and/or *theoretical* moderators may have moderated the overall effect estimates. This moderator analysis would examine study features such as the type of prosociality under scrutiny (e.g. *reciprocal* or *non-reciprocal* measures, *actual* behaviours or *intentions* etc.), the method via which *gratitude* was induced. This moderator analysis should have both *theoretical* (e.g. how the gratitude is different from other positive affects in its link with prosociality) and *practical* implications

(e.g. what aspects of the current *gratitude intervention* regime (See Wood et al. (2010) for a review) the practitioners to modify as to boost its much doubted efficacy in enhancing well-being (Davis et al., 2016; Wood et al., 2010).

1.5.2. Chapter 3: Experiment 1

The meta-analysis revealed that *gratitude* is more strongly linked to *prosociality* among studies which measured i) *direct* reciprocal outcomes and ii) gratitude as a *state*. Hence, via Experiment 1 (which is presented in Chapter 3) I examined the *state* gratitude- *direct* reciprocity in a one-off economic context. In so doing I conducted a one-shot, variant of trust game played by 61 dyads.

Experiment 1 entails as well the scrutiny of how the injunctive norm will contribute to the gratitude-reciprocity link. Specifically, I examined how *normative fairness* (Elster, 2006), which is well-documented to affect intent attribution (See Bicchieri, 2006), would ultimately contribute to a recipient's experience of *gratitude* and his/her desire (and actual decisions) to reciprocate.

An injunctive fairness norm delineates what an average fair person ought to behave in a given context (Kazemi et al., 2016; Paddock, 2005). The injunctive norm under scrutiny in Experiment 1 was—'people should help for free when that help is easy to implement, and when the helpers' potency as a prospective helper was *assigned* but not *earned* (See Frey & Bohnet, 1995).'

Participants were subjected to a dyadic game in which all of them had to roll a die should they intended to win additional money. Each dyad would comprise a loser and a winner. The losers (N=61) —who were later on the prospective *recipients*, were led to believe that the winners —who were later on the potential *helpers*, had won the game by better fortune. On the other hand,

the ‘winners’ (N= 61) also knew their partners were losing—and therefore missed out the bonus money—thanks to poor fortune. The ‘losers’ thus all warranted a ‘donation’ (i.e. a transfer of money-equivalent ‘bonus points’) from their victorious partners to be eligible to earn the bonus money.

Since the winners here were practically ‘assigned’ (via chances) to a victorious position instead of having earned that (Frey & Bohnet, 1995; Hoffman & Spitzer, 1985; Hoffman et al., 1994), the injunctive norm would demand an average fair winner (i.e. P1s hereafter) to help a struggling partner (i.e. P2s hereafter) by making a ‘free’ donation (i.e. to share the proceeds with the latter equally, See Elster, 2006). All P1s first decided whether to help, and should they did they were then inquired whether they would demand some repayment. Thus, P1s decided whether to help *fairly* (i.e. no repayment needed) or *unfairly* (i.e. repayment-bound offers) from a normative standpoint.

Meanwhile, P2s reserved the rights to accept or decline their offers. Crucially, P2s were kept unaware of the list of ‘helping options’ available the P1s. That is, P2s had no idea their P1 partners could decide whether to help with or without the imposition of a repayment clause. As such, P2s here were put in a position where they could only infer the fairness of their help (if they were given any) by whether that help was *conditional* or *unconditional*.

I examined whether an injunctively *unfair* offer (i.e. the conditional offers) would lead to a less desirable giver intent attribution, thus lead to reduced *gratitude* (and greater *indebtedness*) and ultimately a lower desire to reciprocate. Additionally, these recipients were given an avenue to sanction their helpers by under-repaying. P2s who accepted their conditional offers, after being reminded of their obligated repayment, were inquired how much they

would repay (i.e. to repay *fully*, *nothing*, or *partially*). Only P2s who *accepted* the *conditional* offers were aware of this 'defaulting option'. This set-up should enable the author to use this de facto 'voluntary' repayment as a behavioural indicator of how the acceptors a) valued their unfair offers and accordingly, b) responded to their partners' belittlement with normative fairness.

In summary, in Experiment 1 I created a context in which people were bound to rely on the injunctive norm to judge whether they had been fairly treated. In so doing I examined how this evaluation would foster a favourable (negative) interpretation of the donor's intent, and consequently contributed to an elevated *gratitude* (*indebtedness*) by the recipient. I also explored whether this chain of attributions would constitute a similar effect on a recipient's state *indebtedness*, and subsequently tampered with ones' *reciprocal* or *sanctioning* decisions. Importantly, Experiment 1 permits the analyses of whether people would resort to normative fairness to guide their economic decisions; and if yes, how such a fairness inference will affect the *gratitude-prosociality* link.

1.5.3. Chapter 4: Experiment 2

Chapter 4 presents the second laboratory study which was a one-shot game via which I attempted to address an interesting phenomenon—i.e. *Cheap-riding*. *Cheap-riding* is conceptualised in the wider literature as a member's attempts to piggyback on the high contribution of his/her groupmates by giving as little as possible (Asch, Gigliotti, & Polito, 1993; Thompson, 1987).

Analysing the repayment data from Experiment 1, nearly 40% (i.e.38%; N=8) of recipients (of *conditional* offers) had taken advantage of their defaulting options by either under-repaying *fully* or *partially*. Interestingly, scrutiny of these defaulters' responses only revealed one instance of zero back-

transfer (i.e. free-riding), while the rest of the defectors (N=7) overall still repaid 58% of their expected repayment. I interpret such a partial repayment as cheap-riding as it evidently involves the exploitation of others' giving amidst own non-zero contributions (Bchir & Willinger, 2013; Cornes & Sandler, 1984).

Despite the existing coverage in the wider literature (e.g. Krishnamurthy, 2000; Olson & Perl, 2005; Scherer, 1993) it remains unexamined in regards the psychological correlates of *cheap-riding*. Specifically, to date there is still an absence of empirical assessment of the role of *emotions* in inciting (and potentially sustaining) or restraining cheap-riding. While speculations had been made—e.g., giving 'just deserts' (Darley, 2001; Santos & Rivera, 2015; Wenzel & Thielmann, 2006)—in Chapter 3 in relation to the possible psychological correlates on why people may cheap-ride, none were empirically examined in Experiment 1. Thus to date there is still a gap in the literature on the role of emotion in cheap-riding. As a result, in order to fill this gap Experiment 2 was set out to explore the *psychological* (e.g. state gratitude, indebtedness, just deserts, or even Experienced Warm-glow (Ferguson & Flynn, 2016)) and *situational* (e.g. the conditionality of offers) correlates of cheap-riding.

Furthermore, small proportion of recipients i) being 'eligible' to (i.e. 21/ 61= 34%; $N = 21$), and ii) subsequently deciding to (i.e. 8/61= 13.1%; $N = 8$), under-repay (cheap- or free-ride) their *conditional* offers in Experiment 1 would no doubt warrant a relatively *less under-sampled* (N= 135) and more *focused* follow-up investigation in Experiment 2. Hence, via a slightly modified game I aimed to examine i) the 'baseline' response (i.e. to *fully* repay

('cooperate'), *utterly not* repay ('free-ride'), or just *partially* repay ('cheap-ride') to a 'non-free' *conditional* offer (Ma et al., 2014), ii) whether the offer-triggered emotions such as *gratitude* or *indebtedness* would be predictive of cheap-riding, and finally, iii) the motives (e.g. warm-glow, just deserts) behind recipients' repayment—which should allow the disambiguation between the psychological profiles of the *cheap-riders, free-riders* and that of *cooperators* .

In Experiment 2 each participant played in pairs with an anonymous, in reality *fictitious*, 'partner'. Unlike Experiment 1, all participants (N =135) were pre-programmed to 'lose out' on the dice-rolling and were accordingly entitled to zero bonus. Participants then received an offer from their 'fortunate', victorious partners. All these offers were conditional, as recipients learned that upon acceptance they all undertook an obligation to repay. Participants, depending on their assigned conditions, would be due to receive a) an 'Interest-free' (N = 32), or b) a '12.5%-Interest' (N = 33), or c) '25%-Interest (N = 34)', or d) a '50%-Interest (N= 36)' offer. Via this manipulation of the interest charges I subjected participants to varying extent of normative unfairness.

Similar to Experiment 1, the 'winners' should have no *moral legitimacy* to expect *any* repayment from their unlucky partners (Elster, 2006), as the former had not earned the property right to that victory (Fahr & Irlenbusch, 2000; Frey & Bohnet, 1995). Thus the very fact that the offer was rendered *conditional* (repayment-bound) should already represent normative *unfairness* (Ma et al., 2014). Participants were again blind to the range of offers they *could have* received. Acceptors (N = 87) were then allowed to decide whether to a) repay *in full* (i.e. 'cooperating', N = 59), b) to 'cheap-ride' via just giving back *partially* (N = 17); or c) to entirely not repay (i.e. 'free-riding', N = 11).

Experiment 2 was concluded by participants filling out surveys that examined their motives to repay (See Chapter 4 for the items in use) and *dispositional* gratitude (i.e. Gratitude-Questionnaire VI, McCullough et al., 2002).

1.5.4. Chapter 5: Experiment 3

Chapter 5 presents the third experimental study which was an *iterated* game. The key objective of Experiment 3 is to resolve a few unaddressed issues in Experiments 1 and 2, and in so doing I endeavoured to complement the robustness of the present investigation of *gratitude*-induced *prosociality*. Particularly, in Experiment 3 a few new design features—which are summarised below— were incorporated to help tackle certain unaddressed issues in Experiments 1 and 2 (which would be expanded upon in Chapter 5).

First, as opposed to Experiments 1 and 2, recipients in Experiment 3 were no longer naïve to the fact that they *could have* been treated more *generously* or *harshly* by their helpers. This was achieved by having the 133 participants played both roles as a prospective *helper* and *recipient* in the current game. Specifically, participants played as a potential *helper* in the first and the tenth trial, and the eight trials in between during which participants played as a *recipient* (i.e. the 'Recipient Trials') were sandwiched by these two 'Helper Trials'. Thus, the opening trial should notify the players of the choice sets of a helper prior to them playing as a recipient (i.e. Trials 2 to 9). Such a transparency of helpers' options should accordingly allow recipients to appraise their offer receipts not only based on what they *actually* received, but also on what they *could have* been given instead (Charness & Levine, 2007).

Another crucial revised feature of Experiment 3 entails recipients' awareness of the 'options' to under-repay prior to their acceptance (or rejection)

decisions. This new feature was brought in to tackle a possible flaw in Experiments 1 and 2 that might have arguably contributed to the surprisingly high rate of offer rejections (i.e. 36%) in Experiment 2. As previewed above (i.e. Sections 1.5.2 and 1.5.3), recipients in Experiments 1 and 2 only came to realise the feasibility of them under-repaying *after* they accepted their *conditional* offers. Therefore, it is not illogical to argue that acceptors in either Experiments 1 or 2 may have assumed and possibly agreed to the *binding* nature of the repayment contract upon indicating acceptances.

The corollary of this argument is that those who disapproved of the arrangement (i.e. being treated unfairly *normatively* speaking) may have jumped on the very first chance to quit the arrangement. Indeed, evidence in the Impunity Game (IG) literature had well evidenced the feasibility of people 'irrationally' rejecting a financially advantageous arrangement out of perceived infringement of social *fairness* (See Horita & Yamagishi, 2007; Ma et al., 2012; Yamagishi et al., 2012). As such, it might be problematic to refer to the repayment (or the lack thereof) in Experiment 2 as the behavioural index of how people 'valued' being unfairly treated, considering that those who were disgusted by the offer would have been filtered before even being 'eligible' to showcase their discontent. Therefore, via this modified feature I intended to rectify this potential methodological loophole, and in so doing added to the validity of the repayment metrics as the *behavioural* (alongside the existing *emotional*) index of reciprocal or sanctioning decisions in Experiment 3.

Meanwhile, the third key modification enables Experiment 3 to examine an interesting question: does the experience of being 'cheap-rode' prompt more fair plays from the victims? Throughout the present thesis I have drawn

parallels between the mechanisms of *cheap-riding* and that of *altruistic punishment* (Egas & Riedl, 2008; Nelissen & Zeelenberg, 2009). That is, by incurring a cost (despite not having to) a cheap-rider endeavours to warn his/her victim against treating others ungenerously in the future. Nonetheless, it remains to be seen whether cheap-riding does serve that function, and even if so, how well it 'emulates' altruistic punishment in preserving cooperation in the long-run (Barclay, 2006; Fowler, 2005; Jordan, McAuliffe, & Rand, 2015)

Therefore, in Experiment 3 I manipulated partners' repayment among players who gave *conditional* offers in Trial 1(i.e. the initial 'Helper Trial'). Participants were randomly assigned to one out of three conditions in which their *conditional* offers yielded different repayment. These conditions included, first, a 'Fairness' condition ($N = 44$) in which the 'partners' repaid *fully*; second, a 'Cheap-riding' condition ($N = 44$) in which they received part of their obligated repayment. Alternatively, a participant could end up in the 'Free-riding' condition ($N = 45$) in which he/she was subjected to a full-blown free-riding (i.e. no repayment at all) at the hands of his/her 'partner'. All in all, via this new feature I intended to assess the efficacy of cheap-riding in fostering future cooperative decisions in the later phase of the game (i.e. Trials 2 to 10).

Chapter 2

Does Gratitude Enhance Prosociality: A Meta-Analytic Review

A version of Chapter 2 has been re-submitted to the *Psychological Bulletin* for publication (manuscript number: BUL-2016-0614R, currently *under review*):

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2.0. Overview of Chapter 2

As previewed in Chapter 1, this chapter presents a meta-analysis on how *gratitude* relates to *prosociality*. Theoretical models (e.g. the Moral Functional Hypotheses of Gratitude (McCullough et al., 2001; McCullough et al., 2008); Social-Cognitive Model of Gratitude (Wood et al., 2008)) suggest that gratitude is linked to *increased* prosociality. To date, however, there is a lack of a comprehensive quantitative synthesis of results to support this claim, in spite of a host of *empirical* (e.g. Bartlett et al., 2012; Goei, Roberto, Meyer, & Carlyle, 2007; Halali et al., 2016) and *theoretical* evidence (e.g. Emmons, 2004; Frederickson, 2004a; McCullough & Tsang, 2004) pointing to this link.

Given the recurring theme of this thesis pertains to how *gratitude* nurtures *prosociality* (or cooperativeness) in the realm of economic exchanges; it is of paramount importance that I ensure these two constructs are reliably associated in the wider literature. From a *theoretical* standpoint, it is also crucial to examine whether the *gratitude-prosociality* link is moderated by whether the type of interaction concerned involves *reciprocity*. And if so, is *gratitude* more conducive to the *direct* 'tit-for-tat' (Boyd & Richerson, 1989; Brown, 1996; Milinski, 1987) or the *indirect* 'pay-it-forward' (Chang et al., 2012; Gray et al., 2014; Schaumberg & Flynn, 2009) kind of reciprocity?

Hence, via this meta-analysis I endeavoured to 1) examine the overall strength of the association between *gratitude* and *prosociality*, and to 2) identify the theoretical and methodological variables that moderate this link. Two-hundred and fifty-two effect sizes from 91 studies across 65 papers—(Total N = 18,342 participants)—were identified and included in the present review.

The findings revealed a statistically significant, and moderate positive correlation between gratitude and prosociality (r = 0.374). This association was significantly larger among studies that assessed *reciprocal* outcomes relative to *non-reciprocal* outcomes, and in particular among studies examining *direct*—compared to *indirect*—reciprocity. Studies that examined gratitude as an *affective state* reported significantly larger effect size studies assessing gratitude as a *disposition*. Further the results revealed that studies that examined *benefit-triggered* gratitude (in response to other's kindness) had a stronger effect that *generalized* gratitude that focuses on the appreciation of what is valued and cherished in life (See Lambert et al., 2009; Lambert & Fincham, 2011).

Overall, apart from the main effect estimates which practically confirms the existence of a reliable relationship between gratitude and prosociality, the present chapter also shed light upon a few methodological and theoretical attributes that could account for the inconsistencies in the effect sizes as reported from the range of sampled studies. Both *theoretical* and *practical* significance of the present findings will be described and discussed in detail.

2.1. Introduction

2.1.1. Gratitude Revisited

The Latin root of gratitude, 'gratia', translates to include the idea of being thankful. This, thankfulness, can be directed either at others for their help or at events (a beautiful day). Consistent with this gratitude is conceptualized in the scientific literature as either an *emotional* response to other's kindness (*benefit-triggered* gratitude), a *mood* referred to as *generalized gratitude* that focuses on the appreciation of what is valued and cherished in life (e.g., a beautiful day or good friends) (Lambert et al., 2009; McCullough et al., 2002),

or as a *disposition* reflecting a wider life orientation towards appreciating others and the world we live in (Adler & Fagley, 2005; Wood et al., 2010). Although, gratitude has been conceptualized in a variety of ways it has been frequently associated with a variety of social and personal benefits including improved *physical* and *mental health* (Lavelock et al., 2016; Ware, Kosinski, & Keller, 1996), general *well-being* (Froh et al., 2011;Wood, Joseph, & Linley, 2007), and *prosociality* (Emmons, 2004; Frederickson, 2004a; Steindl-Rast, 2004).

Although the literature linking gratitude to health and well-being has been reviewed (see Lavelock et al., 2016; Wood et al., 2010) there is no systematic review of the link with prosociality. A systematic review and synthesis of this link is important for two main reasons. First, gratitude is key to understanding mechanisms (e.g., reciprocity) that underlie the survival of altruism in the population (Nowak, 2006; Nowak & Sigmund, 2005). Thus, exploring the sensitivity of the gratitude-prosociality link to key theoretical distinction (e.g., direct vs. indirect reciprocity) will provide valuable theoretical insights. Second, given both the individual (better health) and societal (increased prosociality) benefits of gratitude, interventions are increasingly been designed to enhance gratitude (see Emmons and McCullough, 2003). However, recently the effectiveness of such intervention had been questioned (Davis et al., 2016; Renshaw & Olinger Steeves, 2016; Wood et al., 2010). One potential explanation for this low efficacy is a lack of understanding about which theoretical factors are most likely to enhance gratitude-outcome links. Should interventions for example highlight *direct* or *indirect* reciprocity? Thus, this meta-analysis will offer insights into ways of enhancing gratitude

interventions to increase prosociality, which also is known to enhance well-being (Weinstein, DeHaan, & Ryan, 2010; Weinstein & Ryan, 2010).

2.1.2. Gratitude and Prosociality

Drawing on the conceptual and theoretical overlaps in the way prosociality has been defined previously prosociality is defined here as a broad range of behaviours, effort or intentions designed to benefit, promote or protect the well-being of another individual, group, organization or society (Bolino & Grant, 2016; Dovidio, Piliavin, Schroeder, & Penner, 2006; George & Brief, 1992; Martin & Olson, 2015; Penner, Dovidio, Piliavin, & Schroeder, 2005). There are a number of potential theoretical mechanisms proposed to explain the *gratitude-prosociality* link and these are discussed below.

First, gratitude may act as a *moral motivator* that underlies direct and indirect reciprocity (Fredrickson, 2004a, 2004b; McCullough et al., 2008; Nowak & Roch, 2007). The principle of reciprocity is fundamental to explaining the survival of altruism in the population (Nowak, 2006). Reciprocity can be divided into *direct* and *indirect* reciprocity. *Direct* reciprocity occurs when the helper (A) is directly repaid by the recipient (B) at some later date (A helps B, B repays A). Thus, both parties benefit, leading to sustained prosociality. *Direct* reciprocity is only effective, however, when the helper and recipient are known to each other and can meet subsequently. When this is not possible *indirect* reciprocity offers another mechanism for the survival of prosociality (Nowak, 2006). *Indirect* reciprocity comes in two flavours: *downstream* and *upstream* (Sigmund, 2010). Downstream indirect reciprocity occurs when the helper (A) gains *reputation* from helping a recipient (B) and this reputation gain increases the probability that they (A) will

be helped by others (C) in the future (Nakamura & Masuda, 2011). Upstream indirect reciprocity occurs when the recipient to help (B) from a benefactor (A) goes on to help someone else (C) (A helps B, then B helps C) (Nowak & Roch, 2007; Nowak & Sigmund, 2005). Gratitude as a moral motivator is a potential mechanism in all three forms of reciprocity (direct, downstream and upstream), but in particular upstream indirect reciprocity (Nowak & Roch, 2007; Nowak & Sigmund, 2005). McCullough and associates (McCullough et al., 2001; McCullough et al., 2008; McCullough & Tsang, 2004; McCullough, Tsang, & Roberts, 2004) argued that gratitude operates to promote prosociality via three moral functions: *barometer*, *motivator* and *reinforcer*. As a *moral barometer* gratitude highlights to the recipients that they have been helped and, as a *moral motivator* it motivates the recipient to act prosocially toward either their benefactor (direct reciprocity) or other people (upstream indirect reciprocity). Indeed, Nowak and Roch (2007) explicitly link the two, suggesting that upstream indirect reciprocity 'hitchhikes' on the back of direct reciprocity, with direct reciprocity acting as the main mechanism for the evolution of prosociality (cooperation in their model). Finally, as a *moral reinforcer,* gratitude encourages continued generosity. It is less clear how gratitude operates for downstream indirect reciprocity, where reputation building is the main mechanism. However, Ferguson (2015) argues that gratitude can still operate here when the prosocial act is towards a group or organisation (e.g., charity). Consistent with the above, gratitude as an emotion has been shown to promote all 3 forms of reciprocity: (1) *upstream indirect* reciprocity (e.g. Chang et al., 2012; Halali et al., 2016), (2) *direct* reciprocity (e.g. Hendrickson &

Goei, 2009; Tsang, Schulwitz, & Carlisle, 2012), and (3) *downstream indirect* reciprocity (e.g. Langan & Kumar, 2015; Romani, Grappi, & Bagozzi, 2013).

Second, Fredrickson's (2004b) *Broaden-and-Build Theory* of positive emotions postulates that positive emotions (e.g., gratitude) *broaden* an individual's momentary repertoire of cognitions and actions which promote enhanced social bonds and help the individual to *build* personal physical and mental resources. Following this reasoning, gratitude may function to enhance social ties and resources that people can subsequently rely on for help when experiencing difficulty (Emmons & Mishra, 2011). As such, gratitude that is triggered by others, as opposed to generalized gratitude about what is personally valued (Lambert et al., 2009; Lambert & Fincham, 2011) should show a stronger association with prosociality. That is, the prosocial response to others is likely to strengthen close social bonds (*direct* reciprocity) or indicate that the person is worthy of help in the future (*indirect* reciprocity).

Third, cultural norms are known to play a major role in the expression of *prosocial behaviour* (Gächter & Schulz, 2016) and *emotions* (Kim & Sasaki, 2014). One important cultural norm with respect to gratitude-prosociality is collectivism. Higher levels of *collectivism* are linked to greater levels of *prosociality* (Lampridis & Papastylianou, 2014) and *gratitude* is linked to *collectivist* ideals that emphasise the maintenance of group harmony and reciprocity (Kee, Tsai, & Chen, 2008). It follows that in more collectivist cultures people should be more likely to experience and respond to gratitude with prosocial acts. Thus, a stronger *gratitude-prosociality* association should be expected in more collectivist cultures.

Fourth, cultural norms of religiosity may offer another explanation of the strength of gratitude-prosociality link. There is evidence that gratitude is associated with higher levels of religiosity (see Emmons & Mishra, 2011; Li & Chow, 2015) and religious observance is associated with prosociality (Henrich et al., 2010). Thus, at an individual level religiosity should mediate the gratitude-prosociality link. As many world religions endorse doctrines that support both gratitude, reciprocity and helping via Golden Rules (e.g. "do unto others as you would have them do unto you" Mathew, 7:12, New International Version), one may also expect that the gratitude-prosociality will be more culturally embedded for countries where religiosity is higher. Therefore, from the perspective of cultural norms of religiosity the *gratitude-prosociality* association should be stronger in countries with higher levels of religiosity.

Fifth, gratitude may be linked to prosociality via a third variable, with other prosocial dispositions (e.g., Agreeableness) known to be associated with prosociality (Zhao & Smillie, 2015) a strong candidate third variable. Indeed, while gratitude has been shown to be associated with other prosocial traits including *empathy* (Kruger, 2011; McCullough et al., 2002) and *forgiveness* (Carlisle & Tsang, 2013; Satici, Uysal, & Akin, 2014), there is no systematic evaluation of the strength of the association between *gratitude* and *prosocial disposition* in general. Showing that gratitude is linked to other prosocial traits will offer some initial evidence that, at least for the trait gratitude-prosociality link, other *prosocial dispositions* may act as a confounder linking the two.

Despite evidence supporting the link between *gratitude* and *prosociality*, and the theoretical reasons outlined above, there is considerable inconsistency regarding the strength of the association. For instance, Soscia

(2007) reports a strong correlation between customer perceived gratitude and their propensity to recommend the store to friends (r = 0.78, p <.01), while Watkins and colleagues (2006) report a more modest association (r = 0.34, p <.05) between *gratitude* and *direct* reciprocity. These differences may reflect both different types of prosociality as well as different domains (i.e. commercial and general). To date, there is no comprehensive quantitative review of the findings of how *gratitude* and *prosociality* are related to one another and what the salient moderator(s) of this association might be.

2.1.3. Gratitude and the Other Prosocial Emotions

Although this review focuses primarily on gratitude and its moderators, it is acknowledged that gratitude is not the only pro-social emotion. Nowak and Roch (2007) contend that other positive emotions can also evolve to support cooperation, but gratitude has particular theoretical importance for reciprocity. Furthermore, McCullough et al. (2008) suggest that gratitude has a wider impact on prosociality than other emotions as it supports high-cost helping. As such, gratitude should have a larger overall effect size (as it contributes more to all forms of prosociality – low and high cost) than other prosocial emotions. Thus, I intend to compare the effect size of the gratitude-prosociality link with effect sizes with the other specific prosocial emotions as well as general *Positive Affect* (PA) and *Negative Affect* (NA). This will also provide valuable information about which prosocial emotions to target interventions. If gratitude has a larger effect size than other prosocial emotions, then gratitude would appear a reasonable focus for interventions.

Other key prosocial emotions were identified primarily from the Appraisal Tendency Framework (ATF) of emotions (See Ferrer, Klein, Lerner,

Reyna, & Keltner, 2016; Lerner & Keltner, 2000). The ATF approach moves beyond exploring just the valence of emotions (NA versus PA) and differentiates among emotions in terms of their appraisal dimensions, such as certainty (e.g. the emotion arises as a consequence of a predictable stimuli) and personal control (e.g., the emotion arises as a consequence of actions/cognition under the person's personal control). The ATF identifies seven specific emotions with a relationship to prosociality: *gratitude*, *hope*, *pride*, *surprise*, *anger*, *guilt*, and sadness (Ferrer et al., 2016).

2.1.3.1. *Hope*, *Sadness*, and *Anger*

Hope refers to a desire for the person to have a better future for themselves and/or others, with the belief that this is achievable. Indeed, hope for a better future is one of the main motives given for volunteering in early stage clinical trials (Catt, Langridge, Fallowfield, Talbot, & Jenkins, 2011). *Sadness* is a key to the Negative State Relief (NSR) model of prosociality (Cialdini et al., 1987; Cialdini et al., 1997). The NSR model suggests people help to manage their own negative mood arising from observing another person's suffering. Finally, *anger* is a crucial motivator that underlies the punishment of free-riders to preserve group cooperation (Fehr & Fischbacher, 2004; Gürerk et al., 2006). That is, one major threat to prosociality is free-riding, whereby people gain a relative advantage by not helping at the expense of another's good deeds. When there is an option to punish free-riding—even if this is not implemented—cooperation increases (Skatova & Ferguson, 2013).

2.1.3.2. *Pride*, *Guilt*, and *Shame*

Pride is defined by the Oxford English Dictionary as a "feeling of deep pleasure or satisfaction derived from one's own achievement". To link pride to

prosociality it is necessary to distinguish *hubristic* (pride linked to arrogance and conceit) from *authentic* pride (linked to achievement), with *authentic* pride linked to *prosociality* (Carver, Sinclair, & Johnson, 2010; Tracy & Robins, 2007). Both *guilt* and *shame* (Amodio, Devine, & Harmon-Jones, 2007) are moral emotions, with guilt a more private and shame a more public self-focused emotion (Amodio et al., 2007; Scheff, 2000; Tangney, 1995). It is argued that individuals are motivated to avoid the *guilt* for not acting prosocially or the *shame* of acting selfishly (Saito, 2015) and indeed, both *guilt* and *shame* have been shown to lead to increased prosociality (Allpress, Brown, Giner-Sorolla, Deonna, & Teroni, 2014; Cunningham, Steinberg, & Grev, 1980; Miller, 2009).

While specific emotions are linked to prosociality there is evidence that both general PA and NA motivate prosociality (see Ferguson, 2016). People may act prosocially to maintain PA and manage and reduce NA (Cialdini et al., 1987; Cialdini & Fultz, 1990; Ferguson, 2016). Thus, as well as exploring if gratitude has a stronger link to prosociality than specific emotions I also examine if the *gratitude-prosociality* link was stronger than for PA and NA.

2.1.4. Moderators of the *Gratitude-Prosociality* Association

Below I detail the predictions from the five main theoretical moderators of the *gratitude-prosociality* link (reciprocity, social relationships, collectivism, religiosity and associations with prosocial dispositions) as well as exploring if the *gratitude-prosociality* association is stronger for *state* or *dispositional* gratitude. A number of methodological moderators will be examined as well.

2.1.4.1. *Theoretical* Moderators and Predictions

i.) *Reciprocity*. Above I argued that gratitude is a potential mechanism in all forms of reciprocity (direct, downstream and upstream). Thus, I predict the

gratitude-prosociality link will be stronger for studies that focus on reciprocity (*direct, downstream* and *upstream*) compared to studies that did not. I further examine whether the *gratitude-prosociality* link will be a stronger for studies that focus on *direct* as opposed to *indirect* (i.e. downstream and upstream) reciprocity. Despite the fact that gratitude has the capacity to incite all forms of reciprocity, it is less likely to be a central mechanism for *downstream indirect* reciprocity. Thus I would expect to observe a stronger *gratitude-prosociality* link for *direct* versus overall (downstream plus upstream) *indirect* reciprocity. Within *indirect* reciprocity I expect the *gratitude-prosociality* link to be stronger for *upstream* compared to *downstream indirect* reciprocity.

ii.) *Social Relationships*. As the prosocial response to others is likely to strengthen close social bonds (*direct* reciprocity) or indicate that the person is worthy of future help (*indirect* reciprocity), I expect that gratitude that is triggered by others (*benefit-triggered*), as opposed to *generalized* gratitude (Lambert et al., 2009), should have a stronger association with prosociality.

iii.) *Country of participants, Religiosity and Collectivism*. Gratitude may be linked to prosociality via increased levels of religiosity, thus the gratitude-prosociality link should be greater in countries with higher levels of religiosity. I explored this by examining the level of religiosity within each country in which each study took place. Likewise, I predict that the *gratitude-prosociality* link will be stronger in *more collectivist countries* where gratitude and reciprocity are stronger cultural norms, again because the expression of gratitude and its link to prosociality are encouraged and supported.

iv.) *Gratitude-Prosociality and Other Prosocial Dispositions*. Gratitude (especially *dispositional* gratitude) may be linked to prosociality simply

because it is associated with other prosocial dispositions such as agreeableness. Indeed, there is evidence linking trait gratitude to other *prosocial* dispositions (e.g. Kruger, 2011; McCullough et al., 2002). However, this link has not been systematically examined. As a first step to explore this potential mechanism for dispositional gratitude it is necessary to establish if there is a reliable association between *dispositional gratitude* and *other prosocial dispositions*. v.) *Gratitude Measure—Emotion, Mood and Disposition*.

Gratitude can be viewed either as a *state* (encompassing emotional reactions and mood) or as a *disposition* (Parrott, 2001). Gratitude as an emotion occurs when an individual is helped by another person, especially if the help was *intentional* (Emmons & Shelton, 2002; Fredrickson, 2004a; Lazarus & Lazarus, 1996), and the recipient did nothing to earn that help (Bertocci & Millard, 1963; Emmons, 2004). Emmons and McCullough (2003) also defined gratitude as a *mood* reflecting neutral daily events like 'waking up in the morning (pp.379)'. These distinctions map onto Lambert et al.'s (2009) distinction between *benefit-triggered* (being grateful to someone) and *generalized* gratitude (grateful for valued and cherished events and people in our lives). With respect to *trait gratitude*, Wood et al. (2010) define it within a life-affirming process as 'noticing and appreciating the positive in the world' (p 891) so as a tendency to experience *gratitude* in response to others' kindness.

Although there is a large evidence base for the link between *state gratitude* and prosociality, Wood and colleagues (2008) highlighted a paucity of empirical evidence for the link between *trait gratitude* and *prosociality*. I, therefore, examined the effect sizes for both *state* and *trait gratitude*. To do this I grouped both aspects of *state* gratitude (emotion/benefit triggered and

generalized/mood) as a single category. As the trait instantiation of gratitude includes both aspects of gratitude, I believe that the comparison between state and disposition is more justified by equating the conceptualization of the two.

Based on the Social Cognitive Model of Gratitude (SCMG) (Wood et al., 2008) and Trait Activation Theory (Tett & Guterman, 2000) I predict that the effect size for *state* gratitude will be stronger than for the *trait* gratitude. The SCMG suggests that *state* gratitude forms an indirect path between *trait gratitude* and *prosociality*. This indicates that *trait* gratitude should be a more distal and weaker direct predictor of prosociality compared to the more proximal state it triggers (see also Ferguson, 2013). Indeed, this pattern of a weaker association between a *trait* than an emotion with respect to a specific outcome is observed by others (see Fredrickson, Tugade, Waugh & Larkin, 2003). Furthermore, the principle of trait activation suggests that any behavioural expression, such as prosociality, linked to a trait requires activation of the trait by trait relevant cues (See Tett & Guterman, 2000). Thus, the assessment of a trait alone should not suffice to fully activate trait tendencies.

2.1.4.2. *Methodological* Moderators and Predictions

A few methodological factors which may influence the *gratitude-prosociality* association were examined. They included, i) gratitude induction, ii) *objectivity* of prosociality assessment, iii) *target* of prosociality—*individual* versus *group*, iv) gratitude measure—*proxy* versus *actual*.

i.) *Gratitude Induction: Laboratory-Studies (Vignettes & Experimental/Economic Games) versus Surveys/Field Studies.*

I explored whether *laboratory-studies*, which use a direct exogenous manipulation of gratitude (e.g. Exline, Lisan, & Lisan, 2012; Tsang, 2007) or

Survey/Field Studies, where participants completed a cross-sectional battery of *gratitude* and *prosociality* measures (e.g. Li & Chow, 2015; McCullough et al., 2002), resulted in a larger effect size. I further delved into a subtle distinction within lab-studies: *Vignettes* (e.g. Graham, 1988; Xia & Kukar-Kinner, 2013) versus Experimental/ Economic Games (e.g. Halali et al., 2016; Tsang, 2006a).

There are several practical advantages in applying the vignette technique. For instance, it is cost-effective and can be easily standardised (Alexander & Becker, 1978; Gould, 1996; Hughes & Huby, 2002). However, the lack of participant involvement in the vignettes may lead participants to simply respond in terms of normative theories of gratitude (Hegtvedt, 1990; Tsang, 2006b; Weiner, Russell, & Lerman, 1979). On the other hand, experimental manipulations/economic games involve laboratory inductions in which participants take an active role. For example, participants might be asked to recall an experience of being generously treated by others (e.g., Siegel, Thomson, & Navarro, 2014); or in an economic game where participants experienced gratitude after receiving a financial benefit (e.g. Leung, 2011). Experimental /economic game induced gratitude should better reflect the participants' genuinely experienced emotion, relative to vignette-induced gratitude, because of the higher extent of involvement (Levine & Moreland, 2004). As a result, I anticipate a stronger *gratitude-prosociality* association for experimental/economic game studies, compared to vignette studies.

ii.) *Objective-Subjective Assessments of Prosociality.* I examined whether the prosociality assessment involved an *actual expenditure of effort or money* (objective); or whether it used *self- or peer-reported* behaviour or *intention to act* prosocially (subjective). Given that subjective tendencies do not always

translate into actual behaviours (Ajzen, 1985, 1991; Ajzen & Madden, 1986), I expect that studies which examined prosociality *subjectively* to show a *stronger* link with gratitude than studies where prosociality was assessed *objectively*.

iii.) *Target of Prosociality—Individual versus Group.* Algoe et al. (2008) reported that gratitude enhances both *dyadic* and *group* relationships. Furthermore, the definition of prosociality I adopted entails helping individuals and groups equally. Thus, it remains unaddressed whether gratitude-inspired prosociality would function similarly when targeted as an *individual* or a *group*.

iv.) *Gratitude Measure—Proxy versus Actual.* Several studies employ *proxy* measures of gratitude. For example, Naito and associates (Naito & Sakata, 2010; Naito, Wangwan, & Tani, 2005) examined feelings of joy, warmth and helpfulness after receiving help. Considering that a *proxy* measure is by definition an *approximate* assessment (Kisch, Kovner, Harris, & Kline, 1969), it is logical to assume that it should, compared to a *direct* assessment, constitute a greater discrepancy between the operational and the conceptual definition of gratitude thus resulting in lower validity (Carver & Scheier, 2008). Therefore, I endeavour to examine if studies that employed *proxy measures* of gratitude would have *smaller effect sizes* compared to studies which adopted a *direct assessment* of participant's gratitude.

v.) *Age.* As there may be developmental trends with respect to experiencing gratitude and the chance to be prosocial I include age as a *continuous* covariate.

2.2 Methods

2.2.1. Main Analyses: *Gratitude-Prosociality* Association

2.2.1.1. Search Strategies

Studies were identified by searching electronic databases (Science, Social Science and general scholarly databases, including ISI Web of Science, PsycARTICLES, PsycINFO, Scopus, EconLit, Google Scholar, British Library EThOS, Applied Social Science Index & Abstracts (ASSIA), Business Source Premier (EBSCO), and Dissertation Online) and the reference lists from relevant articles. The following search terms for the main meta-analysis on *Gratitude* and *Prosociality* were used: *'*Gratitude' , 'Appreciation' and 'Prosocial Behaviours', 'Prosociality', 'Prosocial', 'Altruism', 'Altruistic' , 'Cooperation, 'Helping' , 'Compliance', 'Reciprocity',' Cooperative' and 'Reciprocal'. In the initial screening phase the abstracts and titles of potentially relevant articles (N = 746) were examined, with 420 duplicated entries removed. The full text of the remaining articles was inspected (N = 326), and thereby eliminated entries (N = 257) that were inconsistent with the eligibility criteria (see Sub-section 2.2.1.2). Furthermore, I examined the papers in order to remove entries that shared the same dataset, such as multiple analyses conducted with an identical dataset. Finally, I contacted authors for additional data where whose articles were published (or available online/published as book chapters for unpublished work) *within the last five years* that did not include sufficient information for the effect sizes to be computed.

2.2.1.2. Inclusion and Exclusion Criteria

The following inclusion criteria were used to select studies. First, the search was confined to papers written in *English*. Second, I did not impose any age limit on the participants in the present review but included *age* as a continuous moderating variable instead. Third, all studies had to involve primary and secondary measures of the relationship between gratitude (as a

disposition or a state) and prosociality (e.g. behavioural intention or overt behaviours). Finally, only studies that reported *quantitative* findings were included. Applying all these inclusion and exclusion criteria 65 papers with a grand total of 18, 342 participants, consisting of 91 studies and 252 effect sizes were identified. Figure 2.1 provides the information flow diagram prepared based on the PRISMA Statement (Liberati et al., 2009).

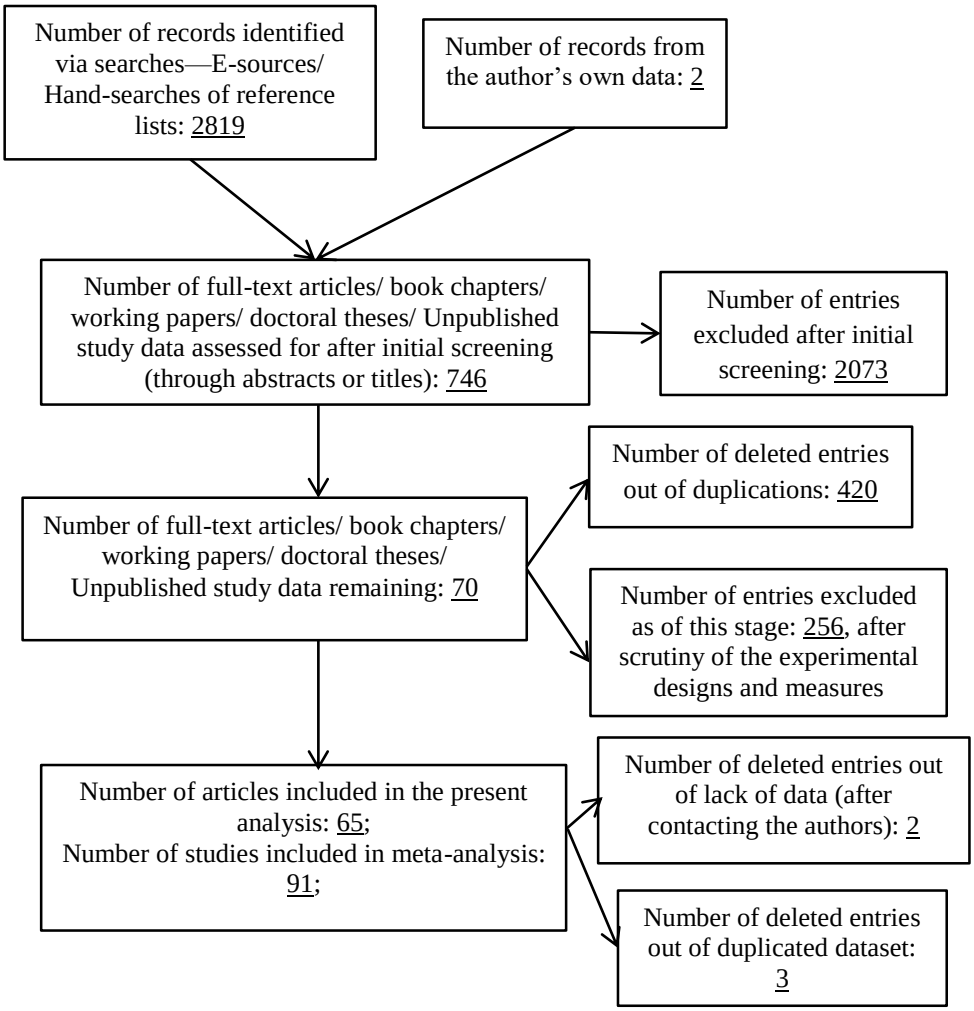

Figure 2.1. PRISMA Statement 2009 Flow Diagram of Information through different phase of the present review (Liberati et al., 2009)

2.2.1.3. Coding Procedures

Below I describe and present examples on how the present theoretical and methodological were coded. Table 2.1 details the specific criteria used.

Table 2.1. Working Definition of the Methodological and Theoretical Moderators

Moderators	**Codes**	**Working Definition**	**Examples in the current sample**
Theoretical Moderator: Reciprocity versus No reciprocity	Non-Reciprocity (k = 16)	Studies which i) measured the effect of gratitude on prosocial behaviours (or behavioural intention) that were not concerned about repaying a benefit received, or ii) were survey-based	i) Participants assigned to the on-going gratitude training session were to indicate 'each day if they had helped someone with a problem or offered someone emotional support (pp. 382)' – (Study 2, Emmons and McCullough, 2003)
	Reciprocity (k = 76)	Studies whose measures of prosocial behaviours or intentions involved: i) Direct Reciprocity: a direct return of favours to the benefactors— an 'A helps B and B helps A' scenario (Novak & Sigmund, 2007); ii) 'Downstream' Indirect Reciprocity: individuals acting prosocially towards those they observed to help others — an 'A helps B, and C helps A' scenario (Novak & Sigmund, 2007); iii) 'Upstream' Indirect Reciprocity: individuals acting prosocially to a third-party after receiving a favour from someone else —an 'A helps B, and B helps C' scenario (Novak & Sigmund, 2007);	i) 'Direct Reciprocity': Participants of the 'Favour' Condition decided how much money (i.e. USD $10) to distribute to their benefactors in the previous round. –(Tsang et al., 2012); ii) 'Downstream' Indirect Reciprocity: Participants of the 'experimental' condition read a vignette of a fictitious company engaging in Corporate Social Responsibility (CSR) investment (e.g. being environmentally-friendly, concern for fair trade, employees' safety and human rights preservation etc.). Participants then rated how grateful or thankful they felt towards that company for her CSR investment, and indicated how likely they would reward her via 'positive word of mouth' and 'advocacy behaviours' –(Romani et al., 2013) iii) 'Upstream' Indirect Reciprocity: Participants recalled a time being treated very generously, and then decided whether to donate their participation allowance to a local charity for children. – (Study 2, Siegel et al., 2014)
	Direct Reciprocity (k = 53)	See the definition above.	See the example above.
	Indirect Reciprocity (k = 16)	See the definitions above. Owing to the relatively small counts of both downstream (k = 5) and upstream (k = 9) studies, together with two studies whose outcomes concerned both types of indirect reciprocities, I combined all these to form a category Indirect Reciprocity in the main analyses.	See the example above.
	Blank Entries (k = 10)	Outcomes involved both *direct* and *indirect* reciprocities, but the authors did not specifically	Refer to the above-mentioned example of an 'ambiguously-targeted' prosociality measure (i.e. Naito et al., 2005).

		report how gratitude was related to each type of reciprocities.	
Theoretical Moderator: Social Relationships (Lambert et al., 2009)	Benefit-triggered Gratitude (k = 69)	The gratitude that results from a specific 'interpersonal transfer of benefit from a beneficiary to a benefactor. (pp. 1194)'	Participants' gratitude towards a confederate who unexpectedly offered the former a soda. –(e.g. Study 1, Goei & Boster, 2005); Patron's gratitude toward the complimentary winery tour and wine-tasting –(e.g. Kolyesnikova & Dodd, 2008)
	Generalized Gratitude (k = 19)	An emotion, or an affective state, which stems from the appreciation of things which 'are meaningful and valuable to oneself. (pp. 1194)'	Gratitude Induction: 'Think back over the past week and write down on the lines below up to five things in your life that you are grateful or thankful for. (pp. 379)' -(Emmons & McCullough, 2003)
Theoretical Moderator: Country of Participations, Religiosity and Collectivism	Continents (k = 91)	I categorised the 91 studies from 16 countries (or regions) on a continental basis. Fifty-five studies were conducted in *North America*, along with 23 in *Asia* and 13 in *Western Europe*.	'North America': Studies that were conducted in the United States of America (e.g. Goei & Boster, 2005) or Canada (e.g. Spence et al., 2014); 'Asia': Studies that were conducted in China (e.g. Tian, et al., 2016), Japan (e.g. Naito & Sakata, 2010), India (e.g. Dewani et al., 2016), South Korea (e.g. Kim & Lee, 2013), Israel (e.g. Halali et al., 2016), Hong Kong (e.g. Zhao, 2010), Thailand (e.g. Wangwan, 2014), or Taiwan (e.g. Chang et al, 2012). 'Western Europe': Studies conducted in the United Kingdom (e.g. Ma, et al., 2014), Germany (e.g. Wetzel et al., 2014), France (e.g. Simon, 2013), Netherlands (e.g. de Hooge, 2014), Italy (e.g. Soscia, 2007) and Norway (e.g. Xie et al., 2015).
	Religiosity	I coded the level of religiosity using the Gallup (2014) International religiosity Index. In particular, the percentage of people from a country identifying themselves as, regardless of whether they attend a place of worship or not, 'a religious person'	Canada: 40%; China; 7%; France: 40%; Germany: 34%; Hong Kong: 26%; India: 76%; Israel: 30%; Italy: 76%; Japan: 13%; Netherlands: 26%; South Korea: 44%; Thailand: 94%; United Kingdom: 30%; United States of America: 56%. (No data was provided for Norway and Taiwan)
	Collectivism	I coded the level of collectivism via Hofstede's (Hofstede, et al., 2010) Individualism-Collectivism index. A higher score indicates higher likelihood of people defining their self-image as 'I' instead of 'we' (i.e. low collectivism)	Canada: 80; China: 20; France: 71; Germany: 67; Hong Kong: 25; India: 48; Israel: 54; Italy: 76; Japan: 46; Netherlands: 80; Norway: 69; South Korea: 18; Taiwan: 17; Thailand: 20; United Kingdom: 89; United States of America: 91.

Theoretical Moderator: Gratitude Measures	State/Mood (*k* = 77)	Gratitude examined or induced as a i) positive emotion upon receipt of an intentional, valued benefit (Tsang, 2006a, 2007) ; or ii) a mood over a designated period of time.	i) Participants rated survey items such as 'I am happy to have been helped by others,' and 'I have benefited from the goodwill of others.' –(Study 2, Spence et al., 2014); ii) Participants rated the amount of gratitude they 'experienced "since yesterday". (pp.637) –(Froh et al., 2009)
	Disposition (*k* = 24)	Gratitude examined as an enduring characteristic of thankfulness that is sustained across contexts and over time (Chan, 2013; McCullough et al., 2002)	Participants to rate themselves using the GQ-VI (McCullough et al., 2002). The sample items included 'I am grateful to a variety of people; As I get older I find myself more able to appreciate people' – (e.g. Tian et al., 2015)
Methodological Moderator: Type of Study (Lab-Studies vs. Survey /Field Studies).	Lab-Studies (*k* = 59)	Studies which employed a direct, exogenous manipulation (or induction) of participants' gratitude mood or affective states. I further classified THESE studies under this code into two sub-categories: 1) Experimental/ Economic Games, and 2) Vignette.	'Experimental/ Economic Games': Participants assigned to the 'Gratitude' condition received a favour from a confederate while working on a tedious task. They then decided whether to help that confederate fill out a time-consuming survey for that confederate. -(Study 1, Bartlett & Desteno, 2006); 'Vignette': Participants read a vignette about a student, who struggled to find the past papers to prepare for his/her admission exam, who was offered the materials by a current graduate student of that programme named Zhang. A year later, the protagonist gets into the programme thanks to Zhang's help and learns that Zhang now needs help. Participants then indicated how eager they were to help Zhang back assuming they were that protagonist. –(Study 2, Yang et al., 2015)
	Survey /Field Studies (*k* = 32)	Studies in which participants completed a battery of questionnaires (i.e. Survey Study). Studies involving manipulation of variables *other than gratitude*, but included *peripheral measures* on participant's gratitude (i.e. Field Study) were also coded as a 'Survey/Field study' in the present analysis.	'Survey Study': Participants filled out a series of questionnaires including the Religiousness Scale (Strayhorn et al., 1990), Spirituality Scale (Delaney, 2005), in addition to the trait gratitude (i.e. GQ-VI) and prosocial behaviours measures (e.g. Peer-helping behaviour scale (Crick, 1996) and Child Altruism Inventory (Ma & Leung, 1991)). -(Li & Chow, 2015); 'Field Study': A between-subject video vignette study which examined the effect of Socio-Economic Statuses (janitor versus doctor) and Favours (whether or not the protagonist had bought a drink for his 'target') on compliance with a date request. Participants imagined themselves as the recipient of a date

			request, and were asked to rate how grateful, appreciative, or thankful they felt towards the protagonist throughout the episode. –(Hendreickson & Goei, 2009)
	Experimental/ Economic Games (k = 34)	Laboratory Induction: Participants were asked to recall being grateful. Economic Games: Participants' gratitude was triggered by a co-player's conferment of a financial benefit in the course of an economic exchange.	'Laboratory Induction': Participants recalled an incident whereby 'another person did something for you that was very kind. (pp.47)' –(Exline et al., 2012)
	Vignette (*k* = 25)	A hypothetical scenario or story in which participants were induced to feel grateful as the protagonist was treated generously by someone.	Refer to the above examples of 'Lab Studies' (e.g. Yang et al., 2015).
Methodological Moderator: Objective versus Subjective Prosociality Assessment	Objective (*k* = 30)	Prosociality assessments that involved an actual expenditure of effort or money (or other money-equivalent resources).	Amount of money (i.e. a $5-dollar note) to distribute to 'another participants who will be in the study at a later time (pp.213)' –(Study 2, Exline and Hill, 2012;)
	Subjective (*k* = 62)	Studies which assessed *self*-, or *peer*-reported prosocial behaviours, or one's *intention* to act prosocially to others.	Customer Purchase Intentions: 1) 'I would be very likely to buy something today'; 2) I would come back to this store.' 3) 'I would likely buy from this store in the future.' (7-point scales) –(Palmatier et al., 2009)
Methodological Moderator : Target of Prosociality: Individual versus Group	Individual (*k* = 44)	Reciprocal, prosocial or cooperative acts or behavioural intentions that were directed toward an *individual* recipient.	Each participant decided how many raffle tickets of his/hers to distribute to his/her in-game partner. -(e.g. Tsang, 2006a, 2007)
	Group (*k* = 34)	Participants' decisions to show appreciation to a group or organisation by i) behaving prosocially toward or ii) harbouring an intention to benefit that organisation in the future.	i) Children participants were given a chance to write a thank-you card to the Parent-Teacher Association for their provision of a multimedia presentation. –(Froh et al., 2014); ii) Loyalty—Advocacy (Lam, Shankar, Erramili & Murthy, 2004): 'I will encourage my friends to use this bank's services' and 'I will recommend this bank to others.' (7-point scales) –(Study 1 and 2, Xia and Kukar-Kinney, 2013)
	Ambiguous (*k* = 15)	Studies which provided i) insufficient information to judge whether prosociality was individually- or group- directed, or ii) studies whose measures of	i) Participants' weekly record of their 'acts of kindness for others (pp.4)' –(Layous et al., 2016); ii) Participants' ratings on the following items were combined to form an overall 'enhanced prosocial behaviours measure': a)

		individual and group helping were combined into a single index of helping.	'Do you want to help your father (i.e. the benefactor in the vignette) if he needs help in similar or other situations?', and b) 'Do you want to help others if they help in similar or other situations?' –(pp. 251, Naito et al., 2005)
Methodological Moderator : Gratitude Measures: Actual versus Proxy	Actual (k = 82)	Studies which adopted a direct or a 'real' measure or induction of gratitude as a state, mood or a disposition.	Customer' Gratitude Scale (adapted from Goei & Boster, 2005): 'I feel grateful/thankful to this company', 'I feel appreciative towards/ a sense of gratitude toward this company'. (9-point scale) – (pp. 607, Simon, 2013); Participants reported a personal experience in which they felt grateful toward someone. – (de Hooge, 2014)
	Proxy (k = 14)	Studies which employed an indirect or a 'surrogate' measure of gratitude.	'Positive Responses' evoked by being helped—e.g. 'Delighted' – (pp. 21, Wangwan, 2014)
Methodological Moderator: Published or not? Times cited, Years of Publications and Participant Age	Published? (k = 91)	I included a 'yes'/ 'no' code on whether the study had been published.	
	Yes (k = 72)	Published journal articles	Tsang (2006a, 2007)
	No (k = 19)	Doctoral theses, Book chapters, and Raw Data collected	Doctoral Theses: Langan & Kumar (2015); Book Chapters: Mikulincer & Shaver (2010); Raw Data Collected; Ma, Tunney & Ferguson (2015)
	Times Cited	I included the times cited metrics (accurate as of 4th August 2016) that are provided by either the Web of Science or Google Scholar.	
	Years of Publication	I included the year of publication (or availability) of the sampled articles/studies data.	
	Participant Age	Sixty-one studies reported the average or the median age range of their participants. I imputed the average age of the 23 studies which only described their sample compositions (i.e. undergraduates) by taking mid-point of the usual age range of the undergraduate student population: 18-24 years (i.e. 21.0 years). Six studies did not provide any age data.	'Mean age given': 12.14 –(Froh et al., 2009); 'Median age range given': 35-44 years—imputed participant age: 39.50 (Romani et al., 2013); 'Only sample description given': 'Undergraduate Psychology students' (e.g. Emmons & McCullough,2003; Tsang et al., 2012); 'No information': Huang (2015); Soscia (2007)
Sensitivity Analysis: Zero-order?	Yes (k = 74)	Zero-order figures (e.g. correlation *r*s, independent sample *t*-values etc.) used to compute the effect estimates.	'Zero-order Correlation rs' – (e.g. Bock et al., 2016); 'Independent samples T-test t-values' (e.g. Hwang & Kandampully, 2015)

	No (*k* = 18)	Imputation of effect sizes from a specific gratitude-prosociality path in a multiple-path model (e.g. Multiple Regressions, Structural Equation Modelling (SEMs))	'SEMs': -(e.g. Study 3, Xia and Kukar-Kinney, 2013); 'Multiple Regressions':

i.) *Reciprocity versus Non-reciprocity and Reciprocity Nature*.

I coded whether the prosociality measures reported were *reciprocally* driven or not. *Reciprocity* (*k* = 76) was defined as an individual's attempt (or motivation) to respond to a positive action with another positive action. These were further differentiated into *Direct* Reciprocity (*k* = 53), that refers to the direct reciprocation of favours received and 2) *Indirect* Reciprocity (*k* = 16) that included both 'downstream' (the individual acts prosocially to someone whom they know to have helped a someone else previously *k* = 5), and 'upstream' (the individual acts prosocially to a third party after being helped by someone else *k* = 9). Ten studies that examined both *direct* and *indirect* reciprocity but did not separately report how gratitude was associated with each type of reciprocity (e.g. Desteno et al., 2010) were excluded from the analysis that focused on the distinction between *direct* and *indirect* reciprocity.

I also coded studies that examined prosocial acts or behavioural intentions that were *not* driven by a need to repay (e.g. Study 1, McCullough et al., 2002) as involving *Non-Reciprocity* (*k* = 16) (See Table 1 for details).

ii.) *Social Relationships*. I coded *benefit-triggered* gratitude as any emotionally felt gratitude in response to another's help (state measures only, *k* = 69) and that *generalized* gratitude as an appreciation of valued people, and events in life assessed as both a state and trait (*k* = 19, see Table 1).

iii.) *Country of participations, religiosity and collectivism*. Altogether sixteen countries are represented. I categorised these countries initially on a continental basis (see Table 1). The majority of the studies were conducted in *North America* (*k* = 55), along with *Western Europe* (*k* = 13), *Asia* (*k* = 23). Each country in the sample was also coded for its level of *religiosity* using the Gallup

(2014) International Religiosity Index and *collectivism* using the scoring procedures of Hofstede, Hofstede, and Minkov (2010).

iv.) *Gratitude Measure—State/Mood versus Disposition*. In the present review I grouped studies that examined gratitude as either a state or a mood under the category *State/Mood* (*k* = 77) or as a *disposition* (*k* = 24) (see Table 1).

v.) *Gratitude Induction: Experiments versus Surveys*. Studies were divided into *Laboratory Studies* (involving a direct manipulation to induce gratitude) (*k* = 59) and cross-sectional *Surveys/Field Studies* (*k* = 32). Lab studies were further sub-divided into: *Experimental/Economic Games* (*k* = 34), and *Vignettes* (*k* = 25). Studies that were coded as *Vignettes* typically triggered participants' feelings of gratitude via hypothetical scenarios in which a protagonist was (or was not) helped. Studies that were coded *Experimental/Economic Games* involved gratitude induction via either laboratory induction of recalling being grateful or economic games during which one's gratitude was elicited via receiving a *financial benefit* in the course of an *economic interaction*. Illustrative examples are given in Table 1.

vi.) *Objectivity of Prosocial Measure*. I coded the objectivity of the prosocial measures reported. An *objective* measure (*k* = 30) was defined as an *actual expenditure* of resources, time or effort and *subjective* measures (*k* = 62) was referred to as a self-reported (or peer-reported) intentions to behave prosocially (i.e., *without* any *actual commitment* of resources) (Table 1).

vii.) *Target of Prosociality—Individual versus Group*. I coded whether the prosocial behaviours or behavioural intentions reported were targeted at an *individual*, a *group*, or an *ambiguous* entity. An *individually*-directed (*k* = 44) prosocial measure is illustrated by Tsang (2006a) in which each participant

decided how much to give to a specific co-player. By contrast, a *group*-directed (*k*= 34) prosocial measure is represented by the participants' decisions to show appreciation—for example, by writing a 'thank-you' note (Froh et al., 2014) — towards a group. Several studies were coded as *ambiguously*-targeted (*k* = 15) due to insufficient information. For example, if measures of *individual* and *group* helping were combined into a single index, or if the item was indexed *helping in general* while the authors did not designate this as either a *group*- or an *individually*- targeted act (see Table 1 for examples).

viii.) *Gratitude Measure—Proxy versus Actual*. I also coded whether the gratitude measurement used was a proxy or an actual measure. A *Proxy* measure (*k* = 14) was defined as a surrogate, or indirect assessment of gratitude. In contrast, I defined an *Actual* measure (*k* = 82) as a direct assessment of gratitude as a *state, mood* or a *disposition* (see Table 1).

ix.) *Times Cited, Year of Publication and Age*.

First, for all the published studies I coded the number of times that each article had been cited. This figure was obtained by examining the *times cited* metrics provided by the databases used in the search (4th August 2016). To avoid double-counting I took the highest count metric available.

Second, I included the years of publication (or availability) of the sampled articles/studies data (*M*: 2010.87, *SD*: 4.88). The earliest publication I included was in the 1980s (Graham, 1988) while the latest one was e-published in June 2016 ((Layous, Nelson, Kurtz, & Lyubomirsky, 2016). For unpublished entries I recorded either the year in which the papers were available, or the year in which the studies were conducted.

I coded *mean* or *median age* of participants reported in each study. Where the only information was a sample description (e.g., American college students) I imputed the age for the average American college student (i.e. (18+24)/ 2= 21.0 years) in the year that the study was administered.

2.2.2. Additional Meta-analysis: Association with other Prosocial Dispositions and Other Prosocial Emotions

2.2.2.1. Prosocial *Dispositions*

To explore the association between dispositional gratitude and other prosocial dispositions I included the following additional search terms ('Agreeableness', 'Conscientiousness', 'Dispositional Empathy', and 'Forgiveness'). I included *Conscientiousness* as a prosocial disposition because there is evidence that it is associated with volunteering behaviours (Ferguson, 2004; Lodi-Smith & Roberts, 2007) along with *Agreeableness* which is associated with prosociality in general (Ferguson, Gancarczyk, Wood, Delaney, & Corr, 2015; Zhao & Smillie, 2015). This resulted in 24 studies with 103 effect sizes with a total sample of 7,677.

2.2.2.2. Other Prosocial *Emotions*

To contextualise the *gratitude-prosociality* association I compared that to association with the other prosocial emotions (i.e. hope, pride, surprise, anger, guilt, and sadness) as well a general Negative Affect (NA) and Positive Affect (PA). A number of existing meta-analyses were identified which addressed prosociality with respect to *NA* (Carlson & Miller, 1987; Dalal, 2005), *PA* (Carlson, Charlin, & Miller, 1988), *guilt* (Boster, Cruz, Manata, DeAngelis, & Zhuang, 2016), *shame* (Lech & Cidam, 2015), *sadness* (Carlson

& Miller, 1987), and *happiness* (Lyubomirsky, King, & Diener, 2005). To date I am yet to locate any meta-analysis for *hope*, *surprise*, *anger* and *pride*.

Literature searches for *surprise* (search terms: ‘Surprise’, ‘Prosocial behaviours’, ‘Cooperation’, ‘Helping’, ‘Compliance’ and 'Prosociality’) and *hope* (search terms: ‘Hope’, ‘Hopeful’, ‘Prosocial behaviours’, ‘Cooperation’, ‘Helping’, ‘Compliance’ and 'Prosociality’) revealed no studies. Literature searches revealed 23 studies on *pride* (search terms: ‘Pride’, ‘Prosocial behaviours’, ‘Cooperation’, ‘Helping’, ‘Compliance’ and 'Prosociality’) and 37 on *anger* (search terms: 'Anger', 'Helping', 'Cooperation', ' Third-party punishment', 'altruistic punishment', 'prosocial behaviours').

The search for *anger* was limited to papers published after Van Doorn and colleagues’ (2014) review on anger and prosocial behaviour and all relevant papers (i.e. the ‘direct evidence’) from Van Doorn et al. (2014) were included. With respect to *anger* the definition of prosociality was extended to include *cooperation* (giving to the public good and contribution of resources, which did not include an option to punish non-cooperators) along with n*orm-enforcing punishment*, whereby non-cooperators are punished either by other players (*second* party) or an impartial observer (*third* party) at a cost to the punisher. Punishment of this type is believed to enforce norms of fairness, with the evidence showing that it leads to greater cooperation (Fehr & Fischbacher, 2004; Fehr & Rockenbach, 2004; Gürerk et al., 2004, 2006). Furthermore, Peysakhovich, Nowak, and Rand (2014) distinguished *cooperation* from *norm enforcement* as two clear and distinct aspects of the cooperative phenotype. Thus it seemed reasonable to distinguish the two. Two additional meta-analyses were conducted to estimate the overall effect sizes for pride and anger on prosocial

behaviours. The same set of *inclusion* and *exclusion criteria* as the main analyses were applied to these additional analyses.

2.2.3. Overview—Data Synthesis, Meta-bias and Additional Analysis

For the present review the correlation *r* was used as the effect-size metric. For studies that only reported the *standardised* βs I had applied Peterson and Brown's (2005)—$r = \beta + 0.05\ \lambda$ (where $\lambda = 1$ for non-negative βs, and $\lambda = 0$ for negative βs)—to impute the corresponding *r*s. I also computed *r* for studies that did not conduct correlational analyses via sample sizes along with *t*-values, χ^2 values, *p*-values, and standardised mean differences (i.e., Cohen's *d*). In addition, several measurements were reverse-scored to assure that each *positive* effect size computed would represent a *direct* association between *gratitude* and *prosociality*.

The *Random-effects* model was adopted to calculate the overall effect size of *gratitude* on *prosociality*. Because the current sample contained studies conducted with noticeably different features I did not follow the Fixed-effects because this assumes that all the studies included are functionally identical and share a single canonical effect size (Borenstein, Hedges, Higgins, & Rothstein, 2010; Hedges & Vevea, 1998). Additionally, the *Random-effects* model allows unconditional inferences (i.e., a generalizable conclusion to situations beyond the sampled studies) of the results (Field, 2001; Schmidt, Oh, & Hayes, 2009).

Many studies were found to have reported multiple *Gratitude-Prosociality* metrics. It was not uncommon for studies to either include *both state* and *dispositional* assessment of gratitude alongside a single prosociality measure (e.g. Spence, Brown, Keeping, & Lian, 2014; Exline et al., 2012), or to have a *single gratitude* measure alongside *multiple prosociality* measures (e.g.

Watkins et al., 2006). These effect sizes that arise from the same study are not independent (Balliet, Mulder, & Van Lange, 2011; Borenstein, Hedges, Higgins, & Rothstein, 2009). As a result, *study* was referred to as the *unit of analysis* meaning that each study included would contribute only one summary effect size to the analysis (see Cooper, 1998). The effect sizes were computed using Cooper's (1998) *Shifting-Unit-of-Analysis* method for studies which report *multiple, non-independent* effect sizes.

Similar to Balliet and Van Lange's (2013) reporting, I detail the 95% Confidence Intervals alongside certain indices of heterogeneity assessment like I^2, i.e. the cross-studies 'inconsistency index' (Higgins & Thompson, 2002; Higgins, Thompson, Deeks, & Altman, 2003), Cochran Q, and tau-squared (the 'study-to-study variances') (Borenstein et al., 2009). The issue of publication bias was addressed via examining the funnel plot in which all effect sizes are plotted against the standard error. To empirically evaluate the extent of the symmetry of the funnel plot, and hence the severity of potential publication bias I examined the following indices, namely (1) the effect size estimates following Duval & Tweedie's (2000) Trim-and-Fill and (2) Egger's regression intercept (Egger, Smith, & Phillips, 1997).

I applied the mixed-effects model in the categorical univariate moderator analyses (e.g., study type, reciprocity nature etc.) so as the meta-regression analyses for the continuous moderators (e.g. times cited and years of publication). It should, nevertheless, be noted that the application of mixed-effects model may, compared to a fixed-effect model, render the analyses over-conservative and therefore susceptible to Type II-errors (Balliet & Van Lange, 2013; Lipsey & Wilson, 2001).

All analyses in the present review were conducted using the Comprehensive Meta-Analysis (CMA) Version 2.0 (Borenstein et al., 2009). Meta-regression models, however, were conducted using CMA Version 3.0 (Borenstein, Hedges, Higgins, & Rothstein, 2014). As a number of effect sizes were derived from multivariate analyses (multiple regressions, path models, ANCOVA etc.), the effect sizes based on *r* may be over- or under-estimated. Therefore, I explored if the effect size estimates would vary as a function of effect sizes that are zero-order (i.e. derived from univariate analyses) or derived from partial coefficients. Similar analyses had shown that it has the effect of generally reducing effect size estimates (Ferguson & Bibby, 2012).

2.3 Results

2.3.1. Overall Analyses

The list of effect sizes of the association between *gratitude* and *prosociality* and study characteristics are contained in Table 2.2. The analysis revealed a moderate positive association between gratitude and prosociality, r = 0.374, 95% confidence interval lower limit (LLCI) /Upper limit (ULCI) = 0.329/0.417, p <.0001. I observed a non-negligible level of variation in the distribution of effect sizes (Tau = 0.232, Tau-squared = 0.054). This might be explained by the considerable extent of heterogeneity (i.e., I^2 = 90.98; Q (90) = 998.16, p <.0001) inherent among the sampled studies.

To address the extent to which publication bias may have impacted upon the analysis I first examined the adjusted effect size estimates following Duval and Tweedie's (2000) Trim-and-Fill procedure using the Random-effects model. No studies were deemed missing below the average effect estimates. In contrast, fifteen studies with imputed effect sizes greater than the mean effect

estimate were filled in, resulting in an effect estimate that was slightly higher than the pre-adjusted mean effect (r = 0.423, LLCI/ULCI = 0.379/ 0.465). This suggested that the present analysis might be potentially biased toward understating, rather than overstating, the summary effect. Such a potential vulnerability to understating the effect is the opposite to what one would normally expect from a review that is confounded by publication bias (i.e. the under-sampling of non-significant effect sizes which are prevalent among unpublished studies (Balliet & Van Lange, 2013; Cooper, 1998). Nevertheless, the non-significant Egger's regression coefficient (intercept = 0.50, standard error = 1.09, LLCI/ULCI = -1.67/2.67, p = .6473 (two-tailed)) should have dispelled any concern about bias toward underestimation. Taken together, all these indicators suggest that the present analysis is not contaminated by publication bias. See Figure 2.2 below for the funnel plot.

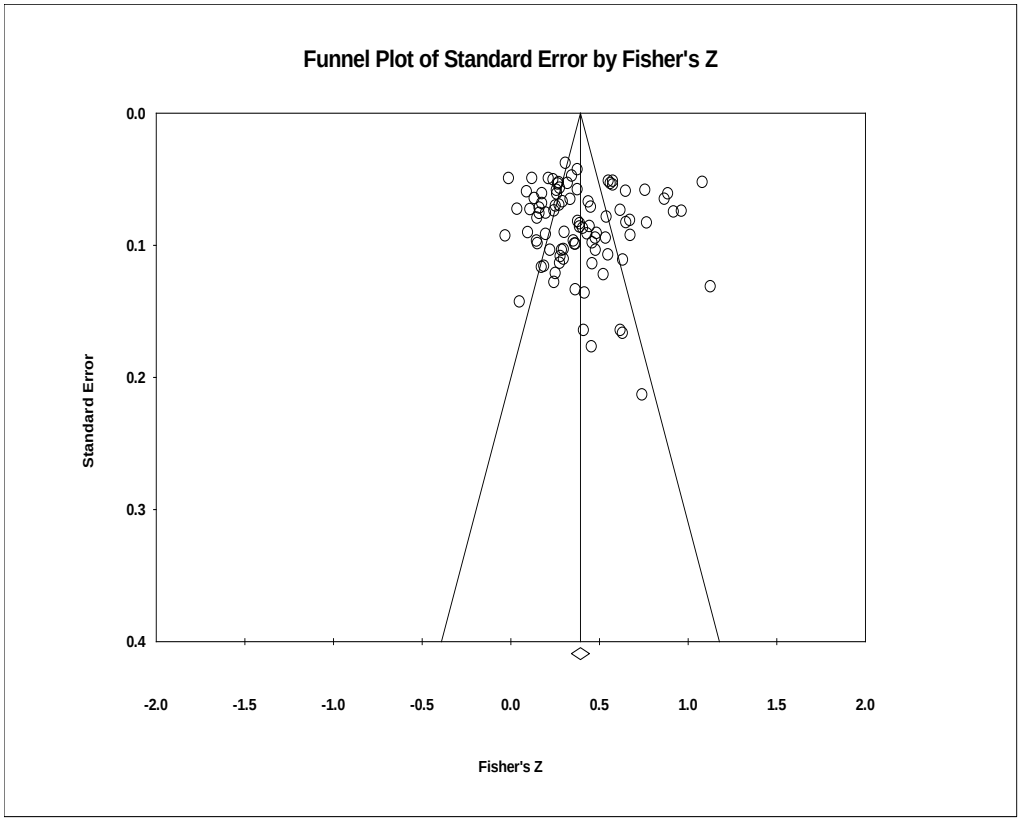

Figure 2.2 The Funnel Plot (with observed studies only)

Table 2.2. Studies on the *Gratitude-Prosociality* Relationship

Studies (*k* = 91)	Gratitude Survey Measures or Direct Inductions	Prosocial Behaviour/ Behavioural Tendencies Measures	Years	*N*	*r*	95% LL/ ULCI
Bartlett & Desteno (2006)						
Study 1	Experiment: Conferment of Benefits	Time Spent on Helping the Confederate	2006	105	0.351	0.1704/0.5018
Study 2	Experiment: Conferment of Benefits	Time Spent on Helping the Confederate	2006	97	0.290	0.0961/0.4627
Study 3	Experiment: Conferment of Benefits	Time Spent on Helping the Confederate	2006	35	0.427	0.1091/0.6654
Bartlett et al. (2012)						
Study 1	Experiment: Conferment of Benefits	Socially Affiliative Decisions	2012	40	0.390	0.0892/0.6254
Study 2	Experiment: Conferment of Benefits	Costly Social Inclusion Behaviours	2012	25	0.630	0.3127/0.8208
Bock, Folse, & Black (2016)						
Study 4	Survey: Gratitude Affect/ Behaviours/ Cognitions	Relationship Continuity (Hess, Ganesan, & Klein, 2003)	2016	226	0.285	0.1604/0.4005
Chang et al., (2012)						
Single-Study	Survey: GQ-VI (McCullough et al., 2002)	Peer-reported Generosity Exhibited throughout a Semester	2012	174	0.160	0.0128/0.3004
Cohen (2012)						
Study 1	Experiment: Recall about Times when Being Benefited (Measures: GAC, McCullough et al (2002))	Self-reported Urge to Reciprocate/ Verbal Reciprocity/ Reciprocal Actions	2012	57	0.394	0.1487/0.5936
Study 2	Experiment: Recall about Times when Helping Someone (Measures: Perceived Recipient's Gratitude)	Perceived Recipients' Urge to Reciprocate/ Verbal Reciprocity/ Reciprocal Actions	2012	59	0.350	0.1026/0.5559
de Hooge (2014)						
Study 1	Experiment: Recall of Personal Gratitude Experience	Total Gift-giving/ Money Willing to Spend on Gifts	2014	271	0.254	0.1395/0.3626
Study 4	Experiment: Recall of Personal Gratitude Experience	Gift-Giving/ Money to Spend/Time Spent on Gift Search	2014	138	0.371	0.2171/0.5065
Desteno, Bartlett et al., (2010)						
Single-Study	Experiment: Conferment of Benefits	Tokens Given: Give-Some Dilemma Game (GSDG)	2010	85	0.290	0.0819/0.4738
Dewani, Sinha & Mathur (2016)						
Single-Study	Survey: GQ-VI and GAC (McCullough et al., 2002)	Purchase Intention (Sweeny, Geoffrey, & Johnson, 1999)	2016	398	0.235	0.1403/0.3261
Emmons & McCullough (2003)						
Study 2	Experiment: Recall of things that 'you are grateful for'	Offer Emotional Support/ Helped Somebody (Yes/No)	2003	157	0.148	-0.0066/0.2954
Exline & Hill (2012)						
Study 2	Experiment: Conferment of Benefits	Donations toward an 'Anonymous Future Participant'	2012	286	0.090	-0.0263/0.2039
Exline, Lisan & Lisan (2012)						
Study 1	Experiment: Recall of an incident when 'another person did something for you that was very kind (pp. 47)'	In-the-moment kindness motives toward Benefactor/ Close Others (i.e. close friends and family)/ Strangers/ Enemies	2012	217	0.175	0.0425/0.3009
Froh, Bono & Emmons (2010)						
Single-Study	Survey: GQ-VI and GAC (McCullough et al., 2002)	Child Social Behaviour Questionnaire (Warden, Cheyne, Christie, Fitzpatrick, & Reid, 2003)	2010	700	0.300	0.2310/0.3660
Froh, Bono, Fan et al. (2014)						
Study 1	Experiment: Conferment of Benefits	Writing Thank-you Cards to the Benefactors	2014	122	0.195	0.0181/0.3605
Froh,Yurkewicz, & Kashdan (2009)						
Single-Study	Survey: GAC and 'Gratitude in response to aids'	Offer Emotional Support/ Helped Somebody (Yes/No)	2009	71	0.172	-0.0545/0.3822
Goei & Boster (2005)						
Study 2	Survey: GAC and 'felt a deep sense of gratitude (pp.293)'	Compliance: Purchase of Raffle Tickets from Confederates	2005	96	0.280	0.0842/0.4549
Goei, Roberto, Meyer, & Carlyle (2007)						
Study 1	Survey: 4-item Gratitude Survey (Goei & Boster, 2005)	Compliance: Purchase of Raffle Tickets from Confederates	2007	64	0.240	-0.0062/0.4587
Study 2	Survey: 4-item Gratitude Survey (Goei & Boster, 2005)	Compliance: Purchase of Raffle Tickets from Confederates	2007	186	0.240	0.0996/0.3711

Graham (1988)						
Study 1	Vignette: The protagonist (i.e. Tim) was selected by captain Bob to join the school baseball team.	A 'Yes/No' response to whether or not Tim would give Bob a new baseball as a thank-you gift for picking him.	1988	119	0.559	0.2958/0.7434
Study 2			1988	105	0.346	0.1653/0.5042
Halali, Kogut & Ritov (2016)						
Study 1A	Experiment: Resource Allocation (Tsang, 2006a)	Resources to Distribute to the Other Player	2016	146	0.371	0.2223/0.5034
Study 2	Experiment: Resource Allocation (Tsang, 2006a)	Resources to Distribute to the Other Player	2016	115	0.444	0.2844/0.5803
Hendrickson & Goei (2009)						
Single-Study	Survey: 4-item Gratitude Survey (Goei & Boster, 2005)	Compliance: Date Request Compliance	2009	115	0.490	0.3371/0.6177
Huang (2015)						
Single-Study	Survey: Customer Gratitude (Palmatier, Jarvis, Bechkoff & Kardes, 2009)	Behavioural Loyalty (De Wulf, Odekerken-Schroder, & Iacobucci, 2001)	2015	239	0.700	0.6290/0.7594
Hwang & Kandampully (2015)						
Single-Study	Vignette: Story about a Hypothetical Grocery Retailer's Corporate Social Responsibility (CSR) commitment	Participation Intention in that Hypothetical Grocery Retail's Prosocial Loyalty Programme (pro-social LP)	2015	350	0.265	0.1669/0.3578
Janakiraman, Meyer & Morales (2006)						
Study 2	Survey: Ratings on the item 'Thankful' (0 to 100)	Purchase Intentions	2006	297	0.253	0.1435/0.3567
Jin & Merkebu (2014)						
Single-Study	Survey: Customer Gratitude (measure via GAC)	Self-reported Favourable Reciprocal Behaviours (FRBs)	2014	398	0.794	0.7526/0.8287
Kim & Lee (2013)/ Lee, Kim & Pan (2014)*						
Single-Study	Survey: Customer Gratitude (measure via GAC)	Self-reported Favourable Reciprocal Behaviours (FRBs)	2013	297	0.640	0.5675/0.7026
Kim, Smith & James (2010)						
Single-Study	Survey: Consumer Gratitude (measure via GAC)	Intention to Reciprocate (Zeithaml, Berry, & Parasuraman, 1996)	2010	272	0.710	0.6456/0.7644
Kolyesnikova & Dodd (2008)/ Kolyesnikova, Dodd & Wilcox (2009)*						
Single-Study	Survey: 3-item measure (example: 'Desire to say "thank-you" to the winery personnel (pp.207)' (Kolyesnikova et al., 2009)	Dollar Amount Spent at Wineries	2008	357	0.510	0.4289/0.5829
Kolyesnikova, Dodd & Callison (2011)						
Single-Study	Experiment: Gratitude-inducing Direct Mail Messages	Purchase Intent/ Future Behavioural Intentions	2011	120	0.588	0.4571/0.6942
Krumrei-Mancuso (2016)						
Single-Study	Survey: Gratitude Disposition (measured via GQ-VI)	Self-reported Altruism (Smith, 2006)	2016	314	0.270	0.1642/0.3696
Kubacka, Finkenauer, Rusbult & Keijsers (2011)						
Study 1	Survey: Gratitude toward Spouses (via GQ-VI)	Relationship Maintenance Behaviours	2011	390	0.311	0.2138/0.4013
Kwak & Kwon (2016)						
Study 2	Vignette: Story about Participants' Favourite Teams Partnering with a Local or an International Charity	Intention to Donate to that Charity	2016	201	0.424	0.3032/0.5310
Langan & Kumar (2015)						
Study 2	Vignette: Story about a Coffee Shop (Local vs. International) Engaging in a Donation (Money vs. Time)	Desire to Reciprocate (example items: 'Frequent that shop more often'; 'Go out of your way to shop there' etc.)	2015	185	0.746	0.6742/0.8037
Layous, Nelson, Kurtz & Lyubomirsky (2016)						
Study 1	Experiment: Specific and General Gratitude Training	Participants' Weekly Acts of Kindness towards Others	2016	233	0.186	-0.0388/0.3930
Study 2	Experiment: General Gratitude Positive Trigger	Participants' Weekly Acts of Kindness towards Others	2016	119	-0.030	-0.2093/0.1503
Leung (2011)						
Study 1	Economic Games: Two-person Public Goods Game Dilemma	Cooperation: Contributions to the Public Account	2011	124	0.450	0.2973/0.5803
Study 2	Economic Games: Two-person Public Goods Game Dilemma	Cooperation: Contributions to the Public Account	2011	84	0.560	0.3928/0.6914

Study 3	Economic Games: Two-person Public Goods Game Dilemma	Cooperation: Contributions to the Public Account	2011	110	0.340	0.1631/0.4957
Study 4	Economic Games: Two-person Public Goods Game Dilemma	Cooperation: Contributions to the Public Account	2011	90	0.500	0.3267/0.6407
Li & Chow (2015)						
Single-Study	Survey: Gratitude Disposition (measured via GQ-VI)	Self-reported and Teacher-reported Prosocial Behaviours	2015	243	0.133	0.0073/0.2546
Ma, Tunney & Ferguson (2014)						
Single-Study	Economic Games: One-shot Variant of Trust Game (TG)	Willingness to Reciprocate (Watkins et al., 2006)	2014	61	0.810	0.7012/0.8819
Ma, Tunney & Ferguson (Unpublished Study 1)						
Single-Study	Economic Games: One-shot Variant of Trust Game (TG)	Cooperation: Percentage of Repayment	2014	135	0.150	-0.0421/0.3317
Ma, Tunney & Ferguson (Unpublished Study 2)						
Single-Study	Economic Games: A repeated version (i.e. Ten Trials, Multiple Roles) of the TG used in Ma et al. (2014)	Decisions to Help (at 'Helper' Trials)/ Percentage of Repayment (at 'Recipient' Trials)	2015	133	0.097	-0.0799/0.2673
Markowitz (2012)						
Study 1	Survey: Gratitude Disposition (measured via GQ-VI)	Responsibility Toward Future Generation (RTFGs)	2012	551	0.360	0.2850/0.4306
Study 2a	Vignette: Story about Past Generation's Contribution (or the lack thereof) to the Transition of the current Fuel-efficient economy	Willingness to Impose an Increase in Taxes on Gasoline	2012	413	-0.011	-0.1074/0.0856
Study 2b	Survey: Gratitude Disposition (measured via GQ-VI)	RTFGs Rating	2012	413	0.210	0.1159/0.3004
Study 3	Vignette: Story about Past Generation's Positive (or Negative) Intent to help Maintain the National Parks for Future Generations	Donation to the National Park Foundation	2012	273	0.175	0.0575/0.2878
McCullough, Emmons & Tsang (2002)						
Study 1	Survey: Peer- and Self-reported Gratitude Disposition (GQ-VI)	Peer-reported Prosocial Behaviours/Tendencies	2002	238	0.324	0.2052/0.4333
Michie (2009)						
Single-Study	Survey: Self-Reported Gratitude toward Subordinates (GQ-VI)	Subordinates' Rating of Supervisors' Prosociality	2009	71	0.247	0.0149/0.4545
Mikulincer & Shaver (2010)						
Single-Study	Experiment: Conferment of Benefits (Bartlett & Desteno, 2006)	Time Spent on Helping (Bartlett & Desteno, 2006)	2010	80	0.430	0.2322/0.5936
Morales (2005)						
Study 2	Vignette: Story about a new luggage Store showing very neat, interesting displays (i.e. 'High-effort' Condition) as opposed to just keeping their displays organised (i.e. 'Low-effort' Condition)	Likelihood of Visiting that Store	2005	88	0.273	0.0677/0.4566
Naito & Sakata (2010)						
Study 1	Vignette: A hypothetical scenario where a protagonist—who lives alone and injured—was helped by a same-sex friend for an extended period of time (Naito et al., 2005)	Enhancement of Prosocial Motivation (e.g. 'More than before, would you want to help your friend, if she were distressed in a similar situation'?)	2010	135	0.386	0.2321/0.5209
Naito, Wangwan & Tani (2005)						
Study 1- Japan	Vignette: Story about an injured protagonist being helped by his/her Parents, Best Friend or a Stranger for an extended period of time	Enhancement of Prosocial Motivation/ Requital: Giving and Verbal-Facial Expression of Gratitude	2005	212	0.446	0.2691/0.5930
Study 1- Thailand	Same as above	Same as above	2005	284	0.418	0.2699/0.5460
Palmatier, Jarvis, Bechkoff, & Kardes (2009)						
Study 1	Survey: Customer Gratitude (measured via GQ-VI)	Customer Purchase Intention/ Customer Commitment	2009	155	0.587	0.4729/0.6814
Study 2	Survey: Gratitude-based Reciprocal Motives	Share of Wallet/ Customer Commitment	2009	446	0.332	0.2466/0.4120

Romani, Grappi & Bagozzi (2013)						
Single-Study	Vignette: Story about a hypothetical business organisation engaging in Corporate Social Responsibility (CSR) activities	Positive Word-of-Mouth/ Advocacy Behaviours	2013	188	0.550	0.4417/0.6425
Rubin (2012)						
Study 1	Economic Games: Iterated Ultimatum Game (UG)	Endowment (USD $100) to Offer to Partner in Next Trial	2012	52	0.050	-0.2260/0.3186
Study 2	Economic Games: Give-Some Dilemma Game (GSDG)	Tokens Given: Give-Some Dilemma Game (GSDG)	2012	96	0.218	0.0186/0.4012
Siegel, Thomson & Navarro (2014)						
Study 2	Experiment: Recall of an instance of being Generously Treated	Donation Behaviours to Charity (i.e. Toys for Tots)	2014	373	0.036	-0.1057/0.1767
Simon (2013)						
Single-Study	Survey: Customer Gratitude Survey (Goei & Boster, 2005)	Repurchase Intent (Maxham & Netemeyer, 2003)	2013	148	0.645	0.5403/0.7306
Soscia (2007)						
Single-Study	Vignette: Stories on various protagonists' Consumption Experience	Positive Word-of Mouth/ Repurchase Intent	2007	182	0.725	0.6484/0.7878
Spence, Brown, Keeping & Lian (2014)						
Study 2	Survey: GQ-VI and State Gratitude Scale (SGS)	Organizational Citizenship Behaviour (OCBs)	2014	67	0.145	-0.0431/0.3240
Study 3	Survey: GQ-VI and State Gratitude Scale (SGS)	Supervisor-, Co-worker-, Organisation-targeted OCBs	2014	104	0.196	0.0500/0.3332
Tian, Chu & Huebner (2016)						
Single-Study	Survey: Gratitude Disposition (measured via GQ-VI)	Prosocial Behaviour Questionnaire (Feng, 2009)	2016	324	0.520	0.4381/0.5933
Tian, Du & Huebner (2015)						
Single-Study	Survey: Gratitude Disposition (measured via GQ-VI)	Prosociality Scale (Zhang, Zeng, & Yu, 2004)	2015	706	0.267	0.1600/0.3552
Tsang (2006a)						
Single-Study	Experiment: Conferment of Benefits	Resource Distribution (i.e. Money Given to Partners)	2006	40	0.550	0.2879/0.7356
Tsang (2007)						
Single-Study	Experiment: Conferment of Benefits (Tsang, 2006a)	Resource Distribution (Tsang, 2006a)	2007	149	0.572	0.4530/0.6712
Tsang, Schulwitz ,& Carlisle (2012)						
Single-Study	Experiment: Conferment of Benefits (Tsang, 2006a)	Resource Distribution (Tsang, 2006a)	2012	80	0.270	0.0541/0.4616
Wangwan (2014)						
Single-Study-High School	Vignette: Same as the ones used by Naito and Associates (Naito & Sakata, 2010, Naito et al., 2005)	Enhancement of Prosocial Motivation (Naito et al., 2005; Naito & Sakata, 2010)	2014	414	0.120	0.0239/0.2139
Single-Study-Undergrad			2014	191	0.109	-0.0334/0.2472
Watkins, Scheer, Ovnicek & Kolts (2006)						
Study 1	Vignette: Story about a protagonist's receipt of a large and unexpected favour (i.e. moving apartment) from a friend	Prosocial Action Thoughts and Tendencies (i.e. PATT, Frijda, 1986, 1988)	2006	107	0.430	0.2618/0.5733
Study 2	Vignette: Story about a protagonist's receipt of a small favour	PATT/Self-reported Altruism	2006	152	0.362	0.2147/0.4924
Wetzel, Hammerschmidt, & Zablah (2014)						
Study 1	Survey: Customer Gratitude Survey (Palmatier et al., 2009)	Sales Growth	2014	192	0.160	0.0207/0.2932
Study 2	Survey: Customer Gratitude Survey (Palmatier et al., 2009)	Sales Growth	2014	302	0.360	0.2576/0.4544

Xia & Kukar-Kinney (2013)						
Study 1	Vignette: Story about a bank willing (or unwilling) to waive a credit card penalty fee that was or (was not) the protagonist's fault	Loyalty— i) Purchase Intention (Lam, Shankar, Eramilli & Murthy, 2004), and ii) Advocacy (Lam et al., 2004); Future Compliance (Xia & Kukar-Kinney, 2013)	2013	290	0.571	0.4879/0.6438
Study 2	Vignette: Story about a protagonist who never had a late payment (or was late several times) missing the deadline by a day (or a month), and the bank was (or was not) willing to waive that charge.		2013	381	0.502	0.4226/0.5734
Study 3	Vignette: Story about a participant who contacted the bank to drop a credit card late fee, and was told the bank would refund them (or compensate them with reward points). Protagonists were then told this was a preferential (or a casual) arrangement by the bank.	Purchase Intention and Advocacy (Lam et al., 2004)	2013	225	0.412	0.2977/0.5154
Xia & Kukar-Kinney (2014)						
Study 2	Survey: Customer's Gratitude toward Preferential Treatment he/she received in the past (via GQ-VI)	Positive Word-of-Mouth (Lacey, Suh & Morgan, 2007)/ Subsequent Purchases (Lam et al., 2004)	2014	206	0.248	0.1151/0.3720
Xie & Bagozzi (2014)						
Single-Study	Vignette: Stories of a Norwegian firm's Corporate Ethical ('Positive Narrative')/Unethical ('Negative Narrative') Actions	Consumer Support for Non-profits	2014	210	0.267	0.1368/0.3886
Xie, Bagozzi, & Grønhaug (2015)						
Single-Study	Vignette: Narrative Scenarios of Corporate Environmental Irresponsibility (vs. Responsibility vs. Control)	Positive Word of Mouth/ Likelihood to Invest in the company	2015	70	0.480	0.2762/0.6425
Yang, Stoeber, & Wang (2015)						
Study 3	Vignette: The protagonist received help from a friend.	Willingness to help that friend in return	2015	165	0.493	0.3679/0.6005
Zhao (2010)						
Study 1	Survey: Gratitude Disposition (measured via GQ-VI)	5-item Helping Tendencies Checklist	2010	381	0.520	0.4427/0.5897
Study 2a	Vignette: Receipt of an unexpected birthday gift from a friend	Prosocial Motivation/ Magnitude of Reciprocation	2010	123	0.406	0.2471/0.5442
Study 2b	Vignette: Receipt of help from a friend	Prosocial Motivation/ Magnitude of Reciprocation	2010	126	0.294	0.1255/0.4460
Random Effects Model				**18,342**	**0.3735**	**0.3287/0.4166**

Table 2.3. Results of the Univariate Categorical Analyses on the *Gratitude* and *Prosociality* Effect Size (*Mixed-Effects* Model used)

	Q	*p-value (two-tailed)*	*k*	*N*	Tau-squared	Standard Error	*Correlation r*	95% Lower Limit / Upper Limit Confidence Intervals
Reciprocity vs. Non-Reciprocity	10.135	.0015**						
Non-Reciprocity			16	3,957	0.024	0.024	0.247	0.165/0.325
Reciprocity			76	14,518	0.060	0.060	0.397	0.346/0.446
Nature of Reciprocity	6.463	.0110**						
Direct			53	9,114	0.056	0.014	0.435	0.378/0.489
Indirect			16	4,075	0.052	0.023	0.282	0.170/0.386
Nature of Indirect Reciprocity	6.655	.0099**						
Upstream			9	2,598	0.020	0.013	0.147	0.043/0.247
Downstream			5	1,003	0.088	0.071	0.484	0.253/0.663
Social Relationships (Lambert et al., 2009)	14.807	.0001***						
Benefit-Triggered Gratitude			69	12,313	0.062	0.014	0.415	0.362/0.466
Generalized Gratitude			19	5,460	0.025	0.011	0.240	0.165/0.312
Country of Participation	1.685	.4306						
North America			55	10,753	0.057	0.015	0.350	0.289/0.408
Asia			23	5,164	0.046	0.017	0.399	0.318/0.474
Western Europe			13	2,425	0.065	0.031	0.425	0.298/0.536
Gratitude Measure	10.253	.0014**						
Disposition			24	5,988	0.035	0.035	0.248	0.171/0.322
State/Mood			77	14,206	0.060	0.060	0.393	0.343/0.441
Type of Study	0.145	.7037						
Lab Studies			59	9,449	0.050	0.050	0.367	0.312/0.419
Cross-sectional Survey			32	8,893	0.058	0.058	0.385	0.308/0.456
Gratitude Induction	1.528	.2164						
Experiment/ Economic Games			34	4,260	0.038	0.013	0.335	0.267/0.399
Vignette			25	5,189	0.059	0.021	0.403	0.316/0.484
Objective/Subjective Prosociality	3.101	.0782						
Objective			30	4,077	0.028	0.010	0.318	0.254/0.380
Subjective			62	14,400	0.060	0.013	0.393	0.337/0.446
Target of Prosociality	2.756	.0969						
Group			34	8,478	0.072	0.020	0.431	0.352/0.504
Individual			44	6,082	0.019	0.007	0.355	0.309/0.398
Proxy/ Actual Gratitude Measure	0.661	.4161						
Actual			82	15,838	0.055	0.011	0.371	0.323/0.417
Proxy			14	3,034	0.047	0.023	0.418	0.312/0.513
Published?	0.429	.5123						
Published			72	14,857	0.054	0.012	0.381	0.331/0.429
Unpublished			19	3,485	0.055	0.025	0.344	0.240/0.440
Zero-order statistics used?	2.408	.1207						
Yes			74	14,337	0.052	0.011	0.388	0.340/0.435
No			18	4,403	0.049	0.021	0.303	0.200/0.399

Note. *k* = Number of studies; *N* = Total number of participants involved; *Q* = Between-group Effect; ** p <.01 (two-tailed); *** p <.001 (two-tailed).

2.3.2. Moderator Analyses

Table 2.3 above shows the results of the univariate moderator analyses. Below I explored every of the *theoretical* or *methodological* moderators.

2.3.2.1. *Reciprocity* versus *Non-Reciprocity*

I coded if the *prosociality* measures reported were *reciprocity* or *non-reciprocity* driven. The results indicated a statistically significant difference, Q (1) = 10.135, p = .0015, with studies which assessed *reciprocal* prosocial outcomes (r = 0.397, LLCI/ULCI= 0.346/ 0.446, k = 76) resulting in a stronger link between *prosociality* and *gratitude* than did studies which focused on *non-reciprocal* prosocial outcomes (r = 0.247, LLCI/ULCI= 0.165/ 0.325, k = 16).

2.3.2.2. Reciprocity Nature: *Direct* versus *Indirect Reciprocity*

Outcomes were coded as either *direct* or *indirect* reciprocity. The results showed that while in both cases the associations were significant, studies that examined *direct* reciprocity (r = 0.435, LLCI/ULCI = 0.378/ 0.489, k = 53) had a stronger association between *gratitude* and *prosociality*, than studies that examined *indirect* reciprocity (r = 0.282, LLCI/ULCI= 0.170/ 0.386, k = 16), Q (1) = 6.463, p = .0110. This indicates that gratitude is a stronger predictor of prosociality in the context of *direct* rather than *indirect* reciprocity.Furthermore, it was worth noting that there are similar effect sizes, Q (1) = 0.259, p = .611, for studies that assessed *non-reciprocal* prosociality (r = 0.247, LLCI/ULCI = 0.165 / 0.325, k = 16) and those which examined *indirect* reciprocity (r = 0.282). Studies which measured *direct* reciprocity (r = 0.435), meanwhile, reported a significantly larger effect, Q (1) = 14.77, p <.001, than studies whose outcomes were *non-reciprocal*. This might suggest the moderating effect of

reciprocity versus non-reciprocity on the *gratitude-prosociality* association may be attributable to the *direct* instead of the *indirect* reciprocal exchanges.

2.3.2.3. *Upstream* versus *Downstream Indirect* Reciprocity

While the associations with both *upstream* (r = 0.147, LLCI/ULCI = 0.043/ 0.247, k = 9) and *downstream* (r = 0.484, LLCI/ULCI = 0.253/ 0.662, k = 5) indirect reciprocity were significant, the association was significantly stronger for *downstream indirect* reciprocity, Q (1) = 6.655, p = .0099.

2.3.2.4. *Social Relationships*

I coded whether *gratitude* was generated by a *benefit-triggered* relationship or *generalized* gratitude. The results revealed a significant difference, Q (1) = 14.807, p = .0001. While both associations were significant, the *benefit-triggered* gratitude (r = 0.415, LLCI/ULCI = 0.362/ 0.466, k = 69) resulted in a significantly larger association than *generalized* gratitude (r = 0.240, LLCI/ULCI = 0.165/ 0.312, k = 19).

2.3.2.5. *Continent* of Participation, *Religiosity* and *Collectivism*

I coded the continents in which the studies were administered and examined if this moderated the link between *gratitude* and *prosociality*. The majority of the studies reported a moderate positive relation between gratitude and prosociality with studies from *Western Europe* (r = .425, LLCI/ ULCI = 0.298/0.536, k = 13) having the largest effect size, then *East Asia* (r = 0.399, LLCI/ ULCI = 0.318/0.474, k = 23) followed by *North America* (r = 0.350, LLCI/ ULCI = 0.289/0.408, k = 55). However, there was no significant moderating effect of continent, Q (2) = 1.685, p = .4307. Regression analyses showed that effect-size estimates did not vary as a function of *religiosity* (β = -0.0012, p = .416) or *collectivism* (β = -0.0008, p = .408) within each country.

2.3.2.6. *Gratitude* Measure: *State* versus *Disposition*

I coded whether the type of *gratitude* examined in the studies was referred to as a *state/mood* or as a *disposition*. The data revealed that the type of gratitude measures did result in different effect sizes, Q (1) = 10.253, p = .001, with the *gratitude-prosociality* association stronger for the studies which reported *state/mood* gratitude measures (r = 0.393, LLCI/ULCI = 0.343/ 0.441, k = 77) than studies that examined *dispositional* gratitude (r = 0.248, LLCI/ULCI = 0.171/ 0.322, k = 24).

2.3.2.7. *Type* of Study *and Gratitude Induction*

I examined whether studies that were classified as *Laboratory* Studies or *Survey/Field* studies had different *gratitude-prosociality* effect sizes. As displayed in Table 2.3, *gratitude* was significantly associated with *prosociality* whether it was *lab*-based (r = .367, LLCI/ULCI = 0.312/ 0.419, k = 59) or a *survey/field study* based (r = .385, LLCI/ULCI = 0.308/ 0.456, k = 32). However, whether the study was *lab-based* or a *Survey/Field* studies did not moderate the effect estimates, Q (1) = 0.145, p = .7037.

I then examined whether the way through which gratitude was induced within the *59 lab-based* studies moderated the relationship between *gratitude* and *prosociality*. It was anticipated that studies that were *experimental/economic game* based would yield a stronger effect-size than *vignettes*. The results, demonstrated that that *gratitude-prosociality* association was significant for both vignettes (r = .403, LLCI/ ULCI = 0.316/ 0.484, k = 25) and experimental/economic games (r = .335, LLCI/ ULCI = 0.267/ 0.399, k = 34), but the two *did not differ significantly*, Q (1) = 1.528, p = .2164.

2.3.2.8. *Objective* versus *Subjective* Prosociality

I coded if the prosociality measure adopted was *objective* (r = 0.318, LLCI/ULCI = 0.254/ 0.380, k = 30) or *subjective* (r = 0.393, LLCI/ULCI = 0.337/ 0.446, k = 62). While both effects are significant the results revealed no significant moderating effect, Q (1) = 3.101, p = .0782.

2.3.2.9. *Target* of Prosociality: *Individual* versus *Group*

I coded if the prosocial behaviours or behavioural tendencies reported were *individually*- or *group*-targeted. The associations between gratitude and prosociality were significant for *group*-directed prosociality (r = 0 .431, LLCI/ ULCI = 0.352/0.504, k = 34), and for the *individually*-targeted prosociality (r = 0.355, LLCI/ ULCI = 0.309/0.398, k = 44). However, these associations were not significantly different from each other, Q (1) = 2.756, p = .0969.

2.3.2.10. *Proxy* versus *Actual* Gratitude Measure

I coded whether *proxy* or *actual* gratitude measures were used. While associations were significant for both proxy (r = 0.418, LLCI/ULCI = 0.312/ 0.413, k = 14) and actual measures (r = 0.371, LLCI/ULCI = 0.323/ 0.417, k = 82), these were not different from each other, Q (1) = 0.416, p = .4161.

2.3.2.11. *Times Cited*, *Years of Publications*, and *Age*

I considered whether a study was frequently cited would have a bearing on its reported effect size. The results demonstrated an absence of significant moderating effect by times cited (β = -0.0002 , p = .63). Categorical comparison between effect sizes from the *published* (r = 0.381, LLCI/ULCI = 0.331/ 0.424, k = 72) and *unpublished* studies (r = 0.344, LLCI/ULCI = 0.240/ 0.440, k = 19) indicated no significant difference , Q (1) = 0.429, p = .5123. There was no effect of year of publication or the year in which the studies were conducted (β = -0.0038 , p = .50). There was no effect of *age* either (β =

0.0034 , p = .17) In short, these results showed that the effect estimates was not affected by how frequently cited the studies were, whether the studies were published, when the studies were conducted, and how old the participants were.

2.3.3. *Dispositional* Gratitude-*Prosocial Disposition* Associations

Table 2.4 details the effect size estimates for the association between indices of *dispositional gratitude* and other *prosocial dispositions*. The Egger's Intercept of 0.293 (95% LLCI/ ULCI= -1.930, 2.514, p = .787 (two-tailed)) revealed no publication bias. The *Random-Effect* Trim-and-Fill analysis indicated zero imputed studies in the current sample, resulting in no change in the effect estimate. There were 24 studies with 103 effect sizes with a total N of 7,677. The overall effect size was positive and significant (r = 0.288, p <.001). Thus, while *dispositional gratitude* and other prosocial *dispositions* are associated this effect is small, and that *dispositional gratitude* cannot be considered as synonymous with a general prosocial *disposition*.

2.3.4. *Gratitude*, *Other Prosocial Emotions* and Differential Predictive Power

Tables 2.5 and 2.6 detail the individual and overall effect estimates for the *pride*-prosociality and *anger*-prosociality links respectively. For *pride* there were twenty-three (k = 23) studies with a total N of 4,509 and 96 effect sizes. The effect was positive and significant (r = .212, p <.001) but smaller than that of the *gratitude*-prosociality link. There was also no evidence of publication bias with a non-significant (p = .257) Eggers' intercept (-2.248, LLCI/ ULCI = -6.257, 1.761). The Random-Effect Trim-and-Fill analysis filled in three studies with imputed effects *larger* than the mean effect estimates, resulting in a slightly *higher* post-adjusted effect estimates (r = 0.250, LLCI/ ULCI: 0.156/0.339) than the initial estimate (r = 0.212, LLCI/ ULCI: 0.114/0.306).

Table 2.4. Studies on the *Gratitude-Prosociality Disposition* Relationship ($k = 24$, $N = 7{,}677$)

Studies	Gratitude Measures	Prosociality Disposition Measures	Years	*N*	*r*	95% LL/ ULCI
Chan (2013)	Dispositional Gratitude	Heartland Forgiveness Scale (Others, Self and Situations)	2013	143	0.420	0.275/0.546
Datu (2014)	Dispositional Gratitude	Heartland Forgiveness Scale (Others, Self and Situations)	2014	210	0.352	0.227/0.465
DeShea (2003)						
Study 2	Dispositional Gratitude	Willingness to Forgive (WTF) Scale/ Transgression-Related Interpersonal Motivations (TRIM)/ Transgression Narrative Test of Forgiveness (TNTF)	2003	42	0.349	0.051/0.590
Dwiwardi et al. (2014)	Dispositional Gratitude	Heartland Forgiveness Scale (Others, Self and Situations)	2014	245	0.490	0.389/0.580
Hill & Allemand (2011)	Dispositional Gratitude	Big Five Factors (BFF)-Agreeableness and Conscientiousness/ Tendency to Forgive Scale (Brown, 2003)	2011	927	0.214	0.152/0.275
Kruger (2011)	Dispositional Gratitude	Dispositional Empathy/ Heartland Forgiveness Scale (Others)	2011	113	0.292	0.113/0.452
*Krumrei-Mancuso (2016)	Dispositional Gratitude	Dispositional Empathy/ Benevolence Subscale (Schwartz,1992)	2016	314	0.334	0.232/0.429
McCullough et al. (2002)						
Study 1	Dispositional/Mood Gratitude	BFFs- Agreeableness and Conscientiousness/ Dispositional Empathy	2002	877	0.283	0.183/0.377
*Study 2	Dispositional Gratitude	BFFs- Agreeableness and Conscientiousness/ Disposition to Forgive	2002	1,228	0.359	0.309/0.406
Study 3	Dispositional Gratitude	BFFs- Agreeableness and Conscientiousness/ Dispositional Empathy	2002	156	0.314	0.165/0.449
Miley & Spinella (2006)	Dispositional Gratitude	Dispositional Empathy/ Heartland Forgiveness Scale (Others)	2006	154	0.245	0.090/0.388
Neto & Menezes (2014)	Dispositional Gratitude	Forgivingness Scale (Lasting Resentment, Sensitivity to Circumstances, and Unconditional Forgiveness) (Mullet et al., 2003)	2014	147	0.200	0.040/0.351
Neto (2007)	Dispositional Gratitude	BFFs- Agreeableness and Conscientiousness/ Forgivingness Scale (Mullet et al., 2003)	2007	152	0.280	0.126/0.421
Rey & Extremera (2014)	Dispositional Gratitude	BFFs- Agreeableness and Conscientiousness/ TRIM	2014	535	0.139	0.055/0.221
Rye et al. (2012)						
Pre-test assessment	Dispositional Gratitude	Forgiveness Scale (Rye et al., 2001)	2012	99	0.322	0.133/0.489
Sandage & Williamson (2010)	Dispositional Gratitude	Disposition to Forgive (McCullough et al., 1997)	2010	203	0.270	0.137/0.393
Satici, Uyal & Akin (2014)	Dispositional Gratitude	Dispositional Forgiveness	2014	331	0.430	0.338/0.514
Smith (2012)	Dispositional Gratitude	BF- Agreeableness/ Heartland Forgiveness Scale (Others,Self, Situations)	2012	191	0.075	-0.067/0.215
Strelan (2007)	Dispositional Gratitude	Heartland Forgiveness Scale (Others,Self)	2007	275	0.365	0.258/0.463
Szczesniak & Soares (2011)	Dispositional Gratitude	TRIM- Motivation to avoid and seek vengeance (reverse-scored)	2011	338	0.281	0.180/0.377
Toussaint & Friedman (2009)	Dispositional Gratitude	Heartland Forgiveness Scale/ TRIM- Avoidance and Revenge	2009	71	0.469	0.264/0.633
Wilks, Neto & Mavroveli (2015)	Dispositional Gratitude	Forgivingness Scale (Mullet et al., 2003)	2015	327	0.143	-0.017/0.296
Wood, Joseph & Maltby (2008)	Dispositional Gratitude	BFFs- Agreeableness and Conscientiousness	2008	398	0.138	0.041/0.233
Wood, Joseph & Maltby (2009)	Dispositional Gratitude	BFFs- Agreeableness and Conscientiousness	2009	201	0.177	0.040/0.308
Random-Effects Model				**7,677**	**0.288**	**0.242/0.332**

*denotes studies that were included in the current main analysis

Table 2.5.Studies on the *Pride-Prosociality* Relationship ($k = 23$, $N = 4{,}509$)

Studies	Pride Measures	Prosociality Measures	Years	*N*	*r*	95% LL/ ULCI
*Soscia (2007)	Experimental Induction	Intent to Repurchase/ Positive Word-of-Mouth	2007	182	0.271	0.130/0.400
*Michie (2009)	Authentic Pride	Supervisor's Prosociality- Social Justice/ Altruism	2009	71	0.247	0.015/0.454
de Hooge (2014)						
*Study 1	Experimental Induction	Money to be spent on a gift/ Total Gift-giving	2014	271	0.230	0.114/0.340
*Study 4	Experimental Induction	Money spent / Total Gift-giving/ Tine spent on gift-search	2014	138	0.290	0.129/0.436
Study 5	Experimental Induction	Money spent / Total Gift-giving/ Tine spent on gift-search	2014	243	0.152	0.027/0.273
Study 6	Experimental Induction	Money spent / Total Gift-giving/ Tine spent on gift-search	2014	242	0.109	-0.018/0.232
Verbeke, Belschak, & Bagozzi (2004)						
Study 1	Authentic Pride	Organizational Citizenship Behaviours (OCBs)	2004	93	0.282	0.083/0.459
Gouthier & Rhein (2011)	Organizational Pride	Customer Services Commitment	2011	733	0.535	0.481/0.585
van der Schalk, Bruder & Manstead (2011)						
Study 1	Fairness (vs. Unfairness)-induced Pride	Money shared with 'Responder'	2011	210	0.061	-0.075/0.195
Study 2	Fairness (vs. Unfairness)-induced Pride	Money shared with 'Responder'	2011	132	0.028	-0.432/0.198
Bureau, Vallerand, Ntoumanis, & Lafreniere (2013)						
Study 2	Authentic and Hubristic Pride	Self-reported Moral Behaviours	2013	296	-0.077	-0.189/0.038
van Leeuwen, van Dijk, & Kaynak (2013)						
Study 1	Collective Pride	Helping of disadvantaged outgroup members	2013	67	0.432	0.214/0.609
Study 2	Collective Pride	Helping of disadvantaged outgroup members	2013	61	0.119	-0.027/0.164
Antonetti & Maklan (2014)						
Study 1	Pride from making purchases with a sustainable brand	Intention to purchase	2014	415	0.069	-0.027/0.164
Study 2	Pride from making purchases with a sustainable brand	Intention to purchase	2014	135	0.149	-0.021/0.310
Dorfman, Eyal & Bereby-Meyer (2014)						
Study 1	Experimental Induction	Fishing Game: Amount of Fishes Returned	2014	83	0.246	-0.006/0.468
Study 2	Experimental Induction	Fishing Game: Amount of Fishes Returned	2014	85	0.199	-0.013/0.393
Cavanaugh, Bettman & Luce (2015)						
Study 3	Experimental Induction	Distant-Others Helping	2015	176	-0.064	-0.209/0.085
Study 4	Experimental Induction	Distant-Others/ Close-Others Helping	2015	206	0.054	-0.083/0.189
Etxebarria, Ortiz, Apodaca, Pascual & Conejero (2015)						
Study 1	Experimental Induction Induction/ Trait Moral Pride	Time Spent on Helping Others	2015	94	0.444	0.173/0.652
Study 2	Dispositional Moral Pride	Time Spent on Helping/ Self-reported Habitual Prosociality	2015	77	0.261	-0.035/0.515
Helm, Renk, & Mishra (2015)	Brand Pride	Brand Citizenship Behaviours	2015	283	0.580	0.497/0.652
Krettenauer & Casey (2015)	Authentic and Hubristic Pride	Self-reported Helping Behaviours	2015	216	0.165	0.033/0.292
Random-Effects Model				**4,509**	**0.212**	**0.114/0.307**

*denotes studies that were included in the current main analysis

Table 2.6. Studies on the *Anger-Prosociality* Relationship (*k* = 38, *N* = 7,857)

Studies (*k* = 38)	**Anger Measures**	**Prosociality Measures**	**Years**	***N***	***r***	**95% LL/ ULCI**
Polman & Kim (2013)						
Study 1	Induction	Contribution in a Public Goods (PG) Dilemma Game	2013	145	-0.416	-0.542/-0.272
Study 3	Induction	Contribution in a Public Goods (PG) Dilemma Game	2013	194	-0.315	-0.436/-0.182
Roberts et al. (2014)						
Study 1	Trait Anger	Friendly Behaviours/ Willingness to Comply with others	2014	99	-0.529	-0.658/-0.371
Seip et al. (2014)						
Study 1	Induction	Costly Non-Cooperator Punishment: Public Goods Games	2014	81	0.377	0.173/0.550
Study 2	Induction	Costly Non-Cooperator Punishent: Sequential Trust Game (STG)	2014	88	0.492	0.307/0.641
Study 3	Induction	Costly Non-Cooperator Punishent: Sequential Trust Game (STG)	2014	38	0.234	-0.092/0.515
Jordan et al. (2015)						
Study 1	Induction	Third-Party Punishment (TPP)	2015	323	0.341	0.240/0.434
Study 2	Induction	Third-Party Punishment (TPP)	2015	96	0.531	0.370/0.661
Gummerum et al. (2016)						
Study 1	Incidental Anger	Third-Party Punishment (TPP)	2016	137	0.265	0.102/0.414
Study 2	Incidental Anger	Third-Party Compensation (TPC)	2016	137	-0.185	-0.342/-0.017
Study 3	*Moral Outrage* versus *Personal Anger*	Third-Party Compensation (TPC)	2016	139	0.166	-0.001/0.323
Landmann & Hess (2016)						
Study 1	Moral Outrage	Third-Party Punishment (TPP) and Compensation (TPC)	2016	136	0.227	0.061/0.381
Study 2	Moral Outrage	Third-Party Punishment (TPP) and Compensation (TPC)	2016	85	0.216	0.003/0.410
O'Reilly et al. (2016)						
Study 1a	Moral Outrage	Third-Party Punishment (TPP) Intention	2016	164	0.560	0.445/0.657
Study 1b	Moral Outrage	Third-Party Punishment (TPP) Intention	2016	136	0.235	0.069/0.388
Study 2	Moral Outrage	Third-Party Punishment (TPP) Intention	2016	409	0.391	0.306/0.470
*Montada & Schneider (1989)	Moral Outrage	Intention to Execute Prosocial Activities	1989	823	0.400	0.341/0.456
*Vitaglione & Barnett (2003)						
Study 4	State and Dispositional Empathic Anger	Intention to Help Victim/ Punish Transgressors	2003	191	0.405	0.279/0.517
*Iyer, Schmader & Lickel (2007)						
Study 1	Moral Outrage	Intention to Compensate, Advocate Withdrawl etc.	2007	194	0.559	0.454/0.649
Study 2	Moral Outrage	Intention to Compensate, Advocate Withdrawl etc.	2007	170	0.386	0.250/0.507
*Wakslak et al. (2007)						
Study 1	Moral Outrage	Support for resource redistribution for under-represneted groups	2007	108	0.476	0.316/0.610
Study 2	Moral Outrage	Willingness to help the disadvantaged	2007	120	0.386	0.223/0.529
*Halperin (2008)						
Study 3	Group-based Anger	Support for education to alter perceptions of outgroup	2008	847	0.370	0.310/0.427

*Halperin, Ruaawll, Dweck, & Gross (2011)						
Study 1	Inter-group Anger Induction	Support for Negotiation with Palestinians	2011	262	-0.010	-0.130/0.111
Study 2	Inter-group Anger Induction	Support for Negotiation with Palestinians	2011	262	0.030	-0.091/0.150
*Lotz, Okimoto, Schlosser, & Fetchenhauer (2011)						
Study 1	Moral Outrage	Intention to Help Victim/ Punish Transgressors	2011	178	0.330	0.192/0.455
*Tagar, Federico & Halperin (2011)						
Study 1	Anger towards Palestinians	Willingness to promote peaceful conflict resolution	2011	501	0.141	0.054/0.226
Study 2	Inter-group Anger Induction	Support of non-violent policies/ conflict resolution	2011	60	0.363	0.085/0.589
*Nelissen & Zeelenberg (2009)						
Study 1	Anger towards Norm Violatiors	Third-Party Punishment (TPP)	2009	91	0.178	-0.029/0.371
Study 2	Anger towards Unfair Allocators	Third-Party Punishment (TPP)	2009	89	0.467	0.287/0.616
**Soscia (2007)		Intent to Repurchase/ Positive Word-of-Mouth	2007	182	-0.280	-0.409/-0.141
**Rubin (2012)						
Study 1	Induction	Money to offer in the next Ultimatum Game trial	2012	52	-0.190	-0.440/0.087
Study 2	Induction	Contribution in a Give-some game	2012	96	-0.125	-0.317/0.078
**de Hooge (2014)						
Study 1	Induction	Money to be spent on a gift/ Total Gift-giving	2014	271	-0.479	-0.566/-0.381
Study 4	Induction	Money spent / Total Gift-giving/ Tine spent on gift-search	2014	138	-0.592	-0.691/-0.471
Study 5	Induction	Money spent / Total Gift-giving/ Tine spent on gift-search	2014	243	-0.260	-0.374/-0.139
Study 6	Induction	Money spent / Total Gift-giving/ Tine spent on gift-search	2014	242	-0.191	-0.309/-0.066
Drouvelis & Grosskopf (2016)	Induction	Public Goods Games Contribution/ Costly Prosocial Punishment	2016	330	-0.007	-0.170/0.156
Random-Effects Model				**7,857**	**0.136**	**0.027/0.242**

*denotes articles quoted in Vam Doorm et al.'s (2014) review as 'direct evidence' of the 'anger-prosocial behaviour' association.

**denotes studies included in the current main analysis

Table 2.6 above details the effect sizes for the *anger-prosociality* link based on 38 studies that altogether included 7,857 participants and 128 effect sizes. The Egger's test (intercept: -2.267, LLCI/ ULCI = -7.006/1.669, p = .220 (two-tailed)) revealed no publication bias. Nonetheless, the Random-effect Trim-and-Fill analysis filled in four studies with effect sizes *smaller* than the initial estimates, resulting in a *non-significant* post-adjusted effect estimate (r = 0.083, -0.0282/0.192, Q = 1028.11). However, as *anger* is related to two very distinct notions of prosociality (i.e. cooperation and norm enforcement) I included this as a moderator. This segregation was based upon *whether punishment was involved* (i.e., *Second-* and *Third-party punishment*) with studies examining '*Third-party Compensations*' (e.g. Study 2, Gummerum, Van Dillen, Van Dijk, & Lopez-Perez, 2016) coded as 'no-punishment'. The difference between studies that involved a *punishment* (k = 18, r = 0.370, 0.312/0.425) and those that did not (k = 27, r = 0.022, -0.121/0.157) was significant, Q (1) = 22.525, p <.001. Taken together, *anger* appears linked primarily to *norm-enforcing punishment* rather than direct *cooperation*.

Table 2.7 below highlights the comparison of the effect sizes derived from the analyses in this paper and meta-analyses reported by others. Overall *gratitude* has one of the largest effect sizes, with Positive Affect the largest. To directly test if the *gratitude-prosociality* link is stronger than either the *Positive Affect- prosociality* or *Negative Affect -prosociality* links I identified 69 studies (with 195 effect sizes) within the database that contain estimates of the associations between other prosocial emotions (*happiness*, *pride*, *anger, guilt*, *shame*) and prosociality that I could compare directly to estimates of the *gratitude-prosociality* association. The results are displayed in Table 2.8. I

combined all other prosocial emotions to differentiate *Positive Affect* (e.g. happiness, pride, elevation, interpersonal liking etc.) and *Negative Affect* (e.g. anger, guilt, sadness, shame combined). Gratitude had a significantly stronger association with prosociality than either *Positive Affect*, Q (1) = 3.928, p = .0475 (two-tailed) or *Negative Affect*, Q (1) = 49.77, p <.001 (two-tailed).

Table 2.7 Comparison of Effect Sizes for *Other Prosocial Emotions*

Emotions	***Authors***	***r***	***Notes***
Gratitude	This study	0.374	
Pride	This study	0.212	
Anger	This study	0.022	- Studies that examined Direct Cooperation (k = 27)
Anger	This study	0.370	- Studies that examined Norm Enforcement (k = 18)
Anger	This study	0.136	-Overall Strength of anger-prosociality (k = 38)
			Previous Meta-Analyses
Negative Affect	Carlson & Miller (1987)	0.268	*Negative Mood* (referred to as 'bad mood') with '*Helpfulness* measured within one hour of the mood-lowering event'
Negative Affect	Dalal (2005)	-0.100	*Negative Affect* and *Organizational Citizenship Behaviours* (OCBs) (defined as 'the intentional employee behaviour that is discretionary and not rewarded but that nonetheless improves functioning of the organisation (pp.1241)'). The *r* reported was the corrected coefficient
Positive Affect	Carlson et al. (1988)	0.540	*Positive Mood* (referred to as 'good mood (pp.213)') with the 'measurement of *Helpfulness* that took place within 30 minutes of the positive mood induction (pp.216)')
Positive Affect	Dalal (2005)	0.340	*Positive Affect* and *OCBs*. The *r* is the corrected coefficient.
Happiness	Lyubormirsky et al.(2005)	0.220	*Happiness* (defined as 'frequent experience of positive emotions (pp.820)') and *Prosocial Behaviour* (represented by 'volunteering', 'helping experimenter', 'donating blood' and so on) across *Experimental , Cross-sectional* and *Longitudinal* studies. The *r* is the calculated weighted overall effect based on their reported results.
Sadness	Carlson & Miller (1987)	0.082	Zero-order correlation between studies coded '*Sadness or Temporary Depression*' (defined as 'the extent to which subjects feel specifically downcast, sad, or depressed as a result of the negative mood induction') with the main effect estimates.
Shame	Leach & Cidam (2015)	0.180	*Shame* (defined as emotional 'experience of a failure to be moral, competent or socially appropriate') and *Prosocial Motivation or Behaviour* (defined as 'any motivation or behaviour intended to benefit another individual or group'). The *r* estimate is derived from the Hedge's *g* of 0.372.
Shame	Leach & Cidam (2015)	-0.060	*Shame* and *Cooperation or Affiliation* (defined as participants' i) cooperation, ii) negotiation, iii) preferences to spend time, or iv) completion of an interdependent task with another party). The *r* estimate is derived from a Hedge's *g* of -0.118.
Guilt	Carlson & Miller (1987)	0.500	Zero-order correlation between studies coded '*Guilt*' (defined as' bad feelings due to perceptions of having caused harm to someone else or otherwise having done something which they shouldn't have lost (pp.96)') with the main effect estimates.
Guilt	Boster et al. (2016)	0.260	Experimentally varied *Guilt* and *Compliance* (defined as behavioural compliance or helping behaviour which take the form of either an 'overt action' or a 'pledge to act (pp.56)')
Anger	Carlson & Miller (1987)	-0.187	Zero-order correlation between studies coded 'Anger' (defined as 'subjects' experience of anger as a result of the induction (pp.96)') with the main effect estimates

Table 2.8.Comparison of Gratitude-Prosociality Association to the Positive-Affect and Negative-Affect Prosociality Associations

	Effect Estimates				Effect Estimates			Differences in Correlation Strengths		
	r	LLCI/ ULCI	Tau^2		*r*	LLCI/ ULCI	Tau^2	*k*/ *N*	Q-value (df)	p-value (2-tailed)
Positive Affect-Prosociality	0.299	0.222/0.371	0.0417	*Gratitude*-Prosociality	0.417	0.3254/0.501	0.0750	28/ 6,479	3.927 (1)	0.0475*
Negative Affect-Prosociality	0.083	0.026/0.140	0.0286	*Gratitude*-Prosociality	0.377	0.320/0.432	0.0384	41/ 9,026	49.767 (1)	<.0001***

* *p* <. 05 (two-tailed); *** p <.001 (two-tailed); k: Number of studies included in the present comparisons; N: Number of Participants; r: Pearson's Correlation rs. Positive Affect (PA): 'Positive Affect/ General Positivity/ Happiness (k = 9)' (e.g. Bartlett and Desteno, 2006; Soscia, 2007); 'Amazement (k= 1)'and 'Love (k= 1)' (i.e. Exline, et al., 2012); 'Trust (k= 3)' (e.g. Kim and Lee, 2013); 'Status Elevation' (i.e. Wetzel et al., 2014); 'Empathy (k= 4)' (e.g. Simon, 2013); 'Humility (k= 3)' (e.g. Exline and Hill, 2012); 'Satisfaction (k= 7)' (e.g. Naito and Sakata, 2010; Zhao, 2010); 'Interpersonal Liking (k= 4)' (e.g. Study 1 and 2, Goei et al., 2007); 'Pride (k= 2)' (e.g. Michie, 2009); Negative Affect (NA): 'Anger/Annoyance' (e.g. Ma et al., 2014; Xie and Bagozzi, 2014); 'Guilt (k= 1)' (e.g. Palmatier et al., 2009); 'Disappointment (k= 1)' (i.e. Rubin, 2012); 'Entitlement (k= 6)' (e.g. Study 1 and 2, Xia and Kukar-Kinney, 2013); 'Indebtedness/Obligation (k= 27)' (e.g. Tsang, 2006, 2007; Study 1 and 2, Watkins et al., 2006), 'Mistrustful (k=1)' and 'Weak/Ashamed (k=1)' (i.e. Exline et al. (2012); 'Sadness/ Negative Affect/ General Negativity (k= 4)' (e.g. Soscia, 2007; Tian et al., 2016); 'Uneasy (k= 1)' (i.e. Rubin, 2012)

2.3.5. Meta-Regression

A meta-regression was run using the three main significant differential predictors from Table 2.3 (i.e. *reciprocity* versus *non-reciprocity*, *benefit-triggered* versus *generalized* gratitude, and *state* versus *trait* gratitude). The results shown in Table 2.9 indicate that only the *state-trait* distinction remained significant. When all predictors from Table 2.3 were included in the meta-regression again only the *state-trait* distinction remained significant.

Table 2.9.Results of the *Meta-Regression*

	B (SE)	95% (L, U)
Intercept	0.230 (0.071)**	0.091, 0.367
Benefit-triggered (1) vs. Generalized (0)	0.074 (0.102)	-0.125, 0.274
Reciprocity (1) vs. no reciprocity (0)	-0.028 (0.103)	-0.213, 0.174
State (1) vs. Trait (0)	0.157 (0.073)*	0.014, 0.230
R^2	0.05	

Note. * $p < .05$, ** $p < .01$.

2.3.6. Sensitivity Analysis

I explored the effect of whether the index of association was derived from a univariate, zero-order association ($r = 0.388$, LLCI/ULCI = 0.340/ 0.435, $k = 74$), or ones that were derived from higher order partials ($r = 0.303$, LLCI/ULCI = 0.200/0.399, $k = 18$). While both effects were significant, they did not significantly differ from each other, Q (1) = 2.41, p =.121 (Table 2.3).

2.4. Discussion

2.4.1. Summary of Evidence

Despite three decades of research on the link between *gratitude* and *prosociality*, there has been an absence of a quantitative synthesis of this link and a systematic exploration of moderators. In this meta-analytic review a

positive moderate association was shown between *gratitude* and *prosociality* (*r* = 0.374). The results further indicated that the *gratitude-prosociality* link was significantly stronger for (1) prosociality that involved a *direct reciprocal* rather than an *indirect reciprocal* exchange, (2), *downstream* versus *upstream indirect* reciprocity, (3) *state/mood* rather than *dispositional* gratitude was assessed, and (4) *benefit-triggered* gratitude in response to others' kindness rather than *generalized* gratitude about valued and cherished aspects of one's life. In the following I discuss the theoretical significance of the current findings and how some of these attributes could be incorporated into existing *gratitude intervention* (or training) regimens.

Before exploring these findings in more detail it is necessary to acknowledge the limitations of the present analyses. This should give the reader a cleaner framework to interpret the findings. I applied the Mixed-effects model in both the categorical moderator analyses and the meta-regression for the continuous moderators. As discussed previously the mixed-effects model is notorious for its over-conservativeness (Balliet & Van Lange, 2013; Lipsy & Wilson, 2001). Nevertheless, it is appropriate to adopt a Mixed-effects model because it assumes the existence of systematic variations in the effect sizes (i.e. moderators) alongside the random population variance (Voss, Kramer, Basak, Prakash, & Roberts, 2010), despite the potential for Type-II errors.

It should be noted that the studies reviewed were a mixture of experimental manipulations of gratitude or cross-sectional assessments. Thus, while it may not feasible to make any clear definitive statement regarding causality, I believe that the experimental work—that *exogenously manipulates gratitude*—provides some evidence that gratitude has a *causal role* with respect

to influencing prosocial behaviour. For causal statements where randomization has not been used causality can be inferred using propensity score matching (See Jackson, Thoemmes, Jonkmann, Ludtke, & Trautwein, 2012) or finding an instrumental variable (Shepherd, O'Carroll, & Ferguson, 2014), as long as the sample sizes are sufficiently large.

Similar to what has been addressed in Balliet, Li, Macfarlan, and Van Vugt (2011), another potential limitation of the current analysis could be attributable to the *sampling* of the studies and the *search strategies*. Although there were 16 countries represented in the sample ($k = 91$), the majority of the studies were from North America or Western Europe ($k = 68$). This could be due to the fact that I had confined the search to papers written in English only. In other words, although I analysed the effect of nationalities (or continent) and failed to observe any significant impact on the effect estimates, the current analysis may not generalise beyond Western, Educated, Industrialized, Rich and Democratic (WEIRD) societies (Henrich, Heine, & Norenzayan, 2010). With these limitations in mind, I now address the main *theoretical* (See Sub-section 2.4.2) and *practical* (See Sub-section 2.4.3) implications of the findings.

2.4.2. *Theoretical* Implications

The overall effect estimates arising from the present meta-analysis identified a positive medium-sized effect between *gratitude* and *prosociality* (Cohen, 1988). This confirms the general expectation in the literature that *gratitude* has the capacity to *engender prosocial exchanges* (McCullough et al., 2001; McCullough et al., 2008; McCullough & Tsang, 2004).

This effect emerges as significant regardless of (1) whether it was a *lab based* manipulation or *survey/field study* based, (2) whether it was based on an

objective or *subjective* estimate of *prosociality*, (3) whether it was targeted at individuals or groups, (4) the continent in which the research occurred, and (5) whether *gratitude* was assessed via a *proxy* or an *actual* gratitude measure, thus the *gratitude-prosociality* association is robust to a number of methodological moderators. This implies, for example, that subjective measures could be used, at least in early stages of research, to gauge effect sizes and test protocols.

Importantly, however, this association was significantly moderated by a number of key *theoretical* constructs. There were stronger associations between *gratitude* and *prosociality* in studies that examined *reciprocity-based prosocial* exchanges, compared to prosociality that did not involve reciprocity. This is consistent with the *moral motivator* function of gratitude (McCullough et al., 2002; McCullough et al., 2001) where gratitude may typify a *reciprocal* instead of a *benevolent* emotion (Yagil, 2015) such as *sympathy* (Wispé, 1986) or *empathy* (Scott, Colquitt, Paddock, & Judge, 2010). This is also consistent with the theory that *gratitude* underlies *reciprocity* (*direct*, *indirect upstream* and *indirect downstream*) (Nowak & Roch, 2007). In addition, I observed a significantly stronger association between *gratitude* and *reciprocity* for *direct* versus *indirect* reciprocal exchanges. That is, while the present data reiterates that gratitude-inspired reciprocity could take on either a direct or an indirect (both upstream and downstream) form of reciprocity (Nowak & Roch, 2007; Schaumberg & Flynn, 2009), gratitude may be more concerned with *direct reciprocal altruism* (McCullough et al., 2008; Trivers, 1971). This may reflect the clearer and stronger social exchange that takes place in *direct reciprocation* where gratitude may also trigger a sense of *obligation* and *indebtedness* (Wood

et al., 2016), both of which encourage a greater tendency to repay the initial favour (see Schaumberg & Flynn, 2009 for a review).

I also find that the association is stronger for *downstream* compared to *upstream* indirect reciprocity. This is of particular theoretical significance as some authors (e.g., Nowak & Roch, 2007) have tied *gratitude* specifically to *upstream* indirect reciprocity. The small *k*s, however, in this specific comparison warrant caution in interpreting this finding. However, it should be noted that in all form of reciprocal exchanges the association is significant.

The *gratitude-prosociality* link was stronger when gratitude was triggered in response to others' kindness (i.e. *benefit-triggered*) rather than as a *generalized* sense of gratitude (Lambert et al., 2009). I argue that this not only reflects a sense of reciprocity but also helps to build strong social ties. That is, returning favours to those we are close to emotionally or physically tends to strengthen the existing social bonds and create new ones which promote the survival of altruism (Preston, 2013). These bonds hold people together under times of need and hence epitomise how *gratitude* might facilitate *well-being* (Emmons & Mishra, 2011). This suggests, with respect to *reciprocal exchanges*, that *gratitude* may have a particularly important role to play with respect to the *evolution* and *maintenance* of *prosocial behaviour*.

However, with respect to the role of *gratitude*, it is important to distinguish exchanges that focus on family (kin) from those that focus on non-kin (McCullough et al., 2008; Nowak, 2006). It has been argued that for exchanges that involve family and relatives, where kin selection models apply, gratitude is less important than for exchanges that involve strangers (McCullough et al., 2008). Kin selection models link prosocial exchange to the

degree of genetic relatedness between individuals. With people showing differential preferable helping towards those to whom they are genetically closer than those who are not (see Nowak, 2006 for review). It is argued that in such kin-based exchanges the prosociality is primarily about maintaining your shared genes in the population. As such, *gratitude* is not needed to motivate *prosociality* in such context. The current data does not allow for this to be tested but it is an interesting and important hypothesis concerning the limits of the *gratitude-prosociality* association.

The present results suggested that both *dispositional* and *state* forms of gratitude motivate prosociality, although *dispositional* gratitude is a weaker motivator than *state* gratitude. The importance of both *dispositional* and *state* gratitude in facilitating prosociality has been extensively discussed in various models in the Social Psychology literature (e.g. Kubacka, Finkenauer, Rusbult, & Keijsers, 2011; McCullough et al., 2001; Wood et al., 2008). The current study does not allow me to test if indeed *state* gratitude mediates the *dispositional* gratitude-prosociality link. However, the finding which indicates that *dispositional* gratitude has a weaker link to prosociality than *state* gratitude does bare the signature of models whereby emotions mediate *dispositional* effects (Fredrickson et al., 2003). This is indeed the main mechanism of the Social Cognitive Model of Gratitude (SCMG) (Wood et al., 2008).

While the meta-regression indicated that the distinction between *state* and *disposition* is the one of primary importance, this does not mean that the *reciprocity* and *benefit-triggered* associations are irrelevant. Rather, I envisaged a *state* manifestation of gratitude that forms a complex around *reciprocity* and *benefit-triggered/generalized* gratitude to motivate prosocial exchanges.

Additionally, it is worth noting that *anger* has a unique role with respect to punishing non-cooperators to enhance cooperation, rather than fostering cooperation directly. This is an important observation as this association between *anger* and *altruistic punishment* has long been hypothesised, but this is first real systematic demonstration that there is a robust effect.

Finally, *dispositional gratitude* was significantly and positively associated with other *prosocial dispositions* (e.g., agreeableness), that are also known to be associated with prosocial behaviour (see Zhao & Smillie, 2015). Whether or not this link to other *prosocial dispositions* accounts for the current link between *disposition gratitude* and *prosociality* remains to be investigated, but these results clearly suggest that this is a possibility.

2.4.3. *Practical* Significance: Implications on *Gratitude Intervention*

Compared to the other *prosocial emotions* examined in this review, gratitude had the largest effect size and was a significantly stronger predictor of prosociality than either *Positive Affect* or *Negative Affect*. This suggests that *gratitude* constitutes a decent target for interventions to enhance *prosociality*. Thus, the main practical application of the findings from this review entails implications for extending and developing intervention around *gratitude* that may enhance *prosocial behaviour* and subjective well-being (see Davis et al., 2016; Renshaw & Olinger-Steeeves, 2016; Wood et al., 2010).

The classic gratitude intervention was originated in studies by Emmons and McCullough (2003) and Froh, Sefick and Emmons (2008) who instructed participants to recall up to five gratitude-inducing events that took place in their recent past. Although this gratitude induction may have focused on transitory feelings of gratitude, it may not have captured some other factors that influence

the link between gratitude and prosociality. Indeed, the moderator analysis indicated that the gratitude-prosociality link was stronger when gratitude was examined as a *state/mood*; and when gratitude was triggered by an 'interpersonal transfer of a benefit (pp. 1194)' (Lambert et al., 2009). Moreover, both *direct reciprocal* exchange and *downstream indirect* reciprocity had the largest effect sizes. Thus exchanges that emphasise *mutual benefit* or attracting help from others—thanks to *previous good deeds*—appear to be crucial. Thus, I propose that future gratitude interventions designed to promote prosociality should include the element of *benefit-triggered* gratitude and reciprocity.

For instance, resembling Watkins and colleagues' (2003) 'Grateful Essay' training, practitioners could instruct the trainees to write about a particular person, for example a close friend, for whom they feel grateful and how they either actually helped, or intended to help, that particular individual back (*direct* reciprocity). Participants could also be instructed to consider how their good deeds may influence others to help them when they need help (*downstream indirect* reciprocity) or how feelings of gratitude may influence them to help others (*upstream indirect* reciprocity).

I acknowledge that some people may spontaneously generate situations of this type in a free response list, but not all will. At a group level this may serve to weaken the effectiveness of any interventions. Even though the changes as suggested at present are small, it is well-known that small changes in frames can significantly alter behavioural and emotional responses (Dolan & Kahneman, 2008). So while the suggested change at present may be small, it still constitutes an empirical question of whether this small change could result in a large impact on the enhancement of effectiveness of gratitude interventions.

Chapter 3

Gratefully received, gratefully repaid: the role of perceived fairness in cooperative interactions

(Experiment 1)

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3.0. Overview of Chapter 3

Chapter 2—which presents the meta-analysis—illustrated that the gratitude-prosociality relation is more prominent among studies which examined *direct reciprocal* outcomes. The meta-analysis also revealed that studies which examined gratitude as a *state* generally reported a larger gratitude-prosociality correlation. Building upon these findings, I intend to examine gratitude, as a *state* symbolising appreciation (McCullough et al., 2002), in the context of a *direct reciprocal* economic exchange. Additionally, this chapter would underline the role of *normative fairness* perception (Elster, 2006) in the gratitude-reciprocity link. That is, how the expectations of what an average fair person *should* do (Bicchieri & Chavez, 2010)—in an economic exchange—would affect how gratitude relates to one's decision to reciprocate.

It is well documented that people are inclined to reward fair behaviours by being cooperative, i.e. *positive* reciprocity (Fehr & Falk, 1999) while penalising unfair behaviours via sanctioning, i.e. *negative* reciprocity (Fehr & Falk, 1999). Fair intentions are expected to elicit *positive* emotions (Greenberg & Westcott, 1983), like *gratitude* (Komter, 2004; Nadler, 2012), triggering cooperative behaviours or behavioural tendencies (Fredrickson, 2004a; McCullough et al., 2001; Tsang, 2007); while intended unfairness is conducive to *negative* affect, like anger or indebtedness (Hatfield & Sprecher, 1983), fostering sanctioning decisions (Ma et al., 2012; Yamagishi et al., 2009).

Nevertheless, Bicchieri (2006) theorised that when people infer the intention of others they do it on the basis of a 'background of expectations (pp.112)'. These expectations, amongst other things, constitute the injunctive norm about what an average person *ought to* do in a given context (Burger et

al., 2010; Cialdini et al., 1990; Kallgren, Reno, & Cialdini, 2000). Thus, the injunctive norm could be instrumental when one evaluates whether he/she was fairly treated, with this attribution already well-documented to shape *gratitude* and *reciprocity* (Kim & Lee, 2013; Komter, 2004; McCullough et al., 2001).

As such, in Experiment 1 I created an economic game setting in which people were bound to make reference to the injunctive norm to determine whether or not they were *fairly* treated. In so doing I examined how this normative fairness attribution would foster a *favourable* (or *negative*) interpretation of the helper's intent, and consequently contributed to a greater feeling of *gratitude* (*indebtedness*) by the recipient. That is, the current experiment scrutinised how this chain of attributions would influence the recipients' desire to reciprocate as well their actual reciprocal behaviours, especially when not cooperating yields no repercussions. Importantly, this experimental design enables me to examine whether people would really rely on normative fairness to guide their decision making; and if yes, how such a fairness perception will contribute to the *gratitude-reciprocity* association.

A hundred and twenty-two students were recruited to play a one-shot, variant of trust game in which half played as potential helpers (i.e. P1s hereafter, $N = 61$) and half as recipients (i.e. P2s hereafter, $N = 61$). Whether a participant was a P1 or P2 was *chance*-determined and all participants knew that. P1s had to decide whether to help P2s and whether to make their offer *unconditional* (no repayment needed) or *conditional* (full or 'taxed' repayment). P2s decided whether to accept a) the offer and b) whatever *conditions* attached but were blind to the list of helping options available to the P1s.

I examined if a normatively *unfair* offer, i.e. to impose a charge for helping knowing that one did not legitimately 'earn' thus entitle him/herself to that position as the helper (Hoffman & Spitzer, 1985), would prompt a less favourable helper intent attribution, thus lead to lower *gratitude* (or higher *indebtedness*) and ultimately a lower desire to reciprocate. Furthermore, these recipients were provided an avenue to sanction the helpers by under-repaying. P2s in this game were free to decide whether to really pay up after supposedly 'agreeing' to the contract. I further examined whether *gratitude* and *indebtedness* will be related to this sanctioning decision.

All in all, via Experiment 1 I empirically addressed the importance of *fairness* and *intent* attribution on the *gratitude-reciprocity* association. In so doing I endeavour to build on the meta-analysis (i.e. Chapter 2)—i.e. gratitude overall correlates stronger with prosociality that involves *direct* reciprocity (e.g. Graham, 1988; Goei & Boster, 2005; Goei et al., 2007)—by outlining what other mediating factor(s) (i.e. injunctive fairness) could contribute to the strength of this particular link. In addition, the present experiment also investigated whether the above chain of attributions will constitute a similar effect on a recipient's *indebtedness*, and subsequently affected the participants' reported tendency so as their actual reciprocal (or sanctioning) behaviours.

3.1. Introduction

Chapter 2 illustrated the consistent relationship between gratitude and reciprocity, in particular *direct reciprocity*, across studies. Indeed, the reciprocity principle stipulates that 'we should attempt to repay, in kind, what another person has provided us (pp.20) (Cialdini, 2001). Fehr and colleagues (e.g.Fehr, Gächter, & Kirchsteiger, 1997; Fehr & Gächter, 1998, 2000) further

make a distinction between the tendency to remunerate (or show cooperation) *fair* behaviours (*positive* reciprocity), and the tendency to sanction (or be hostile towards) acts we perceive as *unkind* or *unfair* (*negative* reciprocity). Additionally, while *positive reciprocity* is mediated by the perceived trustfulness of a giver (Fehr & List, 2004; Fehr & Rockenbach, 2003), negative affect—such as anger toward the transgressors (Drouvelis & Grosskopf, 2016; Seip et al., 2014; Xiao & Houser, 2005)—mediates negative reciprocal acts.

3.1.1. Intention, Gratitude and Indebtedness

The above implies that when a person decides whether to sanction or reward an interactional partner the attribution of the partners' intentions is crucial (Charness & Levine, 2007; Falk, Fehr, & Fischbacher, 2008). In fact, Falk and Fischbacher (2006) theorised that reciprocation (or retaliation) is dependent upon the recipient's evaluation of the helper's intention. Hence, reciprocity should be more than a knee-jerk reaction to a benefit received, as the recipient also ponders why he/she was offered what they were.

Greenberg and Westcott (1983) argued that a prosocial gesture which communicates good intention should elicit *positive affect* such as *gratitude* in the recipient directed toward the helpers (Tsang, 2006b), and these positive emotions can motivate reciprocation (Bartlett & Desteno, 2006; Schaumberg & Flynn, 2009; Tsang, 2007). Indeed, Nowak and Roch (2007) had particularly highlighted the importance of *gratitude* in both triggering and sustaining the *direct* and *indirect* reciprocation mechanisms. Fredrickson (2004a) similarly theorised that *gratitude* drives people to 'behave prosocially oneself, either towards the benefactor, toward others, or both (pp.150).' This definitely echoes

the moderate, positive correlation ($r = 0.454$, $p < .001$) between gratitude and direct reciprocal measures as reported in Chapter 2 across over ninety studies.

The corollary of the above is that *unfair* acts that are perceived to reflect malicious intentions should elicit *negative* emotions such as *indebtedness* (Greenberg & Shapiro, 1971). Indebtedness represents an aversive state of tension (Greenberg & Frisch, 1972) that is associated with negative action tendencies such as *inhibition* and *avoidance* (Watkins et al., 2006). Meanwhile, the effect of indebtedness on reciprocity remains inconclusive. While it is well documented that an indebted person would reciprocate to rid oneself of the unpleasant feeling (Gergen et al., 1975; Schaumberg & Flynn, 2009), Greenberg and Westcott (1983) contended that apart from reciprocation *cognitive restructuring* also constitutes a 'major mode of indebtedness reduction (pp.95)'. These restructuring efforts entail a reassessment of the benefit received which often leads to the recipients discrediting the benefactors and ultimately, feeling less obliged to give back (Greenberg & Westcott, 1983).

3.1.2. Injunctive norm, Objectives of Experiment 1 and the Hypotheses

The above summarised the literature on how intent attribution is crucial to people's emotions and subsequent reward or sanctioning decisions. Meanwhile, it is argued that when people attend to others' intentions they do so not out thin air, as Bicchieri (2006) stated that 'an intention is only good or bad against a background of expectations (pp.112)'. These expectations by definition constitute, amongst other things, the 'injunctive norm' which delineates how an average person *should* behave in a given situation (Kallgren et al., 2000; Perkins, 2002; Raihani & McAuliffe, 2014). That is, Bicchieri (2006) suggested that when evaluating someone else's intent, e.g. being fair or

otherwise, people will base their judgment on whether that 'someone' had acted in accord with what the society expects from an average fair person. Indeed, the idea that injunctive norms underpin fairness judgment has been endorsed by models in the social psychology and social cooperation literature (See Fahr & Irlenbusch, 2000; Irwin & Simpson, 2013). Nevertheless, to date it still remains unaddressed how such a norm-based fairness judgment will incite people's emotions of *gratitude*, or *indebtedness*, and how that may ultimately contribute to an individual's decisions to reciprocate or sanction in an economic exchange.

In Experiment 1 I examined this idea using a one-shot, variant of the trust game (i.e. TG) (Berg, Dickhaut, & McCabe, 1995; Camerer, 2003). Particularly, in this game 1) all players were *anonymous* to each other and the experimenter (thereby eliminating any motive to build reputations); 2) players interacted in pairs with one being a potential helper (i.e. P1) and the other playing a potential recipient (i.e. P2); 3) who ended up as either P1 or P2 in every pair, was by pure *chance* thus removing any ability attributions; and finally, P2s were completely unaware of the options P1s had available to them.

Such unawareness is essential as Charness and Levine (2007) argued that when making an economic decision an agent (i.e. a recipient) would infer a principal's (i.e. a helper) intention also on the basis of the latter's range of possible behavioural options—that is, the option that was picked and those *could have been* picked. Therefore, by keeping P2s naïve to the options available to the helpers, coupled with the double-anonymous (Hoffman et al., 1994) setting, P2s could only rely on their beliefs about what people should do in this context (i.e. injunctive norm) as the basis for evaluating how *fair* (or *unfair*) their P1 partners were with respect to the any help offer they made.

P1s, meanwhile, could offer 'unconditional' help (i.e. asking for no repayment and P2s could not repay) or 'conditional' help (full repayment or repayment with an interest charge). I examined whether this difference in the types of helping offered—unconditional versus conditional—would suffice to induce different attributions of fairness in recipients towards the helper.

The null hypothesis, by contrast, would state that in the absence of information about the options open to the helpers, all help—*conditional* or otherwise—should be perceived as *equally* fair. Alternatively, it is plausible to hypothesise that individuals resort to injunctive norms of fairness (i.e. how people ought to behave). That is, when somebody's good fortune is acquired by *chance* (or assignment), rather than that owner having to earn the 'Property Rights' (Fahr & Irlenbusch, 2000; Frey & Bohnet, 1995; Oxoby & Spraggon, 2008) or the 'Entitlement' (Elster, 2006; Hoffman & Spitzer, 1985; Hoffman et al., 1994) to that good fortune, then in general a 'fair' individual should share that extra resources with other people *equally* (Frey & Bohnet, 1995).

In this case, a typical *fair*, fortunate P1 should help his/her P2 partner and help unconditionally—i.e. 'for free'. This hypothesis has garnered support in the social psychology, social cooperation and behavioural economics literature (See Bicchieri, 1999; Hoffman et al., 1996; Ruffle, 1998). As such, I predicted a difference in attributions of helpers' intentions, even in the absence of any knowledge about the choices facing the helper, purely based on whether or not that offer is unconditional. That is, P2s will rely on the normative belief that fair people will help for free when it is easy to administer and when the helpers' potency to help was 'assigned' (via chance) instead of 'earned'.

In summary,

Hypothesis 1: Conditionality of offers—which denotes difference in normative fairness of the arrangement—should induce different emotional response from the P2s, e.g. the perception of genuine helpfulness, gratitude and so on;

In addition to hypothesising that an *unconditional* offer would incite a more *favourable* appraisal of the helper's intentions, I expected that this should then engender a greater (lower) level of gratitude (indebtedness) (Tsang, 2006a; Watkins, et al., 2006). I further argued that this gratitude would ultimately translate to an elevated desire for recipients to reciprocate in the future (Tsang, 2006b; Schopler, 1970). A *conditional* offer, on the contrary, should not only elicit a more cynical appraisal of P1s' intent but also a lower desire to reciprocate (Tsang, 2006a), with this related to sanctioning (e.g. not fulfilling the repayment obligation) should opportunity beckons (Fehr & Falk, 2002).
In a nutshell, I tested a serial multiple mediational model (Hayes, 2013) which examines if the effect of offer conditionality on reciprocity tendency is mediated *serially* by two psychological processes: 1) initial attributions that the helper *genuinely intends* to be *helpful* and 2) feelings of *gratitude*. Figure 3.1 graphically depicts this hypothesised serial mediational model (i.e. H2).

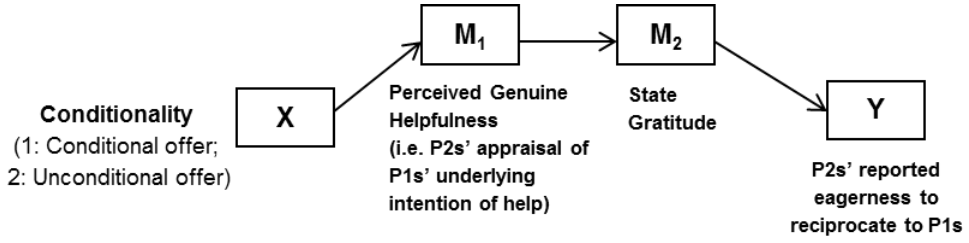

Figure 3.1. Graphical Presentation of H2—the mediational model.

On the other hand, as discussed above *indebtedness* could in theory either strengthen or weaken one's tendency to reciprocate (Greenberg, 1980; Hatfield & Sprecher, 1983). Particularly, Greenberg and Westcott (1983)

theorised that a recipient would prefer cognitive restructuring over reciprocation upon i) a perceived absence of opportunity to reciprocate (Schaumberg & Flynn, 2009); ii) a lack of witness to the initial helping episode; and iii) recipients anticipating *no future exchanges* with the benefactors. All these conditions were compatible with Experiment 1's parameters.

As such, it is conceivable that the present *indebted* recipients may favour *cognitive restructuring*—which should lead to a decreased desire to reciprocate and a higher propensity to *actually* under-repay— in reducing their feeling of indebtedness. As a result, in addition to feeling more *indebted* upon the receipt of the unfair 'conditional' offers, I predicted a lower likelihood among these *indebted* recipients to *want to* and *actually* reciprocate.

H3: P2s who received *conditional* offers, compared to *unconditional* offers recipients, should report higher *indebtedness*, with this indebtedness being negatively related to the *intended* and *actual* repayment by these recipients.

3.2. Methods

3.2.1. Participants

One hundred and twenty-two students from the University of Nottingham participated (72 were females, mean age: 21.8 years, SD = 3.7 years). Each participant received an inconvenience allowance of £2 and could earn up to £3.50 more.

3.2.2. Game and its Rationale

The experiment was administered using Z-Tree (Fischbacher, 2007). To begin with each participant was given a fair die to roll and was seated in a separate cubicle. Each session took place with groups of 4 to 10, with players in

each session randomly assigned to play in dyads, with one participant being the potential helper (P1) and the other playing a potential recipient (P2).

Each player was *initially* endowed with *150* money-equivalent 'Bonus Points' and was informed that he/she needed at least *50* points more to reach the 'Bonus Threshold' (i.e. *200*) to claim the bonus money. The initial endowments were allocated this way to create a situation in which all players were, to begin with, equal in the sense that they were all entitled to *zero* additional bonuses as their initial bonus points stood at 150. It was made explicit to participants that each bonus point would be worth one penny but only when he/she made the 200 point threshold would he/she be entitled to any bonus.

The only way to reach that threshold was by rolling a fair, six-sided die once. The possible payoffs were a loss of -50 points, 0 points, and gains of 50, 100, 150, and 200 points depending on the number on the dice. It was, nevertheless, emphasised that a larger number on the dice did NOT guarantee a more favourable bonus points. The purpose of this die-rolling arrangement was to convince the participants that what differentiated 'winners' (i.e. hitting the threshold) from the 'losers' (i.e. missing the threshold) was simply *better fortune* but not ability. Every player then learned that he/she had an in-game 'partner' who was *unidentifiable* and *anonymous*, and there was no need to collaborate with or compete against that partner throughout the experiment.

Unbeknownst to the participants, all their final bonus scores were *pre-determined*. Regardless of the actual die-roll, P1s always received a 'Die-rolling' score 200 (thus they ended up with 350 points: their initial 150 + 200) that exceeded the 200 point threshold, while P2s would fall short of the threshold. Half the P2s (N = 30) ended up with a final total of 100 bonus point

(that is their initial 150 *minus 50* points from the dice roll) and the other P2s (N = 31) ended up with 150 points (that is their initial 150 *plus zero* from the dice roll). This difference between the P2s was aimed to manipulate the potential *helping costs* for the P1s. P2s with 100 points were in the 'High-Cost' helping condition, and would require twice the magnitude of their partners' (or potential helpers') minimum 'donations' (i.e. 100) compared to the 'Low-Cost' recipients with 150 points (i.e. 50), to reach the 200 point threshold. Each participant then learned about his/her payoff and that of his/her 'partner'.

At this point both P1s' and P2s' gave attributions regarding their partners' Die-rolling scores. Specifically they indicated the extent to which they considered their partners' scores were due to (1) Luck ('To what extent you think that your partner's High (Low) score is attributable to his/her good (bad) luck), and (2) Ability ('To what extent you think that your partner's High (Low) score is because of his/her capability (incompetence)'). Both items were measured using 7-point Likert-type scales (1 = 'Not At All' and 7 = 'Completely'). In so doing I examined if the participants perceived their partners' (good or bad) outcomes as *chance*- or *ability*-based.

3.2.3. *Unconditional* 'Free' versus *Conditional* 'Charged' Offers

3.2.3.1. *Helper's Decisions.*

P1s, who were better off financially than P2s, were then inquired if they would help P2s by transferring part of their excess bonus points (i.e. 350 – 200 'Bonus Threshold' = 150 points). Helpers could help either *unconditionally* (i.e. to donate their points and demand no repayment: repayment free offer), or *conditionally* by attaching a 'repayment clause' to their transfer. All conditional helpers then selected a designated level of repayment (See Figure 3.2) as the

repayment clauses of their offers. All these decisions were made privately and P2s were blind to the list of choices that were available to P1s. Figure 3.2 outlines the flow of a P1's decision.

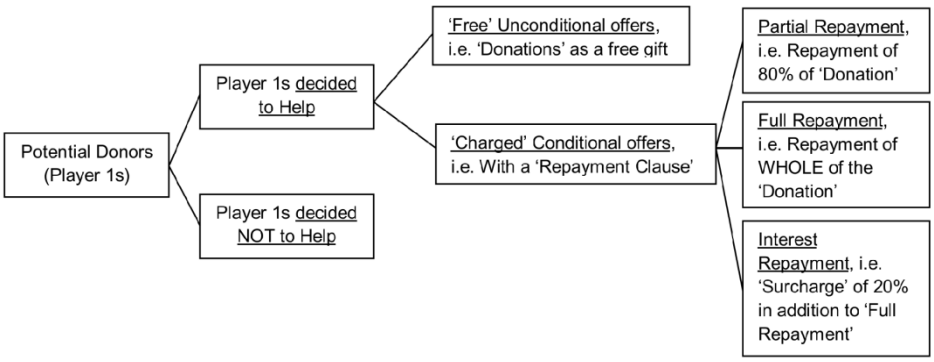

Figure 3.2. The flow of decisions made by a potential helper (P1).

3.2.3.2. *Recipient's Decisions.*

After P1s made the decisions their P2 partners were shown, firstly, whether P1s decided to help; and if so, whether the offers were *conditional* or *unconditional* , and if they were conditional what the *repayment terms* were. P2s who received an offer were then asked to decide whether to accept it. It was emphasised to them that they would always be financially better off receiving any offer of help compared to no offer of help, in spite of the repayment obligation or lack thereof. I then assessed recipients' perception of their helpers' decisions using the following constructs: (1) State *Annoyance* ('I am annoyed by my partner's decision.'), (2) State *Gratitude* (Average of items 'I am thankful for my partner's decision' and 'I feel grateful for what my partner does for me', Cronbach's $\alpha = 0.931$), (3) State *Indebtedness* ('I feel indebted to my partner'), (4) *Obligation* ('I feel obliged to repay my partner'), (5) *Perceived* Genuine *Helpfulness* ('My partner's transfer is motivated by his/her sincere desire to help me'), (6) Perceived *Reasonableness* ('My partner's

decision is reasonable'), and (7) *Reciprocating Tendency* ('I am eager to help my partner out if he/she is in need in the near future'). All items were rated on 7-point Likert-type scales (1 = 'Not At All' and 7 = 'Completely').

I checked that the recipients of *conditional* offers understood that they were always better off economically accepting (rather than declining) the offers, even if they paid back in full. P2s who *accepted* their *conditional* offers were then shown a reminder of their *expected* repayment. Immediately after that P2s decided whether to repay and how much. Importantly by giving P2s the freedom to decide whether or not to honour the agreement these recipients were 'granted' the option of defaulting in the form of a 'breach of contract' (Shavell, 1980). Figure 3.3 illustrates the flow of decisions a P2 was expected to make.

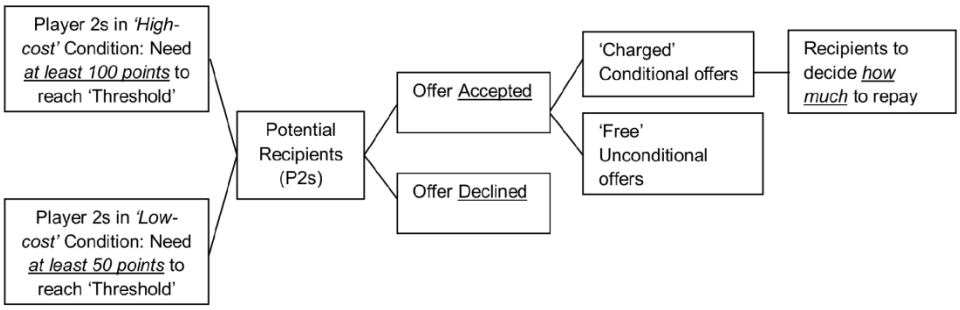

Figure 3.3. The flow of decisions made by a potential recipient (P2)

P2s (who received conditional offers) were initially unaware of this option to default when making their emotional judgements. The procedures were set up as such so to avoid confounding these P2s' emotional judgements--which are the primary outcome in this study, and make these judgements comparable to the *unconditional* offer receipts in which there was no room for any repayment. P2s who received *unconditional* offers were not given a latter option to repay the 'free' gift. This was meant to mirror the real world situation in which a 'free' gift from a stranger with no option to repay—e.g., altruistic organ/blood donation or a graduate school scholarship—would remain as such.

Meanwhile, by offering the recipients an opportunity to sanction their helpers for their breach of normative fairness (i.e. to only help conditionally), Experiment 1 provided a *behavioural* measure of *positive* (full repayment) and *negative* reciprocity (zero back-transfer or partial repayment). Thus the level of under-repayment should constitute an index of how the recipients were irked by P1s' defiance of the injunctive norm. Hence the option to default provides a secondary outcome and one that does not bias the initial emotional judgements.

In this game all P2s' decisions (e.g. acceptance, repayment) were fed back to their P1 partners, thus affecting their final payoffs. Afterwards, all players were shown onscreen their respective compensations. They were then instructed to complete the Marlowe-Crowne Social Desirability Scale (Crowne & Marlowe, 1960). In so doing I intended to address any potential validity issues attributable to Social Desirable Responding (Carver & Scheier, 2008). The experiment ended here and the participants were debriefed and paid.

3.2.4. Ethics Statement

Experiment 1 was approved by the ethics committee of the School of Psychology at the University of Nottingham. All participants were older than 17 and all provided written, informed consent prior to participation as approved by the ethics committee (Approved 11th Feb 2013; Ref. Code: 267).

3.3. Results

3.3.1. Manipulation Check

I intended to inspect, via two pre-offer ratings using 7-point Likert-Type Scales (1= 'Not At All'; 7 = 'Completely'), if the present die-rolling procedure succeeded in leading our participants (both P1s and P2s) to believe that what

segregated the 'winners' (i.e. making the 200 point 'threshold') from the 'losers' (i.e. missing that) in the game was chance rather than ability.

Table 3.1. Statistics for Participants' Rating: Item 'Chance' Attribution

'To what extent you think that your partner's high (low) score is attributable to his/her good (bad) luck'[1]								
		N	*M*	*SD*	*SE*	*df*	*MD*	*t-statistic*	*p* (2-tailed)
Gender	Male	50	4.76	2.37	0.34				
	Female	72	5.24	2.08	0.25				
	Total	122	5.04	2.21	0.20	96.4	0.476	1.147	0.254
Condition	Low-cost	62	5.26	2.20	0.28				
	High-cost	60	4.82	2.21	0.29				
	Total	122	5.04	2.21	0.20	120	0.441	1.106	0.271
Roles	Player 1s	61	4.80	2.29	0.29				
	Player 2s	61	5.28	2.11	0.27				
	Total	122	5.04	2.21	0.20	120	0.475	1.192	0.235

Note. *M*: Mean; *MD*: Mean Difference; [1]1= 'Not At All'; 7 = 'Completely'.

As illustrated in Table 3.1, participants in general (*M:* 5.04, *SD:* 2.21) were convinced that their partners' high or low die-rolling scores were due to chance. The independent samples t-tests evidenced that this high propensity to impute partner's outcomes to chance was not affected by gender, which *cost condition* and which in-game *role* the participant was assigned to.

Table 3.2. Statistics for Participants' Rating: Item 'Ability' Attribution

'To what extent you think that your partner's high (low) score is because of his/her capability (incompetence)' (1= 'Not At All'; 7= 'Completely')								
		N	*M*	*SD*	*SE*	*df*	*MD*	*t-statistic*	*p* (2-tailed)
Gender	Male	50	1.58	1.11	0.16				
	Female	72	1.47	1.01	0.12				
	Total	122	1.59	1.22	0.11	120	0.108	0.558	0.578
Condition	Low-cost	62	1.55	1.28	0.16				
	High-cost	60	1.63	1.16	0.15				
	Total	122	1.59	1.22	0.11	120	0.085	0.384	0.702
Roles	Player 1s	61	1.38	0.86	0.11				
	Player 2s	61	1.80	1.47	0.19				
	Total	122	1.59	1.22	0.11	96.7	0.426	1.955	0.053

Table 3.2 shows that overall participants did not attribute their partners' high or low die-rolling scores to their abilities (*M:* 1.59, *SD*: 1.22). Again, this attributional tendency was unaffected by gender, cost condition and in-game role. Meanwhile, a paired-samples t-test indicated a significant difference, (t (121) = 14.82, p <.001), between participants' average 'Chance' (*M:* 5.04) and 'Ability' attributions (*M:* 1.59). This suggested that participants were prone to attribute their partners' good (or bad) outcomes to *chance* instead of *ability*.

Table 3.3. Statistics for Participants' Attributional Bias

Attributional Bias = Participant's 'Chance' Attribution – 'Ability' Attribution Rating									
		N	*M*	*SD*	*SE*	*df*	*MD*	*t-statistic*	*p* (2-tailed)
Gender	Male	50	3.06	2.90	0.41				
	Female	72	3.72	2.30	0.27				
	Total	122	3.45	2.57	0.23	89.6	0.662	1.348	0.181
Condition	Low-cost	62	3.71	2.55	0.32				
	High-cost	60	3.18	2.59	0.33				
	Total	122	3.45	2.57	0.23	120	0.526	1.132	0.260
Roles	Player 1s	61	3.43	2.51	0.32				
	Player 2s	61	3.48	2.66	0.34				
	Total	122	3.45	2.57	0.23	120	0.049	0.105	0.916

I similarly explored if this attributional bias was explainable by participant's *gender*, assigned in-game *role* or *cost condition*. The data (See Table 3.3) revealed that this bias was not accounted for by any of the three variables. This illustrated that the participants generally had a consensus that chance, instead of ability, accounted for their partners' die-rolling outcomes.

3.3.2. Descriptive Data

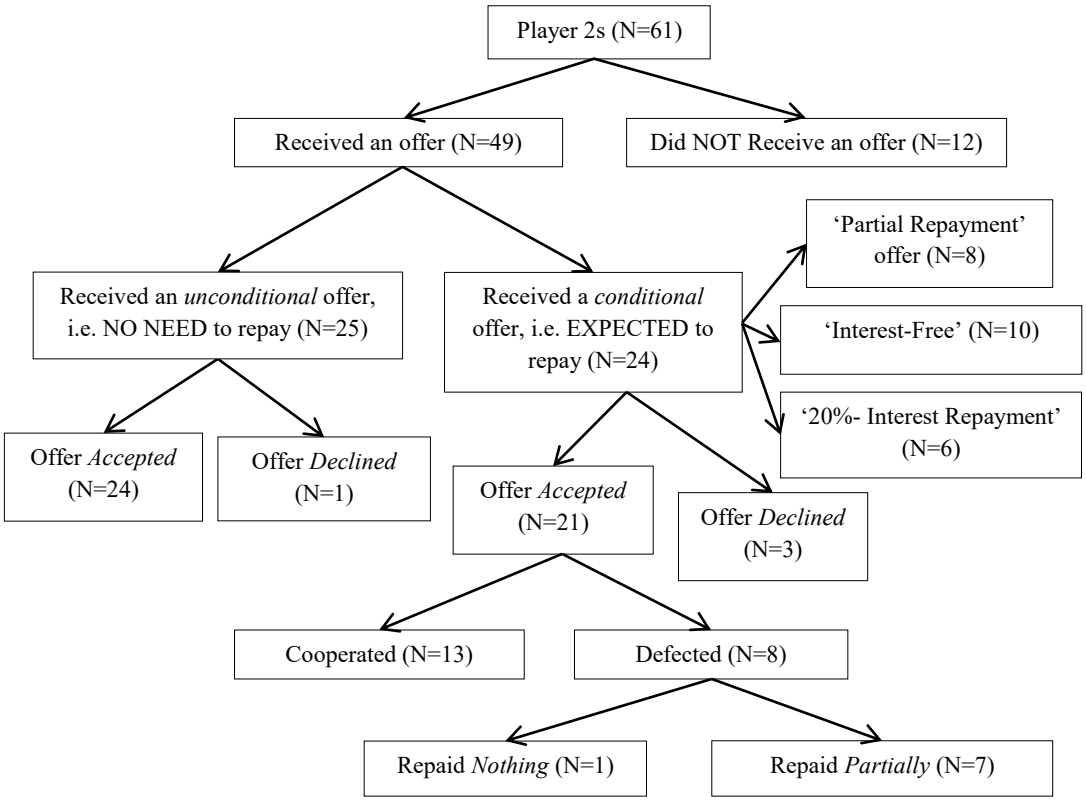

Figure 3.4. Decision Tree of P2s ($N = 61$)

Forty-nine P2s (80%) received an offer and of these 24 received a *conditional* (10 received an 'Interest-Free', 8 a 'Partial Repayment' and 6 an

'Interest Repayment' offer) and 25 an *unconditional* offer. While there was one rejection among unconditional offer recipients (rejection rate: 1/25= 4%: 96% acceptance), three recipients of conditional offers declined theirs (rejection rate: 3/24 = 12.5%: 87.5% acceptance). Figure 3.4 depicts the decision tree of P2s.

3.3.3. General Confounds

The following analysis delves into the potential effect by the three general confounds—namely i) the cost of helping, ii) gender, and iii) Social Desirability Responding—on the current key outcome variables.

3.3.3.1. Cost of Helping

Table 3.4. Effect of Cost of Helping on Player 2s' 1) post-offer ratings, 2) repayment decisions; and on 3) Helpers' Transfer Decisions.

			Testing Condition			
			Low-cost	High-cost		
	t	*df*	*M (SD)*	*M (SD)*	*MD*	*p* (2-tailed)
1. P2s'[1] Post-offer Ratings						
State *Annoyance*	0.54	59	2.29 (1.77)	2.07 (1.43)	0.22	.591
State *Gratitude*	1.06	59	4.79 (2.28)	5.37 (1.96)	0.58	.294
State *Indebtedness*	1.06	59	3.68 (2.20)	4.30 (2.40)	0.62	.294
Perceived Genuine *Helpfulness*[2]	0.41	47	5.54 (1.10)	5.40 (1.32)	0.14	.686
Obligation to Repay[2]	1.16	47	4.58 (1.69)	5.16 (1.80)	0.58	.253
Reasonableness	0.49	52.6	5.10 (5.30)	4.90 (1.81)	0.20	.628
Perceived *Low Cost* of Help[2]	1.35	47	4.54 (1.62)	3.88 (1.81)	0.66	.184
Tendency to *Reciprocate*	0.23	59	5.19 (1.85)	5.30 (1.82)	0.11	.822
2. Recipients'[3] Repayment (in Points)						
Actual Repayment	1.85	13.8	49.0 (17.3)	73.7 (40.4)	24.7	.085
Discrepancy between *Actual* and *Expected* Repayment	0.97	12.5	10.0 (17.0)	25.6 (50.2)	15.6	.349
3. Helpers'[4] Transfer (in Points)						
Discrepancy between *Actual* and Expected (i.e. *Minimum)* Transfer	1.38	41.3	14.4 (21.9)	6.84 (15.5)	7.54	.174

Note. [1] There were 31 P2s in 'High-cost' condition of which 25 received either a conditional or unconditional offer while 24 out of 30 Player 2s in 'Low-cost' condition received either a conditional or unconditional offer. [2] Only P2s who received an offer (N=49) were required to respond to this item. [3] Twenty-one recipients of conditional offers were expected to repay, 10 of them were from 'Low-cost' condition and 11 were from the 'High-cost' condition.
[4]There were 24 Player 1s from the 'Low-cost' condition and 25 from the 'High-cost' condition who decided to help either conditionally or unconditionally

I first examined whether the effect of *cost condition* significantly influenced 1) P2s' post-offer ratings, 2) repayment decisions for P2s who *accepted* a *conditional* offer, 3) helpers' excess transfers and their 4) other helping-related decisions. Independent-sample T-Tests revealed that cost condition overall did not differentiate any of 1) the recipients' post-offer

ratings, 2) repayment decisions of the *conditional* offer acceptors and 3) the helpers' magnitude of 'over-donation'. The results are detailed in Table 3.4.

Additionally, a Chi-square test was used to determine whether there was a significant difference between helpers from 'Low-cost' and 'High-cost' condition in their 1) decisions to help, and 2) conditionality (need to repay or not) of offers made should they agreed to help. Table 3.5 details the results.

Table 3.5. Effect of Cost of Helping on Helpers' Helping Decisions.

		Testing Condition				
		Low-cost	High-cost			
P1s' Helping Decisions		*N*	*N*	χ^2	*df*	*p* (2-tailed)
1. To Help or Not to Help (*N* = 61)	Helped	24	25			
	Not Helped	7	5			
	Total	31	30	.337	1	.561
2. Conditionality of Offer Made (*N* = 49)	Unconditional	13	12			
	Conditional	11	13			
	Total	24	25	.186	1	.666
3. Preferred Repayment Modes (*N* = 24)	Partial	5	5			
	Interest-Free	3	5			
	20%-Interest	3	3			
	Total	11	13		N.A.[1]	

Note. [1] Chi-squared test was not conducted for 'Preferred Repayment Modes'. Fisher's Exact Test was indeed run and the results revealed that preferred repayment modes did not significantly differ by Testing (i.e. Cost of Helping) Condition (p = .882, two-tailed)

The data (See Table 3.5) revealed that neither decisions had significantly differed by testing condition. Furthermore, Fisher's exact test was conducted to determine if helpers' preferences of repayment modes would differ by testing condition. The results indicated no significant effect of testing condition on (*conditional*) helpers' choices of repayment modes.

3.3.3.2. Effect of Gender

I also examined if participants' gender had significantly influenced 1) recipients' post-offer ratings (i.e. emotional responses to offers), 2) repayment decisions for those who received a *conditional* offer, 3) helpers' transfer decisions, and their 4) other helping-related decisions. Independent-sample T-Tests indicated that gender failed to differentiate any of 1) the recipients' post-offer ratings (except that male P2s rated their partners' decisions as more

'reasonable' than their female counterparts), 2) repayment decisions of the recipients of conditional offers and 3) the helpers' magnitude of transfers and 'over-donation'. The results are detailed in Table 3.6.

Table 3.6. Effect of Gender on Player 2s' 1) post-offer ratings, 2) repayment decisions; and on 3) Helpers' Transfer Decisions.

	Testing Condition					
			Male	Female		
	t	*df*	*M (SD)*	*M (SD)*	*MD*	*p* (2-tailed)
1. P2s'[1] Post-offer Ratings						
State *Annoyance*	0.439	59	2.05 (1.40)	2.24 (1.71)	0.194	.662
State *Gratitude*	0.513	59	5.28 (2.14)	4.98 (2.14)	0.299	.610
State *Indebtedness*	0.789	59	3.65(2.23)	4.15 (2.34)	0.496	.433
Perceived Genuine *Helpfulness*[2]	0.122	47	5.50 (1.32)	5.45 (1.18)	0.045	.903
Obligation to Repay[2]	0.179	47	4.81 (1.97)	4.91 (1.67)	0.097	.859
Reasonableness	2.161	59	5.60 (1.39)	4.71 (1.57)	0.893	.035*
Perceived *Low Cost* of Help[2]	0.925	47	3.88 (1.75)	4.36 (1.73)	0.489	.360
Tendency to *Reciprocate*	0.758	59	5.50 (1.67)	5.12 (1.90)	0.378	.452
2. Recipients'[3] Repayment (in Points)						
Actual Repayment	0.401	19	66.7 (42.7)	60.1 (30.3)	6.60	.693
Discrepancy between *Actual* and *Expected* Repayment	1.160	19	33.3 (47.2)	12.1 (33.9)	21.2	.260
3. Helpers'[4] Transfer (in Points)						
Actual Transfer	0.924	47	90.0 (29.9)	82.5 (27.2)	7.54	.360
Discrepancy between *Actual* and Expected (i.e. *Minimum)* Transfer	0.315	47	9.61 (18.3)	11.4 (20.2)	1.74	.755

Note. [1] There were 16 out of 20 male P2s and 33 out of 41 female P2s who received either a *conditional* or *unconditional* offer. [2] Only P2s who had received an offer (N=49) were required to respond to this item.[3] Twenty-one recipients of conditional offers were expected to repay, there were 6 male recipients and 15 female recipients. [4] There were 23 male helpers and 26 female helpers. *p < .05 (two-tailed).

A Chi-square test was used to determine whether there was a significant difference between female and male helpers in their 1) decisions to help (or not), and 2) conditionality of offers made should they agreed to help.

Table 3.7 Effect of Gender on (Potential) Helpers' Helping Decisions

		Male	Female			
P1s' Helping Decisions		*N*	*N*	χ^2	*df*	*p* (2-tailed)
1. To Help or Not to Help (*N* = 61)	Helped	23	26			
	Not Helped	7	5			
	Total	30	31	.501	1	.534
2. Conditionality of Offer Made (*N* = 49)	Unconditional	10	15			
	Conditional	13	11			
	Total	23	26	.987	1	.396
3. Preferred Repayment Modes (*N* = 24)	Partial	5	5			
	Interest-Free	4	4			
	20%-Interest	4	2			
	Total	13	11		N.A.[1]	

Note. [1] Pearson's Chi-squared test was not conducted due to insufficient counts.

As shown in Table 3.7, neither decisions significantly (p >.05) differed by gender. Furthermore, the Fisher's exact test indicated a lack of gender effect (p = .77, two-tailed) on *conditional* helpers' selections of repayment modes.

3.3.3.3. Effects of Social Desirability Responding (i.e. SDR hereafter)

Table 3.8. Effect of SDR on recipients' post-offer ratings and repayment

		Gs	As	Is	Os	Reas	PGH	Cost	Rec	AR	UR
MCSD	*r*	.089	-.142	-.308	-.007	.141	-.073	.098	.159	.224	-.087
	p	.541	.329	.031*	.964	.332	.620	.502	.274	.329	.706
	N	49	49	49	49	49	49	49	49	21	21

Note. MCSD: Marlowe-Crowne Social Desirability Scale; *r*: Pearson's r correlation; *p: p*-value (two-tailed). Gs = State Gratitude; As = State Annoyance; Is= State Indebtedness; Os = Perceived Obligation to Repay; Reas = Perceived Reasonableness of Partners' decision; PGH = Perceived Genuine Helpfulness in partners' help; Cost= Perceived Low Cost of Partners' Help; Rec = Recipients' Eagerness to Reciprocate; AR = Actual Repayment by Recipients of Conditional offers; UR = Subtractions of 'Expected Repayment' from 'Actual Repayment' by recipients who accepted their conditional offers (negative values denote defaulting). * p <.05 (two-tailed)

I examined whether SDR had considerably affected the recipients' post-offer ratings (See Table 3.8), repayment decisions (See Table 3.8), and P1s' range of helping-related decisions (See Table 3.9). The SDR tendencies were operationalized as participants' composite scores in the MCSD (Crowne & Marlowe, 1960). As shown in Table 3.8, there was no evidence that 1) SDR tendency was correlated with most of the recipients' ($N = 49$) post-offer ratings or that 2) SDR tendency influenced the repayment decisions.

Table 3.9. Effect of SDR on helpers' helping decisions

		Help	Conditionality	Tran	Over-donation	RM
MCSD	Pearson's Correlation (r)	-.095	-.121	.290*	.147	.399
	p (two-tailed)	.468	.409	.043	.313	.054
	N	61	49	49	49	24

Note. Help = To Help or Not; Conditionality= Unconditional or Conditional offers Made; Trans = Magnitude of Transfer; RM = Conditional Helpers' Chosen Repayment Modes (i.e. level of conditionality in offers made).

SDR was also not correlated with P1s' decisions to help, *conditionality* of offers made should they helped, preferences for repayment modes, and magnitude of 'over-donation'. The results are presented in Table 3.9. While SDR appeared positively ($r = .290$, $p = .043$) correlated with the helpers' transfer magnitude, this relationship no longer stood (p >.1) once helpers' 'Expected Transfer' (i.e. minimum required transfers) was controlled for.

Taken together, the above analyses failed to demonstrate any associations between MCSD with the current key outcome measures, indicating that SDR may not have substantially contaminated the current analysis.

3.3.4. Hypotheses Testing

As detailed above the current key outcomes measures were not subjected to contamination of the possible confounds. Therefore, in the main analysis I collapsed findings from both cost conditions and genders. Below I detail the results of the main hypotheses (i.e. H1 to H3) testing.

3.3.4.1. Testing Hypothesis 1

H1: Conditionality of offers—which denotes difference in normative fairness of the arrangement—should induce different emotional response from the P2s, e.g. the perception of genuine helpfulness, gratitude and so on;

Table 3.10 Statistics for P2s' Post-offer ratings

	Conditionality of offer received					
			Unconditional[a]	Conditional[b]		
	t-statistic	*df*	*Mean (SD)*	*Mean (SD)*	*MD*	*SED*
State *Annoyance*	1.73	37.9	1.40 (0.71)	1.88 (1.15)	0.475	0.275
State *Gratitude*	2.05*	47	6.30 (1.27)	5.63 (1.02)	0.675	0.330
State *Indebtedness*	0.90	47	4.96 (1.72)	4.46 (2.19)	0.502	0.561
Perceived *Genuine Helpfulness*	2.01*	47	5.80 (1.19)	5.13 (1.15)	0.675	0.335
Obligation to Repay	1.83	47	4.44 (1.73)	5.33 (1.69)	0.893	0.489
Reasonableness	1.82	47	4.72 (1.62)	5.46 (1.18)	0.738	0.406
Perceived Low Cost of Help	1.88	47	3.76 (1.79)	4.67 (1.58)	0.907	0.482
Tendency to *Reciprocate*	2.74**	38.6	6.28 (0.79)	5.46 (1.25)	0.822	0.300

Note. [a]N = 25. [b]N = 24. *SED:* Standard Error of Differences.* p <.05 ; ** p < .01 (2-tailed)

Independent samples T-Tests were conducted to examine if the conditionality of offer alone elicited differential benefit-triggered emotions and interpretations of helpers' intent. Recipients of *unconditional* offers (N= 25) reported (1) more gratitude (p <.05), (2) that the helpers were genuinely more helpful (p <.05), and (3) a higher tendency to reciprocate (p <.01). No other post-offer ratings significantly differed by whether the offer received was conditional or unconditional. Table 3.10 above details the statistics.

3.3.4.2. Testing Hypothesis 2 - The serial mediational model (Figure 3.5)

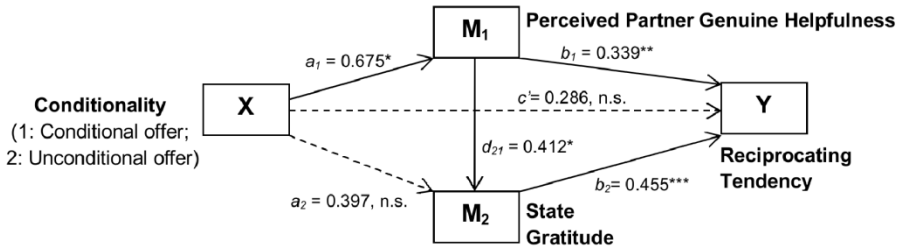

Figure 3.5. A statistical diagram of the hypothesised mediator model.
Note. N=49; n.s.= Not Significant; *p<.05; **p<.01; ***p<.001.

I examined whether the hypothesised mediational model—which was depicted in Figure 3.1—was supported by the current data. This model examines if the effect of offer conditionality on reciprocity tendency is mediated *serially* by (1) the degree that the helper was perceived as genuinely helpful and (2) the recipients' level of gratitude. Indeed, the results supported this serial multiple mediator model (Figure 3.5). I adopted the Bootstrap Confidence Intervals (CI) approach when analysing the current model. Hayes (2013) argued that bootstrapping is 'particularly useful relative to normal approach (i.e. the Sobel's (1982) (pp.110))' as it better handles the non-normality of the sampling distribution (of the indirect effects under scrutiny) which is inherent in smaller samples, thus rendering 'a test with higher power (pp.106).') The results are detailed in Table 3.11 and Figure 3.5.

Table 3.11 Regression Coefficients, Standard Errors, and Model Summary Information for the hypothesised Serial Multiple Mediator Model

		Consequent									
	Mediator 1 (PGH)				Mediator 2 (Gs)				Y (Rec)			
Antecedent		Coef	*SE*	*p*		Coef	*SE*	*p*		Coef	*SE*	*p*
X (Cond*)	a_1		.335	.0497	a_2	.397	.316	.215	*c'*	.286	.214	.188
M1 (PGH)		--	--	--	d_{21}	.412	.132	.003	b_1	.339	.097	.0010
M2 (Gs)		--	--	--		--	--	--	b_2	.455	.098	<.001
Constant	iM_1	4.45	.533	<.001	iM_2	3.12	.759	<.001	i_Y	.876	.591	.145
		R^2 = .080				R^2 = .243				R^2 = .622		
		F (1,47) = 4.058, p = .0497				F (2,46) = 7.368, p = .0017				F (3,45) = 24.71, p = <.001		

Note * Conditionality: '1'denotes receipt of a conditional offer while '2' denotes receipt of an unconditional offer. PGH= Perceived Partner Helpfulness; Gs= State Gratitude; Rec= Tendency to reciprocate

The present data show that the conditionality of an offer indirectly influenced the recipients' tendency to reciprocate through its effect on attributions of helpfulness and gratitude towards the helper. Recipients receiving an *unconditional* offer were more prone to see their helper as truly helpful ($a_1 = 0.675$, $p < .05$), and therefore reported more gratitude ($d_{21} = 0.412$, $p < .01$). This elevated gratitude predicts a greater desire to reciprocate ($b_2 = 0.455$, $p < .001$). A bias-corrected bootstrap CI for this total indirect effect ($a_1d_{21}b_2 = 0.675*0.412*0.455 = 0.127$) based on 5,000 samples excluded zero (0.012 to 0.368), indicating a significant indirect effect (Field, 2013).

Further analyses indicated that receiving an *unconditional* offer indirectly influenced gratitude through attributions of helpfulness—i.e. the bootstrap CI for this effect (a_1d_{21}) did not straddle zero (0.027 to 0.587). However, there was no evidence that receiving an *unconditional* offer influenced reciprocity tendency *directly* ($c' = 0.286$, $p = .19$), or that receiving an *unconditional* offer influenced reciprocity indirectly through gratitude *only*, as the CI for this indirect effect (a_2b_2) included zero (-0.144 to 0.633).

3.3.4.3. Analyses of Behavioural Data (i.e. Repayment)

As revealed in Figure 3.4, twenty-one recipients who had accepted their conditional offers were asked to repay. Of all these 21 recipients eight (i.e. 38.1%) defaulted by either repaying *partially* or *nothing*, while the rest ($N = 13$, 61.9 %) all repaid fully. Interestingly, scrutiny of these eight defaulters response revealed only one instance of *zero* repayment, while the rest ($N = 7$) on average repaid 58.4% of their expected repayment. A Wilcoxon Signed-ranked test demonstrated a significant discrepancy, ($Z = 2.53$, $p = .012$), between the defaulter's 'Expected Repayment' (M: 86.6) and his/her 'Actual Repayment'

(M: 38.9). This suggested that a significant level of defaulting occurred among these conditional recipients and that is attributable to those 8 defaulters.

Table 3.12 Effect of Level of Conditionality on Player 2s' (who received conditional offers (N=24)) decision making (acceptance and repayment)

		Levels of Conditionality of offers received			
		Interest-Free	Partial Repayment	20%-Interest	
		N	*N*	*N*	Fisher's Exact Test *p* (2-tailed)
1. Offer Acceptance (*N* =24)	Accept	8	8	5	
	Decline	2	0	1	
	Overall	10	8	6	.447
2. Repayment (*N* =21)	Cooperate	6	5	2	
	Default[1]	2	3	3	
	Overall	8	8	5	.461

Note. [1] I classify 'defaulters' as recipients (N=21) who accepted their offers but under-repaid (either repaid partially or nothing).

In addition, the Fisher's exact test was conducted to examine whether the level of *conditionality* of the offers would influence his/her decision to (1) accept (or decline) the offer, and (2) to cooperate (i.e. to fulfil his/her obligated repayment) or to under-repay (partly or totally). As shown in Table 3.12, neither decisions were significantly influenced (all *p*s >.05) by the level of conditionality of the offers. This indicated that whether or not recipients of *conditional* offers were treated 'more harshly' did not affect their decisions to accept (or reject) and their decisions to cooperate (or default).

3.3.4.4. Testing Hypothesis 3

H3: P2s who received *conditional* offers, compared to *unconditional* offers recipients, should report more indebtedness, with this indebtedness being *negatively* related to the intended and actual repayment by these recipients

As illustrated in Table 3.10, recipients of *conditional* (*M*: 4.46, *SD*: 2.19) and *unconditional* (*M:* 4.96; *SD:* 1.72) offers felt similarly (p >.05) indebted to their helpers. Contrary to the above review of the literature, indebtedness was found *positively* and *strongly* correlated with positively-valenced emotion items such as gratitude, r (59) = 0.721, p <. 001, and

tendency to reciprocate, r (59) = 0.518, p <. 001, while negatively correlated to annoyance, r (59) = -0.423, p<. 001. P2s' indebtedness, meanwhile, was not correlated (p > .05) to their perception of P1s' genuine helpfulness.

Table 3.13. Effect of P2s' (who received and *accepted* their conditional offers (N=21)) Emotion on their later Repayment Magnitude

		Gs	As	Is	Os	Reas	PGH	Cost	Rec
Repayment	Pearson's *r*	.159	-.058	-.032	.292	-.005	-.029	-.094	.089
Percentage	*p* (two-tailed)	.490	.804	.890	.198	.981	.901	.686	.702
	N	21	21	21	21	21	21	21	21

Note. Gs = State Gratitude; As = State Annoyance; Is= State Indebtedness; Os = Perceived Obligation to Repay; Reas = Perceived Reasonableness of Partners' decision; PGH = Perceived Genuine Helpfulness; Cost= Perceived Low Cost; Rec = Eagerness to Reciprocate. Repayment Percentage = (Actual/ Expected Repayment) * 100%.

In addition, there was no evidence that P2s' (N=21) repayment magnitude was correlated (all *p*s>. 05) with their emotions (e.g. indebtedness, gratitude etc.) the moment they *accepted* that normatively unfair offers. Table 3.13 details the correlational analyses with respect to the magnitude of repayment and emotions of P2s. All in all, the present data did not support H3.

3.4. Discussion

3.4.1. Summary of Results

3.4.1.1. Injunctive Fairness and Gratitude

The present data illustrated that conditionality of an offer of help alone, when the recipient is not aware of the options open to the helper, can significantly influence how recipients interpreted the helpers' intentions, so as the recipients' feelings of gratitude, and their eagerness to reciprocate. In addition, the results show that the greater the perceptions of the helpers' intentions to be helpful (influenced by offer conditionality) the greater the level of gratitude towards them, and resulting in a greater tendency to reciprocate.

Furthermore, the behavioural data revealed that when recipients received a *conditional* offer, nearly 40% of them (8 out of 21) acted uncooperatively despite prior acceptance (of their obligation), either giving less

than they agreed or defecting completely. This indicated that people, albeit lacking awareness of the options open to helpers, still made differential judgments based solely on whether they received a *conditional* or *unconditional* offer. I interpret this as an example of the adherence to the injunctive fairness norm. That is, in the absence of information about the helper, recipients could only infer their helpers' intent to be helpful with reference to the normative beliefs about what an average 'fair' person *should* do when the opportunity to be in a position to help was determined by chance.

3.4.1.2. Gratitude versus Indebtedness

Additionally, the present data showed that people's tendency to want to reciprocate when treated fairly and be less cooperative when treated less fairly (in a *normative* sense) is, in part, dependent on attributional and emotional processing. People who inferred that the helpers' intentions were positive—feel gratitude towards—were more inclined to be eager to help them back in the future. Thus intentions and gratitude are two key processes that help explain the link between conditionality of offers and willingness to reciprocate.

Nonetheless, the same logic does not apply to indebtedness. While indebtedness was positively related to one's desire to reciprocate—contrary to our prediction that P2s here should favour cognitive restructuring over reciprocation, indebtedness was not correlated with offer conditionality and intent perception. This implied that indebtedness, unlike gratitude, was uninfluenced by normative fairness. An indebted recipient may still strive to reciprocate irrespective of the intent of his/her benefactor, and also whether or not that benefit constituted a fair gesture. This indeed echoes Tsang's (2006a) observation that while perceived benevolence was crucial in cultivating

gratitude, recipients experienced similar indebtedness whether their helpers were acting prosocially thanks to a *benevolent* or an *ulterior* reason.

3.4.1.3. The Mediational Model-Role of Normative Fairness Perception on the Gratitude-reciprocity link

Meanwhile, the present mediational model demonstrates that injunctive normative beliefs about fairness are important with respect to intentions (Bicchieri & Chavez, 2010) and emotional responses associated with cooperation and reciprocation (Komter, 2004; Nadler, 2012). Recipients in Experiment 1 had no knowledge about who their helpers were, what their helpers had done in the past, would never meet them, and importantly, knew nothing about the array of options facing the helpers. All they knew was that their helpers' good fortune to be in a position to help was by chance.

Consistent with the notion that under these circumstances people should act generously and help for free (Elster, 2006), the results show clearly that when this is not the case the recipients would view these helpers as *less genuinely helpful*, feel *less gratitude* and are *less likely to want to reciprocate* to them. In fact, this reduced willingness to reciprocate resonates with the indirect reciprocation literature which evidences that we prefer to help 'kind' people with a good reputation (See Milinski, Semmann, & Krambeck, 2002; Nowak, 2005, 2006; Nowak & Sigmund, 2007; Semmann, Krambeck, & Milinski, 2005). Here the results indicate that reputation can be inferred from very minimal information, that is, the conditionality of help only (Ksenia, 2008).

3.4.2. Interesting Observation- Phenomenon of 'Cheap-riding'

While only a relatively small segment of recipients who received conditional offers a non-negligible percentage (38%) of them still showed

uncooperative behaviour, and interestingly all but one of these defectors chose to be somewhat 'altruistic' by repaying the majority of their obligated repayment. It is, important and theoretically intriguing to examine the psychological or situational factors that motivate someone to engage in this kind of 'impure' free-riding or 'cheap-riding' (Asch, Gigliotti, & Polito, 1993; Bchir & Willinger, 2013; Cornes & Sandler, 1984; Krishnamurthy, 2000).

Such cheap-riding may symbolise dissatisfaction with the helpers, the actual value the recipients place on the offer or perhaps a means to irk the helpers by deliberately underpaying on their agreed repayment. This reduced backtransfer has been documented before in trust games when the recipient knew in advance their trustors could have refrained from making their investment conditional (or punishable) (see Fehr & Rockenbach, 2003). Here the data revealed a similar phenomenon in the absence of this knowledge. Fehr and colleagues (Fehr & Rockenbach, 2004; Fehr & Schmidt, 2006) had interpreted this as evidence for 'strong reciprocity' (Wiessner, 2005).

But then one question remains: Why bother to pay back at all, even at a *reduced* level? The recipients in this game, if they wished to, could have retained all the money (with no repercussion). However, it appears that these recipients deliberately under-repaid as to express their disapproval of their helpers' breach of normative fairness. I am inclined to interpret this as a means of giving these unfair helpers their 'just deserts' (Scott, Matland, Michelbach, & Borenstein, 2001; Weinstein, 2011; Wenzel & Thielmann, 2006) and signally disapproval designed to deter their helpers from being ungenerous to others in the future (Gromet & Darley, 2009; Okimoto & Wenzel, 2008). However, taking everything may enrage the helpers and even worse, set the stage for them

to retrospectively rationalise their initial amoral acts (i.e. not being fair) (Carroll & Meeks, 1999; Hartman & Nelson, 1999). It may also result in repugnance at the recipient that generalises to others hence dampened their desire to help in the future. Thus not repaying anything may appear inadvisable.

On the contrary, partial repayment signals to the helpers that they were not nice for starters – did not observe the injunctive norm – by indicating via partial repayment what the recipients believed the (conditional) help was really worth. This, therefore, may serve to make the helper feel guilty. These feeling of guilt are likely to render the helpers more cooperative in the future (See Boster et al. (2016) for a meta-analysis on guilt-compliance link). Thus partial repayment might be a means to enforce normative fairness in a way similar to altruistic punishment (Fowler, 2005). However, at present I could not rule out any alternative perspectives (e.g. the recipients under-repaying out of sheer selfishness) that could account for the observed partial repayment.

3.5. Implications of Chapter 3

The present chapter addressed a few things. First, it demonstrated the importance of injunctive norm in functioning as a basis on which people evaluate whether they were fairly (or unfairly) treated. And importantly, this evaluation would consequently influence people's attribution of the helpers' intent, and therefore influence how much *gratitude* was felt and ultimately one's desire to reciprocate. Additionally, this path model could complement the meta-analysis (presented in Chapter 2) by highlighting another potential moderator— i.e. the perception of normative fairness—in influencing how gratitude (as an affective *state*) relates to one's prosocial intention.

Secondly, the current data acknowledged the divergence of indebtedness and gratitude which was well-documented in the literature (Schaumberg & Flynn, 2009; Tsang, 2006b; Watkins et al., 2006). Particularly, the present analyses highlighted the lack of association between perceived fairness, or perceived genuine helpfulness, and recipients' indebtedness. That is, recipients could still feel indebtedness (and want to reciprocate accordingly) despite the lack of perceived helpful intent or the fact that the arrangement was not injunctively fair. Nonetheless, the present path model (Figure 3.5) indicated that this was clearly not the case for *gratitude*; as gratitude was more strongly experienced upon receipt of injunctively fair offers and when helpers were regarded genuinely helpful. This should add to the literature by highlighting the role of indebtedness—as a psychological state—in fostering reciprocal economic behaviours, considering that indebtedness is, to my understanding, a basically unexplored domain in the experimental economics literature.

Finally, the behavioural data revealed that upon being unfairly treated recipients displayed a non-negligible extent of sanctioning by breaching their agreement. It is, however, still premature to draw many conclusions as only a small fraction of recipients (N = 21, i.e. 34%) had accepted the *conditional* offer (thus giving them the very opportunity to under-repay), not to mention only eight of them had 'cashed in on' this sanctioning option. And although a few speculations have been made on motives behind the observed 'cheap-riding'—e.g. to give the helpers their 'just deserts' (Wenzel & Thielmann, 2006; Santos & Rivera, 2015)—none were scrutinised at present.

Hence, the primary objective of Experiment 2—which will be thoroughly discussed in Chapter 4—was to delve into the *situational* or

psychological factor (s) that would motivate, and potentially perpetuate, this kind of 'cheap-riding' behaviour (Cornes & Sandler, 1984). In particular, via Experiment 2 I intended to examine, and potentially disambiguate the psychological parameters which underlie one's decision to cheap-ride, cooperate, and free-ride on a taxed, or repayment-bound benefit.

Chapter 4

Gratefully received, Gratefully Retaliate, or Cheap-rode? Examining the Role of emotion in Cheap-riding in Economic Exchanges

(Experiment 2)

A version of the present chapter has been presented as:

Ma, L.K., Tunney, R.J., & Ferguson, E. (2014). *Why cooperate if being a nice guy does not pay? Implications on Situational and Dispositional Determinants of 'Cheap-riding' Behaviours.* Poster presented at the British Society for the Psychology of Individual Differences (BSPID) Annual Conference 2014, City University of London

Ma, L.K., Tunney, R.J., & Ferguson, E. (2015). *Gratefully Perceived, Gratefully Rejected, or Repaid?—The role of gratitude and perceived fairness in economic interactions*. Poster presented at the 16th International Conference on Social Dilemmas (ICSD), Chinese University of Hong Kong.

4.0. Overview of Chapter 4

I observed a non-negligible degree of *cheap-riding* in Experiment 1 among participants who accepted the injunctively unfair offers. *Cheap-riding* at present was operationally defined as a recipients' *partial repayment* of an *accepted conditional* offer. While speculations had been raised on why people would engage in such an economically irrational act (e.g. provisions of 'Just Deserts' (Weinsetein, 2011; Wenzel & Thielmann, 2006)) all these warranted empirical scrutiny which belongs to the focus of the present chapter. Additionally, the small fractions of recipients i) being 'eligible' to (i.e. 21/ 61= 34%; N = 21), and ii) deciding to (i.e. 8/61= 13.1%; N = 8), under-repay their *conditional* offers in Experiment 1further necessitates a relatively *less under-sampled* and more *customized* follow-up investigation in Experiment 2.

Despite being a sparingly examined research topic the notion *cheap-riding* has been discussed (or even examined) in different domains. Those included, but not confined to, the i) Public Goods Game (e.g. Asch et al., 1993; Bchir & Willinger, 2013; Isaac, Schmidtz, & Walker, 1989), ii) Marketing (e.g. Krishnamurthy, 2000; Roma & Perrone, 2010; Scherer, 1993), and even the iii) International Relations (e.g. Borcherding, 1981; Kennedy, 2015; Lu, 2016; Oneal, 1990) literature. As such, *cheap-riding*—which is conceptualised as a potential contributor's endeavour to piggyback on the high contributions of others (e.g. ones' teammates) (Stigler, 1974; Thompson, 1987)—should be far from just an experimental artefact.

However, none of the abovementioned empirical works had touched upon the psychology behind cheap-riding. To date there is still a gap in the literature on the role of *emotions* in inciting, and potentially sustaining, *cheap-*

riding. Thus, to fill this gap—which constitutes another crucial objective of the present chapter—I examined the *psychological* correlates (e.g. state gratitude, indebtedness, or even experienced warm-glow (Ferguson & Flynn, 2016) etc.) of *cheap-riding* in the present economic exchange context.

I administered a one-shot game that specifically scrutinised i) the 'baseline' response (i.e. to *fully* repay ('cooperate'), *utterly not* repay ('free-ride'), or just *partially* repay ('cheap-ride') to a 'non-free' *conditional* offer (Ma et al., 2014), and importantly, ii) whether the benefit-triggered state gratitude (Lambert et al., 2009) and indebtedness (Greenberg & Shapiro, 1971; Hatfield & Sprecher, 1983) will be predictive of people's cheap-riding decisions. Via this one-shot, variant of Trust Game I assessed also the motives (which will be discussed later) behind recipients' repayment decisions. In so doing I explored whether *cheap-riding* would, compared to *cooperation* and *free-riding*, be originated from a unique pattern of psychological correlates.

In the present game each participant played in pairs with an anonymous—in reality *fictitious* —'partner'. Unlike Experiment 1, all participants (N =135) were pre-programmed to 'lose out' on the dice-rolling and were accordingly entitled to zero bonus. Participants then received an offer from their 'fortunate', victorious partners. All these offers were conditional, as recipients learned that upon acceptance they all undertook an obligation to repay. Acceptors (N = 87) were then allowed to decide whether to a) pay up *in full* (i.e. 'cooperating', N = 59), b) to 'cheap-ride' via just giving back *partially* (N = 17); or c) to entirely evade the payment (i.e. 'free-riding', N = 11).

In short, Experiment 2 should represent a continuation of Experiment 1 by featuring a more focused and comprehensive investigation into *cheap-riding*.

Additionally, via the psychometrics I examined how *gratitude* (as both a *state* and a *disposition*) and *indebtedness* (as a *state*) would be related to one's trustworthiness or the lack thereof (i.e.to breach a repayment 'contract' via *cheap*- or *free*-riding). Furthermore, by examining the motives of repayment I endeavoured to disentangle the psychology of *cheap-riders* from that of the *cooperators* and that of the free-riders.

4.1. Introduction

4.1.1. What Cheap-riding constitutes?

Experiment 1 showed that participants when subjected to normative unfairness (Elster, 2006) would react by *partially* breaching a repayment obligation. I termed this deliberate, *reduced* back-transfer 'cheap-riding' in the present thesis. Indeed, *cheap-riding* in the wider literature was similarly conceptualised as a contributor's endeavours to just give as little as possible (Asch et al., 1993; Cornes & Sandler, 1984) with the intention to exploit the other contributors' high contributions (Thompson, 1987). Thus, as opposed to a total *avoidance* of contribution which is commonly understood as '*free*-riding' (Krishnamurthy, 2000; Olson & Perl, 2005), *cheap*-riding is characterised by, a) contributing the *bare minimum* (Schmitz, 1995), and crucially, b) the cheap-riders' *cost* (i.e. their non-zero inputs) being outweighed by their share of *benefits* (Kennedy, 2015; Olson & Zeckhauser, 1966).

While the *cheap-riding* has been empirically examined in the Public Goods Games literature (e.g. Bchir & Willinger, 2013; Marks, Lehr, & Brastow, 2006; Isaac et al., 1989), as abovementioned there is also evidence that *cheap-riding* indeed exists in myriads of interpersonal, societal, or even international contexts. Those include and are certainly not limited to, 1)

churchgoers contributing ‘relatively little to their congregations (pp.125)’ as they take advantage of ‘a few members who contribute far more than their “fair” share (pp.125)’ (Olson & Perl, 2005); 2) other nations’ exploitation (via their respective internal healthcare policy-making) of the American pharmaceutical manufacturers’ Research-and-Development efforts (Scherer, 1993); 3) the alleged inadequate efforts from China in global energy security preservation (Kennedy, 2015); and 4) the United States shouldering an overwhelming burden of military defence against the Soviet Union relative to her fellow NATO member states such as Japan and France (Thompson, 1987).

4.1.2. The Main Objectives of the present Experiment

Nonetheless, it remains unaddressed as of what motivates people to cheap-ride (especially when ‘free-riding’ is a feasible option). This phenomenon appears counterintuitive because it deviates from what is normally expected from a rational, profit-maximizing economic agent. As reiterated in Chapter 3, the *conditional* offer acceptors could have defaulted at ‘full throttle’ with neither possibility of being recognised (and accordingly reprimanded), nor incentives to manage reputations (Güney & Newell, 2013) thanks to the double-anonymous, one-shot game design (e.g. Hoffman et al., 1994). Meanwhile, shirking from an obligation, albeit *incompletely*, hardly constitutes the most moral gesture either. Taken together, cheap-riding should make little sense from a *rationality* standpoint, therefore hinting that *psychological* factors may instead underlie people’s decisions to cheap-ride.

As addressed in Section 4.0, despite the existing coverage in the wider literature it remains unexamined in regards the psychological correlates of

cheap-riding. Specifically, to date there is still an absence of empirical assessment of the role of *emotions* in inciting or restraining cheap-riding. Accordingly, one important objective of Experiment 2 concerns the scrutiny of the *psychological* correlates of *cheap-riding*. Importantly, via Experiment 2 I intended to disambiguate the psychology among people who choose to i) stubbornly adhere to a prior commitment despite temptations to default (i.e. *cooperating*); ii) make the most out of every possible means in pursuit of self-gain (i.e. free-riding); and iii) somewhat cross the 'moral' line by breaching the contract (Shavell, 1980) but refuse to free-ride on others (i.e. *cheap-riding*).

The present chapter also shed light on an alternative aspect of the *gratitude-prosociality* association. That is, the role of *state* and *dispositional* gratitude in restraining sanctioning behaviours (i.e. the Moral Motive Hypothesis of Gratitude (McCullough et al., 2001; McCullough et al., 2008)) in *cheap-* or *free-riding* at present. Data of Experiment 1 highlighted the role of *gratitude* in conjunction with *intent* attributions—both of which were subjected to the injunctive norm (Cialdini et al. 1990; Kallgren et al., 2000) — in prompting *prosocial intention* following the receipt of a normatively *fair* benefit. Experiment 2, in contrast, would examine how *gratitude* would serve to mitigate cheap- or free-riding as triggered by a normatively *unfair* benefit.

Analyses of Experiment 1 also revealed the well-documented divergent routes via which *indebtedness* and *gratitude* were related to *increased, direct* reciprocal intentions (Schaumberg & Flynn, 2009; Watkins et al., 2006; Tsang, 2006b). Feeling of *indebtedness* was shown to also correlate with prosocial intentions (i.e. 'willingness to reciprocate'), although this correlation, unlike *gratitude*, was independent of whether the offer was *fair* (See Chapter 3).

Meanwhile, Greenberg and colleagues (Greenberg, 1980; Greenberg and Westcott, 1983) had theorised that indebtedness could instigate a benefit-triggered sanctioning response, following a distorted benefit re-appraisal which is intended to manage one's feeling of indebtedness (Hatfield & Sprecher, 1983) (See Chapters 1 and 3). While this theory was not confirmed (See Chapter 3), the evidently small sample (i.e. $N = 21$) eligible for the analyses in Experiment 1 certainly makes it difficult to rule out the possibility that the theory is still valid. Therefore, via a less under-sampled experiment—in which 135 participants would be subjected to a normatively unfair treatment (i.e. a *conditional* offer receipt)—I strive to re-examine the hypothesised indebtedness-sanctioning link via benefit appraisals (i.e. the Cognitive Restructuring Hypothesis (Greenberg, 1980; Greenberg & Westcott, 1983)).

It is, meanwhile, worthwhile to point out that the chronological order of the present three experiments and the meta-analysis was not identical to the ordering of the present thesis. The meta-analysis (i.e. Chapter 2) on the *gratitude-prosociality* association was indeed conducted after the completion of data collection of Experiments 1 to 3 (i.e. Chapters 3 to 5). Indeed, as outlined in Chapter 2 there is considerable inconsistency in the literature in regards how strongly gratitude (as a *state* or a *disposition*) is linked toward prosociality. This, coupled with the non-existence of empirical works on the indebtedness-prosociality link, should render any valid a-priori power analyses to estimate the necessary Ns very difficult. A more in-depth discussion about the statistical power issue and the null findings of Experiments 1 to 3 will be featured in the *General Discussion* chapter (i.e. Chapter 6).

4.1.3. Possible Factors behind Cheap-riding

Below (i.e. Sub-sections 4.1.3.1 and 4.1.3.2) I discuss certain key factors that may contribute to or undermine *cheap-riding*. They included the provision of 'Just Deserts' (Weinstein, 2011), gratitude and indebtedness.

4.1.3.1. Provision of 'Just Deserts' (Carlsmith, Darley, & Robinson, 2002)

One suggested possible motive behind cheap-riding in Experiment 1 lies in the provision of 'Just Deserts' (Darley, Carlsmith, & Robinson, 2000; Santos & Rivera, 2015). That is, via paying back *partially* the cheap-riders intended to communicate their discontent with their helpers' belittlement of normative fairness, and that the amount repaid should be indicative of what they perceived their helpers 'genuinely deserved'(Ma et al., 2014). This explanation resonates with the 'Just Deserts' Theory of Retribution (Carlsmith et al., 2002; Starkweather, 1992; Wong, 2006) which argues that punishment cannot be equated to sheer vengeance, as a fair punisher will also contemplate how a punishment would measure up to the damage done (Moore, 1997). Accordingly, an *appropriate* sanction should be one that is *proportionate* to the initial harm so that the 'moral balance' can be restored following the implementation of such (Dyckman, 1998; Wenzel & Thielmann, 2006).

Going back to the cheap-riding situation of Experiment 1, participants were aware a priori that their repayment (or the lack thereof) would indeed affect their co-players' ultimate payoffs. As a result, to cheap-ride—when free-riding was also feasible and clearly a more rational choice—in this situation can be interpreted as the recipients trying to convey a message like: 'You are a cheapskate and I am having a go at you for that, and I'd like you to have a re-think of what you did!' Thus, these cheap-riders were making a case that the helpers *deserved* their comeuppances, and hopefully this may get the helpers to

repent a bit and stop doing that to others in the future (Ma et al., 2014). It is worth noting that *cheap-riding* of this kind should transcend beyond the simple negative 'tit-for-tat' (Brown, 1986), given that the main purpose of cheap-riding here is not about avenging oneself (if that is the case people should have 'free-rode' instead), but for preserving mutual adherence to normative fairness in the long run (i.e. '*Strong Reciprocity*', Bowles and Gintis (2004)).

Building on this logic, dodging the repayment in its entirety (i.e. *free-riding*) may have defeated this purpose. That is because the null payment may offer the perfect excuse for any 'free-rode' helpers to retrospectively rationalise their initial miserliness (Carroll & Meeks, 1999). As a result, not only will these norm-breaking helpers refuse to repent their own misdeed, they will also be livid with the recipients' hostile gestures and therefore become even more inclined to be ungenerous to others in the future (Ma et al., 2014).

Cheap-riding, on the contrary, should give rise to a lower likelihood for such a self-justification. I argued that the act of *cheap-riding* may propel the helpers to ponder why they were just *partially* but not *totally* defaulted. Consequently, these 'cheap-rode' helpers should be more able to think through if their initial fairness-violating act (i.e. only helped *conditionally*) was what earned them the *partial* under-repayment (Ma et al., 2014).

Taken together, this whole 'Just Desert' perspective of sanction (Carlsmith et al., 2002; Darley et al., 2000) would argue that *cheap-riding* may serve a similar purpose as altruistic punishment (Boyd, Gintis, Bowles, & Richerson, 2003; Fehr & Gächter, 2002; Fowler, 2005) as an enforcer of normative fairness (Fehr, Fischbacher, & Gächter, 2002) . That is, via incurring a cost to themselves in their *partial* repayment the cheap-riders aimed to incite

more prosociality (in the form of normatively fair behaviours) from their partners in the future (See Boyd et al., 2003; Egas & Ridel, 2008).

4.1.3.2. Gratitude, Indebtedness, and Cheap-riding

As the present thesis primarily concerns the investigation into how *gratitude* nurtures *cooperativeness* in economic exchanges, there is no reason to not look into how *gratitude* (as a state or a trait) may relate to cheap-riding. Below I discuss also how state *indebtedness* might prompt *cheap-riding*.

1) Gratitude and Cheap-riding

McCullough and associates (McCullough et al., 2001; McCullough and Tsang, 2004; McCullough et al., 2008) maintained that *gratitude* fosters endeavours to repay the benefactors' kindness via one of its fundamental functions as a *moral motive* (i.e. the 'Moral Motive' Hypothesis). This *Moral Motive* perspective stipulates that the *gratitude* emotion should push the recipient toward not only contributing to the benefactors' future well-being, but also the inhibition of any destructive acts toward whom gratitude was felt.

Moreover, the *Moral Barometer* Hypothesis of gratitude (McCullough, et al., 2001) argues that the mere presence (or absence) of the *gratitude* experience already signals to a recipient whether a benefit was worth feeling grateful for. People only experience gratitude toward a benefit if certain parameters are met (McCullough et al., 2008). They included; a) a recognition that the benefit symbolises a token of *benevolence*; b) a perception that the benefit is of *value*; and c) the knowledge about the *intentionality* of the conferment of that benefit (i.e. whether that was *by chance* or *deliberate*). Thus, the above two moral hypotheses of gratitude imply that *gratitude* and the punishment of one's benefactor should have no room for co-existence.

Nonetheless, as addressed above *cheap-riding* at present should represent a punitive act which is intended to serve a *correctional* purpose in the long run (Egas & Ridel, 2008; Fehr et al., 2002)—as the *cheap-riders* believed that via giving the helpers their 'just deserts' (Carlsmith et al., 2002; Darley, 2001; Darley et al., 2000) the latter will make amends by showing more adherence to the fairness norm in the future (Ma et al., 2014). As a result, it should be impossible to fathom a situation in which on one hand, someone feels grateful toward a helper's benevolence but at the same time, still feels like punishing that very same helper to assure that he/she will treat others better in the future.

The Social-Cognitive Model of Gratitude (i.e. SCMG (Wood et al., 2008) theorised that people with high *dispositional* gratitude have a tendency to see the *positives* in the daily encounters. Thus these people should be more prone to 'over-rate' the benefits bestowed upon them (by perceiving more kindness in the benefit conferment), which should be translated to more intense feelings of gratitude (Nadler, 2012; Tsang, 2006b), and ultimately a higher tendency to return the kindness (See Tsang, 2006a, 2007; Tsang et al., 2012). Building upon this sequence of reasoning, there is again no reason to argue that *dispositional* gratitude would in any way encourage any punitive acts toward the benefactors in *cheap-* or *free-riding*. On the contrary, the SCMG should predict an *inverse, indirect* relationship between *dispositional* gratitude and defaulting (cheap- and free-riding inclusive) that is serially mediated (Hayes, 2013) by i) *benefit appraisals*, followed by ii) *state* gratitude. See Figure 4.1.

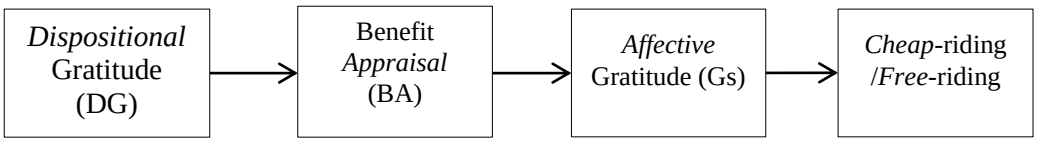

Figure 4.1 The SCMG (Wood et al., 2008) Predictions on Cheap-/Free-riding

2) Indebtedness and Cheap-riding

Greenberg and Westcott (1983) theorised that there are two major avenues through which a recipient could manage his/her benefit-triggered feeling of indebtedness. They are, 1) *direct* or *indirect* reciprocation; and 2) *Cognitive Restructuring*. While the former can be likened to the act of paying off a debt (Schaumberg & Flynn, 2009), the latter entails a re-appraisal of the exchange which, in the first place, prompted *indebtedness* (Greenberg, 1980).

Specifically, a recipient may revisit and accordingly query his/her *initial* appraisal of the whole benefit conferment scenario. Questions such as 'Was the gift really that kind-intentioned?' or 'Was it really that much of a fuss for that helper to have benefited me?' may be pondered over (Greenberg, 1980). All these thinking may—as theorised by Greenberg and Westcott (1983)—give rise to a *negative* and possibly *distorted* re-assessment of the benefit exchange (e.g. 'He/she is helping me because he/she has a 'benevolent' reputation to maintain), which accordingly rids the recipients of any indebtedness-invoked discomfort (Greenberg & Shaprio, 1971).

Additionally, the link between perceived *negative intent* and *sanctioning* has been well documented in the experimental economics literature (Fehr & Gächter, 2002; Falk & Fischbacher, 2006).As reiterated throughout the present thesis, *cheap-riding* in this context exemplifies a form of *sanction* intended for norm enforcement (Ma et al., 2014; Weinstein, 2011). Thus, the Cognitive Restructuring Hypothesis (i.e. CRH) (Greenberg, 1980; Greenberg & Westcott, 1983) will argue that benefit-triggered *indebtedness* may prompt a more cynical benefit re-appraisal—which should comprise a perception of ill intent from the

helper (Hatfield & Sprecher, 1983), with that perceived malice ultimately leading to a sanctioning response in cheap-riding.

4.1.4. Exploratory Aims of Experiment 2

Overall, via Experiment 2 I intended to complement Experiment 1 and examine how emotions (especially *gratitude* and *indebtedness*) would relate to *cheap-riding* in an economic game context. These were achieved with the assistance of a one-shot game that is similar to the one used in Experiment 1 (See Section 4.2 for details about the experimental design and rationales). Below (i.e. Sub-sections 4.1.4.1 and 4.1.4.2) I summarise the two principal exploratory aims and the respective predictions of Experiment 2.

4.1.4.1. Exploratory Aim 1: Does more (normative) unfairness count?

Chapter 3 indicated that people would respond differently upon receipt of a normatively *fair* (i.e. 'unconditional' offer) as opposed to an *unfair* (i.e. 'conditional' offer) treatment. Particularly, the serial mediational model (See Chapter 3) emphatically corroborated the significance of the injunctive fairness norm (Elster, 2006; Paddock, 2005) in guiding a positive helper intent attribution, which then prompts *gratitude* and ultimately a (direct) reciprocal intention. The behavioural data evidenced a display of sanctioning (mostly in the form of *cheap-riding*) among those *unfairly* treated recipients.

Via Experiment 2 I intend to further look into one interesting issue: is there an 'all-or-nothing' threshold for fairness attribution? Data of Experiment 1 revealed an absence of differences in *emotional* and *behavioural* responses from recipients of offers of varying *conditionality*, implying that an increasing extent of unfairness would not bother the recipients. Since the injunctive norm stipulated that a *fair* gesture in Experiment 1 entails the provision of a 'free'

help (Elster, 2006; Frey & Bohnet, 1995), the varying extent of expected repayment (i.e. conditionality) should connote different 'severity' of violation in normative fairness (Cialdini et al., 1990). As such, it should be logical to anticipate a *graded* pattern of i) emotional negativity (e.g. more indebtedness) so as positivity (e.g. less gratitude) towards, or ii) an overt sanctioning (e.g. cheap- or free-riding) against, offers with increasing (normative) unfairness.

Hence, one principal exploratory aim of Experiment 2—in which 135 participants were *all* due to receive an unfair conditional offer— entails how the increasing normative unfairness would relate to more emotional negativity (e.g. more *indebtedness* and less *gratitude*) and sanctioning (i.e. *cheap*-ride or *free*-ride). Similar to Experiment 1, participants acknowledged that their helpers had not legitimately 'earned' their positions as potential helpers (Fahr & Irlenbusch, 2000; Frey & Bohnet, 1995; Hoffman et al., 1994), thus all repayment obligations attached to the offer should constitute no moral legitimacy (Elster, 2006). As a result, *all* offers in Experiment 2 should be in principle normatively *unfair* (Ma et al., 2014), with the severity of this *unfairness* being a manipulation at present (see Section 4.2 for details).

4.1.4.2. Exploratory Aim 2: The Psychology of Cheap-riders

Further to scrutiny over how *situations* (i.e. injunctive unfairness) could contribute to people's decision to *cheap-ride*, a key objective of Experiment 2 entails the analysis the *psychological correlates* of cheap-riding. First, I analysed benefit-triggered emotions (i.e. the 'pre-repayment emotions) would predict people's repayment decisions. The current analysis would in particular shed light upon how state *gratitude* and *indebtedness* will be associated with participants' sanctioning decisions (i.e. *cheap*- or *free*-riding).

1) *Affective* Gratitude, Indebtedness and Sanctioning in Experiment 2

As elaborated above (i.e., sub-section 4.1.3.2) emotional *gratitude*, thanks to its two moral functions as a *moral barometer* and *moral motive* (McCullough et al., 2001; McCullough et al., 2008), should in no way promote endeavours to harm the benefactors' well-being. Hence, a strong *inverse* relationship between recipients' state *gratitude* and the magnitude of under-repayment should be expected. I also predicted a sharp contrast between the cooperators' *state gratitude* and that of both the cheap-riders and free-riders.

By contrast, *state* indebtedness could incite *cognitive restructuring* which results in perceptions of malicious intent in the helpers' gestures, with these negative attributions capable of inciting sanctioning behaviours. Greenberg and Westcott (1983) also stated that a *double-blinded, one-shot* exchange context will constitute an ideal breeding platform for *Cognitive Restructuring* to be resorted to ahead of *reciprocation*. Given that both these features were retained in the present game, it should be logical to predict a positive, indirect link between state indebtedness and magnitude of under-payment which is mediated by benefit appraisals. Figure 4.2 illustrates the path model outlined by this Cognitive Restructuring Hypothesis (CRH).

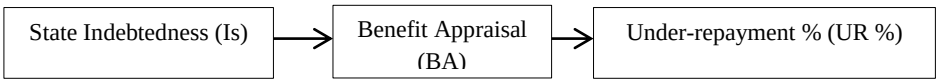

Figure 4.2 The CRH: effect of *state* indebtedness on under-repayment.

2) Differentiating *Cheap-riding*, *Cooperating* and *Free-riding*

Via eleven post-repayment survey items (as detailed in Table 4.1 below) I specifically probed into the motives behind the offer acceptors' various *repayment* decisions (i.e. to cheap-ride, cooperate or free-ride). Four items were adapted from Ferguson and Flynn (2016) to measure experienced warm-glow

(Cronbach's α = .802, *N* of items = 4) Warm-glow is defined as a sense of internal satisfaction derived from a person's act of cooperativeness (Andreoni, 1990; Harbaugh, 1998; Holmes, 2009) which has been extensively proven to underlie *prosociality* in the experimental economics literature (e.g. Khanna et al., 1995; Carpenter et al., 2008; Crumpler & Grossman, 2008). Hence, there is no reason to overlook how warm-glow may at present foster trustworthiness (i.e. cooperativeness) among the acceptors—thus in a way undermine their urge to *cheap*- or to *free-ride*. An example item will be 'It made me feel emotionally positive about myself to have repaid (or have avoided the repayment).'

Table 4.1. Survey Items for Repayment Motives

Questions	**Adapted From**
The fact that I repaid/did not repay made me feel good about myself	Experienced Warm-Glow (EWG) (Ferguson & Flynn, 2016)
It made me feel emotionally positive about myself to have repaid/have avoided the repayment	Ferguson and Flynn (2016)- EWG
I feel irritated by my partner's request for a repayment.	Self-created—NTFT
My repayment left me with a feeling of 'warm glow' inside	Ferguson and Flynn (2016)- EWG
I see my Repayment/Non-repayment as a means to punish my partner for his/her unreasonable request.	Self-created—NTFT
I would feel guilty if I didn't stick with my repayment decision.	Ferguson and Flynn (2016)- EWG
The way I responded to my partner's offer is the most appropriate way to restore fairness between myself and my partner .	Self-created—Just Desert
I see my Repayment/Non-repayment as a retaliatory act against my partner's offer	Self-created—NTFT
I see my repayment/non-repayment best signifies how grateful I feel towards my partner.	Self-created—NTFT*
By repaying/not repaying, I manage to offer my partner what he/she truly deserves.	Self-created—Just Desert
I repaid/ did not repay because I endeavoured to attain a relatively more equal payoff distribution between me and my partner	Self-created—Just Desert

*Reverse-scored for the Reliability Analysis. NTFT= Negative 'Tit-for-Tat'

Three self-created items (Cronbach's α = .649, *N* of items = 3) were included to measure whether the urge to provide the helpers with their 'just deserts' (Carlsmith et al., 2002; Darley, 2001; Weinstein, 2011) will be related to the acceptors' decision to *cheap-ride*. An example item will be 'By repaying or not repaying, I manage to offer my partner what he/she truly deserves'. I further included four self-created items (Cronbach's α = .650, *N* of items = 4) to measure how an acceptor's negative 'tit-for-tat' (i.e. NTFT) mentality

(Brown, 1986; Massey, 2009) will be related to his/her *cheap-* or *free-riding* decisions. An example item will be 'I see my repayment or non-repayment as a means to punish my partner for his or her unreasonable request'.

Taken together, via these eleven survey items (See Table 4.1) I intended to gather an insight into the mindset of a cheap-rider at present. And more importantly, I endeavoured to underscore if, and how, such a cheap-rider's mindset will deviate from that of a *cooperator* and of a *free-rider*.

3) *Dispositional* Gratitude and Under-repayment

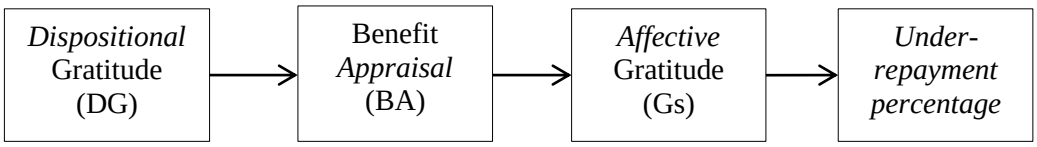

Figure 4.3 The SCMG serially mediated prediction of DG on Under-repayment

I tested how *benefit appraisals* and *state gratitude* could serially *mediate* (Hayes, 2013) the path between *dispositional* gratitude and under-repayment percentage (as an *inverse* measure of trustworthiness). Hence, by testing the path model as illustrated in Figure 4.3 I examined if *dispositional* gratitude would restrain defaulting (both *cheap-* and *free-riding* inclusive) the way as theorised in the Social Cognitive Model of Gratitude (SCMG) (Wood et al., 2008). This under-repayment percentage (i.e. UR %) is given by the formula: '[*Obligated* Repayment - *Actual* Repayment] * 100%.

4.2. Methods

4.2.1. Participants

One hundred and thirty-five students from The University of Nottingham participated in Experiment 2 (81 were females, mean age: 21.1 years, SD = 3.6 years). Each participant was paid an inconvenience allowance of £2 and was aware that they could earn up to £3.50 more.

4.2.2. Game and its Rationale

The experiment was administered using Z-Tree (Fischbacher, 2007). To begin with, each participant was seated in a separate cubicle and asked to read the instructions. Each session took place with groups of four to ten. All participants played the game on their own although they were told that they each would play with a random partner (i.e. a fellow participant) in pairs.

Each player was *initially* endowed with 150 money-equivalent 'Bonus Points' and was aware that he/she needed at least 50 points more to reach the 'Bonus Threshold' (i.e. 200) .Reaching this 200-point mark (i.e. Bonus Threshold'), by rolling the fair, six-sided dice once, was the only way participants could leave the experiment with any additional bonus money. The possible payoffs were a loss of 50 points, 0 points, and gains of 50, 100, 150, and 200 points. It was again emphasised that the magnitude of the number rolled had no direct association with the final allocations of bonus points.

Each player then learned that he/she had an in-game, unidentifiable and anonymous 'partner'. Neither competitions nor collaboration with this 'partner' would be necessary. Regardless of the actual die-roll, participants were all pre-programmed to end up with only 100 points which were below the 200-point threshold, leaving them zero bonuses. Each participant then learned that his/her 'partner' had emerged 'victorious' by acquiring 350 points at the end, entitling them to a £3.50 bonus. In short, unlike Experiment 1, all participants in the current game were potential *recipients* (i.e. P2s).

4.2.3. Receipts of the Normatively Unfair, Conditional offers

All participants then learned that their partners had offered to entrust part of their excess bonus points (i.e. 150) to help them attain that 200-point

mark. Specifically, by *accepting* these offers—which enabled the recipients to earn some bonus money—these acceptors took on the obligation to make varying back-transfers (out of their new earnings) to their helpers. Indeed, the lack of moral legitimacy for a 'winner' to make his/her sharing *conditional* in this particular context had been reiterated throughout the chapters. Thus, all offers as presently constructed should be injunctively unfair.

4.2.4. Manipulating Conditionality: Does more unfairness matter?

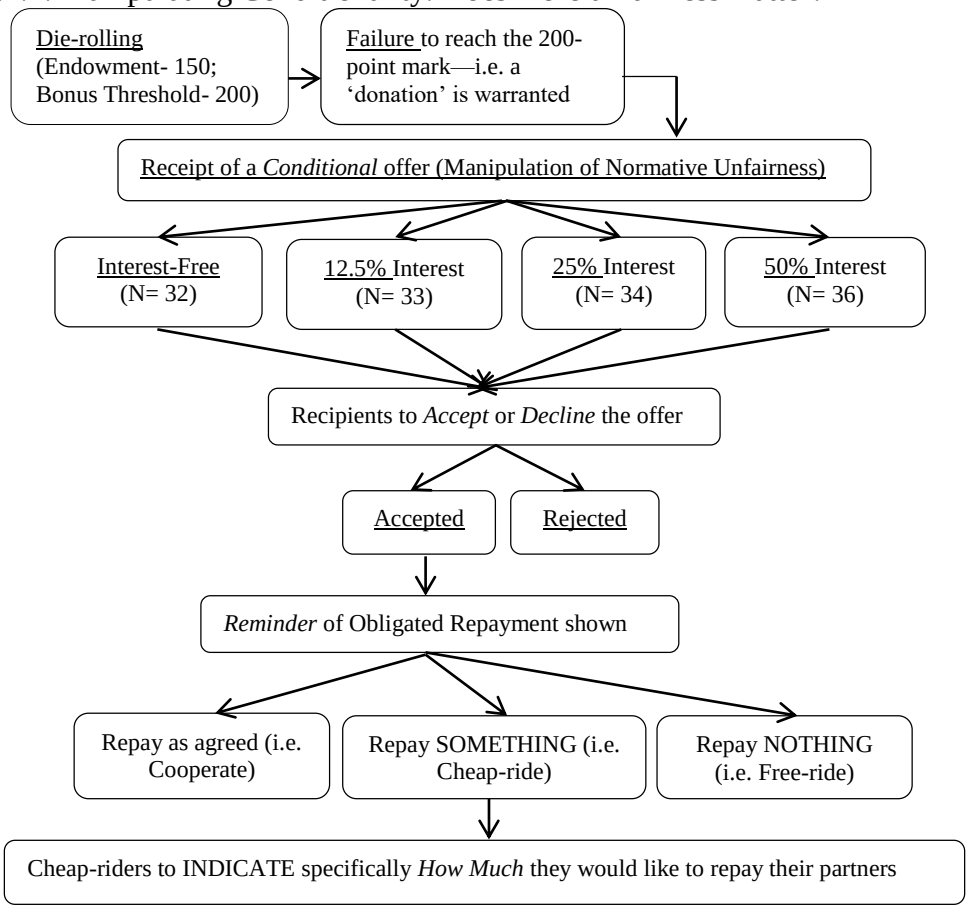

Figure 4.4. The sequence of behavioural decisions to be made by a participant

I manipulated this normative unfairness across offers by subjecting recipients to offers with varying conditionalities. These included; an '*Interest-free*' offer where the *acceptors* only had to repay the 'principal' ($N = 32$); or a 'surcharged' offer whereby acceptors would repay the *principal* plus an interest charge. The levels of these interest charges were further manipulated as a

participant could receive either a *12.5%*- ($N = 33$); or a *25%*- ($N = 34$) or a *50%*-Interest offer ($N = 36$). Recipients were told upfront and later on reminded that it would make more financial sense accepting the offer (in spite of the repayment) than rejecting that. Recipients *at this stage* had no idea about a) the availability of alternative offers; and 2) the non-binding nature of the repayment clause. Figure 4.4 illustrates the flow of behavioural decisions.

4.2.5. Recipients' Emotional Reactions toward the unfair offers

Table 4.2 Pre-repayment Survey Items

Pre-repayment Survey Items	**Constructs**
I am annoyed by my partner's decision	State *Annoyance*
How much gratitude would you feel towards your partner?	State *Gratitude*
I feel indebted to my partner	State *Indebtedness*
How much do you consider your partner's transfer is motivated by his/her sincere desire to help you?'	Perceived Genuine Helpfulness
I feel obliged to repay my partner.	State Obligation
My partner was being fair to have offered his/her help	Perceived Fairness
My partner's decision is reasonable.	Perceived Reasonableness
My partner's decision is understandable.	Understandability
How valuable do you consider that your partner's help is to you?	Perceived Value in Help
How much did it cost your partner to help you (1: *Nothing*; 7: *A Great Deal*)	Perceived Cost to Helper
I am eager to help my partner out if he/she is in need in the near future	Reciprocity Tendency

Recipients then rated items (i.e. pre-repayment ratings) in relation to their offer-triggered emotions. Table 4.2 presents the list of items and the *psychological* constructs represented by each of them. Particularly, the item 'how much gratitude would you feel towards your partner' was adopted to represent the construct *state gratitude* at present, which was used to examine the current gratitude-related hypotheses such as i) the *Moral Barometer* and *Moral Motive* Hypothesis of Gratitude (McCullough et al., 2001; McCullough et al., 2008), and ii) the Social Cognitive Model of Gratitude (SCMG) (Wood et al., 2008). The item 'I feel indebted to my partner' will represent the construct *state indebtedness* that is tested in the present *Cognitive Restructuring Hypothesis* (CRH) (Greenberg, 1980; Greenberg & Westcott, 1983). Additionally, following Wood et al.'s (2008) procedures, I averaged the three items 'Perceived *Value* in Help', 'Perceived *Genuine Helpfulness*' and

'Perceived *Cost to Helpers*' to form the composite 'Benefit Appraisals' (Cronbach's $\alpha = .691$, *N* of items = 3) —which is indeed an integral component in both the Social Cognitive Model of Gratitude (Wood et al., 2008) and *Cognitive Restructuring* Hypothesis (Greenberg & Westcott, 1983).

4.2.6. Behavioural Repayment Decisions

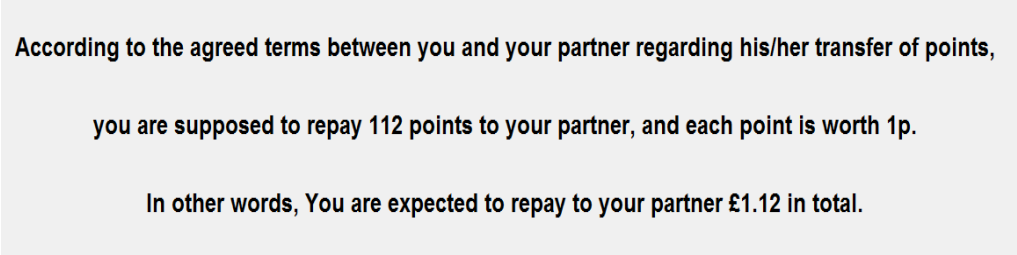

Figure 4.5 An example reminder of obligated repayment among offer acceptors

Participants who accepted their offers were then shown onscreen a *reminder* of how much they were expected to repay (See Figure 4.5). Immediately after that reminder participants were facing a *forced-choices* scenario as they contemplated whether to repay a) 'Nothing At All' (i.e. *free-riding*), b) 'Some of the Agreed Amount' (i.e. *cheap-riding*), or c) 'the Whole of the Agreed Repayment' (i.e. *cooperating*). Participants who selected to cheap-ride were then instructed to specify the magnitude of their desired repayment (See Figure 4.4). I conceded that while this explicit segregation facilitated the categorisation of repayment this may have also brought forth the '*Response Effect*' that might have predisposed the participants into selecting a particular option (Feldman, 1989; Zaller & Feldman, 1992). Refer to the General Discussion chapter (Chapter 6) for more discussions about the 'Response Effect' and how that may be resolved in future investigations.

4.2.7. Reasons for Repaying (or not repaying)

Immediately after repaying participants were instructed to rate 11 items (See Figure 4.5) that served to explore the motives behind the way they repaid or (did not repay). Sub-section 4.1.4.2 detailed the rationales for the inclusion of these items Participants were then shown onscreen their respective compensations, and consequently asked to fill out two questionnaires, namely the i) Impression Management Scale (i.e. IMS) (Paulhus, 1991), ii) Gratitude Questionnaire-VI (i.e. GQ-VI) (McCullough et al., 2002). Via the IMS (Cronbach's α = .797, *N* of items = 20) and GQ-VI (Cronbach's α = .690, *N* of items = 6) I measured respectively, participants' 1) tendencies to make decisions to manage own social impression, and 2) *dispositional* gratitude. The experiment ended here and the participants were debriefed and paid.

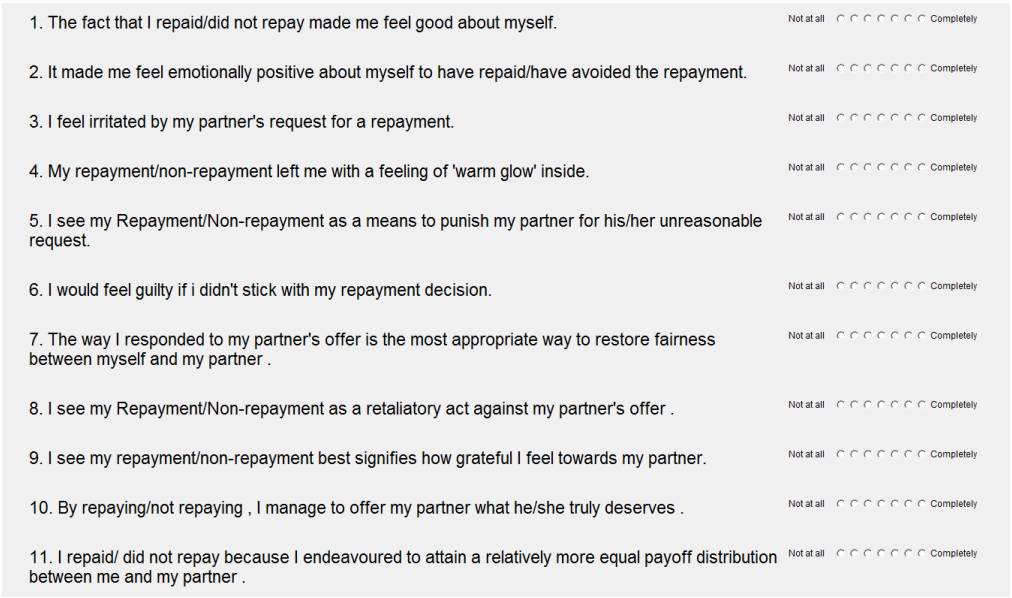

Figure 4.5. Post-repayment Survey Items

4.2.8. Ethics Statement

Experiment 2 was approved by the ethics committee of the School of Psychology at the University of Nottingham. All participants were older than 17 and all provided written, informed consent prior to participation as approved by the ethics committee (Approved 14th Aug 2013; Ref. Code: 332).

4.3. Results

4.3.1. Descriptive Data

All 135 participants received a *conditional* offer (32 received an 'Interest-free', 33 a '12.5%-interest', 34 a '25%-interest' and 36 a '50%-interest' offer). Of these 48 rejected their offers (i.e. 35.56% (48/135) rejection; 64.44% (87/135) acceptance, χ^2 (1, N = 135) = 10.7, p = .0011. Among these 87 acceptors 59 repaid *fully* (i.e. *cooperate*), 11 repaid *nothing* (i.e. *free-ride*) and 17 repaid *partially* (i.e. *cheap-ride*). Particularly, these 17 *cheap-riders* on average repaid 56.4% (*SD*: 31.5) of the obligations, which was not different (U = 54.00, p = .744) from that of the seven cheap-riders from Experiment 1 (*M*: 58.4%). Figure 4.6 illustrates the present decision tree.

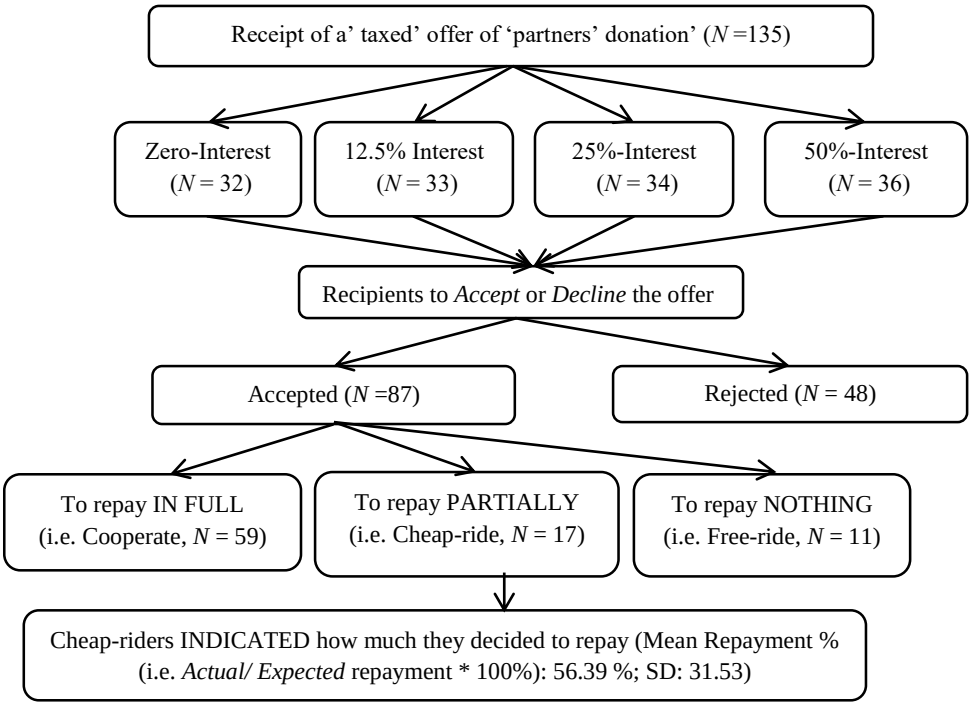

Figure 4.6. Decision Tree of the participants (N = 135)

4.3.2. Exploratory Aim 1: Does more (normative) unfairness count?

One key exploratory aim of Experiment 2 entails whether the increasing severity of normative unfairness would prompt a *graded* pattern of emotional negativity, and ultimately more sanctioning in the form of *cheap*- or *free*-riding.

Below I examined whether the present manipulation of injunctive unfairness (i.e. levels of conditionality) would impact on, a) participants' emotions toward the offers (i.e. Sub-section 4.3.2.1); or b) the acceptors' (N= 87) *categorical* and *numerical* repayment decisions (i.e. Sub-section 4.3.2.2).

4.3.2.1. Levels of Conditionality on Emotions towards offer

A one-way ANOVA was conducted to examine the effect of offer *conditionality* (i.e. Interest-free vs. 12.5%- vs. 25%- vs. 50%-Interest) over participants' offer-triggered emotions. Table 4.3 illustrates the findings.

Table 4.3. Effect of offer conditionality over Pre-repayment Survey Ratings

	Repayment Obligations (i.e. Levels of Offer Conditionality)									
	Interest-Free		12.5%		25%		50%			
	N	*M* (*SD*)	*N*	*M* (*SD*)	*N*	*M* (*SD*)	*N*	*M* (*SD*)	*F*-	*p*
State *Gratitude*	32	4.87 (1.54)	33	5.00 (1.35)	34	4.44 (1.24)	36	4.39 (1.40)	1.67	.177
State *Annoyance*	32	2.34 (1.41)	33	2.00 (1.23)	34	2.29 (1.40)	36	2.86 (1.73)	2.10	.104
State *Indebtedness*	32	4.16 (2.13)	33	4.67 (1.76)	34	4.09 (2.04)	36	4.64 (2.00)	0.81	.490
Obligation to Repay	32	5.13 (1.81)	33	4.67 (1.90)	34	5.09 (1.98)	36	5.03 (1.92)	0.40	.750
Perceived *Help Value*	32	5.00 (1.55)	33	4.76 (1.48)	34	4.50 (1.50)	36	4.81 (1.19)	0.69	.562
Perceived *Fairness*	32	5.06 (1.34)	33	5.03 (1.59)	34	4.97 (1.27)	36	4.58 (1.57)	0.82	.486
Perceived *Helpfulness*	32	4.16 (1.59)	33	4.06 (1.66)	34	3.71 (1.70)	36	4.31 (1.75)	0.80	.498
Perceived Cost to Helper	32	3.66 (1.73)	33	3.88 (1.83)	34	3.59 (1.58)	36	3.92 (1.95)	0.28	.837
Reciprocity Tendency	32	5.03 (1.60)	33	5.24 (1.62)	34	5.50 (1.31)	36	5.17 (1.68)	0.53	.661

Note.'12.5%'= 12.5%-Interest Offer; '25%'= 25%-Interest Offer; '50%' = 50%-Interest Offer

Overall, the data (See Table 4.3) revealed no evidence (all *p*s >. 10) that increasing levels of unfairness had engendered different emotional negativity (or positivity). This echoes Experiment 1's results whereby a more unreasonable offer, which in principle epitomises more injunctive *unfairness*, would not irk the recipients (in an existing unfair arrangement) even further.

Interestingly, results of the independent samples t-tests (See Table 4.4 below) demonstrated that compared to the 87 acceptors, the 48 rejectors reported lower offer-triggered 1) state *gratitude*, 2) state *indebtedness*, and 3) a

less positive overall benefit appraisal (Wood et al., 2008) (all *p*s <. 05, two-tailed). The rejectors also deemed the offers as less *reasonable* (*p* = .048) and were marginally (*p* = .061) less *willing* to *reciprocate* (Ma et al., 2014).

Table 4.4 Supplementary Analysis—Independent samples T-Tests on *Pre-repayment* Ratings by Decisions to whether *Accept* or *Reject* (*N*= 135)

	Rejectors		*Acceptors*					
Pre-repayment Ratings	*N*	*M*	*N*	*M*	*MD*	*df*	*t-value*	*p* (2-tailed)
State *Gratitude*	48	4.29	87	4.87	0.582	76.3	2.173	.033*
State *Annoyance*	48	2.69	87	2.22	0.469	76.8	1.643	.105
State *Indebtedness*	48	3.81	87	4.71	0.900	79.9	2.413	.018*
Obligation to repay	48	4.25	87	5.38	1.129	66.8	3.023	.004*
Perceived *Value* in Help	48	4.19	87	5.08	0.893	75.2	3.330	.001**
Perceived *Helpfulness*	48	3.88	87	4.44	1.062	133	3.690	<.001***
Perceived *Cost* to Helpers	48	3.35	87	3.99	0.634	133	2.019	.045*
Benefit Appraisal	48	3.64	87	4.50	0.863	133	3.948	<.001***
Tendency to *Reciprocate*	48	4.88	87	5.44	0.562	79.3	1.901	.061
Perceived *Fairness*	48	4.75	87	4.99	0.239	133	0.914	.362
Reasonableness	48	4.75	87	5.24	0.491	133	1.998	.048*
Understandability	48	5.27	87	5.03	0.236	133	0.855	.394

Note. *p <.05 (two-tailed); ** *p* <.01 (two-tailed); *** *p* <.001 (two-tailed)

Meanwhile, results of a 2 (Acceptance: Accept vs. Reject) * 4 (Conditionality: Interest-Free vs. 12.5%- vs. 25%- vs. 50%-Interest) Chi-square test revealed no interaction (χ^2 (3) = 3.30, *p* =. 23) between the two variables. These results suggest that not only did the increasing unfairness (i.e. conditionality) of the offers not trigger more emotional negativity (less positivity), there was no effect on offer acceptances (or rejections) either.

4.3.2.2. Levels of Conditionality on Acceptors' Repayment

I then tested whether a 'harsher' offer would prompt more eventual sanctioning in cheap- or free-riding among the 87 acceptors. The present analyses concerned both the *categorical* (i.e. *cheap*-ride vs. *free*-ride vs. *cooperate*) and the *numerical* facets of the acceptors' repayment decisions. The latter was given by the metrics under-repayment percentage (i.e. UR %).

1) Unfairness and Sanctioning (Part 1: *Categorical*)

I had to scrap the originally planned 3 (*Repayment*: Cheap- vs. Free-riding vs. Cooperating) * 4 (*Conditionality*) Chi-square test due to the present

low counts of both cheap- (N = 17) and free-riders (N = 11), as there will be extremely small tally in a few cells (e.g. 1 or 2) which renders the whole analysis meaningless. Thus in this *specific* analysis I aggregated the counts of both cheap- and free-riders to create the category *defaulters*. The 2 (*Defaulters* (N = 28) vs. *Cooperators* (N = 59)) * 4 (*Conditionality*) Chi-square test again indicated no interaction between the two (χ^2 (3) = 1.58, p =. 68), signalling a null effect of varying offer unfairness on acceptors' decisions to default or not.

2) Unfairness and Sanctioning (Part 2: *Numerical*)

I administered a One-way ANOVA on the effect of offer *conditionality* on the acceptors' under-repayment rates (i.e. UR %). The results, which were summarised in Table 4.5 below, indicated no evidence that more unfairly treated acceptors would under-repay more when opportunities beckoned.

Table 4.5. Effect of Offer Conditionality over UR%

One-way ANOVA of effect by Offer *Conditionality* on Acceptors' (N= 87) UR%						
		N	*Mean*	*SE*	df	*F*-value (*p*, two-tailed)
Offer *Conditionality*	Interest-Free	21	16.19	7.29		
	12.5%-Interest	24	16.93	6.96		
	25%-Interest	23	20.83	7.98		
	50%-Interest	19	32.42	10.06		
	Overall	87	21.17	3.99	3	0.802 (.496)

Taken together, the present analyses hinted that a more (or less) deviation from the injunctive norm of fairness —which stipulated in this context that a typical fair 'winner' *should* help the 'loser' *unconditionally* especially when the former simply rode his/her luck to that 'victory' (Ma et al., 2014)—did not matter. The analyses revealed neither signs of 1) more emotional negativity, nor 2) any different pattern of *behavioural* responses in offer *acceptances* and *sanctioning* decisions in *cheap*- or *free-riding*, from recipients of more *unfair* offers. These results seemed to give a resounding no to the present exploratory aim 'does more unfairness matter'. Meanwhile, the results which showed that *acceptors* reported more 1) emotional positivity (e.g.

state gratitude, *reasonableness* and so on), and a 2) more positive *benefit appraisal* than rejectors, is certainly an intriguing and somewhat a surprise finding that warrants a more in-depth discussion later (See Section 4.4.2).

4.3.3. Exploratory Aim 2: The Psychology of Cheap-riders

Another key exploratory aim of Experiment 2 entails the scrutiny of *psychological* correlates of *cheap*-riding. Thus, the upcoming three sub-sections (i.e. 4.3.3.1 to 4.3.3.3) would examine the following topics:

1) *State* Gratitude, Indebtedness and Sanctioning (Sub-section 4.3.3.1);

2) Differentiating *Cooperation*, *Cheap*- and *Free*-riding (Sub-section 4.3.3.2);

3) *Dispositional* Gratitude and *Cheap*-riding (Sub-section 4.3.3.3).

4.3.3.1 *State* Gratitude, Indebtedness, and sanctioning

Throughout this chapter I reiterated why I predicted *state* gratitude to undermine under-repayment (i.e. *cheap*- and *free*-riding inclusive) (the *Moral Motive* and *Moral Barometer* Hypotheses of Gratitude, McCullough et al., 2001). I discussed also how *state* indebtedness will *indirectly* prompt more under-repayment via a more cynical benefit appraisal (Cognitive Restructuring Hypothesis (CRH), Greenberg and Westcott, 1983)) (Refer to Figure 4.2).Hence, this sub-section would feature the following analyses:

i) A Kruskal-Wallis test on the 87 acceptors' pre-repayment ratings (e.g. *state* gratitude and indebtedness), on the basis of their *nominal* repayment decisions (i.e. to *cheap*-ride, *free*-ride or cooperate). Table 4.6 details the statistics;

ii) A Spearman's correlational analysis between the acceptors' pre-repayment ratings and their under-repayment rates (UR %). Table 4.7 details the results;

iii) A path analysis via PROCESS (Hayes, 2013) on the *indirect* effect of state indebtedness over UR%. Table 4.8 and Figure 4.7 present the findings.

Table 4.6 Pre-repayment ratings by Acceptors' repayment decisions ($N = 87$)

	Categorical Repayment Decisions (N= 87)				
	Cooperate[a]	Cheap-ride[b]	Free-ride[c]		
	Median	*Median*	*Median*	*df*	*H(p-value)*
State *Gratitude*	5	5	5	2	0.12 (.942)
State *Annoyance*	2	2	3	2	3.27 (.195)
State *Indebtedness*	5	4	5	2	3.93 (.140)
Obligation to repay	6	5	5	2	3.34 (.188)
Benefit Appraisal[d]	4.67	4.67	4	2	1.02 (.601)
Tendency to *Reciprocate*	6	6	4	2	11.1 (.004**)
Reasonableness	5	5	5	2	0.32 (.322)
Understandability	5	5	5	2	0.20 (.201)
Perceived *Fairness*	5	5	5	2	0.94 (.935)

Note. [a]$N = 59$. [b]$N = 17$, [c]$N = 11$. **$p < .01$ (two-sided).

As detailed in Table 4.6, acceptors' categorical repayment decisions had not differentiated (ps > .10) almost all the pre-repayment survey ratings. The only exception was the item 'tendency to reciprocate' ($p = .004$), where free-riders understandably reported feeling less looking forward—than both *cheap*-riders ($p = .014$) and *cooperators* ($p = .003$)—to helping their helpers back. In short, the present acceptors' differential repayment choice could *not* be attributed to them experiencing varying *gratitude* towards the helpers.

Table 4.7 Spearman's Correlation between pre-repayment ratings and UR%

		Gs	As	Is	Ob	BA	Rec	Res	Und	Fair
Overall	*r*	-.011	.147	-.103	-.187	-.10	-.247	-.155	-.114	-.019
UR%	*p*	.920	.175	.343	.083	.357	.021*	.151	.295	.859
(N= 87)	*N*	87	87	87	87	87	87	87	87	87
Cheap-	*r*	.149	.229	.180	.129	.110	-.010	.045	.240	.200
riders'	*p*	.569	.377	.489	.622	.673	.969	.863	.353	.441
UR%	*N*	17	17	17	17	17	17	17	17	17

Note. Gs = State Gratitude; As = State Annoyance; Is = State Indebtedness; Ob = Perceived Obligation to repay; Fair = Perceived Fairness in; BA= Benefit Appraisal; Rec = Participants' Eagerness to Reciprocate; Res: Reasonableness; Und: Understandability ; Fair: Perceived Fairness; r = Spearman's rho correlation coefficient; *p*: p-value (two-tailed); * $p < .05$ (two-tailed)

The results of the Spearman's correlational analyses (See Table 4.7) further highlighted the absence of associations between the acceptors' benefit-triggered emotions and their subsequent repayment (or the lack thereof). Once again, the data failed to confirm the prediction that *state* gratitude would diminish or even rule out sanctioning (*cheap*- and *free*-riding inclusive).

Nevertheless, the current small portions of *cheap-riders* (N = 17) may render the analysis underpowered thus not allowing many conclusions to be drawn.

Table 4.8 Regression Coefficients, Standard Errors, and Model Summary Information for the Path Model (i.e. Figure 4.7)

		Consequent						
		M (Benefit Appraisal)				Y (Under-repayment Rate, UR %)		
Antecedent		Coef	*SE*	*p*		Coef	*SE*	*p*
X (State *Indebtedness*)	*a*	0.48	0.21	.023	*c*	-0.738	2.37	.756
M (Benefit Appraisal)		—	—	—	*b*	-0.619	1.21	.609
Constant	iM_1	11.24	1.04	<.001	iM_2	33.00	17.80	.067
		R^2 = .0598				R^2 = .0055		
		F (1, 85) = 5.404, p =.0225				F (2, 84) = 0.2332, p =.7925		

As illustrated in Table 4.8 and Figure 4.7 below, results of the PROCESS (Hayes, 2013) failed to confirm the present hypothesised path model (i.e. Figure 4.7). The 95% bias-corrected bootstrap confidence intervals for the present indirect effect (i.e. *ab*: 0.48*-0.74 = -0.30)—based upon 5000 samples—included zero (i.e. -1.72 to 0.69), indicating non-significance (Field, 2013). Contrary to what Greenberg and Westcott (1983) theorised, *state* indebtedness had indeed elicited a more *favourable* (a = 0.48, p = .023) overall appraisal (cost of helping, perceived helper's sincerity, and perceived value in the benefit) of the offers. This positive benefit appraisal (Wood et al., 2008), however, failed to impact upon the under-repayment rate (b = -0.62, p = .61).

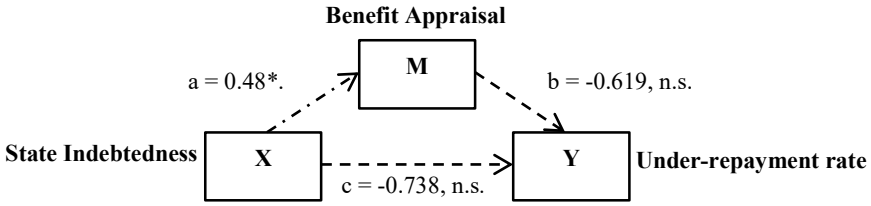

Figure 4.7 A statistical diagram of the path model of the effect of state indebtedness over under-repayment percentage
Note. N = 87; * p <.05 (two-tailed); n,s.= Non-significant at p <.05 (two-tailed)

Additionally, as detailed above *indebtedness* was revealed to have neither a) differentiated acceptors based on their *categorical* repayment (i.e. *cheap*- vs. *free*-ride vs. cooperate, See Table 4.6), nor b) correlated to their *magnitude* of under-repayment (See Table 4.7). Taken together, *state*

indebtedness was not demonstrated to have—*indirectly* or *directly*— impacted on people's sanctioning decisions in Experiment 2 as hypothesised.

4.3.3.2 Differentiating *Cooperation*, *Cheap*- and *Free*-riding

The above suggested that the various emotions upon offer receipt could not account for the acceptors' differential repayment decisions. Hence, the present sub-section would address the principal axis analysis conducted on the post-repayment items (Refer to Table 4.1 for the items). Particularly, through this exploratory factor analysis I aimed to a) garner insight on why acceptors would (or would not) repay in this context and crucially, b) tease apart the psychology of *cheap*-riders from that of *free*-riders and of cooperators.

1) Overview and Results of the present Principal Axis Analysis (PAA)

The Kaiser-Meyer-Olkin (i.e. KMO hereafter) measure verified the 'middling' (Hutcheson & Sofroniou, 1999) *sampling adequacy* (i.e. KMO = .784) of the present PAA. The diagonals of the anti-image correlation matrices were all above .5 (See Table 4.8 below)—which justified the inclusion of each item in the current analysis (Field, 2013). The Bartlett's test of sphericity was significant, χ^2 (55) = 373.43, $p < .001$. Upon inspection of the Scree Plot, three factors with Eigenvalues over 1.0 were extracted. These factors in aggregate explained 54.23 % of the total variance. The items that clustered on the same factors highlight that Factor 1 represented the '*Experienced Warm-Glow*' (Ferguson & Flynn, 2016) (Eigenvalue = 1.73; Factor Loadings = .43 to .88; Cronbach's α = .822), Factor 2 as '*Justice Pursuit*' (Eigenvalue 1.64; Factor Loadings = .40 to .81; Cronbach's α = .770), and Factor 3 as '*Retaliation/ Punitive Responses*' (Eigenvalue = 1.64; Factor Loadings = .55 to .87; Cronbach's α = .711). Reliability analyses showed that

all three factors had 'meritorious' reliabilities (i.e. all αs >.70) (George & Mallery, 2003). Factor loadings below .40 were all suppressed. I also applied the 'separation of .15 guideline' in the event of *double-loading* (Bundick, 2011; Tabachnick & Fidell, 2007; Worthington & Whittaker, 2006). For instance, I suppressed the item DD's (See Table 4.9 below) factor loadings (.428) on the factor 'Experienced Warm-glow' as this item was evidently more strongly loaded (i.e. difference exceeds .15) on the factor 'Justice Pursuit' (.809). Lastly, items of the *same factor* were averaged to form the composite factor scores (El-Alayli & Messé, 2004). Table 4.9 details the key information of the PAA.

Table 4.9 Factor loadings, Eigenvalues, Percentage of variance and Reliability Analyses based on a PAA for the 11 post-repayment survey items ($N = 87$)

	Post-Rotation Factor Loadings			
Post-repayment Survey Items	Experienced Warm-Glow	Justice Pursuit	Retaliation/ Punishment	AIC Diagonals
It made me feel emotionally positive about myself to have repaid (avoided repayment). (EP)	**.877**	.125	.034	.798
The fact that I repaid (did not repay) made me feel good about myself. (FG)	**.810**	.169	-.083	.812
I would feel guilty if I didn't stick with my repayment decision (GA)	**.588**	.232	-.222	.882
My repayment (non-repayment) left me with a feeling of 'warm glow' inside. (WG)	**.547**	.227	.210	.810
I see my Repayment/Non-repayment as a means to punish my partner for his/her unreasonable request. (RP)	-.111	-.073	**.871**	.612
I feel irritated by my partner's request for a repayment. (RI)	-.089	-.133	**.621**	.697
I see my Repayment/Non-repayment as a retaliatory act against my partner's offer. (PR)	.097	.035	**.551**	.665
I repaid/ did not repay because I endeavoured to attain a relatively more equal payoff distribution between me and my partner. (ED)	.000	**.402**	.039	.683
By repaying or not repaying, I manage to offer my partner what he/she truly deserves (DD)	.428	**.809**	-.098	.777
I see my repayment/non-repayment best signifies how grateful I feel towards my partner. (RG)	.399	**.687**	-.170	.811
The way I responded to my partner's offer is the most appropriate way to restore fairness between myself and my partner (RF)	.394	**.514**	-.230	.849
Post-Rotation *Eigenvalues*	2.598	1.725	1.643	—
Post-Rotation *Percentage of Explained Variance*	23.62	15.68	14.94	—
Components' Cronbach's Alpha (α)	.802	.770	.711	—

Note AIC: Anti-Image Correlation. Factor loadings that appear in **bold** were not suppressed. See Sub-section 4.3.3.2 above for a more in-depth discussion about the suppression criteria.

2) Dissociating *Cooperation*, *Cheap*-riding, and *Free*-riding

First, correlational analyses revealed a *strong* association (r (85) = .69, p <.001) between indices 'Experienced Warm-Glow (i.e. EWG hereafter)' (M = 4.97, SD = 1.29) and 'Justice Pursuit (i.e. JP hereafter)' (M = 5.06, SD = 1.28). Meanwhile, index 'Retaliation/ Punitive Responses (i.e. RPR hereafter) (M = 2.72, SD = 1.28) did not correlate with either EWG or FP (both ps >. 05).

One-way ANOVAs were conducted to scrutinise whether *cheap*-riders (N= 17), *free*-riders (N= 11) and *cooperators* (N = 59) would score differently on any of these factor scores (See Table 4.10). The Tukey's HSD post-hoc analyses were administered to break down any significant main effects.

Table 4.10. ANOVA Statistics for Factor Scores by Repayment Decisions

Participants' (N=87) Component Ratings by Repayment Decisions							
	Free-Riding (FR)		Cheap-riding (CR)		Cooperating (CP)		
Component Indices	*N*	*M (SD)*	*N*	*M (SD)*	*N*	*M (SD)*	*F-*
Experienced Warm-glow (EWG) (FG + EP + GA+ WG)/ 4	11	3.80 (1.00)[a]	17	4.68 (1.14)	59	5.26 (1.25)[a]	7.43**
Justice Pursuit (JP) (ED + DD + RG + RF)/4	11	3.64 (1.05)[bc]	17	5.06 (0.92)[c]	59	5.33 (1.25)[b]	9.73***
Retaliatory/ Punitive Response (RPR) (RP + RI+ PR)/3	11	3.91 (1.08)[d]	17	3.55 (1.70)[e]	59	2.27 (1.19)[de]	11.83***

Note. [a] FR < CP (p <.01; Tukey-HSD adjusted) ; [b] FR < CP (p <.001; Tukey-HSD adjusted); [c] FR < CR (p <.01; Tukey-HSD adjusted); [d] FR> CP (p <.001; Tukey-HSD adjusted); [d] FR> CP (p <.01; Tukey-HSD adjusted). ** p <.001 (2-tailed); *** p <.0001 (2-tailed)

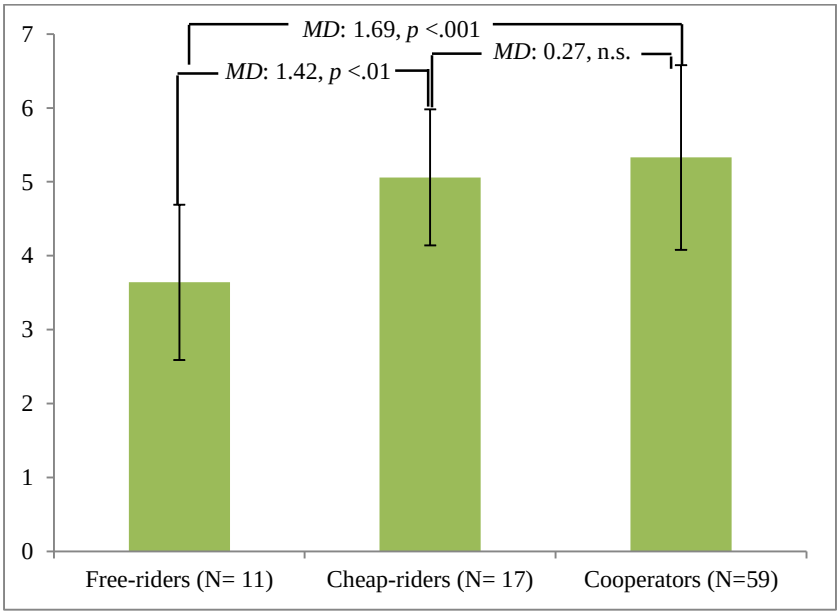

Figure 4.8. Factor Score *Justice Pursuit* by Repayment Decisions (N = 87)

As can be seen in Table 4.10 there are significant main effects (all *p*s <.01) by repayment decisions on all the three factor scores. Specifically, the Tukey-HSD analyses revealed that *cheap-riders* (*M*: 5.06; *SD*: 0.92) were no different (*p* >.05) from *cooperators* (*M*: 5.33; *SD*: 1.25) in using *Justice Pursuit* (e.g. 'giving partner what I think he/she truly deserves') to rationalise their repayment. By contrast, *free-riders* (*M*: 3.64; *SD*: 1.05) were less inclined—than both *cheap*-riders and *cooperators* (both *ps* <.01) —to rationalise their zero back-transfer as a pursuit of justice (See Figure 4.8).

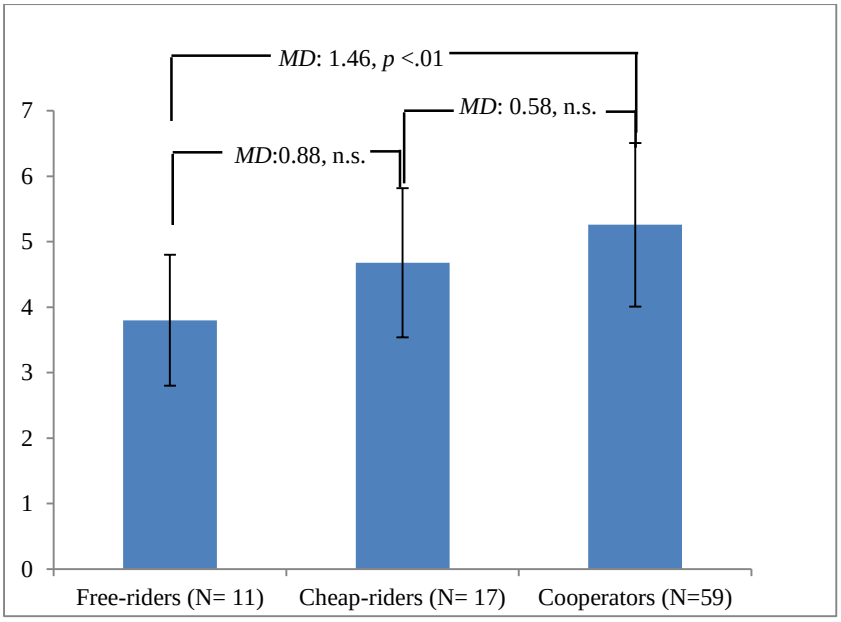

Figure 4.9 Factor *Experienced Warm-Glow* by Repayment Decisions (*N* = 87)

Meanwhile, as highlighted in Figure 4.9, *cheap-riders* (*M*: 4.68; *SD*: 1.14) and *cooperators* (*M*: 5.26; *SD*: 1.25) reported having derived similar levels (*p* >.10) of *Emotional Warm-Glow* from their respective repayment decisions. *Free-riders* (*M*: 3.80; *SD*: 1.00), on the other hand, were understandably more *reluctant* than cooperators (*p* <.01) to cite experienced warm-glow as why they decided to take full advantage of the situation.

Table 4.11. Spearman's Correlation of Factor Scores by Repayment

	Cheap-riders (N= 17)	Cooperators (N= 59)	Free-riders (N= 11)
Spearman's Rho			

Experienced Warm-Glow * Retaliation	0.432	-0.118	0.510
Experienced Warm-Glow * Justice Pursuit	0.630**	0.663***	0.393
Retaliation * Justice Pursuit	0.065	-0.165	0.704*

Note. * p <.05 (two-tailed); ** p<.01 (two-tailed); *** p<.001 (two-tailed).

The above implied that *cheap-riders* tended to both *think* (i.e. to bring justice) and *feel* (i.e. warm-glow) similar to people who earnestly fulfilled their obligation (despite the unfair treatment). This was further evidenced (See Table 4.11) by the significant association (both ps <.01) between the factors 'Experienced Warm Glow' and 'Justice Pursuit' only among *cheap-riders* (r_s = .63) and *cooperators* (r_s = .66) but not the *free-riders* (r_s = .393, p = .232). However, results of the *cocor* analysis (Diedenhofen & Musch, 2015) indicated no significant differences among these three correlation indices.

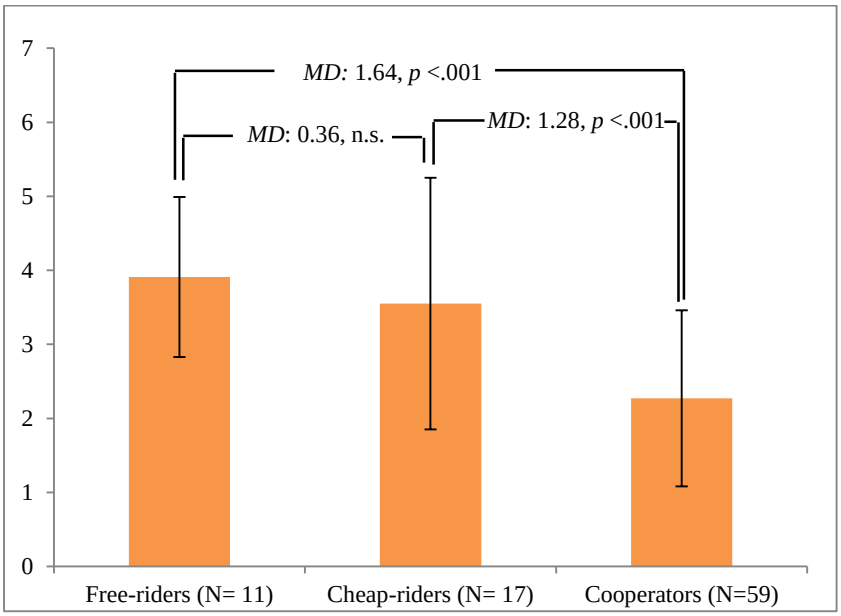

Figure 4.10. Factor *Retaliation/ Punishment* by Repayment Decisions (N = 87)

It is, meanwhile, untrue to claim that *cheap*- and *free*-riders have nothing in common. First, Figure 4.9 highlighted that they reported having acquired similar *emotional warm-glow* (p >.10) from their *refusal to cooperate*. Furthermore, as depicted in Figure 4.10 both *cheap*- (*M:* 3.55; *SD:* 1.70) and *free*-riders (*M:* 3.91, *SD:* 1.08) were evidently more likely (both ps <.001),

compared to cooperators (*M:* 2.27; *SD:* 1.19), to attribute a punitive intent to their repayment decisions. Taken together, both the present *cheap*- and *free*-riders were similarly ($p >$. 10) motivated to exercise their 'rights' to sanction with a *penalising* intent—which was absent among those *cooperators*.

In summary, the above analyses evidenced an emotional ambivalence among the present *cheap-riders*. They felt motivated, like the *free*-riders, to express their irritation with their helpers not acting in accord with the injunctive fairness norm (i.e. *moral outrage*, Batson et al., 2007; Batson, Chao, & Givens, 2009). Nonetheless, just like the cooperators they experienced an internal emotional satisfaction (Holmes, 2009; Ribar & Wilhelm, 2002) from their de facto 'voluntary' back-transfer. Additionally, these *cheap-riders* were pointing to the quest for distributive justice (Tyler, 1994) or provision of 'just deserts' (Carlsmith et al., 2002; Darley, 2001)—like the cooperators—as for why they ended up paying back *partially*. All in all, the present *cheap-riders* appeared to want to cultivate a 'firm but fair' persona (Komorita, Hilty, & Parks, 1991; Preston, 2013) by how they reacted against normative injustice.

4.3.3.3. *Dispositional* Gratitude and *Cheap*-riding

Table 4.12 Regression Coefficients, Standard Errors, and Model Summary Information for the Serial Multiple Mediator Model (Figure 4.11).

		Consequent										
		M_1 (Benefit Appraisal, BA)				M_2 (*State* Gratitude, Gs)				Y (Under-repayment Rate)		
Antecedent		*Coef*	*SE*	*p*		*Coef*	*SE*	*p*		*Coef*	*SE*	*p*
X (*Trait* Gratitude)	a_1	-0.89	0.42	.832	a_2	0.15	0.13	.262	c'	-3.77	4.54	.409
M_1 (BA)		—	—	—	d_{21}	0.17	0.03	<.001	b_1	-0.94	1.33	.485
M_2 (Gs)		—	—	—		—	—	—	b_2	1.24	3.84	.749
Constant	iM_1	13.99	2.28	<.001	iM_2	1.85	0.84	.030	i_y	48.2	30.5	.118
		R^2= .0005				R^2= .2356				R^2= .2356		
		F (1,85) = 0.045, p =.832				F (2,84) = 12.95, p <.001				F (3,83) = 0.368, p =.776		

As outlined above the present investigation into the *cheap-riders'* psychology also entail the link between *dispositional* gratitude and *cheap-riding*. Particularly, I tested the Social-Cognitive Model of Gratitude (i.e.

SCMG; Wood et al., 2008)—which argued that *dispositional* gratitude will restrain under-repayment (i.e. *cheap*- and *free*-riding inclusive) via its serially mediated effect on *state* gratitude through *benefit appraisal* (See Figure 4.11). Thus, the following features the path analysis with PROCESS (Hayes, 2013) on the *serial* mediational model of *dispositional* gratitude over under-repayment rate (i.e. UR %). Table 4.12 and Figure 4.11 detail the statistics.

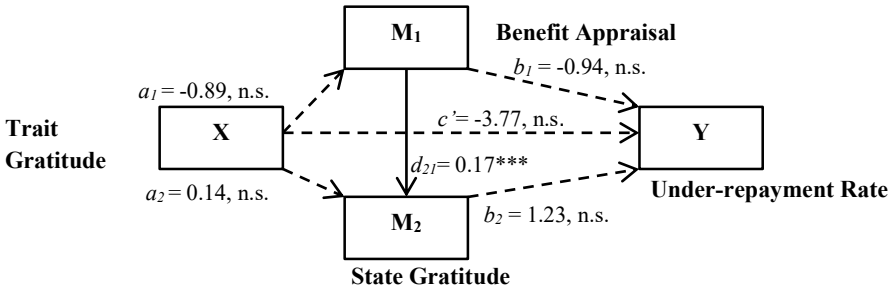

Figure 4.11 A statistical diagram of the serial mediational model (i.e. the SCMG prediction) of the effect of *trait* gratitude over under-repayment rate
Note. $N = 87$; * p <.05 (two-tailed); n,s.= Non-significant at p <.05 (two-tailed).

95% Bootstrap Confidence Intervals was adopted in the present analysis. The results, as detailed in Table 4.12 and Figure 4.11, indicated that almost *none* of the present hypothesised paths were significant at p <.05. The bias-corrected, 5000-sampled Bootstrap Confidence Intervals of the aggregate *indirect* effect (i.e. $a_1d_{21}b_2$ = -0.89*0.17*1.24 = -0.018) included zero (i.e. -1.08 to 0.60), suggesting *non-significance*. In a nutshell, the present data fails to confirm the *indirect* role of *dispositional* gratitude in restraining sanctioning as theorised in the Social Cognitive Model of Gratitude (Wood et al., 2008).

4.3.4. General Confounds

This section addresses the effects (or the lack thereof) by two general confounds, namely *Gender* (i.e. sub-section 4.3.4.1) and *Social Desirability Responding* (i.e. sub-section 4.3.4.2), on the present key study variables (i.e. the 1) in-game survey *ratings* and 2) actual *behavioural decisions*).

4.3.4.1 Gender Differences

1) *Gender* Effect on the Pre-and Post-repayment Ratings

Results of the Independent-samples T-tests revealed no gender effect in both the *pre*- (i.e. Table 4.13, all *p*s >.10, two-tailed) and the *post*-repayment (i.e. Table 4.14, all *p*s > .05, two-tailed) ratings. In short, gender had no role in differentiating either the recipients' (*N* = 135) offer-invoked emotions, or the acceptors' (*N* = 87) self-reported motives behind their repayment decisions.

Table 4.13. Gender and Pre-repayment Ratings (i.e. offer-invoked emotions)

	Male		*Female*					
Pre-repayment Ratings	*N*	*M*	*N*	*M*	*MD*	*df*	*t*	*p (2-tailed)*
State *Gratitude*	54	4.61	81	4.70	0.09	133	0.377	.707
State *Annoyance*	54	2.57	81	2.26	0.32	133	1.216	.226
State *Indebtedness*	54	4.35	81	4.42	0.07	133	0.194	.846
Obligation to repay	54	4.89	81	5.04	0.15	133	0.445	.657
Perceived *Value* in the Help	54	4.83	81	4.72	0.12	92.5	0.442	.659
Perceived *Helpfulness*	54	4.13	81	4.01	0.12	133	0.398	.692
Perceived *Cost to Helpers*	54	3.63	81	3.85	0.22	133	0.715	.476
Tendency to *Reciprocate*	54	5.31	81	5.19	0.13	133	0.474	.636
Perceived *Fairness*	54	5.04	81	4.81	0.22	96.6	0.834	.406
Reasonableness	54	4.98	81	5.12	0.14	133	0.583	.561
Understandability	54	4.91	81	5.26	0.35	89.2	1.227	.223

Table 4.14. Gender and Post-repayment Ratings (i.e. Repayment Motives)

	Male		*Female*					
Post-repayment Ratings	*N*	*M*	*N*	*M*	*MD*	*df*	*t*	*p*
'Feel Good about myself'	36	4.86	51	5.24	0.374	85	1.07	.288
'Feel Emotionally Positive'	36	4.92	51	5.39	0.475	85	1.45	.151
'Feel irritated by repayment request'	36	2.94	51	2.76	0.180	85	0.48	.636
'Feeling of "warm-glow" inside'	36	4.50	51	4.14	0.363	85	0.96	.339
'Punish partners' unreasonable requests'	36	2.58	51	1.98	0.603	63.7	1.55	.126
'Will feel guilty if I did not'	36	4.97	51	5.76	0.792	57.2	1.91	.061
'To restore fairness'	36	5.17	51	5.73	0.559	85	1.56	.122
'To retaliate against partners' offers'	36	2.83	51	3.29	0.461	85	1.08	.285
'To show gratitude toward partners'	36	4.83	51	4.94	0.108	85	0.29	.770
'To give what my partner truly deserves'	36	4.78	51	4.86	0.085	85	0.22	.826
'To attain equal payoff distributions'	36	5.08	51	5.00	0.083	85	0.24	.809

2) Gender Effect on the Behavioural Decisions

Table 4.15. Gender and the *categorical* in-game behavioural decisions

	Male	*Female*			
1. Offer Acceptance (*N* = 135)	*N*	*N*	*df*	χ^2	*p (two-sided)*
Offer *Accepted*	36	51			
Offer *Declined*	18	30			
Overall	54	81	1	0.194	.660
2. Repayment Decisions (*N* = 87)					
2a. Repayment Analysis (i)					
Cooperate (Repaid *Fully*)	27	32			
Default (Repaid *Nothing* or *Partially*)	9	19			
Overall	36	51	1	1.452	.228
2b. Repayment Analysis (ii)					
Cooperate (Repaid *Fully*)	27	32			
Cheap-ride (Repaid *Partially*)	2	15			
Free-ride (Repaid *Nothing*)	7	4			
Overall	36	51	2	8.860	.012

Table 4.15 details the analyses of *gender* effect on both the offer acceptance/rejection (N = 135) and repayment (N = 87) decisions. The data showed no evidence (p = .66) that a particular gender was more inclined to take what was offered to them. While the results highlighted a significant interaction (χ^2 (2) = 8.86, p =.012) between repayment (i.e. *cheap*- vs. *free*-ride vs. *cooperate*) and gender, this interaction vanished (p =.23) once the effect was re-analysed when *cheap*- and *free*-riders were re-grouped as 'defaulters'. Such a re-categorisation was needed because of a few small (i.e. below 5) cell counts (e.g. N= 2 for male cheap-riders, N= 4 for female free-riders) in the initial 2 (Gender)*3 (*cheap*- vs. *free-ride* vs. *cooperate*) Chi-square test.

Additionally, results of an independent-samples t-test indicated no evidence (p = .902) that male (M: 21.8; SD: 40.2, N = 36) and female (M: 20.7; SD: 35.4, N = 51) acceptors had under-repaid differently. Taken together, the data fails to acknowledge any contamination by gender on either the current *psychometric* (i.e. the ratings) or *behavioural* (e.g. repayment) measures.

4.3.4.2. Effect of Social Desirability Responding (i.e. SDR hereafter)

1) Effect of *SDR* on the Pre-and Post-repayment Ratings

Table 4.16. Correlation Table of SDR and Pre-repayment Ratings

		Gs	*As*	*Is*	*Ob*	*PV*	*PGH*	*PCo*	*Rec*	*Res*	*Und*	*Fair*
SDR	*r*	-.06	-.06	.03	-.02	.00	-.08	.07	.04	.01	-.01	-.09
(measured	*p*	.51	.46	.74	.84	.99	.34	.42	.67	.92	.92	.28
via *IMS*)	*N*	135	135	135	135	135	135	135	135	135	135	135

Note. IMS: Paulhus's (1991) Impression Management Scale Composite; *r*: Pearson's Correlation r; *p*: p-value (two-tailed); Gs = State *Gratitude*; As = State *Annoyance*; Is = State *Indebtedness*; Ob = Perceived *Obligation to repay*; Fair = Perceived *Fairness* in the offer; PV: Perceived Value in the offer; PGH: Perceived Helpfulness; PCo: Perceived Cost to Helpers; Rec = Tendency to Reciprocate; Res: Reasonableness; Und: Understandability ; Fair: Perceived Fairness.

Table 4.17. Correlation Table of SDR and Post-repayment Ratings

		FG	*EP*	*IR*	*WG*	*RP*	*GA*	*RF*	*PR*	*RG*	*DD*	*ED*
SDR	*r*	.01	.06	-.07	-.05	-.10	.02	.02	-.06	.15	.11	-.05
(measured	*p*	.93	.61	.50	.62	.35	.87	.84	.58	.16	.30	.64
via *IMS*)	*N*	87	87	87	87	87	87	87	87	87	87	87

Note. *r*: Pearson's Correlation r; *p*: p-value (two-tailed); FG: '*Feel Good* about myself'; EP: '*Feel Emotionally Positive*'; WG = 'Feeling of "*warm-glow*" inside'; GA = 'Will feel *guilty* if I did not.'; RI = '*Feel irritated* by repayment request.'; RP = '*Punish* partners' *unreasonable* requests'; RF = 'To *restore*

fairness'; PR = '*To retaliate* against partners' offers'; RG = 'To *show gratitude* toward partners'; DD = 'To give *what my partner truly deserves*'; ED: 'To attain *equal payoff distributions*'.

Results of the correlational analyses revealed that SDR—which was operationalized by Paulhus's (1991) Impression Management Scale— were not correlated to any of the participants' *pre-* (i.e. Table 4.16, all *p*s >. 10) and *post-*repayment ratings (i.e. Table 4.17, all *p*s >. 10). Hence, how recipients (*N* = 135) reported feeling towards the offers, and the way acceptors (*N* = 87) justified their repayment decisions both appeared not biased by SDR.

Table 4.18. SDR and the categorical in-game behavioural decisions

Social Desirability Responding (Operationalized by IMS, Paulhus, 1991)						
	M	*SD*	*Mean Difference*	*t-value*	*df*	*p (two-tailed)*
1. Offer Acceptance						
Acceptors (*N* = 87)	74.51	18.4				
Rejectors (*N* = 48)	73.13	17.7				
Overall (*N* = 135)	74.01	18.1	1.38	.423	123	.673
2. Repayment Decisions	*M*	*SD*		*F-value*	*df*	*p (two-tailed)*
Cooperators (*N* = 59)	76.76	19.7				
Cheap-riders (*N* = 17)	72.88	14.1				
Free-riders (*N* = 11)	64.91	14.1				
Overall (*N* =87)	74.51	18.4	Not Applicable	2.058	84	.134

Statistical analyses (See Table 4.18) failed to evidence that SDR had segregated, 1) offer *acceptors* from *rejectors* (*p* = .673); 2) acceptors who repaid *fully*, *partially* and *utterly nothing* (*p* = .134). Meanwhile, correlational analyses revealed a negative association between under-repayment rates and SDR (i.e. the IMS composite), r_s (85) = -0.22, *p* = .046 (two-tailed), hinting that acceptors who were more concerned about their social impressions would be more reluctant to sanction 'at full throttle'. Nonetheless, this correlation vanished when I confined the analyses to only *defaulters* (i.e. *cheap-* and *free-*riders inclusive, r_s (26) = -0.26, *p* = .19) or just the *cheap-riders*, r_s (15) = -0.07, *p* = .78. All in all, the data indicated minimal signs that participants' crucial decisions at present were bothered by Social Desirability Responding.

4.4. Discussion

4.4.1. Does more unfairness matter?

Chapter 3 (i.e. Experiment 1) underlined the role of normative fairness in guiding fairness perception, which then contributes to recipients' emotions (*gratitude* in particular) and ultimately a desire to *directly* reciprocate (Ma et al., 2014). Particularly, Experiment 1 showed that recipients could infer reputations (i.e. whether their helpers were *fair* people) —under the guidance of the injunctive norm—based on whether or not the help they received was *conditional*. Crucially, while Chapter 3 indicated an unambiguous divide between perceptions of an *unfair* versus a *fair* offer—which was evidenced by the mediational model (refer to Figure 3.5), it remains unaddressed how people may differentially perceive offers of *varying unfairness*. That is, knowing that people would take issue with an *unfair*—relative to a *fair*— offer, I endeavoured to examine if a more outrageously unfair offer would irk them even further. Or alternatively, there exists a 'fairness-versus-unfairness' threshold and once that was passed, it mattered no more as in how egregious that infringement of fairness became (in comparison to other scenarios).

Indeed, data of Experiment 2 appeared to endorse this 'fairness threshold' perspective. Echoing the observations from Experiment 1, the current manipulation of the degree of unfairness had practically no effect on all the outcome measures (e.g. offer-invoked emotions, offer acceptances and repayment). Taken together, Experiments 1 and 2 may in combination suggested that (injunctive) fairness perception could belong to an 'all-or-nothing' concept: once the border of injunctive fairness had been overstepped people will just pay no mind to the 'severity' of that infringement.

4.4.2. *Gratitude*, *Indebtedness* and Sanctioning in Economic Games

Overall, the results showed little evidence that *gratitude* (as a *state* and a *disposition*) and *indebtedness* were related to the participants' reciprocal or sanctioning decisions (i.e. repayment). This again echoed the results in Experiment 1 whereby recipients' various affective states (e.g. *gratitude*, *indebtedness* etc.)—triggered by the *conditional* offer receipts (N = 21)—were also not predictive of how they actually repaid their helpers (See Chapter 3).

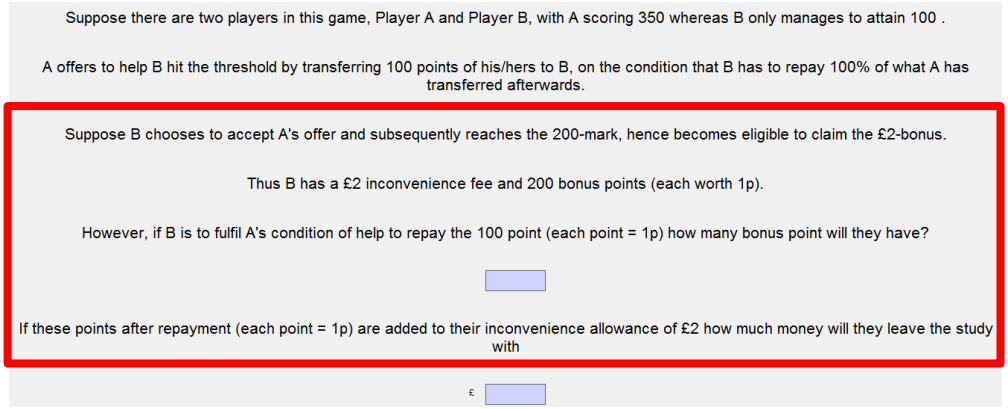

Figure 4.12. Understanding Check Example Screens (Experiment 1)

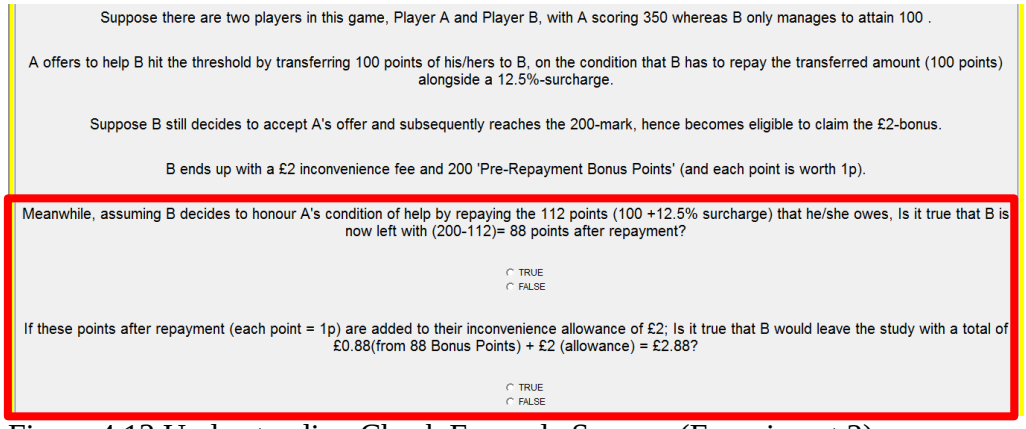

Figure 4.13 Understanding Check Example Screens (Experiment 2)

While it may be convenient to point the finger at the power issue for the absence of expected findings (especially for Experiment 1), another reason could lie in a *shared* experimental feature of Experiments 1 and 2—i.e. the recipients' implicit assumption of the *binding* nature of the repayment conditions before they decided whether to accept. Recipients only came to know of the feasibility of not honouring their repayment after they indicated

acceptances. This, in conjunction with the understanding checks which explicitly highlighted the advantages of acceptances despite the *full repayment* (See Figures 4.12 and 4.13 above), could have ruled out any thoughts to possibly under-repay when recipients decided whether to accept the offers.

As a result, those who were bothered by the injunctive unfairness may have already severed the tie with the helper before even getting to know they could punish. In fact, the evidence of people shunning a financial benefit amidst perceived *injustice* has been well documented in the Impunity Game literature (e.g. Ma et al., 2012; Takagishi et al., 2009; Yamagishi et al., 2012).

Thus it is not unreasonable to speculate that those who remained in the loop (by accepting) were at least not taking so much issue with their (in principle) unfair offers. Indeed, the present findings which highlighted how differently acceptors and rejectors felt about the offers certainly give substances to this speculation (See Sub-section 4.3.2.1). In particular, the data (i.e. Table 4.4) showed that rejectors felt less *grateful*, *indebted*, and gave a *less favourable* appraisal, toward the offers. They also deemed the exchange *less reasonable* and felt *less likely to reciprocate*. All these resonated with a speculation that the current participants, who were not aware of the *non-binding* nature of the condition a priori, may have indeed used their acceptance decision as a proxy sanction (or reward) in response to their helpers' gestures.

Consequently, the repayment may be inflated because those who were discontented with the arrangement, thus more prone to sanction, may have been 'filtered' from the repayment analyses. While logically sound, neither Experiments 1 nor 2 had examined the psychology of the rejectors. Therefore, it

still remains to be seen whether the above does explain why neither *gratitude* nor *indebtedness* was shown to relate to repayment in Experiments 1 and 2.

4.4.3. Disambiguating *Cheap-*, *Free-*riders and *Cooperators*

As a continuation of Experiment 1, one key objective of the present experiment entails finding out why people *cheap-ride* on someone. The results of the Principal Axis Analysis identified three factors of why (or why not) they repaid an injunctively unfair offer even when not doing so yielded no repercussions. Those included, a) Experienced Warm-glow (Ferguson & Flynn, 2016); b) Justice Pursuit; and c) Retaliation/ Punitive Responses.

Importantly, the follow-up analysis of these three factor scores—on the basis of repayment decisions—offered insights upon the unique mentality that underlies cheap-riding. On one hand, cheap-riders would fancy some *paybacks*, just like their free-riding counterparts, out of the irritation at helpers' departure from a moral standard in injunctive fairness (Batson et al., 2007; Batson et al., 2009; Wakslak et al., 2007). On the other, cheap-riding constituted more than just a channel for venting out frustrations (Legkaukas & Jakimavičiūtė, 2007). Cheap-riders were as convinced as the cooperators that their partial repayment would promote *distributive justice* (Tyler, 1994), and meanwhile provided what they believed were the *fair shares* to their helpers (Santos & Rivera, 2015; Weinstein, 2011; Wenzel & Thielmann, 2006). Furthermore, *cheap-riders* also derived a sense of emotional satisfaction (Beccheti & Degli Antoni, 2010; Falk, 2007; Harbaugh, 1998) from their de facto 'voluntary' repayment gesture, as did acceptors who earnestly fulfilled their prior agreement.

All in all, the present *cheap-riders* appeared to capture the gist of the theory of 'Just Deserts' in sanctioning (Carlsmith et al., 2002; Darley et al.,

2000; Starkweather, 1992). Instead of being fixated on vengeance, a 'just' punishment entails giving the offenders a *rightful, deserving* comeuppance (Moore, 1997) that serves to restore the overall moral balance (Wenzel & Thielmann, 2006). This clearly echoes the present *cheap-riders'* pattern of responses toward the post-repayment survey as outlined above.

4.4.4. Cheap-riding in Real-life Settings: Under-contribution of Child Support

Indeed, I argue that one fitting example that epitomises *cheap-riding* in a real-life setting will be the under-contribution of child support in the United Kingdom (See Allbeson, 2016; Savage, 2015). According to the Child Support Act 1991, a '*non-resident* parent' (i.e. the parent who is not involved in the main day-to-day child care) is obliged to financially provide for his/her ex-partner (i.e. the '*receiving* parent') in regards their children's daily living expenditure. This amount of contribution is computed after taking into consideration a basket of factors in relation to the non-resident parent's socio-economic status (e.g. weekly income, whether he/she is on a benefit etc.).

Meanwhile, data from the Child Support Agency (CSA hereafter) suggested that as of September 2014 there is a total of 'uncollectible' child maintenance which amounts to £2,917 million, with over a quarter of the non-resident parents contributing less than half of their obligated amount (See Savage, 2015). A closer look at the data revealed that there were 17.2% of paying parents paying a non-zero but below 80% of the stipulated contribution.

I interpret this 17.2% of under-contribution as a quintessence of *cheap-riding* as presently operationalized (i.e. a *partial* repayment of an affordable, *conditional* offer). As explained above this contribution is supposedly tailored each non-resident parent's earning power, thereby rendering him/her no

grounds to quote affordability as a reason to not pay up. However, the CSA's notoriety in its failure to retrieve the maintenance arrears has been so flagrant that even Mary Quinn, the Head of the CSA in Northern Ireland, once had to publicly apologise for it (See Savage, 2007). Hence, there is no reason to not expect that these non-resident parents should have known of the de facto absence of repercussions even if they *refuse to* contribute *in full*.

In other words, resembling the *conditional* offer acceptors in Experiments 1 and 2, these non-resident parents should know that if they wanted to they could have not contributed anything and practically faced no consequences (although in principle they would still risk imprisonment or forced sales of properties). Nevertheless, the fact that this particular portion of non-resident parents was contributing something clearly captures the essence of cheap-riding, that is, to piggyback on others' high contribution (i.e. former partners' effort and financial expenditure in child-raising) by giving just the bare minimum (See Bchir & Willinger, 2013; Isaac et al., 1989; Marks et al., 2006). Interestingly, the present proportion of cheap-riding (i.e. 17/87= 19.5%) was not too far off from the abovementioned figure from the CSA (i.e. 17.2%) that was calculated based on a total sample of 86,700. Thus I believe this not only emphasises that the present analysis of *cheap-riding* is not without ecological validity (Brewer, 2000; Shadish, Cook, & Campbell, 2002), but also enables the extrapolation of certain crucial findings into the understanding of the child support evasion situation that is discussed in Section 4.5.1 below.

4.5 Implications of Chapter 4

While Section 4.4 summarised and discussed—from a *theoretical* standpoint—several key features of the present analyses, this section would

highlight two important implications. They included, first, how the analyses of *cheap-riders* add to the understanding of child support under-contributions (i.e. Sub-section 4.5.1), and second, a few methodological concerns (of both Experiments 1 and 2) which constitute four unanswered questions (i.e. UAQs 1 to 4) that warrant future investigations (i.e. Sub-sections 4.5.2.1 to 4.5.2.4).

4.5.1. Cheap-riders and Under-Contributing Non-resident Parents

One crucial discovery of the present analyses, as discussed in Sub-section 4.4.3, entails the profiling of the cheap-riders' emotional ambivalence behind their *partial* back-transfers. It is conceivable that such an emotional ambivalence may also underlie why certain non-resident parents would under-contribute their shares of child support. On one hand, just like the present *cheap-riders* these parents may under-repay to show retributions against their exes for the separation and/or the loss of custody (Hetherington & Stanley-Hagan, 1999; Stahly, 2000). But on the other, leaving the entire burden of child-raising to the exes may have crossed the line of what a 'Just Desert' should constitute (Darley et al., 2000; Moore, 1997; Wenzel & Thielmann, 2006) —especially if that ex-partner is far from well-off (See 'A Single Parent's Story' in Savage (2015)). Furthermore, the fact that one contributes, albeit incompletely, could also help lessen that non-custodial parent's guilt over the deprivation of his/her children of a loving family (Arditti, 1992; Braver et al., 1993; Seagull & Seagull, 1977)—and that *guilt aversion* is a defining attribute of 'experienced warm-glow' (Ferguson & Flynn, 2016).

All in all, the above implied that all three present extracted factors—namely, Experienced Warm-Glow, Justice Pursuit and Retaliation/ Punitive Responses—appears to each have a role in egging those non-resident parents on

underpaying their shares. Nonetheless, while the above described how the current analyses may account for why a non-resident parent would contribute insufficiently, it is still arbitrary to make a case that these are the genuine reasons behind. There could still a whole lot of other reasons behind why people would under-contribute their child support. For instance, a non-resident father may have doubt about the paternity of that child he legally 'shares' with his ex-partner (See O'Connor, Kelleher, & Kelleher, 2008), thus not paying the support in full (with the understanding that not doing so will not *practically* bring forth any repercussions) may represent some forms of self-protections. Taken together, qualitative analyses such as *Grounded Theory* or *case study* will be warranted if one endeavours to specifically address the psychology of those non-custodial parents (e.g. Cook, McKenzie, & Natalier, 2015; Klett-Davies, 2016; Skinner, 2013; Wilson, 2006).

4.5.2. Unanswered Questions (i.e. UAQs) that warrant further research

Below (i.e. Subsections 4.5.2.1 to 4.5.2.4) I outline several UAQs (i.e. UAQs 1 to 4) that warrant further empirical scrutiny. These UAQs primarily concern the methodological features of both Experiments 1 and 2.

4.5.2.1 UAQ #1: Does 'more fairness' really not count? Is there really a 'threshold' for fairness perception?

Overall, results of Experiment 2 in combination with Experiment 1 hinted the existence of a threshold in fairness evaluation. Recipients whose helpers had infringed normative fairness seemed not *differentially* offended by the *relative* severity of that violation.

While such an *absolute* construal of a 'fairness-unfairness' divide has been documented in the literature (Bian & Keller, 1999; Eckel & Grossman,

1996b), Wood and colleagues (2011) contended that *relativity* matters as people are sensitive to how their treatment compare against what others were having (i.e. the 'Gratitude Relativity Hypothesis (i.e. GRH hereafter)). Thus, the GRH would challenge this absolute perspective of fairness judgment, as it should predict less discontent with (and more *gratitude* toward) helpers who gave a less unreasonable offer (e.g. an '*Interest-Free*' compared to a '*50%*-interest' offer). In other words, more unfairness should 'count' according to the GRH.

Nevertheless, the lack of transparency as regards helpers' behavioural options (and also what other recipients were getting) in Experiments 1 and 2 prevented the scrutiny of GRH. While the rationale of such information denial has been detailed in Chapter 3, it remains intriguing to test whether this 'fairness-threshold' perspective would prevail in a context in which recipients could infer helpers' intent based on both their *actual* and their '*could-have-chosen*' options (Charness & Levine, 2007). That is, whether the fairness-threshold perspective or the GRH would better portray a recipient's in-game fairness perception should be the focus of a relevant future scrutiny.

4.5.2.2 UAQ #2: Were the lack of opportunities to play both in-game roles to blame for the observed 'over-cooperativeness' in Experiments 1 and 2?

While Experiments 1 and 2 had each recorded a certain extent of cynicism—for instance, the lack of helping (i.e. 19.7% in Experiment 1) and under-repayment (i.e. 38 % and 32% defaulting rate in Experiments 1 and 2 respectively), it is undeniable that a majority of the participants were not behaving as a quintessential profit-maximizer. Discussions on why participants in Experiments 1 and 2 appeared too prosocial would be detailed in Chapter 5.

Indeed, I suspected that a reason behind the relative lack of cynicism in both experiments may be attributed to the fact that both games were *single-role* games. Participants in Experiments 1 and 2 were only assigned to one in-game role. Burks, Carpenter, and Verhoogen (2003) argued that such a single-role design may, relative to a design where players would assume different roles (i.e. both trustors and trustees) in different trials, have *inflated* participants' in-game prosociality. Burks et al. (2003) attributed this *increased* prosociality in a single-role game to participants feeling more *personally responsible* for their co-player's ultimate payoffs (i.e. the '*Reduced Responsibility* Hypothesis'). Thus they might be more uncomfortable acting in accord with their self-interest knowing that their acts were the only determinant of their partners' poor payoffs. Taken together, Experiments 1 and 2 had definitely left the door open for future scrutiny as of whether the *single-role* design was the reason for the observed patterns of over-generous decision making.

4.5.2.3 UAQ #3: Why reject an offer in the present experimental set-up?

One intriguing observation of the present study no doubt entails the almost threefold increase in rejection (35.6%) compared to that of Experiment 1 (*conditional* offer rejection: 3/24: 12.5%). While the above (i.e. Sub-section 4.4.2) alluded to the possibility of participants *rejecting* out of a retaliatory intent against *injustice* (Takagishi et al., 2009; Yamagishi et al., 2009), the psychology of rejectors in this scenario was still yet to be ascertained.

Particularly, *dispositional* indebtedness (Mathews & Green, 2010; Mathews & Shook, 2013) may also constitute a strong correlate of people's decision to shun a financially advantageous arrangement. As detailed in Chapter 1 *dispositionally* indebted people are best characterised by their

extreme endeavours to avoid owing anybody anything (Greenberg & Westcott, 1983), thanks to their religious adherence to the reciprocity norm (Gouldner, 1960). It is, therefore, conceivable the high rejections may be attributable to individual differences along the continuum of *dispositional* indebtedness.

However, neither of the two experiments, as presently constructed, could disentangle two above-mentioned motives of rejections. Therefore, to better examine the role of *dispositional* indebtedness in fostering rejection I suggest amending another shared feature of Experiments 1 and 2: recipients' pre-acceptance unawareness of the *non-binding* nature of the repayment.

By removing this unawareness a true punisher should instead accept the offer and then default, considering that rejection is no longer the most effective punitive strategy. In contrast, a *dispositionally* indebted person should always reject to avoid building up credits with others (Greenberg & Westcott, 1983).

4.5.2.4 UAQ #4: Cheap-riding as a fairness enforcement strategy?

Lastly, I argued that cheap-riding could function in a similar fashion as altruistic punishment (Boyd et al., 2003; Fehr et al., 2002; Fehr & Gächter, 2002; Fowler, 2005). That is, by deliberately under-repaying a *cheap-rider* endeavours to curb any future ungenerous acts from that helper (Ma et al., 2014). Nonetheless, it remains unexamined as of how valid this logic is, and how efficacious *cheap-riding* is as a strategy to be relied upon in implementing normative fairness. Therefore, a future relevant study should constitute a *direct* manipulation of cheap-riding that permits the scrutiny of the effect of being 'cheap-rode' over that 'victim's' propensity to play fair consequently.

Chapter 5

Gratefully Received, Gratefully Retaliate? Analysing the Role of Emotion and Situational variables in Repeated Economic Exchanges

(Experiment 3)

5.0. Overview of Chapter 5

In Experiment 1 I highlighted how normative fairness guides fairness perception, especially in situations where information about others' intent was absent, and how this perception would mould one's *emotional* gratitude and a desire to cooperate. Among those ill-treated (normatively speaking) participants a non-negligible portion of them retaliated in the form of *cheap-riding*.

Experiment 2, meanwhile, offered a more in-depth analysis of *cheap-riding*. The data revealed a pattern of *cheap-riding* that shared resemblance not only to observations in Experiment 1, but also to the reality (i.e. non-resident parents under-contributing their child support). I also disentangled the psychology of cheap-riders from cooperators and that of typical maximizers.

While Experiments 1 and 2 in combination constituted a decent measure of how *state* or *trait* gratitude, aside from factors such as normative fairness or Just Deserts (Carlsmith et al. 2002; Darley, 2001), shapes people's prosociality in economic exchanges, Chapter 4 outlined certain issues that warrant attention. They were, a) recipients' lack of access to 'what my helper *could have* done instead of what he/she did'; b) the 'over-generosity' issue, and c) recipients' unawareness of feasibility to default and their high rates of offer rejections.

As such, the primary objective of Experiment 3 was to address a few unanswered questions (i.e. UAQs hereafter) in Chapters 3 and 4 by incorporating new design features. The below summarises, in bulletin points, the four crucial UAQs and how the five new features could help address them. - *UAQ 1*: Is there really an 'all-or-nothing' threshold for injunctive fairness? - *Design Feature 1*: Recipients in this game were no longer blind to the helping options available to each helper. Such an awareness should enable recipients to

deduce the injunctive fairness by comparing what they *actually* to what they *could have* received. Hence this would allow the scrutiny of whether the observed 'all-or-nothing' pattern of injunctive fairness attribution in Experiments 1 and 2 is moderated by recipients' *knowledge*. That is, now that recipients knew how they could have been treated differently, would a more *unfair* (e.g. a '50%-Interest') offer still prompt a *similar* appraisal (e.g. intent, *gratitude* etc.) than when they were given a fairer (e.g. 'Interest-free') one?

- *UAQ 2*: Were participants in Experiments 1 and 2 too cooperative due to the lack of opportunities to assume *both* in-game roles (i.e. helpers and recipients)?

- *Design Feature 2*: I argued that the *one-shot* nature and specifically, the lack of chances to play both as helpers and recipients may explain the lack of cynicism in either experiment (i.e. over 80% helping in Experiment 1 and over 60% full repayment in both Experiments). Indeed, Burks, Carpenter and Verhoogen (2003) theorised that playing *both* roles may lead players to feel less responsibility for others' payoffs thus remove their guilt over selfish decisions (i.e. the *Reduced Responsibility* Hypothesis). Thus, by having participants play an *iterated, two-role* version of the game I examined whether a different pattern of helping and repayment (than Experiments 1 and 2) would be observed.

- *UAQ 3*: Were rejections in Experiment 2 down to *injustice* or *indebtedness*?

- *Design Feature 3*: It remains unclear if the high rate of rejection (i.e. 35.6%) of *conditional* offers was down to recipients' disapproval of perceived injustice, or a trait indebtedness-induced repulsion against benefit acceptance (Greenberg & Westcott, 1983). I argued that this confusion could originate from Experiment 2's design where players had no awareness of the feasibility of repayment evasion. As such, in Experiment 3 I informed participants of this

feasibility before the acceptance or rejection decisions. While this new feature may not tamper with the indebtedness-motivated rejections, I would expect minimal rejections out of perceived unfairness as there is clearly a more effective strategy to retaliate: to simply take the offers and then not repay.

- *UAQ 4*: Does being 'cheap-rode' prompt more eventual fair plays?

- *Design Feature 4 and 5*: I argued the logic behind cheap-riding should be similar to that of altruistic punishment (Fehr & Gächter, 2002; Fowler, 2005). That is, via their reduced payback cheap-riders endeavoured to prevent future non-generosity of their helpers (Ma et al., 2014). To examine the validity of this argument I included two additional features as summarised below:

First, I manipulated the magnitude of partners' repayment for participants who helped *conditionally* in the first trial (in which they played as a potential helper). A *conditional* offer in Trial 1, depending on a one's assigned condition, may be *partially* repaid (i.e. *Cheap-riding* condition), *fully* repaid (i.e. *Fairness* condition') or *utterly not* repaid (i.e. *Free-riding* condition). Second, participants played as a helper *twice* (i.e. Helper Trials, HTs hereafter) and *eight times* as a recipient (i.e. Recipient Trials, RTs hereafter), while the 8 RTs were sandwiched by the 2HTs (i.e. Trial 1 and Trial 10). During the 8 RTs participants received offers of varying conditionalities (to be discussed later). Overall, features 4 and 5 should permit the analysis of whether being 'cheap-rode' would push people to act more in accord with injunctive fairness. That is, whether the *conditional* helpers (on Trial 1) from the *Cheap-riding* condition would be more inclined to show generosity in the ensuing experimental trials.

5.1. Introduction

5.1.1. The Purpose of Experiment 3

The key objective of Experiment 3 was to address a few unexamined issues in Experiments 1 and 2, and in so doing I intended to complement the robustness of the overall investigation of gratitude-induced prosociality. I aimed also to further the understanding of the *gratitude-(direct) reciprocity* relation—which was a key discovery in Chapter 2—by examining what other variables (situational or dispositional) could have contributed to the strength of this link. The next section (i.e. Section 5.1.2) discusses how the new features of the current experiment could address the UAQs as outlined in Section 5.0.

5.1.2. Unanswered Questions from Experiments 1 and 2

In this section I discuss at length a series of UAQs from Experiments 1 and 2, and importantly, I elaborate on how the new features could help tackle these UAQs. While this section may touch upon a few present predictions and hypotheses, they are to be thoroughly discussed in Section 5.1.3.

5.1.2.1. UAQ 1: Does the *degree* of unfairness really not count?

One intriguing observation of Experiments 1 and 2 was that participants whose helpers breached the injunctive fairness norm (i.e. via giving *conditional* offers with varying repayment expectations) seemed not bothered by the extent of their helpers' non-generosity. Meanwhile, data of Experiment 1 illustrates that people favoured *unconditional* (injunctively fair) over *conditional* (injunctively not fair) offers (e.g. more perceived helpfulness and state gratitude). In sum, data from Experiments 1 and 2 imply that individuals care only about whether injunctive fairness was observed, and crucially when it was *not* a more (or less) severe deviation from fairness would not matter.

The above hinted that there may be a 'threshold' to segregate *fairness* from *unfairness* (in a normative sense). Nonetheless, a shared feature of

Experiments 1 and 2 (i.e. recipients' naivety about the helpers' available options) may render this fairness threshold perspective debatable. In particular, it remains unaddressed as in how differently would recipients respond if they could decipher their helpers' intent based on both the helpers' *selected* and *neglected* options (Charness & Levine, 2007)? Hence, one key new feature of Experiment 3 would be the recipients' awareness of the helpers' list of options.

In the present study participants played an *iterated* game in which they had opportunities—at different trials—to assume the role as a helper or a recipient. Participants played as a potential helper (i.e. P1s hereafter) in Trial 1 and 10 and as a potential recipient (i.e. P2s hereafter) for the eight trials in between. The trials were arranged this way to notify the participants, prior to playing as P2s, of the various behavioural options a P1 will be endowed with.

Via this revised experimental set-up I measured whether the transparency of P1s' options would affect participants' *offer appraisals* and *behavioural decisions* during the Recipient Trials. Specifically, I assessed whether the well-informed recipients in this game would replicate the 'all-or-nothing' pattern of fairness perceptions as indicated in Experiments 1 and 2.

As such, I tested two competing hypotheses. The Gratitude Relativity Hypothesis (i.e. GRH) (Wood, Maltby & Brown, 2011)—which predicts a *graded* offer appraisal (i.e. how an offer is appraised is subject to its *relative* worth to other offers), and the Inelastic Fairness Construal (i.e. IFC) notion—which argues the fairness threshold perspective as mentioned above (i.e. how fair an offer is *only* hinges on whether it is consistent with the injunctive norm). Section 5.1.3 features a detailed discussion in relation to GRH and IFC.

5.1.2.2. UAQ 2: Was the lack of both perspectives as helpers and recipients to blame for the observed over-cooperativeness in both Experiments 1 and 2?

Another issue surrounding Experiments 1 and 2 was that participants seemed to be too cooperative. Below I discuss why I came to this conclusion and how the current revised design may subject that to scrutiny.

a) How were participants in Experiments 1 and 2 'overly cooperative'?

While I addressed the moral rationale of P1s in Experiment 1 to not only help but also help *unconditionally* (Elster, 2006), the fact that one was willing to entrust something to an *anonymous*, *one-time* fellow co-player (even with expectations to be repaid) alone already constitutes a prosocial gesture (Berg et al., 1995; Fehr & Rockenbach, 2003). Gergen and colleagues (Gergen, Diebold, Seipel, & Gresser, 1969; Gergen et al., 1975) even argued that gifting *unconditional* benefits in economic games may engender suspicion as recipients may ponder why the helpers were being *that* benevolent. Considering also that the one-shot, double-blinded design of Experiment 1 should have removed any reputation-incentivized helping (Güney & Newell, 2013), the observed 80% helping rate (and with the majority being *unconditional* helping, i.e. 51%) should evidence an over-generosity issue among helpers in Experiment 1.

Meanwhile, the meta-analysis by Johnson and Mislin (2011) over 160 Trust Games revealed an average trustworthiness index (i.e. TI hereafter) of 37%. This TI was the ratio of a trustee's *actual* back-transfer to the *highest possible* back-transfer. Indeed, the TIs of both Experiments 1 and 2 (i.e. Experiment 1 (N = 21): *M:* 81.4%; *SD:* 33.4; Experiment 2 (N = 87): *M:* 78.8%; *SD:* 37.2), which were above the mean estimates (i.e. 37%) by some distances, were not far off from the maximum TI (i.e. 81.2%) in Johnson and Mislin's

(2011) review. In this regard, it should *not* be unreasonable to argue that participants in Experiments 1 and 2 were overly trustworthy.

b) Single-role vs. Two-role Games: Analysing why people over-cooperate?

I suggested that the one-shot, single-role design of Experiments 1 and 2 may account for why people seemed to be overly prosocial in either study. In particular, Burks et al. (2003) put forth two hypotheses on how playing both roles (as senders and receivers) could contribute to participants' in-game prosociality. The *Reduced Responsibility* Hypothesis (i.e. RRH)—which argues that playing both roles reduces the players' helping and reciprocating intentions, and the *Golden Rules* Hypothesis (i.e. GRH)—which argues the otherwise. See Section 5.1.3.2 for more details about RRH and GRH.

As such, the present revised design should help tackle UAQ 2 by testing the RRH and the GRH. In particular, I examined whether a similar pattern of *over-helping* or *over-repayment* would still be found in a context where players could garner perspectives of both *helpers* and *recipients*.

5.1.2.3. UAQ 3: Was *negative reciprocity* or *trait indebtedness* to blame for rejecting a financially advantageous, albeit injunctively unfair, offer?

I discussed that the lack of prior knowledge on one's 'defaulting' rights (in both Experiments 1 and 2) may account for the high rejection rate (i.e. 36%) of offers in Experiment 2. Evidence in the impunity game (IG) literature has documented the feasibility of players irrationally rejecting a financial benefit out of preservation of *social fairness* (e.g. Yamagishi et al., 2009; Yamagishi et al., 2012). Hence, in Chapter 4 I speculated that participants may have utilised their 'acceptance/rejection' options to retaliate against the helpers' belittlement of injunctive fairness, instead of their repayment options as originally expected.

Nonetheless, I also argued that *trait* indebtedness (Mathews & Green, 2010; Mathews & Shook, 2013) may be an alternative reason why people quitted on a financial benefit. A *dispositionally indebted* person is characterised by an ever-present disgust to be the one to *receive* in an exchange (Greenberg & Westcott, 1983). Thus, such an indebtedness-driven rejection should not relate to endeavours to protest against injustice. However, in Experiment 2 there was no way to rule out either of the motives behind the observed rejections.

Therefore, to unravel this confusion I brought in another modification in Experiment 3. Instead of keeping recipients in the dark they were informed, before they accepted (or rejected) the offers, that the *acceptors* had the freedom to, later on, decide *whether or not* and *how much* to repay. This new feature should enable the disentanglement of the two abovementioned motives behind the rejections. Obviously, knowing that the 'default' option is now open a punisher is expected to accept and later on default. In contrast, *dispositionally indebted* participants should still reject nonetheless as they always strive to refrain from owing anybody (Greenberg & Westcott, 1983).

5.1.2.4. UAQ 4: Does being 'cheap-rode' make the 'victims' act more fairly?

Throughout the thesis I contended that the logic to cheap-ride in an economic game could be analogous to that of *altruistic punishment* (Fehr & Gächter, 2002). That is, by incurring a cost (i.e. making a *partial* repayment despite not having to) the *cheap-riders* strive to prevent their miserly helpers from 'victimising' other people in the future. Nonetheless, it remains to be seen how valid this notion is, and even if cheap-riding is resorted to how effective it is in enforcing fairness. Thus here I examined whether being 'cheap-rode' would prompt more eventual fair (or generous) gestures from the 'victims'.

In Experiment 3 I manipulated partners' repayment among players who made *conditional* offers in Trial 1 where they played as P1s. Thus, by having different *conditional* givers experience different levels of partners' repayment (i.e. cooperating, cheap- or free-riding) I examined specifically whether (and how) being cheap-rode (compared to other treatments) would succeed in inciting more generous decisions in the ensuing trials (i.e. Trial 2 to 10).

5.1.3. Exploratory Aims and Hypotheses

In this section, I present the list of hypotheses and exploratory aims of the current chapter and the respective predictions were summarised in Table 5.1. I intend to discuss at length the procedures of the current experimental design in section 5.2 (i.e. the Method section).

5.1.3.1. Inelastic Fairness Construal vs. Gratitude Relativity Hypothesis

I addressed, in Section 5.1.2.1, how granting participants the perspectives of both helpers and recipients should facilitate the analyses of whether injunctive fairness is an *absolute* or a *relative* judgment (i.e. 'whether more unfairness really does not count (i.e. UAQ 1)'. There were two hypotheses making competing predictions on the matter, and they were, a) the Inelastic Fairness Construal argument and, b) the Gratitude Relativity Hypothesis (Wood et al., 2011). Both hypotheses are discussed below.

i) Inelastic Fairness Construal Argument

Eckel and Grossman (1996b) introduced the concept of fairness elasticity in the context of economic games. In particular, they contended that an *inelastic* fairness demand, as characterised by the consistent sanctioning against unfair treatment despite the *increasing* cost of doing so, should originate

from the punisher's stubborn allegiance to a set of *moral* principles (Gilligan, 1982) like the injunctive norms (Biel, Von Borgstede, & Dahlstrand, 1999).

The above fairness perspective hinted that the crucial criterion for fairness perception lies in the *injunctive norm*. That is, whether a treatment was fair should hinge on how that specific treatment compares to *normative fairness*. Thus, relativity should be irrelevant as people would focus on how a specific treatment fares against the 'standard' fair treatment, but not against other treatments. Indeed, such an *inelastic* (or absolute) fairness construal is documented in the literature (Bian & Keller, 1999; Eckel & Grossman, 1996b).

Building upon this inelastic fairness construal (IFC) notion, the information of whether a helper could be nicer (or nastier) should have no role in offer appraisals. Recipients are expected to evaluate, subject to the guidance of injunctive norm (Ma et al., 2014), whether the offer on its own constituted a *fair* (or an *unfair*) gesture. As such, the IFC should predict an 'all-or-nothing' threshold in fairness attribution similar to Experiments 1 and 2. In other words, only a dichotomous 'fair vs. unfair' contrast in intent and emotions between receipts of *unconditional* and *conditional* (i.e. 'the interest-free', '25%-interest' and '50%-interest') offers should exist. The IFC would also predict all conditional offer receipts, regardless of conditionalities, to trigger similar emotions and intent, considering that the imposition of repayment clauses alone already breached the injunctive fairness norm (Elster, 2006; Ma et al., 2014).

ii) Gratitude Relativity Hypothesis (i.e. GRH hereafter) (Wood et al., 2011)

The GRH argues that the experience of *gratitude* does not function in an absolute manner. *Relativity* matters as people are vigilant to how their benefits 'rank' among what others have acquired. Accordingly, a person who is

convinced that his/her benefit does not compare favourably should barely experience any gratitude. Thus, the GRH implies that recipients seldom judge a benefit in isolation as recipients always contemplate its *relative* worth.

Hence, the newfound transparency of helpers' options in the present study should facilitate the comparisons of offers that one *actually* received with what he/she *could have* received. This comparison should then elicit a *graded, linear* fairness appraisal of offers of varying conditionalities. For instance, knowing that a helper could have helped *unconditionall*y, a recipient should think even *less highly* of helpers giving 50%-interest offers than those 'interest-free' offer givers. Therefore, the GRH would hypothesise not only different intent attributions (and emotions) between receipts of *conditional* and *unconditional* offers, but also across receipts of *conditional* offers of varying repayment obligations (i.e. 'interest-free' vs. 25%-interest' vs. '50% interest').

5.1.3.2. Playing Both Roles and Over-generosity

I highlighted in Section 5.1.2.2 how the present repeated and two-role design allows scrutiny over the two competing hypotheses by Burks et al (2003). They were the *Golden Rules* Hypothesis and the *Reduced Responsibility* Hypothesis. Via the current analysis I examined whether the lack of opportunities to play both in-game roles (i.e. single-role game) was indeed the reason why people over-cooperated in Experiments 1 and 2 (i.e. UAQ 2).

i) Golden Rules Hypothesis

Burks et al. (2003) contended that the playing both roles could raise the salience of the Golden Rule (i.e. 'Do unto others as you would have them do unto you'). Therefore, playing both roles should raise trustfulness (i.e. willingness to give offers) and trustworthiness (i.e. reciprocity) from the

participants, as they would fancy the same to occur to them (Burks et al., 2003). In short, the Golden Rule Hypothesis should predict even more 'over-helping' and 'over-repayment' in the present experiment than in Experiments 1 and 2.

ii) Reduced Responsibility Hypothesis

Burks and colleague (2003), nonetheless, also theorised that playing both roles may undermine prosociality, especially when the game involves *partner-switching* and *independent* trials. Burks et al. (2003) argued that playing both roles under such a setting may diminish prosociality because of a decreased sense of personal responsibility for partner's earning in each trial. As a result, participants may feel less uncomfortable making selfish decisions.

Participants at present were told they each had a different 'partner' in each trial, and that their final outcome depended on the performance on a randomly selected trial. Therefore, the Reduced Responsibility Hypothesis should predict more selfish responding such as refusal to i) make any offers in the helper trials, or ii) fulfil the repayment obligations, in the present experiment than in Experiments 1 and 2.

5.1.3.3. Cheap-riding as Normative Fairness Enforcement?

The present manipulation of 'partners' repayment' (i.e. partial, full or zero repayment) permits the scrutiny of the feasibility of *cheap-riding* in enforcing normative fairness. Thus, this norm-enforcement hypothesis (i.e. NEH hereafter) would predict that being cheap-rode should incite more generosity from the victims as they make amends for their initial violation of normative fairness (i.e. not helping for 'free' in Trial 1) (Ma et al., 2014).

As such, the NEH should predict that relative to *conditional* helpers (of Trial 1) whose offers were *fully* repaid or *utterly not* repaid, helpers whose

recipients *partially* repaid (i.e. Cheap-riding Condition) should demonstrate more fair plays in subsequent trials such as helping *unconditionally* in Trial 10. The null hypothesis, nevertheless, would state that being 'cheap-rode' have no specific role in fostering more fair gestures from the victims.

5.1.3.4. Recurring Research Themes

In addition to the exploratory aims and hypotheses detailed above, this chapter also comprises the analyses of two recurring research themes of the thesis. In particular, this section highlights how *Gratitude* and *Indebtedness*, as a *state* or *trait*, would influence cooperation or sanctioning in Experiment 3.

a) Recurring Research Theme 1: Gratitude and Decision Making

Since the present thesis *primarily* concerns the scrutiny of how gratitude nurtures *prosociality* in economic exchanges, there is no reason to overlook how *state* or *trait* gratitude would relate to the range of cooperative (or retaliatory) decision-making (e.g. helping and repayment) in Experiment 3.

ai.) State Gratitude and Prosociality (or Antisociality) in Economic Games

The *Moral Barometer* hypothesis (McCullough et al., 2001) suggests the logical fallacy in people feeling gratitude while harbouring intentions to harm the benefactors (See Chapter 1 for a detailed discussion). The *Moral Motive* hypothesis (McCullough et al., 2001) also addressed the role of *state* gratitude in cultivating a desire to repay the benefactors. Taken together, both the above Moral Functional Hypotheses of Gratitude should dismiss any motives to default by any grateful recipients. Therefore, these two moral gratitude hypotheses should predict minimal *state* gratitude from recipients under-repaying (partially or completely) their *conditional* offers (See Table 5.1).

aii.) Trait Gratitude and Prosociality (or Antisociality) in Economic Games

Furthermore, gratitude as a *trait* should also discourage retaliation in Experiment 3. Particularly, the Social Cognitive Model of Gratitude (i.e., SCMG, Wood et al., 2008) outlined how *trait* gratitude could indirectly dictate *state* gratitude by altering recipients' appraisal of benefits (Wood et al., 2008), with *state* gratitude being theorised to restrain antisociality towards the helpers (McCullough et al., 2001). Thus, by testing the below path model (See Figure 5.1) examined whether *trait* gratitude would also restrain defaulting (i.e. under-repayment) the way as theorised by Wood et al. (2008).

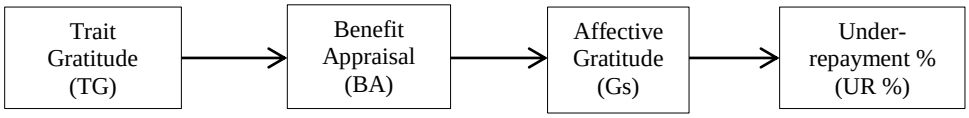

Figure 5.1. The SCMG (Wood et al., 2008) Prediction on Defaulting

b) Recurring Research Theme 2: Indebtedness and Decision Making

This chapter also includes the analysis of how indebtedness, as a *state* or a *trait*, would impact upon participants' decision making in Experiment 3.

bi.) *State* Indebtedness and Prosociality (or Antisociality) in Economic Games

Indebtedness as an unpleasant *state* that results from a benefit receipt (Greenberg & Shapiro, 1971) should also have some bearing on participants' in-game decisions. Greenberg and Westcott (1983) suggested that a *double-blinded* experimental set-up constitutes the perfect breeding grounds for indebted recipients to adopt *Cognitive Restructuring* to manage his/her arousal.

Given that in Experiment 3 I retained the double-anonymity (Hoffman et al., 1994) feature, an emotionally *indebted* recipient may be tempted to berate their helpers' intent as to rid oneself of the indebtedness-invoked discomfort (Hatfield & Sprecher, 1983). Such a distorted intent attribution may consequently trigger *retaliation* (Falk & Fischbacher, 2006) in the form of *cheap*- or *free*-riding. Taken together, this Cognitive Restructuring Hypothesis

(i.e. CRH hereafter) of indebtedness (Greenberg and Westcott, 1983) should predict a *positive, indirect* effect of *state indebtedness* on participants' under-repayment (i.e. UR %) via benefit appraisal (i.e. BA) (See Figure 5.2).

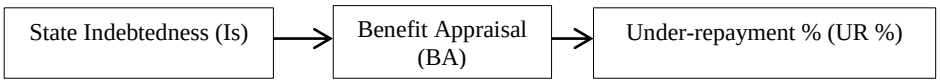

Figure 5.2. CRH's (Greenberg & Westcott, 1983) Prediction on Defaulting

Nonetheless, Greenberg (1980) argued that reciprocity could also help relieve an *indebted* recipient of his/her arousal. Therefore, echoing the Negative State Relief Model (Cialdini al., 1987), an *indebted* recipient would strive to escape the psychological shackle (Greenberg & Shapiro, 1971) by giving back. Contrary to the CRH, this indebtedness-reciprocity notion would predict a *negative* association between *state* indebtedness and participants' UR %. Recipients feeling *more* indebted (toward the helpers) should have a higher propensity to *want to* and *actuall*y make restitutions in the present game.

bii.)*Trait* Indebtedness and Prosociality (or Antisociality) in Economic Games

Dispositionally indebted individuals derive utilities via putting (and keeping) other people in debt (Greenberg & Westcott, 1983), as they enjoy being under-benefited in an interaction (Hatfield & Sprecher, 1983). Thus, I expected a more *dispositionally indebted* participant to help *unconditionally*, since giving to a stranger (while stressing that there is *no need* to pay up) should place the helper in the most disadvantageous position.

A *dispositionally indebted* person is also characterised by an extreme sensitivity to (the *state* of) owing and a rigid allegiance to the reciprocity principle (Greenberg & Westcott, 1983). Therefore, I would anticipate acceptors who under-repaid (either completely or partially) their *conditional* offers to be less *dispositionally indebted* than the fully-repaying acceptors.

Table 5.1. Unanswered Questions, Hypotheses and Predictions of Experiment 3

Unanswered Questions (UAQ)	**Hypotheses/ Exploratory Aims**	**Predictions/ Expected Results**
1. Does degree of unfairness really 'not count' as suggested in Experiments 1 and 2?	*Yes*: Inelastic Fairness Construal (IFC) (Eckel & Grossman, 1996b)	*IFC*: Differences in intent attributions (and emotions) *only* based on if the offer was *Conditional* (i.e. injunctively unfair) or *Unconditional* (i.e.injunctively fair);
	No: Gratitude Relativity Hypothesis (i.e. GRH) (Wood et al., 2011)	*GRH*: Differences in intent perceptions, and emotions, across offer conditionalities (i.e. *Unconditional* vs. *Interest-Free* vs. *25%-* vs *50%-Interest*).
2. Was the lack of chances to play both roles in Experiments 1 and 2 to blame for the 'over-generosity' issue?	*Yes*: Golden Rule Hypothesis (Burks et al., 2003) *No*: Reduced Responsibility Hypothesis (Burks et al., 2003)	*Golden Rule Hypothesis:* More cooperativeness (i.e. helping in both Helper Trials; Reciprocity in Recipient Trials) in Experiment 3 than both Experiments 1 and 2 *Reduced Responsibility Hypothesis*: More selfish responding in Experiments 3 than Experiments 1 and 2 (i.e. less helping and reciprocity in both HTs and RTs)
3. Were rejections in Experiment 2 attributable to *negative reciprocity* or trait indebtedness?	Negative Reciprocity-driven Rejections (Bolton, Katok, & Zwick, 1998)	The percentage of *Conditional* offer rejections at present should be *significantly lower* than that of Experiment 2 (i.e. 35.6%).
	Trait Indebtedness-driven Rejections (Greenberg & Westcott, 1983)	*Conditional* offer *Rejectors* should score *higher* than *acceptors* in the Indebtedness Scales-Revised (i.e. IS-R) (Elster, Maleki, McLeod, & Watkins, 2005)
4. Does being 'cheap-rode' prompt more eventual fair plays from the 'victims'?	'Norm-Enforcement' Hypothesis (i.e. NEH) (i.e. *Cheap-riding* performs a similar function as Altruistic Punishment)	*NEH*: *Conditional* helpers (at Trial 1) from '*Cheap-riding*' condition, compared to helpers from '*Fairness*' or '*Free-riding*' conditions, should show more *generosity* in the eventual trials (e.g. give *unconditional* or *less unfair*) offers in Trial 10)
(Recurring Research Theme 1) Does Gratitude (as a *state* or a *disposition*) nurture cooperation in economic exchanges?	*State* Gratitude: *Moral Barometer* and *Moral Motive* Hypotheses of Gratitude (McCullough et al., 2001)	*Both Moral Barometer and Motive Hypotheses: Conditional* offer acceptors who repaid fully (i.e. *Cooperators*) should report more *state* gratitude than recipients repaying partly (i.e. *Cheap-riders*) or nothing at all (i.e. *Free-riders*).
	Trait Gratitude: Social Cognitive Model of Gratitude (SCMG) (Wood, et al., 2008)	*SCMG*: Trait Gratitude should restrain defaulting (i.e. partial or zero repayments of *conditional* offers) via Benefit Appraisal and Emotional Gratitude (See Figure 5.1)
(Recurring Research Theme 2) Does Indebtedness (as a state or as a trait) prompt cooperative or retaliatory economic	*State* Indebtedness: 1. Cognitive-Restructuring Hypothesis (Greenberg & Westcott, 1983); vs. 2. Indebtedness-driven Reciprocity	*Cognitive-Restructuring Hypothesis: State* Indebtedness should *promote* under-repayment via Benefit Appraisal (See Figure 5.2). *Indebtedness-Driven Reciprocity:* Recipients feeling indebted should, compared to defaulters, be more likely to make restitutions by repaying fully

decision making in the current context?	*Trait* Indebtedness: 1. Trait Indebtedness-motivated rejection (See UAQ 3 above); 2. Indebtedness-driven helping (Greenberg & Westcott, 1983); 3. Indebtedness-driven reciprocity	1. See UAQ 3 above. 2. *Unconditional* helpers (in both Helper Trials) should be the most dispositionally indebted (i.e. scoring higher in IS-R than *Non-helpers* and *Conditional* helpers); 3. *Conditional* offer acceptors who under-repaid (*partially* or *completely*) should report lower IS-R than those who repaid in full (i.e. cooperated).

5.2. Methods

5.2.1. Participants

One hundred and thirty-three students from The University of Nottingham participated in the experiment (79 were females, mean age: 22.7 years, *SD* = 4.7). Each participant was guaranteed a basic allowance of £3 and was automatically entered into a prize draw in which they may win a cash prize up to £35. Participants knew that in each session (N= 8-10) one of them would be randomly drawn as the winner thus became entitled to that cash prize.

5.2.2. Game Design

The present experiment was a modified, repeated version of Experiments 1 and 2 that was conducted with software Z-Tree (Fischbacher, 2007). To begin with each participant was given a fair die to roll and was seated in a separate cubicle. Each experimental session took place with groups of 8 to 10 participants. Participants were told the game included multiple (but had no idea how many) trials, and that they would each be partnered with a different, randomly assigned (and anonymous) fellow participant in each trial.

5.2.2.1. Similarities with Experiments 1 and 2

Each player was initially endowed with 150 money-equivalent ‘Bonus Points’, and therefore he/she needed 50 additional points to reach the ‘Bonus Threshold’ (i.e. 200 points) to have a chance to be entered into the lottery draw to win a cash prize. It was made clear to every player that each bonus point he/she gained would be worth 10 pennies but only when that player made the ‘200 point threshold’ was he/she entitled to the possibility to win any cash prize.

The only way to reach that threshold was by rolling a fair, six-sided die once. The possible payoffs were a loss of -50 points, 0 points, and gains of 50, 100, 150, and 200 points depending on the number on the dice. It was emphasised that a larger number on the dice did NOT guarantee a more favourable allocation of bonus points. Also, in each trial every player learned that he/she was partnered with an unidentifiable and anonymous fellow participant. Participants were reminded that 'neither joint effort nor competition with your partner is warranted'. Again, participants were led to believe they played with one another although in reality they played on their own. All 'post-die roll' scores were pre-determined with no reference to the actual die-roll.

5.2.2.2. New Features of Experiment 3

In this section I highlighted several new features of Experiment 3. They included the 'Jackpot Trials'; perspectives of both helpers and recipients; prior knowledge of defaulting rights; and the manipulation of partners' repayment.

a) The 'Jackpot Trials'

A new feature of Experiment 3 was that participants were not paid for every trial they played. They were informed that, amongst each session, one participant would be drawn as the winner. And for that winner his/her cash prize would be based on the performance of a *randomly* picked trial—the 'Jackpot Trial' (i.e. JT)— he/she participated. For instance, if a player who won the draw and had achieved 350 points on his/her JT, then he/she would win a £35-cash prize. It was made clear to the participants that a final score below the 200 point threshold at the JT means that his/her cash prize would remain ZERO. Thus, participants were recommended to take each trial seriously.

b) Perspectives of both Helpers and Recipients

Participants played as a potential helper (i.e. P1s) twice and as a recipient (i.e. P2) eight times in this game. Meanwhile, they did not know a priori in which trial they would play as a P1 or as a P2. The next sections (i.e., b i) and b ii)) detail the chain of decisions a participant had to make.

b i.) The Helper Trials (HT)—the first and the final (10th) trial

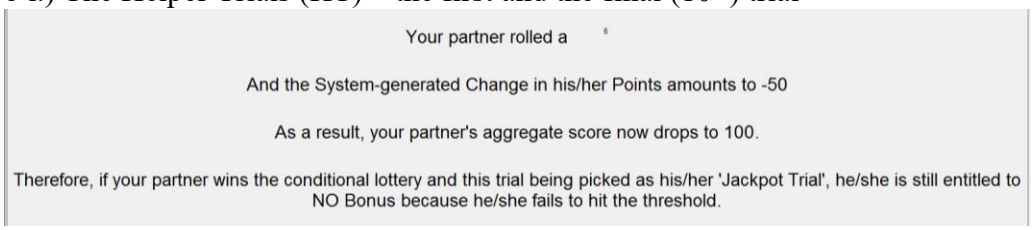

Figure 5.3. Notification of partners' failure to win any bonuses

An HT was almost identical to Experiment 1. All participants, after their die-rolling, were told they ended up with a final score of 350. Thus, they could win a £35-cash prize if they also won the prize draw. They were also informed that their 'partners' acquired just 100 points (i.e. below the threshold) thus would win no bonuses, even if the latter won the lottery (See Figure 5.3).

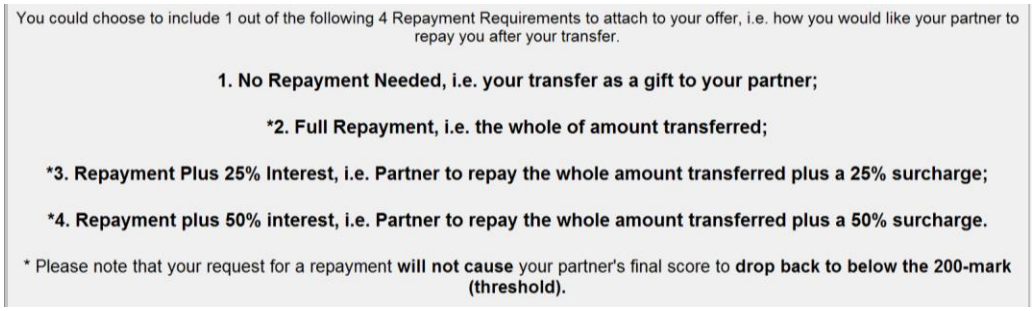

Figure 5.4. Range of helping options (Helper Trials)

As a result, participants were asked if they would help by transferring part of their excess points (i.e. 350 – 200 'Threshold' = 150 points), so that in case that partner won the conditional lottery he/she could still acquire some bonus money. Participants could make either an *unconditional* or a *conditional* transfer (See Figure 5.4). *Conditional* offer givers would then decide a level of repayment out of three alternatives (i.e. Interest-Free, 25%-, or 50%-Interest

Offer) as the repayment clause (See Figure 5.5). All offer givers then specified the transfer magnitude (from the range 100- 150 points).

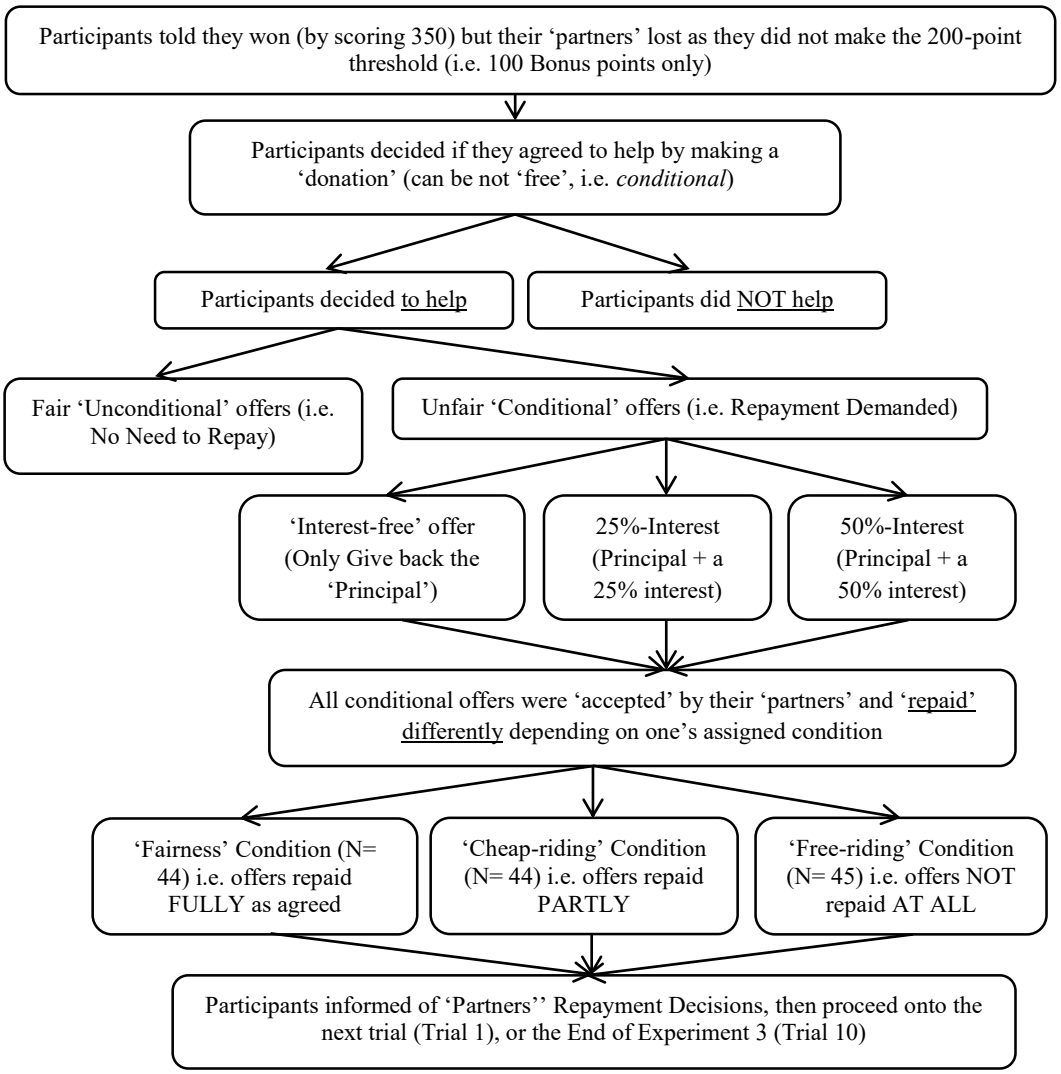

Figure 5.5. The Flow of Decisions in the Helper Trials (Trial 1 and Trial 10)

Conditional helpers were then notified of their recipients' actual repayment decisions—which belonged to another new feature of Experiment 3 (to be discussed later in e))—despite their previous agreement. The trial ceased here with participants knowing what their cash prizes would be if they won the lottery Figure 5.5 outlines the flow of decisions in a helper trial. Participants played as a potential helper in the first (i.e. T1) and the tenth trial (i.e. T10).

b ii). The Recipient Trials (RTs, the 2nd to 9th Trial)

All participants purportedly 'lost' the die-rolling game, as they ended up with only 100 Points, and therefore entitled to NO cash prize (for that trial) even if they eventually won the lottery. Nonetheless, every player then learned that his/her 'partner', who 'won' like he/she did in Trial 1 (i.e. getting 350 Bonus Points), offered to help by giving away part (i.e. 100 points) of his/her excess bonus points (i.e. 350-200 (Bonus Threshold) =150 points).

Table 5.2 Counter-balancing of the sequences of the offers received (in blocks)

Trial/ Block	**1st**	**2nd**	**3rd**	**4th**	**5th**	**6th**	**7th**	**8th**	**9th**	**10th (Last)**
A	Helper	25%	I-F	50%	UC	I-F	UC	50%	25%	Helper
B	Helper	25%	UC	I-F	50%	UC	50%	I-F	25%	Helper
C	Helper	25%	50%	UC	I-F	50%	I-F	UC	25%	Helper
D	Helper	25%	50%	UC	50%	I-F	I-F	UC	25%	Helper
E	Helper	25%	I-F	I-F	UC	50%	UC	50%	25%	Helper
F	Helper	25%	UC	50%	I-F	UC	50%	I-F	25%	Helper

Note. Helper = Helper Trials; UC = Unconditional Offers; I-F= 'Interest-free' Offers; 25% = '25%-Interest' Offers; 50% = '50%-Interest' Offers.

Participants took turn to receive two unconditional offers, 'interest-free', '25%-interest' and '50%-interest' offers over their eight trials as recipients (Refer to Table 5.2). I counter-balanced, using a Latin square, the order of the conditionality of offers a player would receive in six blocks (block A to F). Participants were randomly assigned to one out of these six blocks.

Recipients of *conditional* offers were told their offers were not 'free'. By accepting they undertook an obligation that, in case they eventually won the lottery they would return part of their earnings. For example, a recipient who accepted an 'interest-free' 100-point offer (i.e. £10) on the 5th trial, who then won the draw (and trial 5 being the JT) thus became entitled to a £20-cash prize, was expected to repay £10. Meanwhile, *unconditional* offer recipients were told they were not expected to repay. Participants then decided whether to accept (or decline) the offers. They were reminded that a rejection would yield them ZERO payoffs even if they won the lottery. This reminder served to assure that participants understood the financial risks in rejecting any offers.

c) Prior Knowledge of one's 'right to default'

Recipients were told they could 'decide whether to repay, and the amount of repayment later' before their acceptance decisions. After indicating offer acceptances (or rejections) participants would rate ten items (shown in Table 5.3) about their emotions toward their helpers.

Table 5.3 Measurement of Recipients' Emotions toward their Offers

Item	Questions	Constructs Represented
1	I feel indebted (i.e. feeling like I owe something) to my partner.	Indebtedness (State)
2	How much gratitude would you feel towards your partner?	Gratitude (State)
3	I am annoyed by my partner's decision.	Annoyance (State)
4	I would have done the same thing were I in my partner's shoes.	Perspective-Taking
5	I feel obliged to repay my partner	Obligation
6	How much do you consider your partner's transfer is motivated by a sincere desire to help?	Perceived Helpfulness (Wood et al., 2008)
7	My partner could have been more generous to me.	Generosity
8	How much do you think it cost your partner to have offered you help? ('1'= 'Nothing'; 7= 'A Great Deal')	Perceived Cost of the Benefits (Wood et al., 2008)
9	I am eager to help my partner out if he/she is in need in the near future	Willingness to Reciprocate (Ma et al., 2014)
10	How valuable do you consider that your partner's help is to you?	Valuableness of the Benefits

Note. Items 6, 8 and 10 were averaged to form a *Benefit Appraisal* composite.

d) Avenues for Restitutions among *Unconditional* offer recipients

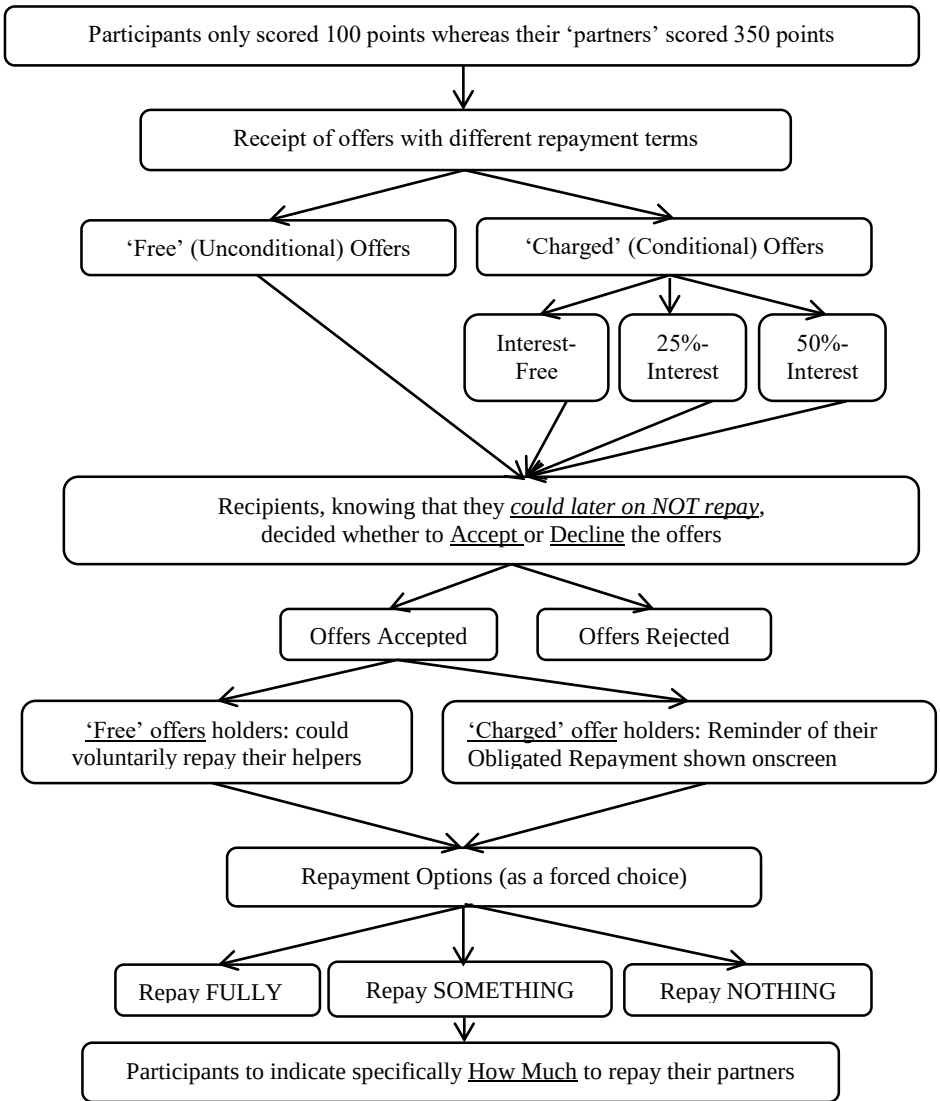

Figure 5.6. The Flow of Decisions in the Recipient Trials (Trials 2-9)

Acceptors of *conditional* offers were then shown a reminder of their *expected* repayment. Immediately after that they would decide whether to repay 'nothing at all', 'some', or 'the whole' of the repayment. Acceptors who chose to give back 'some' then specified their intended repayment (See Figure 5.6). Meanwhile, the present experiment permitted acceptors of *unconditional* offers to repay voluntarily (See Figure 5.7 below). It was, however, reiterated to the acceptors that the helpers 'did not expect to receive any repayment'.

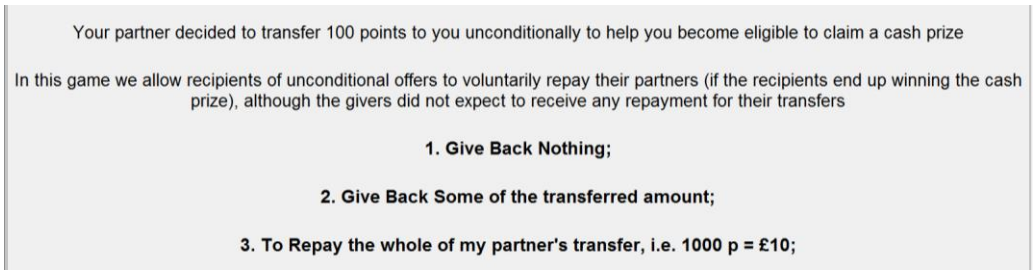

Figure 5.7. Available Avenues to Repay for *Unconditional* Offer Acceptors

e). Manipulation of Partners' Repayment (Trial 1 and 10)

The fifth new feature of Experiment 3 entails the manipulation of repayment decisions by a participant's 'partner' when the participant played as P1s. I randomly assigned participants into one out of three conditions in which their *conditional* offers yielded different repayment (See Figure 5.5). These conditions included: first, a *Fairness* condition ($N = 44$) in which the 'partners' repaid *fully* (i.e. cooperation). Alternatively, participants could be assigned to the *Cheap-riding* condition ($N = 44$) where their offers were *partially* repaid. This partial repayment was a randomly-generated figure between 1p and the amount that 'partner' *should* repay. Or, a participant could end up in the *Free-riding* condition ($N = 45$) in which he/she received *utterly no* repayment.

At the end of each helper trial, every *conditional* helper was shown the repayment he/she *should* receive and the repayment he/she *actually* received (See Figure 5.8). This serves to highlight to the participants whether their

partners had breached the agreement. Participants who *refused* to help or helped *unconditionally* in Trial 1 were 'immune' to this manipulation.

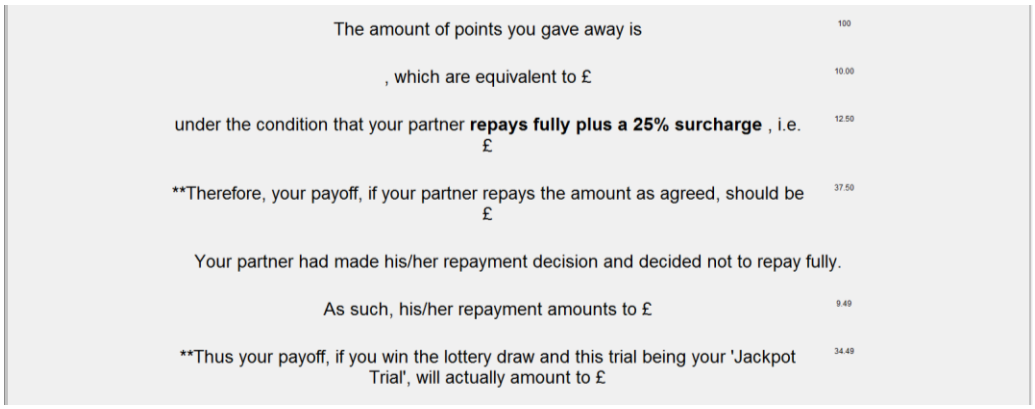

Figure 5.8 Notification of 'Partners'' Repayment (*Cheap-riding* Condition)

5.2.2.3. Analysing Individual Differences in the Game

While I retained the assessment of *Trait Gratitude*—measured by Gratitude-Questionnaire-VI (McCullough et al, 2002) (Cronbach's $\alpha = .729$, $k = 6$), and *Social Desirability Responding*—assessed via Paulhus' (1991) Impression Management Scale (Cronbach's $\alpha = .787$, $k = 19$), in Experiment 3 I added the measure of *Trait* indebtedness. I adopted the Indebtedness Scale-Revised (Elster et al., 2005) (Cronbach's $\alpha = .902$, $k = 22$) in examining how dispositionally indebted a participant was. All these surveys were conducted after Trial 10. The entire experimental session ended here and participants were debriefed and received their compensations.

5.2.3. Ethics Statement

The present experiment was approved by the ethics committee of the School of Psychology at the University of Nottingham. All participants were aged above 17 and all gave written, informed consent prior to participation as approved by the ethics committee (Approved 14th Oct 2014; Ref. Code: 531).

5.2.4. Outcomes Section

In this section I outline and operationalize several crucial test variables in relation to the hypotheses and exploratory aims (Refer to Table 5.1.).

5.2.4.1. Key Variables in the two Helper Trials (HTs, i.e. Trial 1 and 10)

a) Incidence and Conditionality of Helping

One primary indicator of cooperative responding (in the two HTs) was the percentage of helping. Apart from exploring the overall participants' cooperativeness (by helping an unknown co-player), one key aspect of this analysis was the *change* in one's helping decisions over the two HTs.

Another indicator of prosociality during the HTs was the levels of repayment a P1 requested from the recipient (i.e. *unconditional*, *interest-free*, *25% interest* and *50% interest*). I scrutinised also the presence (or absence) of *change* in conditionalities of offers made when participants played P1s again in Trial 10. Both variables are crucial in relation to certain hypotheses. For instance, the *norm-enforcement* hypothesis would argue that 'cheap-rode' *conditional* offer givers (in Trial 1) would give *unconditional* offers in Trial 10. Therefore, I specifically coded 1) whether or not *conditional* helpers at Trial 1 would give *unconditional* offers in Trial 10; and 2) whether or not conditional helpers at Trial 1 would make more *generous* offers in Trial 10 (e.g. gave '50%-interest' offer at Trial 1 but made '25%-Interest' offer at Trial 10).

b) Magnitude of Transfers

In Experiment 3 prospective helpers were granted freedom to decide the range (from 100- 150) bonus points to transfer in both Helper Trials. Hence, I regarded also the transfer magnitude as a token of prosociality at present.

5.2.4.2. Key Variables in the Recipient Trials (RTs, i.e. Trial 2 to 9)

a) Offer Acceptances and Rejections

I examined the proportion of players accepting (or declining) their offers while knowing that they could just take the offers and forget about repaying.

b) Pre-repayment Ratings

I measured participants' evaluations of their offers via a series of ratings in 7-point Likert scales (i.e. pre-repayment ratings). As a recurring theme of the thesis, here I specifically inspect how *state* gratitude and indebtedness would relate to the recipients' cooperativeness (e.g. repayment or the lack thereof).

In each RT an item '*My partner could have been more generous to me*' served as a manipulation check. Via this I ascertained participants' awareness of the various levels of injunctive fairness implied by different offers (i.e. unconditional, 'Interest-free', 25%-interest' and '50%-interest'), and the fact that their helpers could have acted more generous or selfish. Additionally, recipients gave an overall offer appraisal (Wood et al., 2008) by indicating their i) perceived genuine helpfulness, ii) perceived values of the offers, and ii) perceived costliness to the helpers (for helping). I created a composite 'Benefit Appraisal' in each RT by averaging participants' ratings on items (i) to (iii).

c) Categorical and Numerical Repayment Decisions

In the present analysis I used the same categorical classifications as Experiment 2 of *conditional* offer acceptors based on their repayment. Acceptors who repaid completely were the *cooperators.* Those who repaid nothing were termed the *free-riders* as did Krishnamurthy (2000). Participants whose repayment was short of the expected amount were classified as the *cheap-riders* (Cornes & Sandler, 1984). In addition to these *categorical* classifications, I also quantified participants' sanctioning via computing the

under-repayment percentage for each acceptor for every receipt. This under-repayment metric (i.e. *UR%* hereafter) was computed by the formula [100%- (Participant's *Actual* Repayment/ *Expected* Repayment)* 100%].

This UR% metrics and the above nominal classifications were NOT applicable in the two RTs where participants received *unconditional* offers, since these recipients were not expected to repay unless they volunteered to. Thus, I operationalize the cooperativeness of these unconditional offer acceptors by the magnitude of any voluntary repayment they made.

5.2.4.3. Variables in the *Dispositional* Assessment

In the present experiment I employed the Impression Management Scales (Paulhus, 1991) (Cronbach's $\alpha = .787$, $k = 19$), the Gratitude Questionnaire-VI (McCullough et al., 2002) (Cronbach's $\alpha = .729$, $k = 6$), and the Indebtedness Scales-Revised (Elster et al., 2005) (Cronbach's $\alpha = .902$, $k = 22$), respectively as the validated assessment of *Social Desirability Responding*, *Dispositional Gratitude* and *Dispositional Indebtedness*. I computed a composite score for each of these inventories via averaging, after reverse-scoring, participants' ratings on these scales. All participants' ratings were on 7-point Likert Scales (i.e. '1': 'Strongly *Disagree*'; '7': Strongly *Agree*').

5.3. Results- Methodological Features

In this chapter I split the results section into two halves (i.e. Sections *5.3* and *5.4*). Importantly, the present section entails the scrutiny over hypotheses related to the *methodological* features of Experiment 3 (i.e. *one-shot* versus *iterated* design, partner repayment manipulation etc.). This sub-section includes also analyses over whether the present crucial (decision) variables were confounded by factors such as *gender* or *social desirability responding*. Meanwhile, analyses on unanswered questions (i.e. UAQs hereafter) on how *state* or *trait* emotions (e.g. gratitude or indebtedness) impacted decision-making (in the present game) are discussed in Section *5.4*.

5.3.1. UAQ 2: Was the lack of perspectives of both the helpers and recipients to blame for the over-generosity issue in Experiments 1 and 2?

I examined two competing hypotheses on how the presence (or lack thereof) of perspectives of both in-game roles would foster prosociality in Experiment 3. The *Reduced Responsibility Hypothesis* (Burks et al., 2003)—which predicts playing both roles should *undermine* prosociality (e.g. less helping or repayment) in Experiment 3 relative to Experiments 1 and 2 (which are both one-shot games); and the *Golden Rules Hypothesis* (Burks et al., 2003)—which argues that playing both roles should instead *encourage* prosociality in the present context in comparison to Experiments 1 and 2.

5.3.1.1. Statistical Analyses

a) Comparing *Helping* Decisions (Experiment 1 vs. Experiment 3)

To test both the *Reduced Responsibility* and the *Golden Rule Hypotheses* I compared several crucial decisions in the present game to that of Experiments 1 and 2 (i.e. E1 and E2 hereafter). First, I compared the helping

decisions (i.e. rate of helping and offer conditionality etc.) in E1 to both Helper Trials (i.e. Trial 1 and 10) in the present game. Data from Trial 1 was NOT excluded as participants were aware (before the start) that they could be 'either in a position *to help* or to *receive help*', depending on their (and their partners') 'final score at each trial'. I analysed the difference across experiments (i.e. E1 vs. Trial 1; or E1 vs. Trial 10) on *binary* outcome measures (e.g. help or no help) using a 2 (Experiments)*2 (Outcomes) Chi-square test. Here I dichotomized offer conditionality into *unconditional* or *conditional* thanks to the low counts of *conditional* offer givers of each specific category in E1. I also replaced the analysis of transfer magnitude with whether helpers 'over-donated' (i.e. gave away more than required) as half of the potential helpers ($N = 30$) were stipulated to make bigger donations in E1. Table 5.4 illustrates the data.

b) Comparing Repayment Decisions (Experiments 1 and 2 vs. Experiment 3)

To further examine the Reduced Responsibility and *Golden Rule Hypotheses* I also explored how *repayment* decisions of the present experiment compared to that of Experiments 1 and 2.

b i.) Comparing Repayment Decisions (Experiment 1 vs. Experiment 3)

Owing to the relative low counts ($N = 21$) of *conditional* offer acceptors in E1 I dichotomize these acceptors' repayment into *cooperators* (i.e. repaying fully) or *defaulters* (i.e. *partial* or *complete* under-repayment). I excluded repayment data from the two present RTs where recipients were *unconditionally* gifted; since *unconditionally* gifted recipients in E1 ($N = 25$) were not offered any avenues to repay (See Chapter 3). I included also the comparison of under-repayment rate (i.e. UR% hereafter) in E1 with that of the current experiment. Again, the analysis with nominal binary outcomes (i.e.

cooperate or default) was done via a 2 (Experiments)*2 (Outcomes) Chi-square test. The numerical variable (i.e. UR %) was examined using the Mann-Whitney U Test. Table 5.5 and Table 5.6 outline the results.

b ii.) Comparing Repayment Decisions (Experiment 2 vs. Experiment 3)

To analyse how *categorical* repayment (i.e. repay *all*, *nothing* or *partially*) of *conditional* offer acceptors differed across experiments (i.e. Experiment 2 vs. Initial and vs. Second receipts in Experiment 3)—while controlling for offer *conditionality*—I resorted to the Multinomial Logit Model (i.e. MNLM hereafter). The MNLM permits comparisons of multiple groups via a combination of binary logistic regressions (El-habi, 2012), thus allows each category (e.g. repaying *all* or *nothing*) of the outcome variable to be contrasted with a *reference* category (e.g. repaying *some*) (Fredua, 2015). The current model— based upon a *generalized logit* link function (Horton & Lipsitz, 1999)—was examined with a *robust* variance estimator (Zeger & Liang, 1986) and an exchangeable 'working' correlation structure. This specific analysis was conducted via SAS-callable SUDAAN Release 9.0.1, as statisticians (Hardin & Hilbe, 2003; Horton & Lipsitz, 1999) generally agreed that SUDAAN is one of the only packages that can handle nominal multinomial regressions (i.e. *generalized logi*t link). The findings are detailed in Table 5.7.

Additionally, I examined, via independent samples t-tests, whether the UR% were different in E2 in comparison to *both* receipts of *conditional* offers in Experiment 3. Table 5.8 details the findings.

5.3.1.2. Results

a) Is helpfulness more prevalent in a single- (E1) or a both-role (E3) design?

As shown in Table 5.4, there was no evidence (both *p*s >. 05) that the rate of offer provisions of Experiment 1 (i.e. 49/61 = 80.3%) differed from that of Trial 1 (i.e. 95/133= 71.4%) or Trial 10 (i.e. 104/ 133= 78.2%). Similar results emerged (χ^2 (1) = 0.824, p = .364) when I contrasted the rate of provisions of Experiment 1 (i.e. 71.4%) with the combined rates (computed via summing both Trial 1 and Trial 10) of Experiment 3 (i.e. 199/266 = 74.8%)

Table 5.4 Comparison of Helping Decisions (E1 vs. E3T1 vs. E3T10)

	Experiment 1 (E1) vs. Experiment 3-Trial 1 (E3T1) vs. -Trial 10 (E3T10)						
		E1	E3T1	E3T10	E1 vs. E3T1		E1 vs. E3T10	
		N	*N*	*N*	χ^2	*p*	χ^2	*p*
1.Helper vs. Non-helper	Non-Helper	12	38	29				
	Helper	49	95	104				
	Total	61	133	133	1.73	.22	0.11	.74
2.Offer Conditionality	Conditional	24	83	84				
	Unconditional	25	12	20				
	Total	49	95	104	24.95	<.001	16.21	<.001
3. Over-donated?	Yes	14	12	17				
	No	35	83	87	4.70	.030	3.08	.079

Note. All statistical tests were two-tailed. χ^2 = Pearson's Chi-square.

The rate of making an *unconditional* (instead of conditional) transfer was higher (both *p*s <.001) in Experiment 1 (i.e. 25/49 = 51.0%) than both Trial 1 (i.e. 12/95= 12.6%), Trial 10 (i.e. 20/104= 19.2%) and the overall rates (i.e. 32/199= 16.1%, χ^2 (1) = 27.12, p <. 001) of Experiment 3. The rate of over-donation in E1 (i.e. 14/49 = 28.6%) was higher (p = .030) than that of Trial 1(i.e. 12.6%) but was only marginally different (p = .079) from that of Trial 10 (i.e. 17/104 = 16.3%).The comparison is again significant (χ^2 (1) = 5.376, p = .020) when I compared the over-donation rate of Experiment 1 (i.e. 28.6%) to the combined rate of Experiment 3 (i.e. 14.5%). Overall, the current analyses failed to offer conclusive evidence as of whether playing both roles would haunt or promote generosity when participants played as a potential helper.

b i.) Are people more trustworthy in a single-(E1) or a both-role (E3) design?

The analyses (See Table 5.5 below) offer no evidence (all *p*s >. 05) that the propensity to cooperate (or default) was different in a one-shot scenario (i.e. E1) versus in a context where participants played both roles (i.e. E3).

Table 5.5 Comparison of Repayment Decisions (E1 vs. E3a vs. E3b)

Experiment 1 (E1) vs. Experiment 3-Initial Receipt (E3I) vs. Second Receipt (E3S)										
		E1	E3I	E3S	E1 vs. E3I		E1 vs. E3S		E1 vs. E3(Total)	
		N	*N*	*N*	χ^2	*p*	χ^2	*p*	χ^2	*p*
CN										
'-20%'	Cooperate	5	—	—						
	Default	3	—	—						
'I-F'	Cooperate	6	92	91						
	Default	2	38	37						
	Total	8	130	128	No analysis done due to insufficient sample in E1.					
'20%'	Cooperate	2	—	—						
	Default	3	—	—						
'25%'	Cooperate	—	73	55						
	Default	—	39	65						
'50%'	Cooperate	—	39	36						
	Default	—	68	70						
Overall	Cooperate	13	204	182						
	Default	8	145	172						
	Total	21	349	354	0.007	.933	0.505	.478	0.170	.680

Note. CN: Conditionality; '-20': offers of which acceptors only had to repay 80% of the transfer; 'I-F': Interest-Free offers; '20%': offers of which acceptors were liable to a 20%-interest in addition to repaying the 'principal'; '25%': 25%-Interest offers; '50%': 50%-Interest offers. Cooperators: Acceptors who repaid *fully*; Defaulters; Acceptors repaying *partly* or *nothing at all*.

Table 5.6 Comparison of Numerical Repayment (E1 vs. E3a vs. E3b)

	Experiment 1 (E1) vs. Experiment 3-Initial Receipt (E3I) vs. Second Receipt (E3S)								
	E1		E3I		E3S		E1 vs. E3I	E1 vs. E3S	E1 vs. E3 (All)
CN	*N*	*UR%* (SD)	*N*	*UR%* (SD)	*N*	*UR%* (SD)	*Mann-Whitney U (p,2-tailed)*	*U (p, 2-tailed)*	*U (p, 2-tailed)*
-20%	8	26.6 (40.3)	—	—	—	—			
Interest -Free	8	6.37 (17.6)	130	20.8 (35.6)	128	18.6 (32.7)	No analysis done due to insufficient sample in E1.		
20%	5	26.5 (41.5)	—	—	—	—			
25%	—	—	112	22.5 (37.3)	120	32.4 (38.6)			
50%	—	—	107	40.4 (39.1)	106	38.5 (37.3)			
Overall	21	18.86 (33.5)	349	27.4 (38.2)	354	29.2 (37.0)	3370.0 (.488)	3168.0 (.219)	6538.0 (.327)

Note. CN: Conditionality; '-20': offers of which acceptors only had to repay 80% of the transfer; 'I-F': Interest-Free offers; '20%': offers of which acceptors were liable to a 20%-interest in addition to repaying the 'principal'; '25%': 25%-Interest offers; '50%': 50%-Interest offers.

The Mann-Whitney U tests (See Table 5.6) indicated no evidence (all *p*s >. 05) either that the UR% in Experiment 1 (M: 18.9%) were different from either the *initial* (M: 27.4%) or the *second* receipts (M: 29.2%) of Experiment 3. Considering also the comparisons of helping decisions among E1 vs. E3 (i.e. Table 5.6), overall the current data seemed to endorse *neither* of the Golden

Rule nor the Reduced Responsibility Hypothesis, as the experience of playing both roles apparently *neither* depressed *nor* stimulated more trustfulness (i.e. more helping) and trustworthiness (i.e. less under-repayment or defaulting).

b ii.) Are people more trustworthy in a single- (E2) or a both-role (E3) design?

Table 5.7 Comparison of Repayment (Offer Conditionality as covariate)

			Variable: *Experiment*	
	Odds Ratio (ORs)	Intercept	*Experiment 2* vs. *E3-Initial* (RC)	*E3-Second* vs. *E3-Initial* (RC)
1. Reference Category (RC):				
- *Experiment 3-Initial Receipt* (i.e. E3-Initial) (Experiment) &				
- Repay *Partially* (Repayment)				
Variable: *Categorical* Repayment				
-Repay *All* vs.	Odds Ratio	0.81	2.41*	0.82
Repay *Partially* (Reference)	95% LLOR	0.56	1.13	0.65
	95% ULOR	1.17	5.13	1.04
-Repay *Nothing* vs.	Odds Ratio	0.19	3.70*	1.45
Repay *Partially* (Reference)	95% LLOR	0.10	1.28	0.96
	95% ULOR	0.36	10.73	2.17
2. Reference Category (RC):				
- *Experiment 3 (i.e. E3)-Second Receipt* (Experiment) &			*Experiment 2* vs.	*E3-Initial* vs.
- Repay *Partially* (Repayment)			*E3-Second* (RC)	*E3-Second* (RC)
Variable: *Categorical* Repayment				
-Repay *All* vs.	Odds Ratio	0.66*	2.94*	1.22
Repay *Partially* (Reference)	95% LLOR	0.45	1.36	0.96
	95% ULOR	0.99	6.32	1.55
-Repay *Nothing* vs.	Odds Ratio	0.28*	2.56	0.69
Repay *Partially* (Reference)	95% LLOR	0.17	0.95	0.46
	95% ULOR	0.45	6.88	1.04
3. Reference Category (RC):			Variable: *Experiment*	
- *Experiment 3-Initial Receipt* (Experiment) &		Intercept	*Experiment 2* vs.	*E3-Second* vs.
- Repay *Nothing* (Repayment)			*E3-Initial* (RC)	*E3-Initial* (RC)
Variable: *Categorical* Repayment				
-Repay *All* vs.	Odds Ratio	4.25*	0.59	0.50*
Repay *Nothing* (Reference)	95% LLOR	2.25	0.23	0.32
	95% ULOR	8.04	1.55	0.77
-Repay *Partially* vs.	Odds Ratio	5.27*	0.24*	0.60*
Repay *Nothing* (Reference)	95% LLOR	2.78	0.08	0.38
	95% ULOR	9.98	0.71	0.95
4. Reference Category (RC):			Variable: *Manipulation*	
- *Experiment 3-Second Receipt* (Experiment) &		Intercept	*Experiment 2* vs.	*E3-Initial* vs.
- Repay *Nothing* (Repayment)			*E3-Second* (RC)	*E3-Second* (RC)
Variable: *Categorical* Repayment				
-Repay *All* vs.	Odds Ratio	2.13*	1.19	2.00*
Repay *Nothing* (Reference)	95% LLOR	1.25	0.49	1.29
	95% ULOR	3.64	2.86	3.08
-Repay *Partially* vs.	Odds Ratio	3.15*	0.41	1.67*
Repay *Nothing* (Reference)	95% LLOR	1.93	0.15	1.05
	95% ULOR	5.14	1.10	2.65

Note. LLOR: Lower Limit Odds Ratio; ULOR: Upper Limit Odds Ratio. * denotes that the 95% CIs did not overlap the null value (i.e. OR=1), implying statistical significance (Szumilas, 2010).

Below I examined whether *Golden Rule* or the *Reduced Responsibility* Hypothesis would explain any differences in trustworthiness between that of Experiments 2 and 3. The present multinomial model, which comprises

experiments (i.e. Experiment 2 vs. Experiment 3-Initial vs. Experiment 3-Second Receipt) as predictor and *conditionality* (i.e. Interest-free vs. 25%- vs. 50%-Interest) as covariate, is significant (Wald χ^2 (12) = 123.9, p <.001). Both variables have significant main effects (i.e. *experiments*, Wald χ^2 (4) = 17.18, p =.002; *conditionality*, Wald χ^2 (6) = 55.7, p <.001) over repayment (i.e. repay *fully* vs. *partially* vs. *nothing*). There was, however, no evidence that these two variables had interacted to influence repayment (Wald χ^2 (7) = 11.1, p =.13). I detail in the following a breakdown of the significant effect by *experiments*.

Scrutiny of the odds ratios (i.e. OR hereafter) suggested that acceptors from experiment 2 (N= 87), compared to either receipt in Experiment 3, were, in general, more reluctant to partially repay (i.e. cheap-ride). For instance, acceptors from E2 were, compared to the *initial* acceptance in E3, 2.41 times (i.e. 95% CI (1.13, 5.13)) more likely to repay *all* than to repay *partially*. They were also 3.70 times (i.e. 95% CI (1.28, 10.73)) likelier to repay *nothing* rather than to repay *partially*, compared to acceptors of initial offers in Experiment 3.

A similar trend was observed (See Table 5.7 above) when contrasting the *categorical* repayment in E2 to that of the second offer acceptance in E3. The former were 2.94 times likelier (i.e. 95% CI (1.36, 6.32)) than the latter to repay *fully* instead of *partially*. There was marginal evidence that (i.e. 95% CI (0.95, 6.88)) the former would prefer (OR: 2.56) repaying *nothing* than *partially*, compared to the present participants who accepted their conditional offers in their second receipts. There was no evidence that E2 participants' decisions to repay *fully* or *none* differed from that of in Experiment 3. Taken together, the current analyses imply an 'all-or-nothing' repaying mindset of acceptors in Experiment 2. They appeared to prefer either honouring their

obligations totally or simply shirking from that altogether. Thus, again there is no clear-cut evidence that people are overall more (or less) *trustworthy* in a *single*- vis-à-vis a *both-role* context. Neither the *Golden Rule* nor the *Reduced Responsibility* Hypothesis was confirmed.

Table 5.8 Comparison of Numerical Repayment (E2 vs. E3a vs. E3b)

	Experiment 2 (E2) vs. Experiment 3-Initial Receipt (E3I) vs. Second Receipt (E3S)											
	E2		E3I		E3S		E2 vs. E3I		E2 vs. E3S		E2 vs. E3 (All)	
CN	*N*	*UR%* (SD)	*N*	*UR%* (SD)	*N*	*UR%* (SD)	*df*	*t (p)*	*df*	*t (p)*	*df*	*t (p)*
Interest -Free	21	16.2 (33.4)	130	20.8 (35.6)	128	18.6 (32.7)	149	0.56 (.579)	147	0.31 (.759)	277	0.45 (.651)
12.5%	24	16.9 (34.1)	—	—	—	—	No analysis was done.					
25%	23	20.8 (38.3)	112	22.5 (37.3)	120	32.4 (38.6)	133	0.19 (.847)	141	1.32 (.190)	253	0.81 (.418)
50%	19	32.4 (43.9)	107	40.4 (39.1)	106	38.5 (37.3)	124	0.80 (.424)	123	0.63 (.529)	230	0.76 (.450)
Overall	87	21.2 (37.2)	349	27.4 (38.2)	354	29.2 (37.0)	434	1.36 (.175)	439	1.81 (.071)	788	1.67 (.096)

Note. UR%: Mean Under-repayment rate; CN: Conditional offers; t: t-value; p: p-value (2-tailed)

Meanwhile, comparison on the UR% (See Table 5.8) offered no evidence (all *p*s >.05) that acceptors were differentially retaliatory in a *one-shot*, vis-à-vis in an *iterated* context where they had experiences playing both in-game roles. Taken together, apart from the sporadic evidence (See Table 5.7) that acceptors in Experiment 2 were less likely to *cheap-ride*, overall the current data barely supports either the *Golden Rule* Hypothesis or the *Reduced Responsibility* Hypothesis. Participants who *accepted* their *conditional* offers appeared more or less trustworthy with (i.e. Experiment 3) or without (i.e. Experiment 2) the perspectives of both helpers and recipients.

5.3.1.3. Conclusion

All in all, the above extensive comparisons of decision-making in Experiments 1 and 2 to that of Experiment 3 did not supply any clear-cut evidence that the present *two-role*, *repeated* game design had either raised or undermined participants' in-game generosity or trustworthiness. Therefore, the

current analyses failed to justify the over-generosity issues evident in Experiments 1 and 2 by the *single-role* design in both experiments.

5.3.2. UAQ 3: Were rejections in Experiment 2 attributable to *negative* reciprocity or *dispositional* indebtedness?

I predicted that, thanks to the recipients' knowledge of a more efficient way to retaliate (i.e. take the offer and not repay), the offer rejection rate should be noticeably lower in Experiment 3 than in Experiment 2 (i.e. 35.6%) if *negative reciprocity* was what engendered rejections in Experiment 2 (See Table 1) . I also hypothesised that rejections at present should be primarily driven by *trait* indebtedness and as a result, the rejectors of conditional offers should be more *dispositionally indebted* than the acceptors. Below I include the comparison of rejections across Experiments 2 and 3, alongside the analyses over the psychology of the present rejectors. It should be noted that analyses relating to how indebtedness (as a *state* or *trait*) contributed to prosociality (e.g. *helping* or *repayment*) in Experiment 3 will be featured in Section 5.4.

5.3.2.1. Statistical Analyses

a) Comparing the Rates of Conditional offer rejections in Experiment 2 and 3

There was no evidence that conditionality had affected offer rejections in Experiment 2, χ^2 (3) = 3.30, p = .23 (See Chapter 4). Thus, via a Generalised Estimating Equation (i.e. GEE hereafter)—with an exchangeable correlation structure—I first examined whether *conditionality* would alone, or interacted with the order of receipt (i.e. initial vs. second) to, predict rejection in Experiment 3. In this analysis I excluded data from the two Recipient Trials during which participants received *unconditional* offers.

Second, to directly compare the *overall* landscapes of *conditional* offer rejections in both experiments (i.e. E2 and E3 hereafter) I carried out a three-way, 2*2*4, Log-linear analysis. The two-level factors were *Experiments* (i.e. E2 vs. E3) and *Acceptances* (or rejection), while the four-level factor was *Conditionality* (i.e. Interest-Free vs. 12.5% vs. 25% vs. 50%-Interest).

b) Trait Indebtedness and Offer Rejections

The Mann-Whitney U test was adopted in examining whether offer rejectors would, as predicted, report higher *trait* indebtedness than acceptors. Participants' each receipt of their eight offers (of varying conditionality) was separately analysed. Table 5.9 details the findings.

5.3.2.2. Results

a) Comparing Conditional offer Rejections in Experiment 2 and 3: Is *Negative Reciprocity* (i.e. to sanction unfair helpers) driving rejections in Experiment 2?

First, results of the GEE revealed *only* a significant main effect by conditionality on rejection in Experiment 3 (Wald χ^2 (2) = 29.3, p <.001). Particularly, scrutiny of the odds ratios indicated that compared to the '*Interest-free*' offers; the odds of rejecting (over accepting) was 10.53 times (Wald χ^2 (1) = 17.87, p <.001) higher upon receipts of *50%-Interest* offer, and the odds of rejecting was also 8.13 times higher (Wald χ^2 (1) = 11.63, p <.001) when participants received the *25%-Interest* offers. Meanwhile, the odds of rejection were 2.35 times higher (Wald χ^2 (1) = 11.16, p <.001) when participants received a *50%-* instead of a *25%-interest* offer.

Secondly, the log-linear analysis showed that the model which retained only the two-way interactions (e.g. *Experiments* * *Acceptance* and *Conditionality* * *Acceptance*) would fit the data best, Likelihood Ratio χ^2 (3) =

7.88, p = .056. The significant *Conditionality* * *Acceptance* interaction (partial χ^2 (3) = 37.5, p <.001) echoes the above GEE analysis which highlights that rejections would hinge on *conditionality*. More importantly, the significant *Experiments** *Acceptance* interaction (partial χ^2 (1) = 39.8, p <.001) shed light upon the noticeable difference between offer rejections in Experiment 2 (i.e. 48/135 = 35.6%) and Experiment 3 (i.e. 95/ 798 = 11.9%). This shows that the availability of an alternative (and more cost-effective) punishing means in Experiment 3 has significantly diminished offer rejections, thus hinting that negative reciprocity may have underlain the high rejections in Experiment 2.

b) Testing the Dispositional Indebtedness-driven Rejection Hypothesis

Table 5.9 Effect of Trait Indebtedness on rejection (by Conditionality)

Average Indebtedness Scale-Revised (IS-R) Scores (by Offer Acceptances)							
Receipt	Conditionality	Accept		Reject		*Mann-Whitney U*	
		N	*Mean (SD)*	*N*	*Mean (SD)*	*N*	*U*
Initial	*Unconditional*	129	4.89 (0.95)	4	5.22 (1.25)	No Analyses Done[a]	
Second	*Unconditional*	130	4.89 (0.94)	3	5.42 (1.44)	No Analyses Done[a]	
Initial	*Interest-Free*	130	4.92 (0.95)	3	4.36 (1.07)	No Analyses Done[a]	
Second	*Interest-Free*	128	4.92 (0.94)	5	4.71 (1.41)	No Analyses Done[a]	
Initial	*25%-Interest*	112	4.95 (0.91)	21	4.68 (1.13)	133	1012.0
Second	*25%-Interest*	120	4.96 (0.97)	13	4.42 (0.66)	133	520.5*
Initial	*50%-Interest*	107	4.95 (0.94)	26	4.72 (0.99)	133	1213.5
Second	*50%-Interest*	106	4.96 (0.96)	27	4.69 (0.90)	133	1203.0

Note. [a] No analysis was done owing to insufficient counts of rejectors. * p <. 05 (2-tailed)

The present data (See Table 5.9) sternly dispels the speculation that *dispositional* indebtedness was responsible for the observed irrational rejections in the current experiment. Rejectors overall appeared similarly *dispositionally* indebted compared to the acceptors (at almost all levels of conditionality). Nonetheless, it is worth noting that the current lop-sided acceptor-rejector distribution (See Table 5.9) may render the results questionable.

5.3.2.4. Conclusion

Predictably, the knowledge about the availability of an alternative avenue to retaliate clearly contributed to the lower rejections in Experiment 3. It was worth noting that the participants still displayed a higher propensity to shun

the highly 'taxed' offers (i.e. the 25%- and 50%-interest offers) despite knowing that they 1) were financially better off accepting that and, 2) could default as they wished. Such an economically irrational rejection, contrary to what was predicted in this chapter, appeared unrelated to *trait* indebtedness.

5.3.3. UAQ 4: Does being 'cheap-rode' prompt the victims to play fair?

The present norm-enforcement hypothesis (i.e. NEH hereafter) predicted that the 'cheap-rode' *conditional* helpers in T1 ($N = 30$) should, compared to those whose 'taxed' offers were *fully* repaid ($N = 24$) or utterly *not* repaid ($N = 29$), exhibit a greater pursuit of injunctive fairness eventually via making more *generous* offers in T10 or avoid under-repaying any accepted (conditional) offers. The following presents the analyses of the NEH.

5.3.3.1. Statistical Analyses

Here I examined two crucial outcome variables. They included a) categorical repayment of accepted *conditional* offers, and b) whether a more *generous* offer (See 5.2.4.1 for details) was made in T10.

To analyse the effect of repayment manipulation (Trial 1, i.e. fairness vs. free-ride vs. cheap-ride), while controlling for offer *conditionality* (i.e. Interest-free vs. 25%- vs. 50%-Interest), on participants' (N= 83) *categorical* repayment (i.e. repay *partially* (or cheap-ride) vs. *nothing* (or free-ride) vs. *fully* (or cooperate)) I once again resorted to the Multinomial Logit model (Hardin & Hilbe, 2003). The current model, which was based upon a *generalized logit* link function, was analysed with a *robust* variance estimator and an *exchangeable* correlation structure. This specific analysis was administered using SUDAAN (Bieler & Williams, 1997; Shah, 1998). Table 5.10 illustrates the results.

Meanwhile, the GEE analysis on whether partner repayment manipulation (in T1) predicted increased generosity in T10 (i.e. *Yes* or *No*) was conducted using SPSS v. 22. Both nominal regression analyses would exclude *non-helpers* and *unconditional* helpers (Total $N = 50$) from T1 as both parties were *unaffected* by the repayment manipulation. Furthermore, the analysis of categorical repayment only entailed cases in which participants *accepted* their *conditional* offers, as the current analysis primarily concerns how people repay their 'taxed' offers subject to how their own recipients repaid them previously

5.3.3.2. Results

a) Testing if being 'Cheap-rode' prompted more trustworthiness during the RTs

The overall model, which comprises *manipulation* (i.e. Conditions 'Fairness' vs. 'Free-ride' vs. 'Cheap-ride') as the predictor and *offer conditionality* (i.e. Interest-free vs. 25%- vs. 50%-Interest) as a covariate, is significant (Wald χ^2 (10) = 86.87, p <.001). Both variables were shown to have significant main effects (i.e. *manipulation*, Wald χ^2 (4) = 16.70, p =.002; offer *conditionality*, Wald χ^2 (4) = 36.63, p <.001) over *repayment* (i.e. repay *fully* vs. *partially* vs. *nothing*). There was, meanwhile, no evidence (i.e. Wald χ^2 (8) = 12.01, p =.15) that *manipulation* had interacted with *conditionality* to significantly influence repayment. In the following I detail the scrutiny of the main effect *manipulation*. Table 5.10 below summarises the findings.

Scrutiny of the odds ratios (See Table 5.10 below) revealed that, compared to helpers whose initial offers were *fully* repaid ($N = 24$, i.e. 'Fairness' condition), *partially* repaid helpers ($N = 30$, i.e. 'Cheap-riding' condition) were 4.84 times more likely (i.e. 95% CI (1.88, 12.48)) to also *partially* repay their own conditional offers than repaying *fully*. Meanwhile,

compared to helpers whose initial offers were *utterly* not repaid (i.e. 'Free-riding' condition, $N = 30$), 'cheap-rode' helpers were also 3.85 times (i.e. 1/0.26) more likely (i.e. 95% CI (1.03, 14.3)) to also *partially* repay than *not* repay their conditional offers. Nonetheless, the current data offered no indication that 'cheap-rode' helpers, compared to *fully repaid* or *'free-rode'* helpers, were more propelled to repay *fully* or to refrain from totally defaulting on their (accepted) *conditional* offers.

Table 5.10 Repayment Manipulation on Repayment (Conditionality as co-variate)

			Variable: *Manipulation*	
	Odds Ratio (ORs)	Intercept	*Fairness* vs. Free-riding (RC)	*Cheap-riding* vs. Free-riding (RC)
1. Reference Category (RC): - *Free-riding* (Manipulation) & - Repay *Partially* (Repayment)				
Variable: *Categorical* Repayment				
-Repay *All* vs. Repay *Partially* (Reference)	Odds Ratio	1.96*	3.65*	0.71
	95% LLOR	1.01	1.37	0.31
	95% ULOR	3.82	9.69	1.62
-Repay *Nothing* vs. Repay *Partially* (Reference)	Odds Ratio	0.35	0.58	0.26*
	95% LLOR	0.11	0.14	0.07
	95% ULOR	1.12	2.38	0.97
2. Reference Category (RC): - *Free-riding* (Manipulation) & - Repay *Nothing* (Repayment)				
Variable: *Categorical* Repayment				
-Repay *Nothing* vs. Repay *All* (Reference)	Odds Ratio	0.18*	0.18*	0.36
	95% LLOR	0.05	0.04	0.08
	95% ULOR	0.59	0.82	1.58
-Repay *Partially* vs. Repay *All* (Reference)	Odds Ratio	0.51*	0.29*	1.38
	95% LLOR	0.26	0.11	0.60
	95% ULOR	0.99	0.76	3.17
3. Reference Category (RC):			Variable: *Manipulation*	
- *Fairness* (Manipulation) & - Repay *All* (Repayment)	Odds Ratio (ORs)	Intercept	*Cheap-riding* vs. Fairness (RC)	*Free-riding* vs. Fairness (RC)
Variable: *Categorical* Repayment				
-Repay *Partially* vs. Repay *All* (Reference)	Odds Ratio	0.60	4.84*	3.50*
	95% LLOR	0.27	1.88	1.32
	95% ULOR	1.32	12.48	9.28
-Repay *Nothing* vs. Repay *All* (Reference)	Odds Ratio	0.60	2.02	5.58*
	95% LLOR	0.27	0.41	1.22
	95% ULOR	1.32	10.05	25.57
4. Reference Category (RC):			Variable: *Manipulation*	
- *Fairness* (Manipulation) & - Repay *Nothing* (Repayment)	Odds Ratio (ORs)	Intercept	*Cheap-riding* vs. Fairness (RC)	*Free-riding* vs. Fairness (RC)
Variable: *Categorical* Repayment				
-Repay *Partially* vs. Repay *Nothing* (Reference)	Odds Ratio	3.82	2.00	0.63
	95% LLOR	0.81	0.31	0.12
	95% ULOR	18.04	13.10	3.39
-Repay *All* vs. Repay *Nothing* (Reference)	Odds Ratio	6.16*	0.39	0.19
	95% LLOR	1.21	0.05	0.03
	95% ULOR	31.42	3.04	1.18

Note. LLOR: Lower Limit Odds Ratio; ULOR: Upper Limit Odds Ratio. * denotes that the 95% CIs did not overlap the null value (i.e. OR=1), implying statistical significance (Szumilas, 2010).

In short, the data revealed that instead of feeling more prompted to *cooperate* or refrain from *free-riding* (as hypothesised), participants who were subject to others' cheap-riding acts tended more to 'pay it forward' when opportunity beckons. Participants were simply mimicking the *cheap-riding* antics from their own recipients (T1) when it became their turns to decide how they would respond to a *conditional* offer (i.e. the 8 Recipient Trials). In other words, the norm enforcement hypothesis of cheap-riding was not supported.

b) Testing if being 'Cheap-rode' prompted more Generosity in Trial 10

The data revealed a significant effect of partner repayment in T1(Wald χ^2 (2) = 5.99, p = .050) on whether a more generous offer was shown in T10. Specifically, scrutiny of the ORs indicated that, compared to a '*free-rode*' helper, a 'cheap-rode' helper was 4.78 times more likely (Wald χ^2 (1) = 5.75, p = .017) to showcase more generosity in T10. There was, however, no evidence (Wald χ^2 (1) = 0.19, p = .67) that the odds of giving a more generous offer in T10 were different among the *fully-repaid* and 'cheap-rode' conditional helpers.

5.3.3.3. Conclusion

The present data offered minimal endorsement to the norm-enforcement hypothesis—which predicted that experience of being 'cheap-rode' should inspire more eventual fair (or generous) gestures from the victims. Particularly, the analyses on the *categorical* repayment decisions (See Table 5.10) hinted that the manner through which a conditional helper was repaid had established an unofficial guideline for that helper to follow through when he/she was to repay his/her own taxed offers. Furthermore, analyses on whether the experience of being 'cheap-rode' had contributed to more generosity in Trial 10 (i.e. second Helper Trial) also revealed inconclusive findings. While 'cheap-

rode' helpers were more tempted to showcase more generosity than 'free-rode' helpers, there was no evidence that these 'cheap-rode' helpers were more inclined to be generous compared to the *fully repaid* helpers. All in all, the present analyses overall fails to confirm the notion that *cheap-riding* could help safeguard normative fairness in a fashion that is relatable to altruistic punishment.

5.3.4. General Confounds

This sub-section explores the effects of two general confounds, namely 1) Gender, and 2) Social Desirability Responding (SDR), on the key variables.

5.3.4.1. Effect of Gender

The below presents the analyses on the potential role of gender in participants' decision-making in the two *Helper* Trials (i.e. HTs, Trial 1 and 10) and the eight *Recipient* Trials (i.e. RTs, Trial 2 to 9) in between.

a) Statistical Analyses

I examined several crucial decisions in both HTs. They included a) decisions to help or not; b) conditionality of offers made (i.e. *unconditional*, *interest-free*, *25%* or *50%*-Interest) ; c) *transfer* magnitude and d) whether or not a more *generous* offer was made in T10 among *conditional* helpers in T1 (N = 83). Meanwhile, scrutiny of gender effect on decisions in the eight Recipient Trials entails the following: i) offer *acceptance* (or rejection); ii) *categorical* repayment (i.e. *partial*, *zero* or *full* repayment); and iii) *numerical* repayment (i.e. repayment amount and *under-repayment* rate (UR %)).

Unless otherwise specified, the GEE method—with an *exchangeable* correlation structure—was employed to examine if gender significantly influenced any of the decision variables. Particularly, I analysed the effect of

gender, while including *trial order* (i.e. T1 vs. T10) as a covariate, on the conditionality of offers made—which is a nominal *multinomial* variable—via SUDAAN. I also adopted SUDAAN (Bieler & Williams, 1997; Shah, 1998) when examining whether gender affected participants' *categorical* repayment (while including *repayment terms* as a covariate). These two models—both based upon a *generalized logit* link function—were analysed with a *robust* variance estimator and an *exchangeable* working correlation structure.

b) Results

bi.) *Gender* and Decision-making in the two Helper Trials (Trials 1 and 10)

First, the analyses revealed no signs that gender had a) on its own (Wald χ^2 (1) = 1.23, p = .27), or b) interacted with *trial order* (i.e. T1 vs. T10) (Wald χ^2 (1) = 1.75, p = .19), to affect decisions of whether to help. Gender was also found *not* to have independently (Wald χ^2 (1) = 0.11, p = .74), nor interacted with *trial order* (Wald χ^2 (1) = 2.00, p = .16) to influence *transfer magnitude* should a participant agreed to help. Additionally, gender was not instrumental (Wald χ^2 (1) = 0.027, p = .87) in predicting whether a *conditional* helper (N = 83) in T1 be more generous in T10.

Meanwhile, the multinomial analyses revealed that gender had neither on its own (Wald χ^2 (1) = 0.03, p = .87), nor interacted with *trial order* (Wald χ^2 (3) = 5.55, p = .14), to affect *helpers'* preferred repayment clauses (i.e. No repayment needed vs. Zero- vs. 25%- vs. 50%-Interest) to attach to their offers.

bii.) *Gender* and Decision-making in the Recipient Trials (i.e. T2 to T9)

The gender effect on offer acceptance was qualified by a significant *Gender* Conditionality* (i.e. unconditional vs. interest-free vs. 25% vs. 50%) interaction (Wald χ^2 (3) = 12.51, p = .006). While gender (Wald χ^2 (1) = 1.74, p

= .187) did not predict more acceptances, the effect of *conditionality* remained significant (Wald χ^2 (3) = 38.4, $p < .001$). Breaking down the interaction, there was no evidence (all *p*s >.05) that the odds of female (over male) rejection (instead of acceptances) differed across levels of *conditionality*.

Analyses over instances of *conditional* offer acceptances indicated neither *direct* (Wald χ^2 (1) = 0.28, $p = .60$) nor *indirect* (interaction with conditionality) (Wald χ^2 (1) = 0.91, $p = .64$) effects of gender on under-repayment rate. Lastly, multinomial analyses revealed no signs that gender had a) on its own (Wald χ^2 (2) = 3.09, $p = .21$), or b) interacted with *conditionality* (i.e. Unconditional, Interest-free, 25%, 50%-Interest) (Wald χ^2 (6) = 3.05, $p = .80$), to affect repayment (i.e. repay *partially* vs. repay *nothing* vs. repay *fully*).

c) Conclusion

All in all, the current data indicated a near total absence of gender effect on the list of crucial behavioural variables in Experiment 3. In other words, participants did not, in this context, show glimpses of varying trustfulness (as a helper) or trustworthiness (as a recipient) subject to gender.

5.3.4.2. Effect of Social Desirability Responding (i.e. SDR hereafter)

In the below sub-section I detail the analyses on the role of SDR may play in participants' decision-making throughout Experiment 3.

a) Statistical Analyses

Similar to the above gender effect analyses, here I examined how SDR would relate to the same set of decision variables (e.g. whether to help; offer acceptances etc.) in Experiment 3. As addressed in Section 5.2.4.3 at present I

operationally defined participants' SDR as their average composite ratings in Paulhus's (1991) Impression Management Scale (i.e. IMS hereafter).

b) Results

bi.) *SDR* and Decision-making in the two Helper Trials (Trials 1 and 10)

Table 5.11 Effect of SDR on Decision Making over the two Helper Trials

Average Impression Management Scale (i.e. IMS, Paulhus, 1991) Scores[a]								
1. To Help or Not to Help?	*N*	Mean	*SD*	*df*	*t*	*MTr* [b]	*SD*	*ρ*
1.1. Helper Trial 1 (Trial 1)								
- Helper	95	3.960	0.816			—	—	—
- Non-Helper	38	3.963	0.959			—	—	—
Overall	133	3.961	0.855	131	0.02	—	—	—
1.2. Helper Trial 2 (Trial 10)								
- Helper	104	3.995	0.792			—	—	—
- Non-Helper	29	3.837	1.059			—	—	—
Overall	133	3.961	0.855	37.2	0.75	—	—	—

2. Change in Helping Decisions				Kruskal- Wallis Test		
(T1 vs. T10)	*N*	*Mean*	*SD*	*df*	Chi-square	*p*
- Helpers in both T1 and T10	81	4.037	0.789			
- Helpers (T1) turned Non-Helpers (T10)	14	3.515	0.852			
- Non-Helpers (T1) turned Helpers (T10)	23	3.849	0.800			
- Non-Helpers in both T1 and T10	15	4.137	1.170			
Overall	133	3.961	0.855	3	3.290	.349

3. Conditionality of Help								
3.1. Helper Trial 1 (Trial 1)	*N*	Mean	*SD*	*df*	*F*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	12	4.693[12]	0.222			106.7	16.14	.361
- Conditional (CN)								
- Interest-Free (CNIF)	49	3.860[1]	0.100			105.6	15.53	.243
- 25%-Interest (CN25)	24	3.825[2]	0.170			105.7	13.76	.298
- 50%-Interest (CN50)	10	3.895	0.320			100.0	0.00	N.A.[c]
Overall	95	3.960	0.837	3,91	4.08**	—	—	—
	N	Mean	*SD*	*df*	*t*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	12	4.693	0.770			106.7	16.14	.361
- Conditional (CN) Total	83	3.854	0.770			105.0	14.06	.232*
Overall	133	3.961	0.855	93	3.53***	105.2	14.26	.240*

3.2. Helper Trial 2 (Trial 10)	*N*	Mean	*SD*	*df*	*F*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	20	4.253	0.783			110.0	18.85	.328
- Conditional (CN)								
- Interest-Free (CNIF)	54	4.033	0.825			107.4	17.26	.056
- 25%-Interest (CN25)	21	3.797	0.579			106.0	15.62	.099
- 50%-Interest (CN50)	9	3.661	0.932			100.0	0.00	N.A.[c]
Overall	104	3.995	0.792	3,100	1.76	—	—	—
	N	Mean	*SD*	*df*	*t*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	20	4.253	0.783			110.0	18.85	.328
- Conditional (CN) Total	84	3.934	0.786			106.3	15.94	.067
Overall	104	3.995	0.792	102	1.63	107.0	16.51	.140

Note. [1.] Tamhane's T2 indicated a significant difference (MD: 0.83, $p = .021$) in IMS of UC and CNIF givers. [2.] Tamhane's T2 indicated a significant difference (MD: 0.87, $p = .029$) in IMS between UC and CN25 givers. * $p < .05$ (2-sided); ** $p < .01$ (2-sided); *** $p < .001$ (2-sided).
ρ = Spearman's Rho (2-tailed) on MTr and IMS. [a] 7-point Likert Scales (1-7), higher scores signal a greater propensity to maintain impression. [b] Mean Transfer Magnitude (in Bonus Points).
[c] No analysis was done owing to zero variance in CN50 givers' transfer magnitude

As illustrated in Table 5.11 above, overall there were merely sporadic signs that SDR was related to participants' decisions over the two Helper Trials.

For instance, Spearman's correlation showed that Impression Management Scale score was positively—albeit weakly—associated with, a) (all) helpers' (*N* = 95) transfer magnitude (r_s (93) = .24, *p* = .019); and b) *conditional* helpers' (*N* = 83) transfer magnitude (r_s (81) = .23, *p* = .035) in T1. However, both correlations ceased to exist (all *p*s >.05) in Trial 10 (See Table 5.11).

bii.) *SDR* and Decision-making in the Recipient Trials (i.e. T2 to T9)

Table 5.12 a. Effect of SDR on Offer Acceptance (by Conditionality)

Average Impression Management Scale (IMS) Scores (by Offer Acceptances)							
Receipt	Conditionality	Accept		Reject		*Mann-Whitney U*	
		N	*Mean (SD)*	*N*	*Mean (SD)*	*N*	*U*
Initial	*Unconditional*	129	3.94 (0.85)	4	4.59 (0.67)	No Analyses Done[a]	
Second	*Unconditional*	130	3.94 (0.85)	3	4.77 (0.69)	No Analyses Done[a]	
Initial	*Interest-Free*	130	3.95 (0.86)	3	4.23 (0.94)	No Analyses Done[a]	
Second	*Interest-Free*	128	3.95 (0.85)	5	4.19 (1.00)	No Analyses Done[a]	
Initial	*25%-Interest*	112	3.97 (0.81)	21	3.91 (1.09)	133	1085.00
Second	*25%-Interest*	120	3.93 (0.84)	13	4.22 (1.01)	133	648.00
Initial	*50%-Interest*	107	3.91 (0.82)	26	4.15 (0.97)	133	1230.00
Second	*50%-Interest*	106	3.91 (0.83)	27	4.17 (0.92)	133	1211.00

Note. [a] No analysis was done owing to insufficient counts of rejectors.

As shown in Table 5.12a, the present data provided no substances to the speculation that SDR prompted more offer acceptances (or rejections). The similar conclusion applied to acceptors' repayment decisions in that (See Table 5.12b) IMS overall failed to differentiate acceptors who repaid *fully*, *partially*, and *nothing at all*, after they accepted the offers. SDR was not correlated (*p*s >.05) to almost all under-repayment rates of offers of different conditionality.

Table 5.12b Effect of SDR on Repayment Decisions (by Conditionality)

Average Impression Management Scale (IMS) Scores (by Repayment)										
Rec	Cond	Zero Repayment		Partial Repayment		Full Repayment				
		N	*Mean (SD)*	*N*	*Mean (SD)*	*N*	*Mean (SD)*	*F-*	*UR%*	*r*
1st	*UC*	31	3.78 (0.98)	62	4.07 (0.79)	36	3.86 (0.85)	1.441	-	-
2nd	*UC*	39	3.88 (1.00)	50	3.97 (0.64)	41	3.97 (0.94)	0.141	-	-
1st	*I-F*	7	3.78 (1.22)	31	3.85 (0.73)	92	4.00 (0.87)	1.116[a]	20.81	-.068
2nd	*I-F*	8	3.41 (1.45)	29	4.06 (0.74)	91	3.97 (0.81)	2.037[a]	18.56	-.103
1st	*25%*	8	3.48 (1.19)	31	3.70 (0.71)[b]	73	4.14 (0.76)[b]	8.592[b]	22.49	-.215*
2nd	*25%*	19	3.94 (1.18)	46	3.79 (0.75)	55	4.05 (0.76)	3.129[a]	32.38	-.036
1st	*50%*	12	3.57 (1.07)	56	3.88 (0.83)	39	4.06 (0.72)	1.760	40.38	-.178
2nd	*50%*	16	3.62 (1.14)	54	3.91 (0.78)	36	4.04 (0.75)	1.446	38.45	-.140

Note. UC: Unconditional; I-F: 'Interest-Free'; 25%: '25%-Interest'; 50%: '50%-Interest'. Rec: Receipt; 1st: Initial Receipt; 2nd: Second Receipt. UR%= Under-repayment rate. UR% was not calculated for every receipt of unconditional offer. [a] Kruskal-Wallis was conducted and the results (Chi-Square) were not significant (p >.1) (two-tailed). [b] Kruskal-Wallis was run and the results were significant, χ^2 (2) = 8.59, *p* = .014. Pairwise comparison illustrated a significant difference in IMS between cheap-riders (M: 3.70) and cooperators (M: 4.14). *r* = Pearson's *r* (two-tailed) on under-repayment rates and IMS.
* IMS was negatively correlated to UR% for the initial receipt of 25%- offer (*r* (110) = -.215, *p* = .023).

c) Conclusion

While the present analyses indicated sporadic signs (e.g. associations between transfer magnitude and IMS) that SDR was related to participants' prosociality, overall the results did not suggest that SDR had consistently heightened both participants' trustfulness (i.e. in both HTs) and trustworthiness (i.e. in T2 to T9). As such, the present analyses should speak volumes about the minimal interference of SDR with the present major decision variables.

5.4. Results- Gratitude, Indebtedness and Decision Making

In this sub-section I present analyses tackling the unanswered questions (UAQs) or research themes that involved *state* and *trait* gratitude (and indebtedness) and decision making in Experiment 3.

5.4.1. UAQ 1: Does more unfairness really 'not matter'?

To tackle UAQ 1 the present analysis tested two competing hypotheses on how transparency of helpers' available options would influence recipients' offer appraisals. The Inelastic Fairness Construal (i.e. IFC) argument—which predicts a dichotomous 'fair versus unfair' contrast in intent and emotions among the *unconditional* and the *conditional* offers (i.e. more (or less) unfairness would *not* count); and Wood et al.'s (2011) Gratitude Relativity Hypothesis (i.e. GRH) —which proposes a *graded, linear* fairness appraisals of offers of varying repayment obligations (i.e. more unfairness matters).

5.4.1.1. Statistical Analysis

I adopted a 2 (Offer Receipts: *Initial* vs. *Second*) X 4 (Conditionalities: *Unconditional* vs. *Interest-Free* vs. *25%*-Interest vs. *50%*-Interest) *repeated* ANOVA on all the pre-repayment rating items. Two-tailed Bonferroni analyses were conducted to further examine any significant main effects or interactions.

5.4.1.2. Results

a) Manipulation Check: Perceived Inadequate Generosity

In each recipient trial the item '*My partner could have been more generous to m*e' was included to examine if participants were aware of the varying extent of injunctive fairness (or lack thereof) of each offer they received. The results revealed *only* a main effect of *Conditionality* (F (2.07, 273) = 131.7, p <.001). Importantly, the Bonferroni post-hoc analyses revealed

a significant *linear* trend, F (1,132) = 212, p <. 001, which illustrated that perceived inadequacy generosity was *highest* among receipts of *50%-interest* offer (*M:* 5.12; *SD:* 0.15) and *lowest* upon receipts of *unconditional* offers (*M*: 2.17, *SD*: 0.13) (See Table 5.13 and 5.14). Therefore, the present conditionality manipulation succeeded in leading recipients to acknowledge the varying extent of generosity (implying different injunctive fairness) across offers.

Table 5.13 Effect of Conditionality on Pre-repayment Emotional Ratings

	Level of Conditionality				
	Unconditional	I-F	25%	50%	F-values
	M (SE)	*M (SE)*	*M (SE)*	*M (SE)*	
Inadequate Generosity	2.17 (.13)	3.66 (.16)	4.44 (.13)	5.12 (.15)	131.7***
State Indebtedness	5.37 (.13)	4.79 (.15)	4.03 (.15)	3.60 (.17)	63.1***
State Gratitude	6.12 (.10)	5.16 (.12)	4.28 (.13)	3.64 (.15)	145.9***
Annoyance	1.67 (.10)	2.27 (.13)	3.09 (.14)	4.02 (.17)	90.35***
Obligations (to repay)	4.98 (.16)	5.29 (.14)	4.80 (.14)	4.17 (.16)	20.77***
Eagerness to Reciprocate	5.91 (.10)	5.25 (.12)	4.51 (.13)	3.98 (.15)	109.5***
Perspective-Taking	4.06 (.15)	4.97 (.15)	3.82 (.14)	3.02 (.14)	39.16***
Benefit Appraisal	5.66 (.10)	4.68 (.11)	4.04 (.10)	3.52 (.12)	167.3***

Note. *** p <.001 (2-tailed); 'I-F': Interest-Free; '25%': 25%-Interest Offer; '50%': 50%-Interest Offer

Table 5.14 Bonferroni Post-hoc analyses of the Main Effects in Table 5.13

	Bonferroni Post-hoc Comparisons (Two-tailed)					
	UC vs. I-F	UC vs. 25%	UC vs. 50%	I-F vs. 25%	I-F vs. 50%	25% vs. 50%
	MD (SE)	*MD (SE)*	*MD (SE)*	*MD (SE)*	*MD (SE)*	*MD (SE)*
IG	1.50 (.17)***	2.27 (.18)***	2.95 (.20)***	0.78 (.12)***	1.46 (.15)***	0.68 (.10)***
Is	0.58 (.13)***	1.34 (.15)***	1.77 (.17)***	0.75 (.12)***	1.19 (.14)***	0.44 (.13)**
Gs	0.97 (.10)***	1.84 (.14)***	2.49 (.16)***	0.88 (.11)***	1.52 (.14)***	0.64 (.11)***
As	0.60 (.13)***	1.42 (.15)***	2.35 (.19)***	0.82 (.14)***	1.75 (.17)***	0.93 (.14)***
Os	0.31 (.14)	0.18 (.16)	0.81 (.19)***	0.50 (.10)***	1.12 (.14)***	0.63 (.13)***
RC	0.65 (.10)***	1.40 (.12)***	1.93 (.15)***	0.74 (.09)***	1.27 (.12)***	0.53 (.09)***
PT	0.91 (.19)***	0.24 (.21)	1.04 (.19)***	1.15 (.17)***	1.95 (.18)***	0.80 (.14)***
BA	0.98 (.09)***	1.62 (.11)***	2.14 (.13)***	0.64 (.08)***	1.16 (.11)***	0.52 (.08)***

Note. IG: Inadequate Generosity; Is: State Indebtedness; Gs: State Gratitude; As: State Annoyance; Os: Obligation to Repay; RC: Eagerness to Reciprocate; PT: Perspective Taking; BA: Benefit Appraisal; *MD*: (Absolute) Mean Differences; *SE*: Standard Errors; ** p <.01 (2-tailed); *** p <.001 (2-tailed).

b) The rest of pre-repayment ratings (e.g. state gratitude, indebtedness etc.)

The results (See Table 5.14) revealed significant main effects of *Conditionality* (all *p*s <.001) on all pre-repayment ratings. Breaking down these main effects, post-hoc analyses (See Table 5.14) overall revealed that the *higher* the *expected* repayment the *lower* the positive affective responses (e.g. feeling less affective gratitude). Recipients also felt more *annoyed* by; less *obligated* to repay, and interestingly, less *indebted* to helpers who demanded *more* back-transfer. Recipients' disapproval of more unfair offers was also

evidenced by their *graded* responses (See Table 5.13) on item ‘I would have done the same thing were I in my partner’s shoes’ (i.e. Perspective-Taking).

5.4.1.3. Conclusion

Instead of feeling similarly unimpressed with a more blatant deviation from normative fairness, the current data sternly refuted the IFC as recipients demonstrated a *graded* appraisal (i.e. emotions and intent) with respect to the varying fairness connoted across offers. This suggests that injunctive fairness does not function in an ‘all-or-nothing’ fashion as *relativity* matters. I interpret this as an example of recipients computing the worth of their offers *relative* to what they *could have* got, thanks to their newfound access to helpers’ behavioural alternatives in Experiment 3. In short, the GRH was confirmed.

5.4.2. Recurring Research Theme 1: Does gratitude, as a *state* or a *trait*, nurture prosociality (or undermine antisociality) in economic games?

In this sub-section I examine how *state* gratitude would on its own (i.e. the *Moral Barometer* and *Moral Motive* Hypotheses of Gratitude, McCullough et al., 2001), or in conjunction with *trait* gratitude (i.e. the Social Cognitive Model of Gratitude; Wood et al., 2008) in preserving cooperation (or undermining sanctioning) in the present experiment.

5.4.2.1. Moral Barometer and Moral Motive Hypotheses of Gratitude

Both Moral Hypotheses of Gratitude (McCullough et al., 2001) would dismiss *partial* or *complete* under-repayment among any grateful *conditional* offer *acceptors*. As such, a sharp contrast between *state* gratitude of *cooperators* and that of both *cheap*- and *free*-riders should be expected.

a) Statistical Analyses

A generalised estimating equation (i.e. GEE) method with an *exchangeable* correlation structure was used to analyse the data. I excluded participants' (N = 133) responses in their two RTs in which they received *unconditional* offers. I also excluded the 95 instances of rejections as this analysis focused only on how *state* gratitude relates to repayment preferences of the *acceptors*. In summary, here I examined how participants' categorical repayment (i.e. *cooperate*, *cheap-ride* or *free-ride*) would be related to their *state* gratitude in each of their six receipts of *conditional* offers (total receipts = 133* 6 = 798). Bonferroni pairwise comparisons were administered to further break down how this relationship may differ across each level of repayment. Additionally, I ran a hierarchical linear regression inspecting how *state* gratitude would predict under-repayment rate (i.e. UR %), with *expected* repayment and order of receipts (i.e. *initial* or *subsequent*) being the *first* and *second* variables entered prior to *state* gratitude.

b) Results

The results of GEE indicated a significant difference (Wald χ^2 (2) = 38.54, p <.001) on the *state* gratitude (toward helpers) rating that preceded *cheap*-riding, *cooperation* and *free*-riding. Post-hoc analyses illustrated that instances of *cooperation* (N = 386; M: 5.01; SE: 0.12) were preceded by higher *state* gratitude (both ps <.001) than both *cheap*-riding (N = 247; M: 4.22; SE: 0.16) and *free*-riding (N = 70; M: 3.49; SE: 0.31). There was marginal (p = .057) evidence suggesting that *cheap*-riding were preceded by higher *state* gratitude (toward the offers) than *free*-riding.

The regression analysis gave substance to the argument that *state* gratitude should restrain under-repayment (i.e. Moral Motive Hypothesis). Beta

coefficients for the three predictors were *expected* repayment, β = .159, t = 4.19, p < .001; receipts, β = .021, t = 0.58, n.s.; and *state* gratitude, β = -.159, t = -5.01, p < .001. The best fitting model for predicting under-repayment rate is a linear combination of expected repayment and *state* gratitude (R = .280, R^2 = .076, F (2,700) = 29.9, p < .001). The data suggested that a) the *higher* the *stipulated repayment* the *greater* extent the acceptors would under-repay; and b) the more *gratitude* the acceptors felt the less prone they were to under-repay at full throttle. Additionally, a simple linear regression analysis showed that the magnitude of *voluntary* repayment upon *unconditional* offer acceptances (N= 259) could be predicted by *state* gratitude, β = .130, t (257) = 2.11, p = .036.

c) Conclusion

Overall the analyses were mostly in support of the Moral Barometer and Moral Motive Hypotheses of Gratitude (McCullough et al., 2001). Specifically, results of the GEE highlighted that cooperators consistently experienced more *state* gratitude toward the offers than both *cheap*- and *free*-riders In addition, results of the hierarchical regression demonstrated that *state* gratitude *inversely* predicted one's extent of under-repayment (while holding *expected* repayment constant). Thus, consistent with the Moral Motive Hypothesis which argues that *state* gratitude should restrain intent to undermine benefactors' well-being, at present *state* gratitude seemed to put a handbrake on how people defaulted.

Nonetheless, it is worth noting that although instances of defaulting (*cheap*- and *free*-riding inclusive) were preceded by *lower gratitude* than were cooperating, cheap-riders still on average reported a non-negligible extent of *gratitude* (*M*: 4.22; *SE*: 0.16). Thus, contrary to my prediction (See Table 5.1) the experience of *gratitude* still would not entirely eradicate defaulting.

5.4.2.2. Social Cognitive Model of Gratitude (i.e. SCMG) (Wood et al. 2008)

While the above addresses the *direct* effect of *state* gratitude over defaulting, in this sub-section I examined how the *interplay* between *dispositional* and *state* gratitude, as theorised in the SCMG, would contribute to people's decisions to under-repay in Experiment 3.

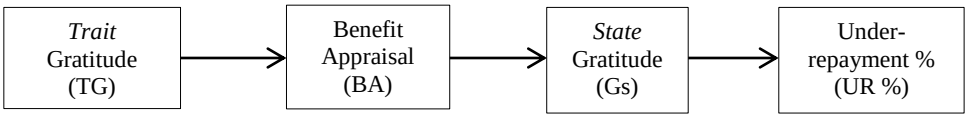

Figure 5.9. The SCMG (Wood et al., 2008) Prediction on Defaulting

a) Statistical Analyses

I analysed the path model in Figure 5.9 via Hayes's (2013) PROCESS using the Bootstrap Confidence Intervals (CI) approach (5000 Bootstrap samples). Participants' individual receipt of their six *conditional* offers was separately analysed. I subjected the model (Figure 5.9) to the analyses which focus on *under-repayment rate*, composite *benefit appraisal* and *state* gratitude for each receipt of offer of varying conditionality (i.e. Interest-free, 25% and 50%-Interest). The current analyses only included the 703 instances (i.e. *N*= 703) in which a *conditional* offer was *accepted*. Table 5.15 details the results.

b) Results and Conclusion

Table 5.15 Regression Coefficients, Standard Errors (*SE*s), and Model Summary Information for the Path Model (Figure 5.9) under scrutiny

Indirect Effect of Trait Gratitude on UR% (serially mediated by Benefit Appraisal then State Gratitude)							
Receipt	Con	*N*	Co-efficient	Bootstrap SE	Bootstrap LLCI	Bootstrap ULCI	Significant at p <.05?
Initial	*I-F*	130	-0.0453	0.7394	-1.7596	1.3332	No
Second	*I-F*	128	-0.9404	1.0162	-3.5422	0.6298	No
Initial	*25%*	112	-0.6537	0.7317	-3.1263	0.2293	No
Second	*25%*	120	-0.9614	1.0144	-4.4140	0.1465	No
Initial	*50%*	107	0.0540	0.7206	-1.0530	2.1998	No
Second	*50%*	106	-0.2700	1.2000	-3.1236	1.7962	No

Note. N = Number of offer *acceptors;* Con: Conditionality; 'UC': Unconditional; 'I-F': Interest-Free; '25%': 25%-Interest; '50%': 50%-Interest.; Boots LLCI (ULCI): Bootstrap CIs Lower (Upper) Limit

The results (See Table 5.15) revealed that the hypothesised total indirect effect (i.e. *Trait* Gratitude→ Benefit Appraisal→ *State* Gratitude→UR %) was

non-significant at every *conditional* offer receipt. This statistical non-significance was represented by the CIs which include zero at 95% levels of confidence (See Table 5.15). Hence, similar to Experiment 2, there was again no evidence that *state* gratitude (along with composite benefit appraisal) would mediate the path between *trait* gratitude and prosociality.

5.4.3. Recurring Research Theme 2: Does indebtedness, as a *state* or a *trait*, nurture prosociality (or undermine antisociality) in economic games?

This sub-section details the analyses on hypotheses (e.g. Cognitive Restructuring Hypothesis) in relation to how indebtedness, as a *state* or a *trait*, affected participants' cooperativeness or the lack thereof in Experiment 3.

5.4.3.1. Does *state* indebtedness promote or suppress trustworthiness?

As summarised in Table 5.1 the present chapter entails scrutiny over two competing predictions on how *state* indebtedness would influence participants' decisions to under-repay or repay fully. The *Indebtedness-driven Reciprocity* Argument—which argues an *inverse* relationship between *state* indebtedness and under-repayment, and the *Cognitive Restructuring Hypothesi*s (i.e. CRH) (Greenberg & Westcott, 1983)—which argues that indebtedness should *indirectly* promote under-repayment via more cynical benefit appraisals.

a) Statistical Analyses

To test the *Indebtedness-driven Reciprocity* argument I adopted a GEE with an *exchangeable* correlation structure. I examined how participants' *categorical* repayment (i.e. *cooperate*, *cheap*- or *free*-ride) decisions related to their *state* indebtedness in each of their receipts of *conditional* offers (total receipts = 133* 6 = 798). Bonferroni pairwise analyses were used to delve into how this relationship may differ across each level of repayment. I also ran a

hierarchical linear regression testing how *state* indebtedness predicted under-repayment rate, with *expected* repayment and receipts (i.e. *initial* or *subsequent*) being the *first* and *second* variables entered prior to *state* indebtedness.

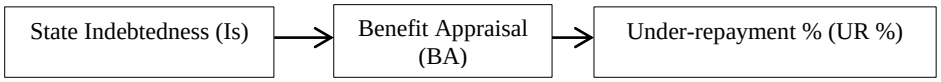

Figure 5.10 CRH's (Greenberg & Westcott, 1983) Prediction on Defaulting

To test the CRH I examined the path model (See Figure 5.10) using the PROCESS (Hayes, 2013) with 5000 Bootstrap samples. This analysis examined only the 703 instances (i.e. $N = 703$) in which a *conditional* offer was *accepted*, while controlling for variables *expected* repayment (i.e. Interest-Free, 25%-Interest and 50%-Interest) and offer receipts (*initial* or *second* receipts). The results are summarised in Table 5.16 and Figure 5.11.

b) Results

bi.) Testing if *State* Indebtedness would enhance *trustworthiness*

The results of GEE indicated a significant effect of repayment (Wald χ^2 (2) = 18.71, p <.001) on *state* indebtedness. Bonferroni analyses indicated that, similar to *state* gratitude (See Section 5.4.2.1), instances of *cooperation* were (N = 386; M: 4.64; SE: 0.15) preceded by higher *state* indebtedness (both ps <.05) than both *cheap*-riding (N = 247; M: 4.01; SE: 0.18) and *free*-riding (N = 70; M: 3.67; SE: 0.35). There was no evidence (p = .997) that *cheap*-riders and *free*-riders experienced different *state* indebtedness toward their helpers.

The regression analysis provided further confirmation of *Indebtedness-driven Reciprocity* argument. Beta coefficients for the three predictors were *expected* repayment, β = .186, t = 4.99, p < .001; offer receipts, β = .029, t = 0.81, n.s.; and *state* indebtedness, β = -.147, t = -3.95, p < .001. The best fitting model for predicting under-repayment rate is a linear combination of *expected* repayment,

and *state* indebtedness (R = .257, R^2 = .063, F (2,700) = 24.7, p < .001). The data suggested that a) the *higher* the *stipulated repayment* the *greater* extent of under-repayment would be; and b) the more indebtedness experienced by the acceptors the more they would give back. Additionally, a simple linear regression analysis revealed that the magnitude of voluntary repayment of *unconditional* offers (N= 259) significantly regressed on *state* indebtedness, β = .262, t (257) = 4.36, p < .001. Taken together, the current data confirmed the *Indebtedness-driven Reciprocity* argument as *state* indebtedness appeared to both preserve trustworthiness and foster voluntary back-transfer.

bii.) Would State Indebtedness lead to *increased* under-repayment via more cynical benefit appraisals (i.e. Cognitive Restructuring Hypothesis (CRH)))?

Table 5.16 Regression Coefficients, Standard Errors, and Model Summary Information for the CRH prediction on under-repayment rate (Figure 5.11)

		Consequent						
		M (Benefit Appraisal)				Y (Under-repayment %)		
Antecedents		*Coeff*	*SE*	*p*		*Coeff*	*SE*	*p*
X (*State* Indebtedness)	*a*	0.342	0.02	<.001	*c*	-1.354	0.81	.0940
M (Benefit Appraisal)		--	--	--	*b*	-4.122	1.21	<.001
Co1: Expected Repayment		-0.011	0.00	<.001		0.029	0.01	<.001
Co2: First or Second Receipt		-0.078	0.09	.3601		1.892	2.73	.4881
Constant	iM_1	4.319	0.32	<.001	iM_2	12.53	11.5	.2775
		R^2 = 0.3133				R^2 = 0.0821		
		F (3,699) = 106.3 , p <.0001				F (4,698) = 15.62 , p <.0001		

Note. Co 1 and Co2: Covariates 1 and 2

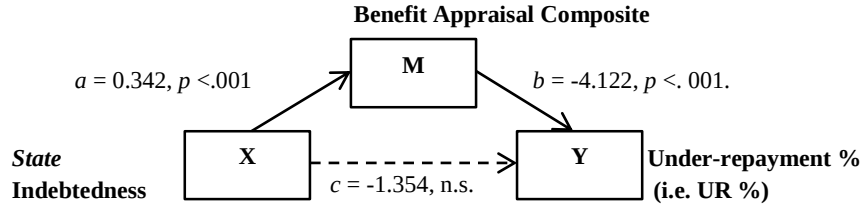

Figure 5.11 Statistical diagram of analysis over the CRH prediction on UR%

The data, as detailed in Table 5.16 and Figure 5.11, refuted the CRH as it reflected that *state* indebtedness *indirectly* affected under-repayment in an *opposite* direction as anticipated by the CRH. The bias-corrected bootstrap 95% CI for the present *indirect* effect (ab = 0.34*-4.12 = -1.41) did not include zero (-2.32 to -0.58), thus implying statistical significance. Acceptors feeling *more*

indebted were *more* inclined to think *positively* about their 'taxed' offers (a = 0.342, p <.001). Understandably, this positive evaluation will predict a lower under-repayment rate (*increased* trustworthiness) (b = -4.12, p <.001).

c) Conclusion

All in all, *state* indebtedness in Experiment 3 appeared to facilitate cooperative economic decisions in a similar fashion as *state* gratitude. Although the current experimental features (e.g. *double-blinded*, *partner-switching* design etc.) should in theory breed *cognitive restructuring* (Greenberg, 1980; Greenberg & Westcott, 1983), the observed positive link between *indebtedness* and *benefit appraisal* dispels the idea that people would try to escape from their feeling of indebtedness by negatively appraising their offers (which includes demonising one's helper, or belittling the value of the benefits that they were bestowed upon (Hatfield & Sprecher, 1983)). People, meanwhile, still considered *direct reciprocity* as the 'major mode of indebtedness reduction (pp.95)' (Greenberg & Westcott, 1983), even in a situation where the absence of the need to repay was very much emphasised (i.e. *unconditional* offer).

5.4.3.2. Does *trait* indebtedness promote or suppress cooperation?

This sub-section features the analyses on the two hypotheses regarding how *trait* indebtedness would influence participants' decisions in the helper trials (i.e. Indebtedness-driven Helping ('Helping Argument' hereafter)) and in the recipient trials (i.e. Indebtedness-driven Repayment ('Repayment Argument' hereafter)). Specifically, the 'Helping Argument' predicts that *dispositionally* indebted helpers will be more likely to help *unconditionally*. Meanwhile, the 'Repayment Argument' states that *dispositionally* indebted recipients will always refrain from partially or fully defaulting (See Table 5.1).

a) Statistical Analyses

To test the 'Helping Argument' I examined several outcome measures of the two helper trials (i.e. Trial 1 and 10). They were i) decisions to *help or not*; ii) whether (and how) participants *changed* their 'Help versus No Help' decisions in Trial 10; iii) *conditionality* of offers made; and the iv) transfer *magnitude*. The results are summarised in a nutshell in Table 5.17.

To test the 'Repayment Argument' I ran a hierarchical regression analysis examining how *trait* indebtedness would predict under-repayment rate, while controlling for both *expected* repayment (i.e. Interest-Free, 25%- and 50%-Interest) and offer receipts (i.e. *initial* or *subsequent receipts*). I also tested how *trait* indebtedness would relate to acceptors' categorical repayment (i.e. *cooperate*, *cheap*- or *free-ride*), while controlling for *expected* repayment, using the GEE method with an *exchangeable* correlation structure. In both the hierarchical regression and GEE I focused only on the 733 instances in which a *conditional* offer was *accepted* (N = 703).

I included two separate analyses on effects of *trait* indebtedness on participants' repayment in the two RTs where they received *unconditional* offers. I conducted a hierarchical regression examining how *trait* indebtedness predicts repayment magnitude while controlling for offer receipts (*initial* or *second*). I also adopted the GEE method to explore how *trait* indebtedness would relate to *unconditional* offer acceptors' *categorical* repayment, while controlling for offer receipts (i.e. *initial* or *subsequent* receipts).

b) Results

The analyses, as detailed in Table 5.17 below offered no indication that *trait* indebtedness had *any* significant *direct* influence on the various decisions

(e.g. to help or not to help; the magnitude of transfer; and conditionality etc.) in Trials 1 and 10 (all *p*s >. 05). Therefore, contrary to the *Indebtedness-driven Helping* argument, helpfulness (or trustfulness) in Experiment 3 appeared unrelated with indebtedness as a *disposition*.

Table 5.17 Effect of Trait Indebtedness on Trial 1 (T1) and Trial 10 (T10)

Average Indebtedness Scale-Revised (IS-R) Scores[a]								
1. To Help or Not to Help?	*N*	Mean	*SD*	*df*	*t*	*MTr* [b]	*SD*	*ρ*
1.3. Helper Trial 1 (Trial 1)								
- Helper	95	4.82	0.95			—	—	—
- Non-Helper	38	5.12	0.93			—	—	—
Overall	133	4.90	0.95	131	1.64	—	—	—
1.4. Helper Trial 2 (Trial 10)								
- Helper	104	4.85	0.91			—	—	—
- Non-Helper	29	5.10	1.09			—	—	—
Overall	133	4.90	0.95	131	1.25	—	—	—

2. Change in Helping Decisions (T1 vs. T10)	*N*	*Mean*	*SD*	Kruskal- Wallis Test *df*	Chi-square	*p*
- Helpers in both T1 and T10	81	4.76	0.91			
- Helpers (T1) turned Non-Helpers (T10)	14	5.14	1.15			
- Non-Helpers (T1) turned Helpers (T10)	23	5.15	0.86			
- Non-Helpers in both T1 and T10	15	5.06	1.06			
Overall	133	4.90	0.95	3	5.029	.170

3. Conditionality of Help								
3.1. Helper Trial 1 (Trial 1)	*N*	Mean	*SD*	*df*	*F*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	12	4.80	1.02			106.7	16.14	-.371
- Conditional (CN)								
- Interest-Free (CNIF)	49	4.82	0.93			105.6	15.53	.181
- 25%-Interest (CN25)	24	4.54	0.85			105.7	13.76	-.395
- 50%-Interest (CN50)	10	5.50	1.00			100.0	0.00	N.A.[c]
Overall	95	4.82	0.95	3,91	2.52	—	—	—
	N	Mean	*SD*	*df*	*t*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	12	4.80	1.02			106.7	16.14	-.371
- Conditional (CN) Total	83	4.82	0.95			105.0	14.06	-.050
Overall	95	4.82	0.95	93	0.06	105.2	14.26	-.091

3.2. Helper Trial 2 (Trial 10)	*N*	Mean	*SD*	*df*	*F*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	20	4.85	0.94			110.0	18.85	-.226
- Conditional (CN)								
- Interest-Free (CNIF)	54	4.76	0.89			107.4	17.26	.032
- 25%-Interest (CN25)	21	5.15	0.79			106.0	15.62	-.260
- 50%-Interest (CN50)	9	4.67	1.20			100.0	0.00	N.A.[c]
Overall	104	4.85	0.91	3,100	1.06	—	—	—
	N	Mean	*SD*	*df*	*t*	*MTr* [b]	*SD*	*ρ*
- Unconditional (UC)	20	4.85	0.94			110.0	18.85	-.226
- Conditional (CN) Total	84	4.85	0.91			106.3	15.94	.010
Overall	104	4.85	0.91	102	0.01	107.0	16.51	-.048

Note. * p <.05 (2-sided); ** p <.01 (2-sided); *** p <.001 (2-sided). [a] 7-point Likert Scales (1-7), higher scores signal greater overall dispositional indebtedness. [b] Mean Transfer Magnitude (in Bonus Points). [c] No analysis was done owing to zero variance in CN50 givers' transfer magnitude; ρ = Spearman's Rho (two-tailed) on MTr and Trait Indebtedness (given by IS-R).

Results of the hierarchical regression also offered minimal clear-cut evidence for the hypothesis that *dispositional* indebtedness undermines under-repayment, despite a trend is shown. Beta coefficients for the three predictors

were *expected* repayment, β = .213, t = 5.77, p < .001; offer receipts, β = .024, t = 0.65, p = .514; and *trait* indebtedness, β = .070, t = 1.91, p = .057. Crucially, the inclusion of *trait* indebtedness did not bring a significant improvement to the model (p = .057). Meanwhile, results of the GEE also revealed no differences (Wald χ^2 (2) = 2.11, p = .348) in *trait* indebtedness, after controlling for *expected* repayment, among cooperators (*M*: 4.92, *SE*: 0.08), *cheap*-riders (M: 4.92, *SE*: 0.08) and *free*-riders (*M*: 4.92, *SE*: 0.082). In short, the data did not confirm the present (Trait) Indebtedness-driven Repayment hypothesis.

Furthermore, the regression analysis examining how *trait* indebtedness predicts unconditional offer repayment also did not show any significant effect, β = -.015, t (256) = -0.25, p = .807. The results of the GEE also offered no evidence (Wald χ^2 (2) = 0.034, p = .983) that *trait* indebtedness differentiated the *categorica*l repayment decisions among *unconditional* offer takers.

c) Conclusion

Overall, the above analyses suggested a total *absence* of *direct* effects of *trait* indebtedness on participants' decision-making throughout Experiment 3. While at present I will not dismiss any potential *indirect* effects *trait* indebtedness may have on the present decision variables, it is, however, beyond the scope of the current thesis to examine issues as such.

5.5. Discussion

The key objective of Experiment 3 entails the scrutiny of a few unexplored domains—which were summarised by the four unanswered questions— in Experiments 1 and 2. While these UAQs predominantly concerned the *methodological* facets (i.e. UAQs 2 to 4), UAQ 1 touched on how *state* and *trait emotions—gratitude* and *indebtedness* in particular— would be related to *cooperativeness* and *sanctioning* within economic games. In so doing, I endeavour to render the present thesis a more all-round and robust investigation of the *gratitude* (or indebtedness)-*prosociality* association in the realm of *Behavioural Economics*.

In this section I discuss three key findings at present. They included, a) the fairness-enforcing aspect of *cheap-riding* (i.e. the norm-enforcement hypothesis, See Sub-section 5.5.1 below); b) the moderating effect of knowledge on recipients' fairness attributions (i.e. would the transparency of helpers' options inspire recipients to quit adopting an 'all-or-nothing' perspective when evaluating fairness? See Sub-section 5.5. 2), and c) how *gratitude* and *indebtedness* were related to the display of prosociality at present (i.e. See Sub-section 5.5.3). Sub-section 5.5.4 will conclude the discussion by reviewing some other findings (e.g. whether the single- or both-role design would explain the 'over-generosity' issue) in relation to the other methodological UAQs.

5.5.1. The Norm-enforcing Role of *Cheap-riding*

The comparison of repayment between Experiments 2 and 3 provided insights on how prevalent *cheap-riding* is resorted to may depend on whether it could serve a norm-enforcing function. Specifically, analyses of the odds ratios

showed that acceptors in a *one-off* setting (Experiment 2) were less likely to cheap-ride than acceptors in the present *iterated* context (See Table 5.7).

I argued throughout this thesis the principal motive for *cheap-riding*—in this particular context—entails conveying a 'fair but firm', 'cannot be exploited' image of an unfairly treated victim (Komorita et al., 1991; Komorita et al., 1992; Komorita & Parks, 1999), and in so doing the punisher attempts to curb further fairness infringement by that offender (Ma et al., 2014). This should capture the essence of the current *norm-enforcement hypothesis* (NEH).

Building on this logic, it makes sense that a participant would prefer *cheap-riding* in a context knowing that his/her 'perpetrator' may have another 'shot' to continue being unkind to other people —that is, a *repeated* game. Participants in Experiment 3 were explicitly told that it was a multiple-shot experiment, and that everyone would be partnered with a *different* co-player in each round. By contrast, in Experiment 2 participants were told it was a *one-shot* game and there were neither explicit instructions nor implications of *repeated* (or multiple) interactions and *partner-switching* throughout.

Taken together, Experiment 3 should constitute a better platform for *cheap-riding* that is intended to serve a norm-enforcing purpose. That is because participants knew that their 'perpetrators' (i.e. ungenerous helpers) would have at least another chance to subject others to unfair treatment. On the contrary, in Experiment 2 participants were under an impression that this was the *one* and *only* trial and the helpers had no room to continue their 'unfair' streak even if they wished to. Accordingly, a fairness-enforcing cheap-riding should make more sense in an iterated rather than in a one-shot context. Thus, the present data which showed that cheap-riding was less prevalent in

Experiment 2 than in Experiment 3 may symbolise the participants' capability of selecting the 'right' circumstance to optimise the norm-enforcing capacity of cheap-riding. I, therefore, interpret this as a *partial* confirmation of the NEH.

While the above suggests that people tended to *cheap-ride* more in a context in which doing so better serves its immediate purpose, here I particularly delve into how *efficacious* cheap-riding can be relied upon in implementing normative fairness. Overall, the results (See Sub-section 5.3.3, i.e. testing UAQ #4) indicated that *conditional* offer givers (N = 83) who had been '*cheap-rode*' would—compared to the '*free-rode*' givers— tend more to give a more generous offer in Trial 10; although the results showed no differences in likelihood for more generosity among the '*cheap-rode*' and *fully-repaid* givers.

In addition, cheap-rode' givers were more likely to also *partially* repay the *conditional* offers that they accepted during their six ensuing recipient trials. There were, however, *no* signs of them—compared to the 'fully repaid' and 'free-rode' givers—being more inclined to repay *fully* or refrain from free-riding (See Table 5.10). That is, being 'cheap-rode' did not incite more *trustworthiness* or a *less blatant breach* of normative fairness, but a 'tit-for-tat' pattern of *cheap-riding* directed to others—thus the NEH is *not fully* supported.

All in all, while the cross-study comparisons alluded to the notion that people prefer to *cheap-ride* more in a context in which fairness enforcement should be more feasible (i.e. the *iterated* game), analyses of the present repayment's manipulation revealed that being 'cheap-rode' did not incite more eventual fair plays from the victims. This could be understood as participants expecting that their decisions (i.e. cheap-riding) may have a certain

consequence (i.e.to prompt more eventual compliance of the injunctive fairness norm) which did not actually turn out to be the case.

5.5.2. Moderating Effect of Recipients' Knowledge about Helpers' Options on Fairness Attribution, Benefit Appraisal, and Experienced *Gratitude*

One key objective of Experiment 3 involves the analyses of whether the hinted 'threshold' mentality in fairness evaluation in Experiments 1 and 2 will be moderated by the recipients' awareness of helpers' options. Does more unfairness still not count (i.e. UAQ #1)? Or that *Relativity* should matter as theorised in the Gratitude Relativity Hypothesis (*GRH*) (Wood et al., 2011)?

Both the present *psychometric* and *behavioural* evidence were in support of the GRH (Wood et al., 2011). Recipients at present had shown the hypothesised *graded* pattern of emotional ratings towards offers of varying unfairness. Recipients reported considerably more *prosocial* emotions (e.g. emotional *gratitude*, a desire to reciprocate etc.) and less disapproval (e.g. annoyance) towards receipts of 'fairer' (i.e. *unconditional* or 'interest-free') offers. Interestingly, as opposed to the literature (e.g. Tsang, 2006b; Watkins et al., 2006) a more salient demand for a return favour (i.e. a 'harsher' repayment clause) at present had triggered i) *lower* state *indebtedness* and, ii) the perceived *obligation* to repay. In Schaumberg and Flynn's (2009) words, a more *unfair* offer at present seemed to have undermined both the '*want–to*' (i.e. *gratitude*-inspired) and the '*have-to*' (*indebtedness*-invoked) motives to reciprocate.

Aside from the survey ratings, the current behavioural data also challenged the notion that there exists a 'fair versus unfair' threshold—thus more unfairness should not matter once that threshold had been overstepped. First, as recapped in Sub-section 5.3.2.2 the odds of offer rejections were higher

among the more unfair offers (e.g. 25%-, 50%- Interest offers); despite knowing *in advance* that it is possible to default. Meanwhile, the data from a supplementary Multinomial Logit Model Analyses (See Table 5.18) further evidenced the impact of conditionality (while controlling for whether the recipients were affected by the 'Partner's Repayment Manipulation (in Trial 1)') on acceptors' categorical repayment of their *conditional* offers.

Table 5.18 Supplementary analysis: Comparison of Repayment by *Conditionality* (Repayment Manipulation[1] as a covariate, N= 133)

	Variable: *Conditionality* (RC: 'Interest-Free' offers)			
	Odds Ratio (ORs)	Intercept	*25%- Interest* vs. *Interest-Free*	*50%-Interest vs. Interest-Free*
1. Variable: Categorical Repayment				
-Repay *Partially* vs. Repay *All* (Reference Category)	Odds Ratio	0.22*	1.92*	4.50*
	95% LLOR	0.13	1.32	2.81
	95% ULOR	0.37	2.81	7.21
-Repay *Nothing* vs. Repay *All* (Reference Category)	Odds Ratio	0.08*	2.70*	4.18*
	95% LLOR	0.03	1.69	2.32
	95% ULOR	0.02	4.33	7.55
	Variable: *Conditionality* (RC: '25%-Interest' offers)			
	Odds Ratio (ORs)	Intercept	*Interest-Free* vs. *25%- Interest*	*50%-Interest vs. 25%-Interest*
2. Variable: *Categorical* Repayment				
-Repay *Partially* vs. Repay *All* (Reference Category)	Odds Ratio	0.42*	0.52*	2.34*
	95% LLOR	0.26	0.36	1.64
	95% ULOR	0.70	0.76	3.33
-Repay *Nothing* vs. Repay *All* (Reference Category)	Odds Ratio	0.22*	0.37*	1.55*
	95% LLOR	0.10	0.23	1.00
	95% ULOR	0.49	0.59	2.39

Note.[1] the 50 participants who were unaffected by the partner repayment manipulation (i.e. *refused* to help or help *unconditionally* at Trial 1) was coded 'unaffected' in this analysis.
LLOR: Lower Limit Odds Ratio; ULOR: Upper Limit Odds Ratio. * denotes that the 95% Confidence Intervals did not overlap the null value (i.e. OR=1)—suggesting statistical significance (Szumilas, 2010).

Indeed, this supplementary analysis (See Table 5.18) indicated that the odds of acceptors *under-repaying* (i.e. *partial* or *zero* back-transfer) over *cooperating* (i.e. repay fully) were higher among the 'harsher' offers compared to the 'lenient' ones (e.g. 50%- vs. 25%-Interest and 25%- vs. Interest-Free etc.). Overall, the behavioural evidence suggested that not only were the more unfair offers more frequently *rejected*, they were also more likely to yield a *sanctioning* response (i.e. repay *partly* or *nothing*) from those who accepted.

Taken together, both the in-game *survey ratings* (i.e. appraisals and emotions) and the *actual decisions* (i.e. acceptances and repayment) were in

support of the GRH (Wood et al., 2011). Participants reacted both *emotionally* (e.g. feeling more *gratitude, indebtedness* etc.) and *behaviourally* (e.g. repaying despite 'not having to') towards offers of varying extent of normative fairness (or unfairness). I interpret this as a result of recipients being allowed to compute the *relative* worth of each offer they received (Wood et al., 2011), thanks to the newfound transparency of the helpers' range of alternatives. Any discrepancy in this relative worth (across offers) is subsequently translated into different *emotions* and displays of *prosociality* (or the lack thereof).

5.5.3. *Gratitude, Indebtedness,* and the present Display of *Prosociality*

In this section, I recap and discuss how *gratitude* and *indebtedness* had *directly* or *indirectly* contributed to prosociality in the present game. First, the results overall showed that acceptors feeling more gratitude (for the *conditional* offers) would less likely default on their helpers (by partial or total under-repaying). The magnitude of voluntary backtransfer to *unconditional* helpers was also regressed on *state* gratitude. While all these observations were consistent with the *Moral Motive* Hypothesis (McCullough et al., 2001; McCullough et al., 2008) in that *gratitude* will undermine endeavours to harm the benefactors, the results, nonetheless, demonstrated that the experience of *gratitude* would not entirely rule out defaulting as hypothesised (i.e. *Moral Barometer* Hypothesis (McCullough et al., 2001; McCullough et al., 2008)). Indeed, instances of cheap-riding were still preceded by a considerable degree of experienced *gratitude* (towards one's conditional offer givers).

Interestingly, an almost identical pattern of findings emerged for *state* indebtedness and recipients' trustworthiness. Acceptors who reported feeling more *indebted* would also more likely *cooperate* than *under-repay* (both

partially and *totally*). Confirming the present *Indebtedness-driven* reciprocity notion (Schaumberg & Flynn, 2009), state *indebtedness* had a) an *inverse* association with the under-repayment rate of *conditional* offers; and b) a *direct* correlation with the voluntary repayment of *unconditional* offers.

Furthermore, echoing the analyses in Chapter 4, the current design features (e.g. double-anonymity and partner-switching) did not encourage the helper vilification as Greenberg and Westcott (1983) suggested as a route via which a recipient can eradicate his/her benefit-triggered *indebtedness*. Data from the path analysis revealed quite the opposite results: state *indebtedness* would indirectly *promote* trustworthiness (i.e. suppress under-repayment) via more favourable benefit appraisals.

In summary, the findings appeared to challenge the well-documented (e.g. Watkins et al., 2006; Tsang, 2006b) *divergence* in how *gratitude* vis-à-vis *indebtedness* are related to *prosociality*—which was replicated in a relatively under-sampled Experiment 1 (N= 49) (See Chapter 3). Participants at present felt less *appreciative* and *indebted* upon receipts of increasingly *unfair* offers, and that both affective states were predictive of *trustworthiness*, so as sheer *generosity*, in a *similar* fashion under an *iterated* economic exchange context.

Additionally, analyses on whether *dispositional* gratitude would shape prosociality as suggested in the Social-Cognitive Model of Gratitude (Wood et al. 2008) revealed insignificant results. Echoing the analysis of Chapter 4, the present investigation again failed to confirm that *dispositional* gratitude would enhance trustworthiness via the serial mediation (Hayes, 2013) via i) *benefit appraisals* followed by ii) *state* gratitude.

Finally, analyses on hypotheses in relation to how *dispositional* indebtedness would foster helping (i.e. Indebtedness-driven Helping) or trustworthiness (i.e. Indebtedness-driven Repayment) again produced null findings (See Sub-section 5.4.3.2 and Table 5.17). In a separate analysis (See Table 5.9) I showed that *dispositional* indebtedness was not segregating offer acceptors from the rejectors, even though I also acknowledged that (See Sub-section 5.3.2.2) the present lop-sided acceptor-rejector distribution may render this particular analysis unfit to be drawn any conclusions from.

Taken together, unlike the *state* measures, neither *dispositional* gratitude nor indebtedness was found to have affected participants' crucial decisions as hypothesised (See Table 5.1). These seemed to be in line with the key findings in the meta-analysis (See Chapter 2) whereby the display of gratitude-triggered prosociality should depend more on whether the gratified person is *feeling* grateful at that instant, rather than whether or not that said individual is *endowed with* a grateful *character* or *life orientation* (Wood et al., 2010). The current data hinted that this very same line of reasoning may also apply to how indebtedness would be related to prosociality as well.

5.5.4. Solutions to other Methodological Unanswered Questions (UAQs)

Below (i.e. Sub-sections 5.5.4.1 and 5.5.4.2) I review how the present results would address the two other methodological UAQs. They included, firstly, the UAQ # 2 (See Sub-section 5.1.2.2) which questioned if the lack of opportunities to play both as helpers and recipients was to blame for the 'over-generosity' issue in Experiments 1 and 2; and secondly, the UAQ # 3 (See Sub-section 5.1.2.3) which inquired if the high rejections (i.e. 35.6%) in Experiment

2 were propelled by *negative reciprocity* (Bolton et al., 1998), or *trait* indebtedness (Greenberg & Westcott, 1983; Mathews & Shook, 2013).

5.5.4.1. Solution to UAQ #2: Playing Both Roles and Over-generosity

Indeed, the analyses had confirmed neither the *Golden Rule* nor the *Reduced Responsibility Hypothesis* (Burks et al., 2003). Overall there was no clear-cut evidence to conclude that the present a) *both-role*, and b) *iterated* design had enhanced either, i) trustfulness or, ii) overall trustworthiness.

Although as discussed in Section 5.5.1 acceptors in Experiment 2 were, compared to Experiment 3, more likely to prefer *fully* or *utterly not* repaying than cheap-riding on their offers, the comparison of under-repayment rates (i.e. UR%) in Experiments 2 and 3 revealed no significant differences. Thus, despite the fact that acceptors in Experiments 2 and 3 had adopted different categorical repayment options, such a difference was not translated to the overall measure of trustworthiness (i.e. UR %). All in all, the 'over-generosity' concern in Experiments 1 and 2 appeared to persist despite the present modified design.

5.5.4.2. Solution to UAQ #3: Were the High Rejections in Experiment 2 attributable to *Negative Reciprocity* or *Dispositional* Indebtedness?

Predictably, the incidence of offer rejections was significantly lower in the present experiment (i.e. 11.9%) than in Experiment 2 (i.e. 35.6%). I attribute such a drastic decline to a modified feature at present—that is, a pre-acceptance notification of the *non-binding* nature of the repayment clause. This new feature should rule out the possibility of recipients rejecting as to express their discontent with the normative unfairness that they were putting up with (See Chapter 4), as they could do so via taking the offers and then not repay.

While it is crystal clear that recipients at present should harbour minimal incentives to reject and the data showed exactly that, the important question here is: had this reduced rejections translated to more sanctioning in Experiment 3? Indeed, as discussed above the cross-study comparison had evidenced a higher prevalence of cheap-riding (over cooperating and free-riding) at present compared to Experiment 2. Additionally, the overall incidence of *cooperating* (over defaulting in general) was significantly higher (p = .022, two-tailed) in Experiment 2 (i.e. 59/ 87= 67.8%) compared to Experiment 3 (i.e. 386/ 703= 54.9%; for conditional offers only), although such a difference in *categorical* repayment was not translated to the present overall index of trustworthiness (i.e. under-repayment %).

Taken together, the results hinted that participants did utilise the current set-up to penalise their helpers via defaulting, and more importantly, an overwhelming segment of these defaulting involved *cheap*- (i.e. 77.9%) rather than *free*-riding (i.e. 22.1%). Thus, contrary to the convenient speculation that the present recipients refused to reject so they could fully exploit their helpers, they appeared to instead scrap rejections for cheap-riding—whereby the correctional, 'Just Desert' (Carlsmith et al., 2002) aspect of cheap-riding had been thoroughly discussed in Chapter 4. As a result, the present observed sharp decline in rejections should add substance to the speculation that a large chunk of rejections in Experiment 2 may have originated from a desire to penalise unfairness—and in a way confirming the Negative Reciprocity Hypothesis.

Additionally, I tested whether *dispositional* indebtedness will have a role in prompting rejections. The data overall failed to show any signs of *dispositionally* indebted individuals being more propelled to accept or reject.

Impression management (Paulhus, 1991) was also a non-factor in acceptance. The current acceptance decisions seemed to primarily hinge on *situational* parameters such as offer conditionality, as the data indicated that the odds of rejection were evidently *lower* for offers with a more *lenient* repayment clause (e.g. 'interest-free'). Nonetheless, the overall rare occurrence of rejections (See Table 5.9) may have limited the scope and the validity of analyses as of why people would bother to reject an offer in the present experiment.

5.6. Implications and Future Directions

Overall, Experiment 3 offered a robust response toward the list of unanswered questions (UAQs) from Experiments 1 and 2. While some UAQs remain unresolved, for instance, the overall 'over-generosity' issues emanating from Experiments 1 to 3 and that *cheap-riding* was not norm-enforcing as expected, the current chapter had complemented Experiment 2's investigations into *cheap-riding*, and more importantly, the role of *gratitude* and *indebtedness* in fostering *reciprocity* (or restraining *sanctioning*) in economic exchanges. In subs-sections 5.6.1 and 5.6.2 I review two unresolved UAQs and discuss how that could be addressed in future investigations.

5.6.1. Unresolved UAQ 1: Over-generosity: *Social Heuristics* (Rand et al., 2012; Rand et al., 2014) or the *House Money* Effect (Clark, 2002) to blame?

The cross-study analyses failed to show that participants under the present *iterated* and *multiple*-roles experimental design was more *prosocial* or *profit-maximizing* than those of the *one-shot*, *single*-role Experiments 1 and 2. Thus, it is conceivable that other facets of Experiments 1 and 2 may account for the observed 'over-cooperativeness'.

Rand and associates (2014) theorised that human beings developed a *social heuristics* through numerous day-to-day interactions during which *cooperation* is inevitable and advantageous. As a result, people acquire a cooperative 'intuitive defaults' (Bear & Rand, 2016; Jordan, Peysakhovich, & Rand, 2014; Peysakhovich & Rand, 2016) and may inadvertently let it dictate their decision-making in situations where being *cooperative* is not advantageous (e.g. one-shot, double-blind games like Experiments 1 and 2).

Indeed, the above notion which attributes people's reluctance to be pro-self in one-shot games to their inherent cooperative instinct had garnered extensive attention from the research community. While Rand and associates (e.g. Rand et al., 2014) had consistently demonstrated the *inverse* relation between conscious reflection and prosociality in a series of public good games (i.e. PGs hereafter), Verkoeijen and Bouwmeester (2014) failed to replicate this intuitive-cooperation effect in their five studies that were modelled after those of Rand and colleagues'. Additionally, Verkoeijen and Bouwmeester (2014) meta-analysed 28 experiments that scrutinised this *intuitive-cooperation* effect in PGs and identified an *immaterial*—albeit still statistically *significant*—overall effect estimate (i.e. Cohen's $d = 0.087$, $R^2 = 0.0018$).

Taken together, while theoretically possible it is still not without doubt that intuitive kindness is the bona fide solution to the present unresolved over-generosity problem. Indeed, analyses of Experiments 1 to 3 had not included the decision time measures, nor had I directly manipulated the *time allowed* for the present decision making —for instance, the '*time-delay* versus the *time-pressure* conditions (Rand et al., 2012). Therefore, a future design that

incorporates either of these two features should be warranted for a more in-depth investigation into the *intuitive-over-cooperation* effect.

Meanwhile, one shared feature of Experiments 1 to 3 entails the absence of needs for participants to legitimately 'earn' their endowment. Potential helpers (i.e. P1s) and recipients (i.e. P2s) throughout Experiments 1 to 3 knew and acknowledged that *chance* rather than *competence* was what 'entitled' them to the positions (the one to *help* or to *expect help*) they were in. As such, it should be legitimate to argue that P1s in Experiment 1 and 3 were basically playing with the 'House Money' (Clark, 2002) when deciding whether or not and how to help their partners out. On the other hand, P2s were also aware that it was not their partners' fault that they 'lost', and therefore it should be fair—at least from an *absolute* standpoint—that their partners just walked away with their proceeds without sparing them a glance. Furthermore, in Experiments 1 to 3 communications between players were strictly forbidden. P2s were practically 'spoon-fed' the offers without even having to ask (not to mention earning that), unlike Andreoni and Rao's (2011) design in which recipients had to at least send a verbal request for help. Therefore, it should be reasonable to argue that the P2s in Experiments 1 to 3 were also playing with the house—or at least 'unearned' (Oxoby & Spraggon, 2008)—money. Indeed, the tendency of participants to be *more generous* or *less risk-averse* playing with house money is well documented in the behavioural economics literature (e.g. Cherry et al., 2002; Harrison, 2007). By contrast, when endowment has to be *earned* people would act more in accord with *rationality* (Oxoby & Spraggon, 2008).

In a nutshell, an alternative avenue to enhance the present resolution of the over-generosity issue entails a slight change in the ways endowment was

allocated to the participants. Specifically, instead of the present dice-rolling set-up the experimenter could consider having the players partake in a general knowledge quiz, and telling them that a certain criterion (e.g. 8 out of 10) had to be met for any contestants to claim any bonus money. The experimenter could then manipulate the difficulty of these questions. For instance, participants assigned to the 'helper' condition could have a 'cakewalk' to that target, while those assigned to the 'recipient' condition would face way tougher questions. Additionally, a recipient in this modified design could be stipulated to at least need to make a 'donation request' should they fancy being bailed out. While these new features are not without flaws (e.g. the competence attribution and one's propensity to proactively seek help etc.), they should, however, in combination provide a more valid assessment by taking out one well-documented reason to be overly cooperative: the house money effect.

5.6.2. Unresolved UAQ 2: Cheap-riding not *Norm*-enforcing—the partner-switching design or the lack of justifications (from the punishers) to blame?

The present analyses highlighted that while *cheap-riding* would more likely be resorted to in situations where doing so better serves its immediate norm-enforcing purpose (i.e. an *iterated* instead of a *one-shot* game); there is no guarantee of how well it necessarily works. Nevertheless, I suspected that this lack of efficacy could be liable to the present partner-*switching*, or the *stranger* design as coined by Fehr and Gächter (2000).

Contrary to a *partner-matching* design (Fehr & Gächter, 2000) in which participants played an *iterated* game with the same co-player(s), at present participants were supposedly playing with a different co-player every round—while staying *anonymous* throughout. Indeed, results of a meta-analysis on over

126 social dilemma experiments (Balliet, Mulder, & Van Lange, 2011) suggested that the *punishment-cooperation* link was significantly stronger in a *partner-matching* than both *partner-switching* and *one-shot* designs, and that the number of *iterations* was practically a non-factor in how effective *punishment* is in inciting cooperativeness. As such, it is conceivable that the current absence of norm-enforcing efficacy of *cheap-riding* could be ascribed to the *partner-switching* nature of Experiment 3.

Meanwhile, Xiao and Tan (2014) contended that *justification* is integral to the proper functioning of any punishment institutions, as people despise those that work in favour of the enforcer's self-gains. Indeed, the failure of a morally *unjustified* punishment in enhancing cooperation has been well documented in the Trust Game literature (e.g. Fehr & Rockenbach, 2003; Fehr & List, 2004). In Experiments 2 and 3 neither the '*cheap-rode*' nor '*free-rode*' victims were given any justifications why they were repaid as such. This absence of justifications—together with the fact that the *cheap-riding* enforcers were still 'profiting' from the exchange—may undermine the legitimacy of the cheap-riding (Xiao & Tan, 2014) and not surprisingly, the efficacy of *cheap-riding* to get the victims to act more in accord with normative fairness.

In a nutshell, I argue that the present analyses of *cheap-riding* could be complemented with two minor design modifications as discussed in the following. They included, first, an iterated partner-*matching* to replace the current partner-*switching* design. Similar to Güney and Newell's (2013) procedure, participants would be informed they would play with an *anonymous* partner for *multiple* rounds. Meanwhile, the experimenter withholds details on i) the total number of trials (which was already part of the present game's

features); and importantly, ii) *when* participants would *swap partners*. In so doing a '*cheap-rode*' helper in a *preceding* trial should be more incentivized to make amends for his/her initial 'wrongdoing' by being cooperative in the *next*, knowing that he/she might encounter the same interactional partner again.

Meanwhile, a second recommended adjustment entails the helpers' access to their offer acceptors' post-repayment ratings (that probed into their *motives* behind their repayment or the lack thereof). Alternatively, similar to Xiao and Tan's (2014) *Justification Treatment* the punisher—at present a *cheap*- or *free*-rider—could be stipulated to give an explanation to the helper why he/she under-repaid. Either of these new features should highlight to the helpers *explicitly* why—justified or otherwise—their offers were repaid as such. Indeed, data from Xiao and Tan (2014) demonstrated that when 'punishment is profitable for the enforcer (pp.14)' a justification could render a) a perception of *legitimacy* in the sanction, and importantly, b) more efficiency of that sanction in *promoting prosociality*.

Meanwhile, Xiao and Tan (2014) also showed that the need to justify a sanction would confine the enforcer to only penalise people for their misdeed rather than for ulterior reasons. Taken together, I argue that by allowing the defaulters (*cheap*- and *free-riders* inclusive) to account for their non-repayment, or at least making their *post-repayment* survey responses known to their helpers, should render a more overall legitimate analysis of how *cheap-riding* could emulate altruistic punishment. Table 5.19 below provided a summary of the list of currently unresolved UAQs (i.e. UUAQs hereafter) and the above-listed recommended measures to address each of them.

Table 5.19 List of recommended changes for the scrutiny of the UUAQs

UUAQs	Possible Reasons	Recommended Changes
1. What caused people to be overly generous in the present experiments?	*Social Heuristics* Hypotheses (i.e. SHH, Rand et al., 2012; Rand & Nowak, 2013; Rand et al., 2014; Jordan et al., 2014)	1. A more *time-sensitive* software package to record participants' decision time—e.g., the *Psychopy* (Peirce, 2007). The currently adopted version of Z-Tree (version 3.3.1.2; Fischbacher, 2007) could only record decision time precise to *seconds*; or 2. A *direct* manipulation of permitted decision time—e.g. Rand et al.'s (2012) time-*delay* vs. time-*pressure* conditions.
	House Money Effect (i.e. HME, Cherry et al., 2002; Clark, 2002; Harrison, 2007; Thaler & Johnson, 1990)	1. To have players 'earned' their endowment via a knowledge quiz. 2. To maintain the 'Helper versus Recipient' set-up I adjust the difficulty of the questions presented to those assigned to the 'Helper' or the 'Recipient' Groups; 3. Recipients need to proactively ask for help (e.g. Andreoni & Rao, 2011)
2. Why *was Cheap-riding* unable to enforce normative fairness as hypothesised?	The current *Partner-switching* design, and the *One-shot* nature of Experiment 2	To inform participants that they would each play with an anonymous fellow player for *multiple* rounds; and I withhold information in relation to a) how many trials the experiment would entail, and b) when the *partner-switching* would take place (e.g. Güney & Newell, 2013).
	Lack of *Justification* from the enforcer of *cheap*- or *free*-riding	1. A *mandatory explanatory note* to be sent to his/her helper if an acceptor is to *partially* or *completely* under-repay (e.g. Xiao & Tan, 2014); or 2. To show the post-repayment survey responses of the *cheap*- or *free*-riders to their respective helpers afterwards.

Chapter 6

General Discussions

6.0. Overview of the Present Thesis

Throughout Chapters 2 to 5 a recurring theme was thoroughly examined, that is, how gratitude as a construct would relate to prosociality or sanctioning in a Behavioural Economics context. Every empirical study casts light upon certain elements of the *gratitude-prosociality* association, with each ensuing experiment providing a follow-up analysis to *validate*, as well as, to *build upon*, those particular observations.

In particular, while the meta-analysis (i.e. Chapter 2) revealed that the *gratitude-prosociality* link is stronger among studies which examined gratitude as a *benefit-triggered* (Lambert et al., 2009), *transitory* affective *state*, or studies which adopted *direct*, *reciprocal* prosociality measure, Experiment 1 (i.e. Chapter 3) built upon that by examining how this *gratitude-reciprocity* association would be subject to helper intent attribution, and how the injunctive fairness norm (Elster, 2006; Paddock, 2005) could contribute to that and thus shaped recipients' feeling of gratitude (or indebtedness) throughout the episode, and ultimately his/her urge to *directly* reciprocate. Experiment 2 (i.e. Chapter 4) shifted the spotlight towards recipients' emotional and reciprocal reactions towards *injunctively unfair* (i.e. *conditional*) offers (Ma et al., 2014), and tested whether *dispositional* gratitude would contribute to prosociality (or sanctioning) in this context as predicted by the Social Cognitive Model of Gratitude (i.e. SCMG, Wood et al., 2008). Meanwhile, Experiment 3 (i.e. Chapter 5) served as a round-up by examining a few unanswered questions (see Chapters 4 and 5) from the two prior studies, via a game with a different design (i.e. an *iterated* game with *role-switching*).

Meanwhile, Experiments 2 and 3 entailed a series of comprehensive follow-up analyses on an interesting while relatively under-researched economic observation from Experiment 1— i.e. the *cheap-rider* problem (Isaac, Schmidtz, & Walker, 1989). While speculations and theories (e.g. 'Just Deserts' theories of punishment (Carlsmith et al., 2002)) on why people may cheap-ride—instead of free-ride—had been raised, Experiment 2 offered a customised and more robust scrutiny over the *occurrences* and *motives* behind cheap-riding. Experiment 3 further built on that by examining, i) how cheap-riding could emulate altruistic punishment (Fehr & Gächter, 2002; Fowler, 2005) as a means to enforce the injunctive fairness norm (i.e. the *Norm Enforcement Hypothesis* (NEH), See Chapter 5 for details), and importantly, ii) how *gratitude* and *indebtedness* would relate to this particular type of defaulting that should make little sense *rationality-wise* (See Chapter 4 for details). Figure 6.1 below—which illustrates an integrated model derived from the key observations—could offer insights on how the present thesis would add to the gratitude and behavioural economics literature. Detailed discussions in relation to the *theoretical* and *practical* significance of the current thesis will, meanwhile, be featured in Sections 6.1 and 6.2 respectively.

6.1. Theoretical Contributions

This section discusses at length how this thesis contributes to both the behavioural economics and gratitude literature. Sub-section 6.1.1 features the discussion on how the observations from the three lab studies add to the existing models (e.g. SCMG, Wood et al., 2008). Sub-section 6.1.2 sheds light upon certain novel specific paths generated from the empirical studies (e.g. how the helper intent will depend upon the injunctive norm), and the subtleties

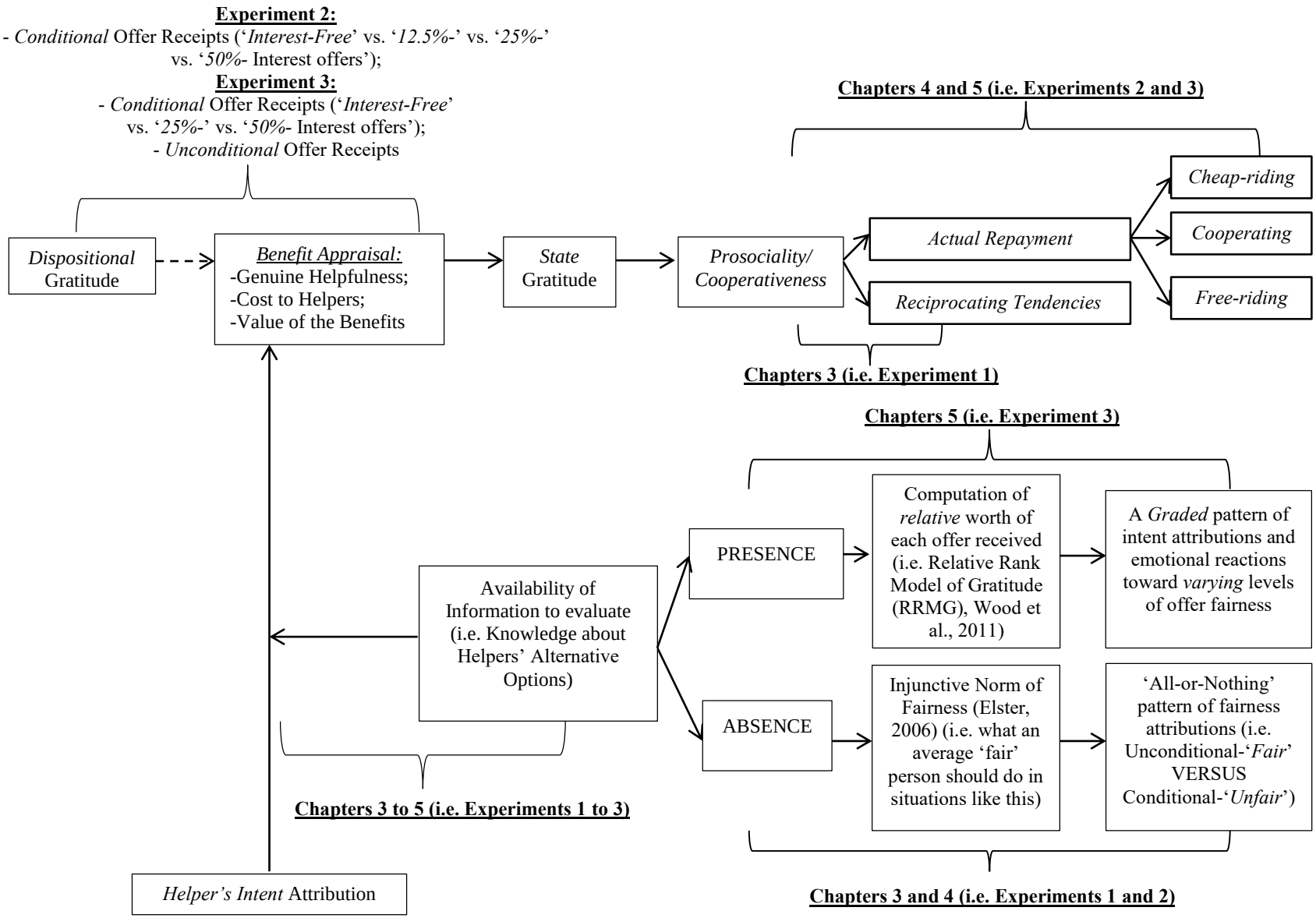

Figure 6.1 Overall Summary Diagram of the Thesis (while incorporating the Social Cognitive Model of Gratitude (Wood et al., 2008)

as regards some of these paths (e.g. how the use of injunctive norm or the *Relative Rank Model of Gratitude* (i.e. RRMG, Wood et al., 2011) was indeed dependent on recipient's knowledge about helpers' options). Sub-section 6.1.3 outlines how the present overall observations of *cheap-riding* would relate to, and therefore builds on, the existing findings on the 'cheap-rider' problem in the public goods game literature (e.g. Isaac et al., 1989; Marks et al., 2006).

6.1.1. How do the Model and the Current Findings Add to the Literature?

While several theories, for example, the Moral Functional Hypotheses of Gratitude (McCullough et al., 2001; McCullough et al., 2008) and the Social Cognitive Model of Gratitude (SCMG) (Wood et al., 2008), had either *hinted* or *overtly* stated that gratitude is predictive of prosociality, the present integrated model (see Figure 6.1) has outlined a complex that underlies the whole *gratitude-prosociality* association. Particularly, this integrated model incorporates the Theory of Reciprocity (or Fairness) (e.g. Falk & Fischbacher, 2006; Falk et al., 2008) by acknowledging the importance of perceived intent or fairness, and importantly, how this fairness attribution could be subjected to manipulation of certain parameters (e.g. knowledge about helpers' behavioural alternatives) and thus elicits varying levels of experienced *gratitude* which is ultimately translated into direct *reciprocation* (or *sanctioning*) in an economic exchange context. All these observations could be mapped onto the results of the moderator analyses (i.e. Chapter 2) where *gratitude* is more strongly linked to *prosociality* when certain parameters were met. These included, (1) *state* rather than *trait* gratitude being examined; (2) *direct* reciprocity instead of *indirect* or *non*-reciprocal outcomes being measured; and (3) *benefit-triggered* instead of *generalized* gratitude (Lambert et al., 2009) being examined.

The present thesis also attempted to shed light upon the well-documented (e.g. Schaumberg & Flynn, 2009; Watkins et al., 2006; Tsang, 2006b) divergence in the mechanism via which *gratitude* and *indebtedness* facilitate prosociality. While the relatively under-sampled (*N*= 49) Experiment 1 appeared to replicate such a divergence—whereby *gratitude* rather than *indebtedness* was found to vary as a function of injunctive (normative) fairness, such a pattern was not replicated in either of the larger samples in Experiments 2 (*N* = 87) or 3 (*N* = 133). In fact, analyses of Experiment 3 even showed that both states *gratitude* and *indebtedness* would relate *directly* toward injunctive fairness, and both were predictive of subsequent *direct reciprocal* behaviours (in the forms of *trustworthiness* or *generosity*). Taken together, the results seemed to challenge the prevalent idea in the emotions literature where *indebtedness* is commonly conceptualized as a negatively-valenced affect that is doomed to haunt cooperation in the long-run (e.g. Greenberg, 1980; Greenberg & Westcott, 1983; Hatfield & Sprecher, 1983), whereas *gratitude* is expected to invoke and nurture prosociality via an entirely different route (Komter, 2004; Watkins, 2014; Watkins et al., 2006). Additionally, these findings would echo my argument in Chapter 2—where in *direct reciprocal* exchanges *gratitude* may also trigger a sense of *indebtedness* or *obligation* (Wood et al., 2016), and that both of which should contribute to a desire to return other's prosocial gestures (Schaumberg & Flynn, 2009).

Furthermore, as addressed in Chapter 1 the current thesis constitutes the first ever empirical examination of how *indebtedness* (both as a *state* or a *disposition*) as a *psychological* construct would mould people's economic decisions. While *dispositional* indebtedness (along with *dispositional*

gratitude) was not shown to be particularly impactful on participants' current cooperative or retaliatory decisions, potentially due to the power issue (Refer to Section 6.3 for more details), the present data have consistently demonstrated that *indebted* recipients did not only feel *prompted* to reciprocate (i.e. Experiment 1), but also engaged in *direct* reciprocal behaviours (i.e. Experiment 3) and restrained sanctioning behaviours, even when the helpers had stated 'upfront' that there was *no need* to repay (i.e. *unconditional* offer receipts, Experiment 3). All in all, the present thesis suggests that the (state) *indebtedness*- (direct) *reciprocity* link was robust to a range of experimental manipulations (i.e.*one-shot* vs. *iterated*, *single*- vs. *multiple*- roles, *fair* vs. *unfair* offers), and hence should serve as a starting point for any future analyses on the role of *indebtedness* in the realm of economic exchanges.

6.1.2. Subtleties of Specific Paths in the Above Integrated Model (Figure 6.1)

As suggested above the present integrated model is not without its subtleties. This section features a closer scrutiny of the two 'plot twists' of the model—one being the 1) moderating effect of recipients' knowledge of helpers' options on the fairness perception and accordingly benefit appraisal (i.e. Sub-section 6.1.2.1), with the other being 2) how *cheap-riding* (which is defined and discussed in the following Sub-section 6.1.2.2) would serve its fairness-enforcing purpose differently in a *one-shot* (i.e. Experiment 2) versus an *iterated*, *partner-switching* (i.e. Experiment 3) interactional context.

6.1.2.1 Moderating Effect of Recipients' Knowledge about Helpers' Options on Fairness Attribution, Benefit Appraisal, and Experienced Gratitude

First, as illustrated in Figure 6.2 the path between 'Helper Intent Attribution' and 'Benefit Appraisal' is dependent upon the recipients'

knowledge about the helpers' list of alternative options. When such knowledge is *absent* or *denied* (Experiments 1 and 2) people were inclined to rely on the injunctive fairness norm—which delineates what an average *fair* person is expected do in a given situation (Elster, 2006; Paddock, 2005)—to guide their helper's intent attributions (Ma et al., 2014). Analyses in Chapters 3 and 4 hinted that such a reliance on injunctive normative fairness appeared to drive recipients to adopt an *all-or-nothing*, 'threshold' mentality in fairness evaluation. Recipients only attended to whether helpers played *fair* from a normative standpoint (i.e. to share their *unearned* resources *equally* with others (Frey & Bohnet, 1995) by helping *unconditionally*), and when the helpers chose not to do so (i.e. to impose a 'helping' fee via donating *conditionally*) the recipients did not show incremental negativity (or decreasing positivity) toward a more injunctively *unfair* arrangement (e.g. a *50%-Interest* versus an *Interest-Free* offer). See Chapters 3 and 4 for more in-depth discussions.

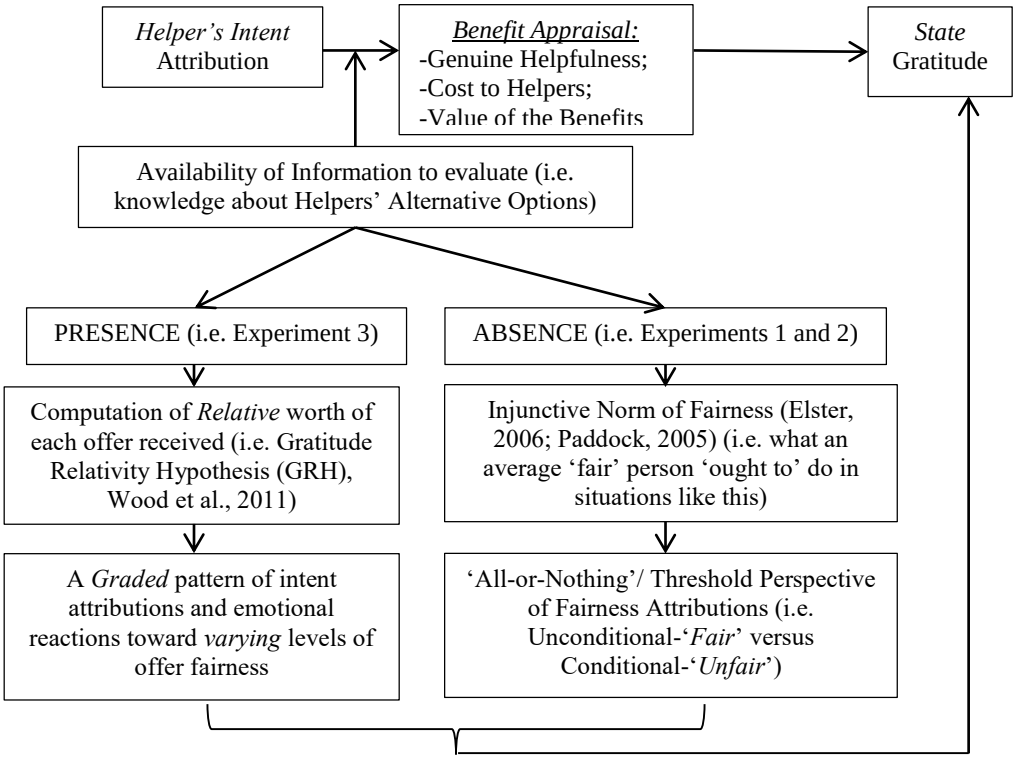

Figure 6.2 Extracts of the Present Overall Summary Diagram (Figure 6.1)

Consequently, recipients who were helped *fairly* (i.e. *unconditionally*) tended to evaluate their offers in a more positive fashion, which then led to more experienced *gratitude* and ultimately, increased *prosocial* intention (Experiment 1). In contrast, recipients who were treated *unfairly* (i.e. helped *conditionally*)—albeit in varying extent (i.e. different 'taxed' offers) — did not differ in their *benefit appraisal, gratitude*, or *actual repayment* (Experiment 2).

However, analyses of Experiment 3 revealed a very different pattern of results once recipients were no longer *unaware* of how differently their helpers *could have* treated them (i.e. access to helpers' alternative in-game options). Both the psychometric (i.e. survey ratings) and behavioural (i.e. acceptances and repayment) evidence were in support of the *Gratitude Relativity Hypothesis* (GRH)—in which Wood et al. (2011) theorised that people's *perception* of and *gratitude* towards a favour should be a function of that favour's *relative* worth : how that favour *compares against* the others.

As a result, this knowledge on how helpers could have acted *nicer* or *nastier* (Experiment 3) should permit the recipients' computation of how their *actual* receipts would 'rank' among those *'could-have'* receipts (Wood et al., 2011). These 'rankings' were indeed translated into 1) a *graded* pattern of both *benefit appraisal* and *gratitude* (so as *indebtedness*) toward the offers of varying conditionalities (or normative fairness), and importantly, 2) fewer *rejections* of and *defaulting* (i.e. *cheap*- and *free-riding* inclusive) on the *fairer* (i.e. the *unconditional* and those 'low-tax' offers) offers (See Chapter 5).

6.1.2.2. When to *Cheap-ride*? And How Reliable could *Cheap-riding* be in emulating Altruistic Punishment (i.e. Norm-Enforcement Hypothesis)?

Another subtlety of the present integrated model would involve the other main focus of this thesis—i.e. *cheap-riding,* which was operationalized at present as a recipient's *partial* repayment of an *accepted, conditional* offer. The analyses of *cheap-riding* became of interest when data of Experiment 1 revealed that a sizeable (i.e. 38.1%) proportion of *conditional* offer acceptors opted to just *under-repay,* instead of dodging the repayment in its entirety (defined as *'free-riding'* at present), upon the realisation that the 'repayment clauses' that they previously agreed to was indeed *not binding* (See Chapter 3).

Importantly, while *cheap-riding* was consistently observed across Experiments 1 to 3 it is interesting to notice that people are more likely to *cheap-ride* in an *iterated, partner-switching* (Experiment 3) than in a *one-off* (Experiment 2) exchange setting (Refer to Sub-section 5.3.1.2, Chapter 5 for the detailed analyses). I interpreted this as a partial confirmation of the Norm-enforcement Hypothesis (NEH), which draws parallels between *cheap-riding* and *altruistic punishment* in safeguarding cooperation in the long-run.

I argued throughout this thesis that one principal motivation of *cheap-riding* at present should entail the communication of an unfairly treated recipient's 'fair but firm', 'cannot be exploited' image (Komorita et al., 1992; Komorita & Parks, 1999) toward the helper, and in so doing the *cheap-rider* strives to prevent that helper from by being ungenerous to other people, and therefore breaching the fairness norm once again (i.e. NEH) (Ma et al., 2014). Hence, it makes perfect sense for a *fairness-enforcing* cheap-rider to *cheap-ride* more in a *repeated*, *partner-switching* context during which doing so should better serve its correctional purpose, as an ungenerous helper in an *iterated* game should have more chances (i.e. the upcoming trials) to continue

to treat others *unfairly*. In contrast, Experiment 2 should be less of a breeding ground for any norm-enforcing cheap-riding, as its one-shot nature should put any prospective *cheap-riders* under the impression that those unfair helpers could not carry on violating normative fairness even if they wanted to. Thus, cheap-riding in Experiment 2 would understandably be less prevalent that of Experiment 3, and thereby offering some confirmation to the *NEH*.

Nonetheless, analyses of Experiment 3 offered minimal evidence that being *cheap-rode* would give rise to more subsequent *fair plays* in subsequent trials. Overall, the data showed that 'cheap-rode' helpers (in Trial 1) were more likely to also *partially repay* the *conditional* offers that they later on accepted. There were, however, neither signs of these 'cheap-rode' victims being more inclined to1) repay fully (i.e. cooperate), nor 2) to abstain from *free-riding* on others—than helpers whose initial conditional offers (in Trial 1) were either *fully repaid* or *not repaid*, thus in a way challenging the *NEH*.

Taken together, while participants seemed capable of finding the 'optimal' circumstance to cheap-ride (i.e. 'preference' to cheap-ride in an *iterated* instead of *one-off* context), there is, nevertheless, no guarantee of how efficacious cheap-riding could be in maintaining normative fairness. I interpret these as the epitome of participants' belief that their decisions may have a consequence that did not turn out to be the case. (See Chapter 5 for more discussions on how Experiment 3's *partner-switching* design may account for the lack of norm-enforcing effect by the 'cheap-riding' manipulation)

6.1.3. Cheap-riding at Present and Cheap-riding in the Wider Literature

Despite being an overall scarcely researched topic *cheap-riding* has been explored in the Public Goods Game (e.g. Bchir & Willinger, 2013;

Rondeau, Schulze, & Poe, 1999), and even in the International Relations (e.g. Borcherding, 1981; Oneal, 1990; Thompson, 1987) and the Religiosity Study (e.g. Olson & Perl, 2005) literature. This implies that *cheap-riding* is far from just an experimental artefact which bears zero ecological validity or generalisability across various research domains. It is, therefore, important to discuss how the present observations on *cheap-riding* would relate to those in the wider literature. However, since papers which discussed cheap-riding in an *International Relations* context are mostly *not experimentally-based* (e.g. Thompson's (1987) analysis on how the NATO allies had 'cheap-rode' on the Americans by contributing significantly less in fending off the military aggression of the Soviet Union), therefore this section will highlight how the present observations on *cheap-riding* would compare against, and accordingly add to, the existing empirical findings in the *Public Goods Game* literature.

6.1.3.1. Operationalizing Cheap-riding in the Public Goods Games literature

Several experimental economists (e.g. Bchir & Willinger, 2013; Isaac et al., 1989; Rondeau et al., 1999) had adopted a variant of the classic Public Goods Game (PG) to investigate *cheap-riding*. Contrary to classic PG in which people who contribute nothing can still claim a share of the resources (as long as the provision point is reached), in this modified game players who refused to contribute a 'membership fee' are prohibited from taking a share (Bchir & Willinger, 2013). Thus, the '*cheap-rider*' problem is defined in this context as the '*unequal* distribution of *contributions* (pp.223)' (Isaac et al., 1989)—which is exemplified by someone contributing merely the 'membership fee' with the intention to exploit his/her groupmates' above-average contributions (Asch et al., 1993; Bchir & Willinger, 2013; Marks et al., 2006; Thompson, 1987).

6.1.3.2. Contrasting Cheap-riding at present and that in the PG Literature

The above conceptualisation of *cheap-riding* does not deviate from that of several other researchers (e.g. Krishnamurthy, 2000; Olson & Perl, 2005; Stigler, 1974), and is indeed not at odds with how *cheap-riding* was referred to at present—both of which concern the exploitations of others' contributions amidst the cheap-riders' own *non-zero* contributions. However, how cheap-riding was examined in the PG literature (e.g. Bchir & Willinger, 2013; Isaac et al., 1989) seemed quite different from what I did in the present thesis.

First, as addressed above this point-provision PG (Bchir & Willinger, 2013; Marks et al., 2006) should *categorically* rule out *free-riding*, as it is stipulated a priori that non-contributors would be *ineligible* to acquire any bonuses even if the group's contribution exceeds the provision point. As such, the only 'legitimate' defaulting option in such a context is *cheap-riding* (i.e. to just pay the membership fee). This is clearly different from the set-up of Experiments 1 to 3 in which offer acceptors were allowed to decide whether to pay up *in full* ('cooperate'), *partially* ('cheap-ride'), or *nothing* ('free-ride').

Additionally, there is an element of *risk* associated with cheap-riding in the Point-provision PG. This is particularly true in a '*No-Refund*' scenario (Bchir & Willinger, 2013) in which participants would 'lose their contributions in the event of provision failures (pp.27)'. Indeed, even in a *'Full-Refund'* context a cheap-rider may still incur the *risk* of his/her 'success' to cheap-ride on others thanks to the potential provision failures. That is, being able to *only* retrieve his/her membership fee should be indicative of a failed endeavour to piggyback on someone else's high contributions. Needless to say, this risk is further complicated by Bchir and Willinger's (2013) direct manipulations of

both 1) the provision thresholds and 2) the membership fees (e.g. 'Low-' versus 'Medium-' versus 'High-Threshold (Membership Fee)' conditions).

In comparison, defaulting in general (*cheap-* and *free-riding* inclusive) in the present experimental set-up appears highly 'risk-free'. Offer acceptors in Experiment 1 to 3 were aware of their bonus money (i.e. helpers' *conditional* transfer) prior to deciding whether they would default. Thus there should not be any risk in relation to 1) *cheap-riders* being denied any monetary payoffs at the end, or 2) *cheap-riding* not being impactful at all. Meanwhile, the *double-blinded*, *one-shot* nature of Experiments 1 and 2 should shield both the *cheap-* and *free-riders* from any retribution. Additionally, the *partner-switching* nature of Experiment 3 should offer assurance to the prospective *cheap-* and *free-riders* in Experiment 3 about the absence of repercussions of under-repayment.

6.1.3.3. Cheap-riding at Present and Cheap-riding in the PG Literature

Taken together, the present thesis should offer new perspectives to the Point-provision PG literature in relation to *cheap-riding* in a two crucial aspects. First, the present experiments added back the option to 'free-ride' (i.e. zero contribution) which is essentially forbidden in the abovementioned PGs (e.g. Bchir & Willigner, 2013; Marks et al., 2006). Not only would this re-addition 1) enable a more *refined* (or possibly *less inflated*) measure of one's *cheap-riding* decision—as *cheap-riders* at present need to also resist the temptations to default in its entirety, it also 2) permits the present dissociation between the mentality of the *cheap-riders*, *co-operators* and the *free-riders* (See Chapter 4). Apart from such a psychological profiling which is, to my knowledge, yet to be examined in the literature, analyses of Experiment 3 further underlined the segregation between *cheap-* and *free-riding* in terms of

how being '*cheap- rode*' (in Trial 1), as opposed to being '*free-rode*' (in Trial 1), would elicit a different pattern of decisions (i.e. repayment in the Recipient Trials and willingness to help in Trial 10) in the later trials (See Chapter 5).

Furthermore, as addressed above (i.e. Sub-section 6.1.3.2) the present investigation (i.e. Experiments 1 to 3) removed the *risk* element which is evident (and even directly manipulated) in Bchir and Willinger (2013). Hence, I argue that the *cheap-riding* examined at present should be more reflective of personal preferences rather than a rational decision following one's 'risk assessment'. As addressed in the Dual-process framework (Dijksterhuis & Nordgren, 2006; Kahneman, 2011) System 1 is characterised by *susceptibility toward emotions*, whereas System 2 epitomises *rationality* and *reasoning* (Kahneman & Frederick, 2007). Considering that the recurrent theme of this thesis primarily concerns how economic cooperation is susceptible to emotions in *gratitude* and *indebtedness* (i.e. System 1), the present *cheap-riding* measure which took away the risk assessment (i.e. System 2) part should on the whole add to the legitimacy and validity of the experiments.

6.2. Practical Contributions

While there is no denying that the present thesis is predominately *theoretically* driven, there are two main *practical* implications to be derived from the key findings. They included implication on, a) how practitioners of Gratitude Interventions (i.e. GIs hereafter) could modify the existing regime to boost its efficacy in promoting prosociality (i.e. Sub-section 6.2.1), and b) the observed under-contribution of child support by the non-custodial 'paying' parents (Allbeson, 2016; Savage, 2015) in the UK (i.e. Sub-section 6.2.2).

6.2.1. The Meta-analysis (Chapter 2) and Gratitude Interventions (GI)

The classic, quintessential GI was originated in studies by Emmons and McCullough (2003) and Watkins et al. (2003) during which *participants* (or *trainees*) were instructed to recall and write about a few *gratitude*-inducing events that occurred in their recent past. For example, participants in Watkins et al.'s (2003) *Gratitude* condition were asked to recall and write about things that they did over the previous summer that they felt *grateful* for, whereas trainees assigned to the *Control* condition would simply list things that they did not manage to do during that very same designated period.

Nonetheless, in their recently published meta-analysis Davis et al. (2016) had questioned the efficacy of these typical GI practices in enhancing trainees' well-beings. Their results generally failed to confirm that GI could consistently outperform either the *control* (i.e. measurement-only), or the *alternative activities* (e.g. listing of daily hassles, Emmons and McCullough, 2003) conditions. Meanwhile, via their qualitative review Wood et al. (2010) had also weighed in with their scepticism about the effectiveness of the existing GI regime. While neither Davis et al. (2016) nor Wood et al. (2010) had specifically critiqued the existing GI's efficacy in promoting *prosociality*, the present suggested modifications—which will be briefly discussed below—should still be worth the practitioners' considerations, thanks to the relative *ease* of *implementing* such and the *theoretical* bases behind.

Building on the key findings of the moderator analyses (See Chapter 2 for details), I suggested that practitioners of existing GI practices (e.g. 'Grateful Essay' training, Watkins et al., 2003) could have their trainees write about an experience of 1) being generously treated by a particular person (i.e. *benefit-triggered* (Lambert et al., 2009) gratitude *affect*), and 2) how the

trainees either actually helped, or intended to help that, that particular benefactor back (i.e. *direct* reciprocity). Trainees could also be instructed to think about 3) how their good deeds (e.g. to return others' benevolence) might draw generosity from others when they need help (i.e. 'downstream' *indirect* reciprocity, Nowak and Roch, 2007), or 4) how their feelings of gratitude for someone's kindness could propel them to contribute to others' well-being (i.e. 'upstream' *indirect* reciprocity, Nowak and Roch, 2007). Overall, the meta-analysis should serve to inform and advise the GI practitioners on ways to optimise its efficacy amidst doubts raised in the research community (e.g. Davis et al., 2016; Renshaw & Olinger-Steeves, 2016; Wood et al., 2010).

6.2.2. The Psychology of those Child Maintenance Under-contributors

Additionally, the present analyses on the *cheap-riders'* unique *psychological profiling* (See Chapter 4) should have implications on the phenomenon of non-custodial parents under-contributing (but not outright escaping) their child support obligations. Since each paying parent's contribution is, according to the Child Support Agency (i.e. CSA hereafter), tailored to his/her financial situation—hence removing any concerns about affordability, coupled with the public knowledge on the CSA's inefficiency in retrieving the maintenance arrears (Savage, 2007)—i.e., the absence of repercussions for not paying, I, therefore, drew parallel between these child support under-payments and the observed *cheap-riding* in Experiments 1 to 3.

Particularly, in Chapter 4 I speculated that those under-contributing parents may have experienced a similar emotional conundrum just like the *cheap-riders* in Experiment 2. While their deliberate under-repayment may be indicative of these paying parents' intent to *show retributions* against their

exes for their losses of custody (or for the split) (Stahly, 2000), they were mindful of not going overboard thus crossed the line of a '*Just Desert*' (Moore, 1997; Wenzel & Thielmann, 2006). They may also contribute partially hoping to lessen their sense of guilt over their failure to provide their children with a loving family (Arditti, 1992; Braver et al., 1993)—whereby guilt aversion is a defining attribute of *Experienced Warm-Glow* (Ferguson & Flynn, 2016).

Meanwhile, analyses of Experiment 3(i.e. Chapter 5) indicated that *cheap-riding* is more likely resorted to in an *iterated* rather than a *one-off* exchange context. This could be mapped onto the situation in which a divorced or split couple might still continue to interact with one another for their children's sake. Nonetheless, results of Experiment 3 further illustrated that despite the more prevalent attempts to *cheap-ride* in an *iterated* interactional context, there was no evidence suggesting that *cheap-riding* could be relied on to enforce fairness (or subsequent cooperativeness). In other words, the present data hinted that any under-contributors who wish to use their partial under-payment to serve a correctional purpose might wish to think twice about that.

6.3. Caveats

While I had outlined—in every preceding empirical chapter—the shortcomings or limitations that pertains to each individual chapter, in this section I intend to shed light on four recurring *methodological* concerns of the three present experiments. The following sub-sections (i.e. 6.3.1 to 6.3.4) will also entail discussions on, (i) what have been done at present to address these issues (i.e. Sub-section 6.3.1), or (ii) alternative measures to be taken in the future as to properly tackle each of these concerns (i.e. 6.3.2 to 6.3.4).

6.3.1. Statistical Power and the Null Findings on the *Trait-Prosociality* links

One major methodological limitation of Experiments 1 to 3 concerns the absence of any a-priori power analyses to determine the minimum required *sample sizes* (*N*) to detect the hypothesised effects (Everitt, 2002). This may account for the present list of insignificant findings—throughout Experiments 2 to 3—as regards the link between *dispositional* gratitude (and indebtedness) and prosociality (or the absence of sanctioning).

To examine whether insufficient *N*s were indeed to blame for the null findings I conducted a post-hoc power analysis using the effect estimates of the *trait* gratitude-*prosociality* link from the meta-analysis (i.e., r (5,988) = 0.248, p <.001 (two-tailed); see Chapter 2). G*Power Version 3.1.9.2 (Faul, Erdfelder, Buchner, & Lang, 2009; Faul, Erdfelder, Lang, & Buchner, 2007) was used to carry out this analysis. The results revealed that with power equals 0.80, a total *N* of 125 would be warranted for the effect (i.e. r = 0.248) to be significant at $p \leq .05$ (two-tailed). While the actual Ns of both Experiments 2 (N = 135) and 3 (N = 133) may have exceeded the suggested N of 125, it should be noted that in Experiment 2 only 87 data points—thanks to the high rate of rejections (N = 48)—were used to examine how *dispositional* gratitude was related to participants' *trustworthiness*. As a result, it is conceivable that the list of insignificant findings—especially on the *trait*-prosociality associations—of Experiment 2 could be due to the insufficient N; while Experiment 3 appeared reasonably powered despite the fact that a similar pattern of null findings were still observed (See Chapter 5 for details).

I also tested if Experiments 1 to 3 were adequately powered to examine the *state* gratitude-*prosociality* association. A similar power analysis, while

adopting the effect estimates of the *state* gratitude-*prosociality* link from the meta-analysis (i.e., r (14,206) = 0.393, p <.001 (two-tailed); see Chapter 2), was carried out using G*Power (Faul et al., 2009; Faul et al., 2007). The results showed that with power equals 0.80, a total N of 48 would be warranted for the effect (i.e. r = 0.393) to be significant at $p \leq .05$ (two-tailed). Hence, the present three empirical studies (i.e. N = 49 for Experiment 1, N = 87 for Experiment 2; and N = 133 for Experiment 3) should be sufficiently powered to detect the association between *state* gratitude and *prosociality*.

6.3.2. Over-reliance on Offer Provisions as the Current Gratitude Induction

Throughout Experiments 1 to 3 I relied solely upon the provision of a financial benefit (i.e. *conditional* or *unconditional* offers) to induce recipients' momentary feelings of *gratitude* (or *indebtedness*). Leung (2011) contended the logic behind was that since contributing in an anonymous game setting involves i) *sacrificing self-interest* and ii) running the *risk* of being taken advantage of, hence the recipients would be inclined to think of those contributions as a token of *benevolence* from the givers—which accordingly elicits the former's feeling of appreciation toward the latter (Wood et al., 2011). However, unlike a classic Public Goods game (i.e. PG hereafter) where Leung (2011) based her above argument on, a contribution in the present experimental set-ups is clearly different from that in a PG in several aspects.

First, a *conditional* offer (especially the offers with high surcharge, e.g. the 25%- or 50%-Interest ones) at present is hardly indicative of the helper's sacrifice of self-gains. Although recipients in Experiments 3 might perceive their helpers to have incurred a default risk making an offer, the same could barely apply to Experiments 1 and 2. Indeed, I suggested that the implied

binding nature may account for why recipients (in particular Experiment 2) may have instead utilised their options to accept or reject as the avenue to vent out their frustrations toward their helpers (See Chapter 4 for details). Thus, there is no reason to believe that a recipient in Experiments 1 or 2 would feel that his/her *conditional* offer giver would have incurred any *risk* for such a quasi-prosocial gesture. All in all, the above suggests that it might be arbitrary, or even erroneous, to take it for granted that an offer provision under the present gaming parameters would constitute a legitimate *gratitude* (or *indebtedness*) induction as Leung (2011) did in the context of the PGs.

Hence, a future investigation may consider including a specific gratitude induction session in the form of a *cover task* prior to the economic game in which prosociality or cooperation is measured (e.g. Bartlett et al., 2012; Desteno et al., 2010; Tsang et al., 2012). For instance, Desteno et al. (2010) had participants undertook a computerized word perception task—during which they would encounter a 'technical failure' only to be then 'resolved' by the confederates (i.e. the '*Gratitude Manipulation*', Study 1 to 3, Bartlett & Desteno, 2006)—prior to partaking in a Give-Some Dilemma Game (Nelissen et al., 2007; Parise et al., 1999) in which *cooperation* was examined.

6.3.3. Concerns with Playing with *Real* versus *Simulated* Counterparts

Aside from Experiment 1 during which participants *actually* played with one another, participants of Experiments 2 and 3 were indeed interacting with a set of *pre-programmed responses* as opposed to real persons. While it is not uncommon to administer trust games with *simulated* instead of *real* counterparts (e.g. Evans & Revelle, 2008; Johansson-Stenman et al., 2009), Mislin and associates (Bottom, Holloway, Miller, Mislin, & Whitford, 2006;

Johnson & Mislin, 2011) noted that belief about playing with a computer (vis-à-vis a real person) should give rise to different in-game decision-making. In particular, Johnson and Mislin (2011) underlined that—via their meta-analysis over 162 Trust Games—people tended to be more *trustful* when they were convinced that they were playing with *real* persons, although the authors did *not* examine whether this belief would also influence people's *trustworthiness*.

While in Experiments 1 to 3 I had not specifically probed into participants' beliefs about whether they were playing with real persons, there were indeed *occasional* post-experiment inquiries or even doubts expressed in relation to the 'authenticity' of one's exchange 'partners'. As such, in any future investigations the experimenter may consider adopting Experiment 1's (random) *partner-matching* design, but meanwhile maintaining the control over certain key parameters (e.g. i) whether or not, and ii) what, offers are given) just like Experiments 2 and 3. For example, similar to Experiment 1 the experimenter might stipulate a recipient to wait his/her turn while the partner is taking the time necessary to decide, thus hinting to the former that the whole episode is *not* simulated. In the meantime, while both the *partnering* and *waiting* may be real, participants may still be shown the indeed pre-determined 'partner's decisions'—just like what was done in Experiments 2 and 3.

6.3.4. Issues with the Forced-choice Repayment (i.e. Experiments 2 and 3)

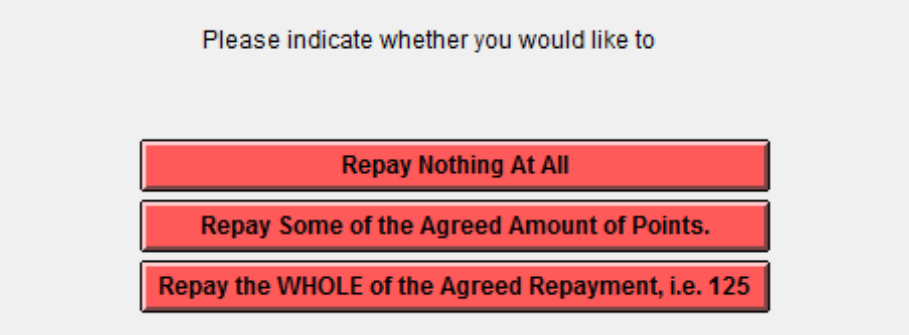

Figure 6.3. The *Forced-choice* Repayment Screen in Experiments 2 and 3

One additional recurring methodological concern involves the *forced-choice* entries of participants' repayment in Experiments 2 and 3. Offer *acceptors* in these two experiments were cornered into making a selection out of whether to 1) repay 'the *whole* of the agreed repayment' (i.e. *Cooperation*); 2) repay '*some* of the agreed amount' (i.e. *Cheap-riding*), or to 3) repay '*nothing* at all' (i.e. *Free-riding*) (See Figure 6.3).Participants who chose to cheap-ride were then instructed to indicate their repayment (See Figure 6.4).

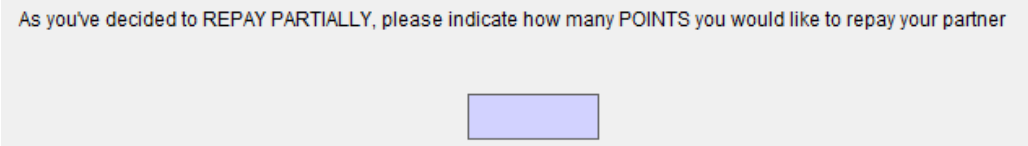

Figure 6.4 Follow-up Entry Screen for Acceptors who *Partially* Repaid

While such an explicit segregation may have eased the categorization based on the participants' repayment, it may also give rise to the 'Response Effect': whereby respondents may have relied upon the given response sets to guide their preferences or decisions (see Feldman, 1989; Zaller and Feldman, 1992). In other words, such an explicit mention of the available repayment avenues might have inadvertently swayed the participants toward repaying in a particular fashion. Therefore, it may be advisable for any future replications to replace the current *forced-choice* entry with a *free-entry* measure. As illustrated in Figure 6.5, the experimenter might just leave it *open* to the participants to *freely* key in how much they intend to give back to the helpers.

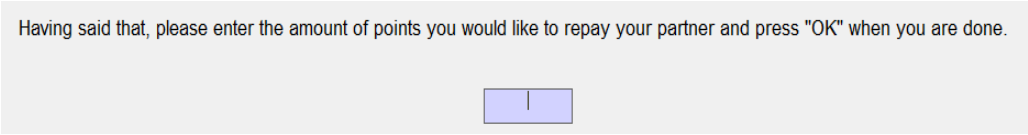

Figure 6.5 The '*Free-Entry*' Repayment Measure as suggested

6.4. Conclusion

The current thesis primarily concerns the scrutiny over the *gratitude-prosociality* association in an economic exchange context. Via the meta-

analysis (i.e. Chapter 2) and three empirical studies (i.e. Chapters 3 to 5) in the form of economic games, the results overall confirmed the role of *state* gratitude in *facilitating* economic *cooperation* in addition to *restraining sanctioning*. Crucially, the present thesis helped to fill a gap in the literature by combining *Psychometrics* (i.e. measurement of personality and attitudes via various surveys) and *Experimental Economics* (i.e. the use of economic games) in the analyses of how *gratitude*, and a similar benefit-triggered emotion in *indebtedness*, would predict people's subsequent economic decisions. Using both the *single-role*, *one-shot* (Experiments 1 and 2) and *multiple-role*, *iterated* (Experiment 3) game designs the present thesis also shed light upon an economic phenomenon which has both *reality* and *theoretical* implications—that is, *cheap-riding*. While the present analyses failed to indicate any associations between *gratitude* (or *indebtedness*) and *cheap-riding* in particular, the results had nonetheless demonstrated the unique psychological profiling of people who still chose to *cheap-ride* when they could have 'free-rode' instead. The data also partially acknowledged a norm-enforcing, functional aspect of *cheap-riding*, although its efficacy in fairness enforcement would warrant confirmation in future related investigations.

All in all, I sincerely hope that my thesis could inspire any prospective investigators to carry on their endeavours to contribute to the understanding of the *prosocial* side of *gratitude*. I also hope that my thesis could encourage further efforts into the study in relation to the role of *indebtedness*—as a psychological construct—in economic exchanges.

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Appendices

Appendix 1. Scale Items of the Marlowe-Crowne Social Desirability Scale (Form-C) (Crowne & Marlowe, 1960)

Listed Below are a number of statements concerning personal attitudes. Please read each item and decide whether the statement is *TRUE* or *FALSE* as it pertains to you personally

1. It is sometimes hard for me to go on with my work if I am not encouraged*.
2. I sometimes feel resentful when I don't get my way*.
3. On a few occasions I have given up doing something because I thought too little of my ability*.
4. There have been times when I felt like rebelling against people in authority, even though I knew they were right*.
5. No matter who I'm talking to, I'm always a good listener**.
6. There have been occasions when I took advantage of someone*.
7. I am always willing to admit when I made a mistake**.
8. I sometimes try to get even rather than forgive and forget*.
9. I am always courteous, even to people who are disagreeable**.
10. I have never been irked when people expressed ideas very different from my own**.
11. There have been times when I was quite jealous of the good fortune of others*.
12. I am sometimes irritated by people who ask favours of me*.
13. I have never deliberately said something that hurt someone's feelings**.

* Add 1 point if FALSE is marked

** Add1 point if TRUE is marked.

Appendix 2. Scale Items of the Impression Management Scale (i.e. IMS, Paulhus, 1991)

Using the scale below as a guide, please indicate how much you agree with the following statements. There are no Right or Wrong answers.

('1'= *Not* True to '7' = *Very* True)

1. I sometimes tell lies if I have to*.
2. I never cover up my mistakes.
3. There have been occasions when I have taken advantage of someone*.
4. I never swear (i.e. use foul languages).
5. I sometimes try to get even rather than 'forgive and forget'*.
6. I always obey laws, even if I am unlikely to get caught.
7. I have said something bad about a friend behind his/her back*.
8. When I hear people talking privately, I avoid listening.
9. I have received too much change from a salesperson without telling him/her*.
10. I always declare everything at customs.
11. When I was young I sometimes stole things*.
12. I have never dropped litter on the street.
13. I sometimes drive faster than the speed limit*
    (Please leave this item blank in case you do not drive).
14. I never read sexy books or magazines.
15. I have done things I don't tell other people about*[1].
16. I never take things that don't belong to me.
17. I have taken sick-leave from work or school even though I wasn't really sick*.
18. I have never damaged library book or store merchandise without reporting it.
19. I have some pretty awful habits*.
20. I don't gossip about other people's business.

* Reverse-scored for the computation of the IMS composite score.

[1]Item 13 was removed when re-administered in Experiment 3.

Appendix 3. Scale Items of the Gratitude-Questionnaire-VI (McCullough, Emmons, & Tsang, 2002)

Please indicate, using the scale below, how the following items describe yourself. There are no Right or Wrong answers.

('1'= *Strongly* Disagree; '2'= Disagree; '3'= *Slightly* Disagree; '4'= Neutral; '5'= *Slightly* agree; '6'= Agree; '7'= *Strongly* Agree)

1. I have so much in life to be thankful for.
2. If I had to list everything that I felt thankful for, it would be a very long list.
3. When I looked at the world, I don't see much to be thankful for (reverse scored).
4. I am thankful to a wide variety of people.
5. As I get older I find myself more able to appreciate the people, events, and situations that have been part of my life history.
6. Long amounts of time can go by before I feel grateful to something or someone (reverse scored).

Appendix 4. Scale Items of the Indebtedness-Scales Revised (Elster, Maleki, McLeod, & Watkins, 2005)

Please indicate, using the scale below, how the following items describe yourself. There are no Right or Wrong answers.

('1'= *Strongly* Disagree; '2'= Disagree; '3'= *Slightly* Disagree; '4'= Neutral; '5'= *Slightly* agree; '6'= Agree; '7'= *Strongly* Agree)

1. If a friend did me a favour, I would make sure to repay them as quickly as possible.
2. Owing someone a favour makes me uncomfortable.
3. I would not borrow money from a friend unless it was absolutely necessary.
4. Asking for another's help gives them power over your life.
5. Never a borrower or a lender be.
6. I'd be embarrassed if someone had to remind me of a debt I owed them.
7. As a rule, I don't accept a favour if I can't return the favour.
8. If someone paid for my dinner or invited me to eat at their place, I would feel obligated to buy the dinner the next time or to invite them to eat at my place.
9. I would be very upset if I discovered that I had forgotten to return something I borrowed.
10. If someone goes out of their way to help me, I feel as though I should do more for them than merely return the favour.
11. When someone does me a favour it often bothers me because I immediately wonder how I will repay them.
12. I like to make sure I don't owe anybody anything.
13. I find myself worrying about whether I have repaid all the favours I have received.
14. When someone gives me something or provides a favour to me, I usually feel somewhat uncomfortable at first.
15. I'd rather do things myself than have someone help me because I wouldn't like feeling obligated to return their favour.
16. I don't receive gifts very well.
17. If someone bought me an expensive gift, I would worry a lot about whether I would be able to repay them.
18. In good friendships you should make sure that you pay back all the favours you have received from your friend.
19. If someone does me a favour, I usually try to pay them back as soon as possible.
20. I would be uncomfortable right now if someone surprised me with a large or expensive gift.
21. Being able to repay a favour or gift brings me great relief.
22. I have trouble enjoying gift from others because I am concerned about what I would give them in return.

Appendix 5. On-screen Instructions for Experiment 1

Example: A P1 (Potential Helper)-P2 (Potential Recipient) pair in *'Low-cost'* condition with P1 making a *Conditional Offer* and P2 *Accepting* it

I. Opening (All participants)

Each of you is given 150 points at the beginning of this game. In this game there is a bonus threshold of 200 points, i.e. you will be entitled to a bonus (in addition to your £2-inconvenience allowance) only if you manage to hit 200 points (or above).

Each point above the threshold (200 points) is worth 1p in addition to the basic bonus £2. For example, if your final score is 300 points, the total bonus for you will be: Basic bonus for reaching 200 points: £2 + Extra bonus: (300-200)* 1p= £ 1→Total £ 3 The maximum points you can get is 350, meaning that you could at most end up with a bonus of £3.50 (£2-basic bonus+150*1p)

In this game you will be asked to roll a fair die once, and afterwards enter the number you get to the computer. The system will then generate a CHANGE in your score based on the number you get out of the die-rolling. Dependent on your die-rolling results, there are SIX possible changes could be brought to your final scores, namely: i) -50, ii) Zero change, iii) +50, iv) +100, v) +150, vi) +200.

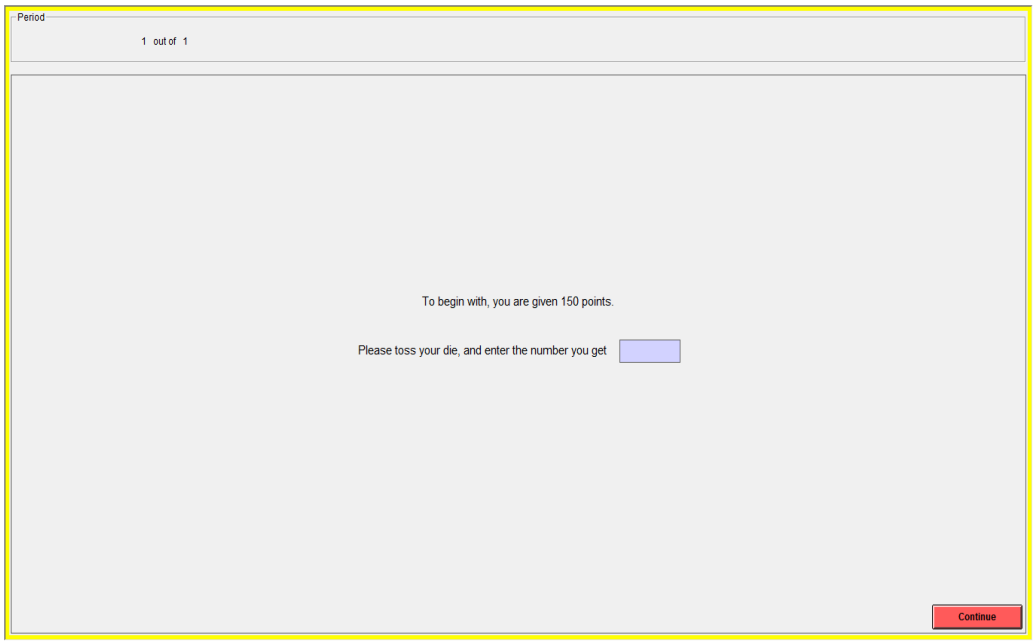
Please note that a large number rolled does not necessarily mean a more favourable change and vice versa. Instead, the level of your final bonus is jointly determined by WHAT YOU AND THE OTHER PARTICIPANTS HAVE GOT; in a way that the RARER number you get relative to others, the MORE FAVOURABLE change in your score. For instance, If there are 20 players and you roll '1' and five others roll '2', another five people roll '3', four people get a '5' and five others roll '6', you'll get the most bonus because you had the rarest number.

In this game, everyone has a partner. Nevertheless, neither joint effort nor competition with your partner will be required to capture the bonus. You and your partner are NOT IDENTIFIABLE to each other and will remain anonymous. The payoff of yours and your partner's will be shown on-screen shortly after you key in the number you get after rolling the die.

Please note that in this gaming session partners interact with one another and they will at times take turn to make decisions. As a result, you will see a couple of waiting screens while your partner is making his/her decision. A few waiting screens could last for 5-6 minutes, depending on how many decisions your partner is asked to make. Similarly, while you are asked to make a series of decisions, it will be your partners' turn to be shown the waiting screen.

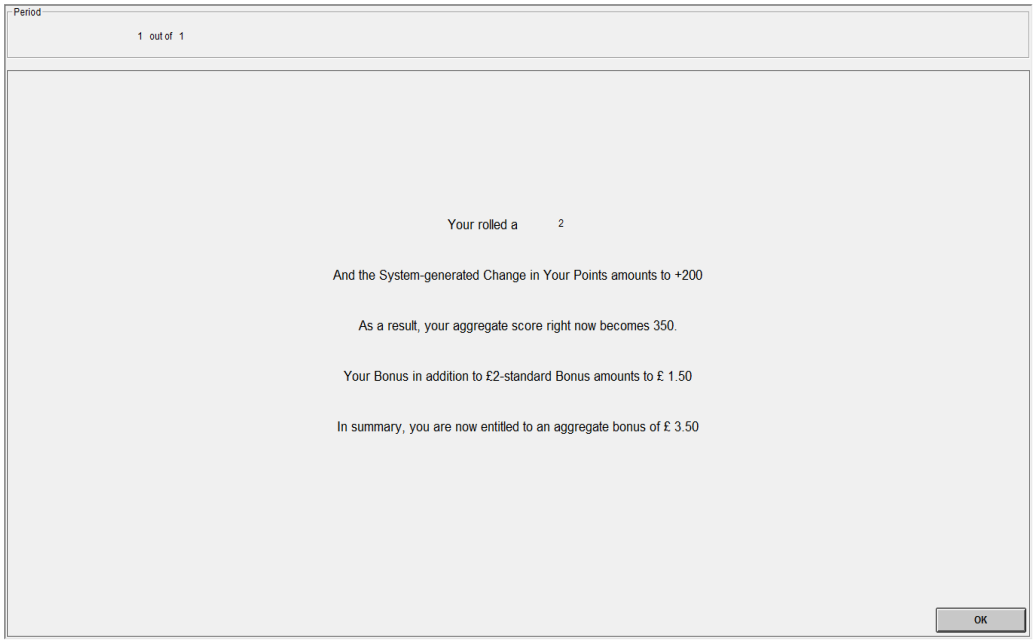
Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

II. Die Rolling and Entry of results (All Participants)



III. Display of INITIAL payoffs of players and that of their partners

Player 1s : **Own** Payoff Display



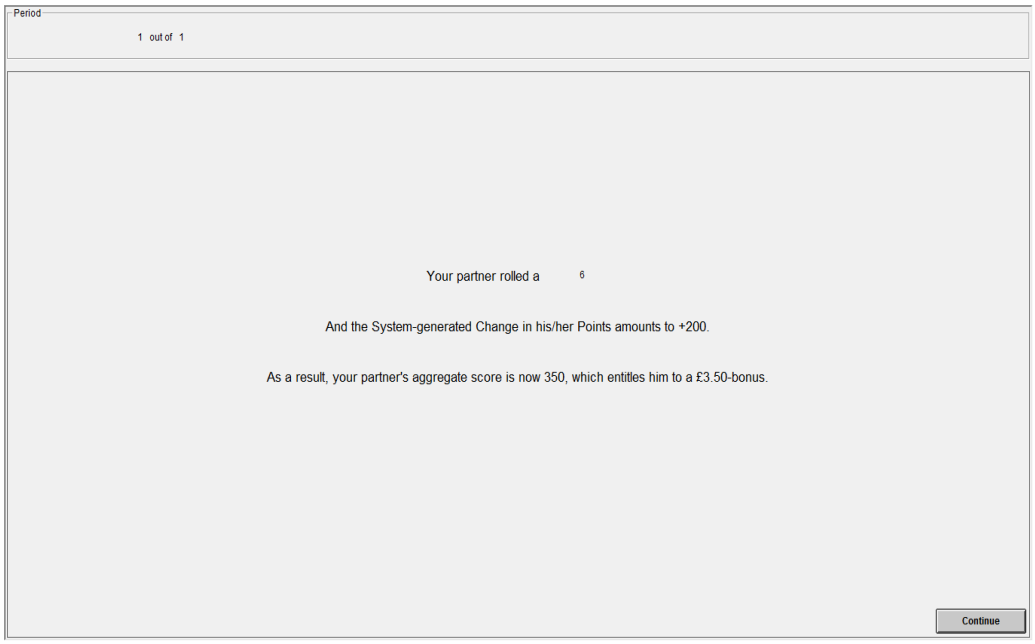
Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

Player 2s : **Own** Payoff Display

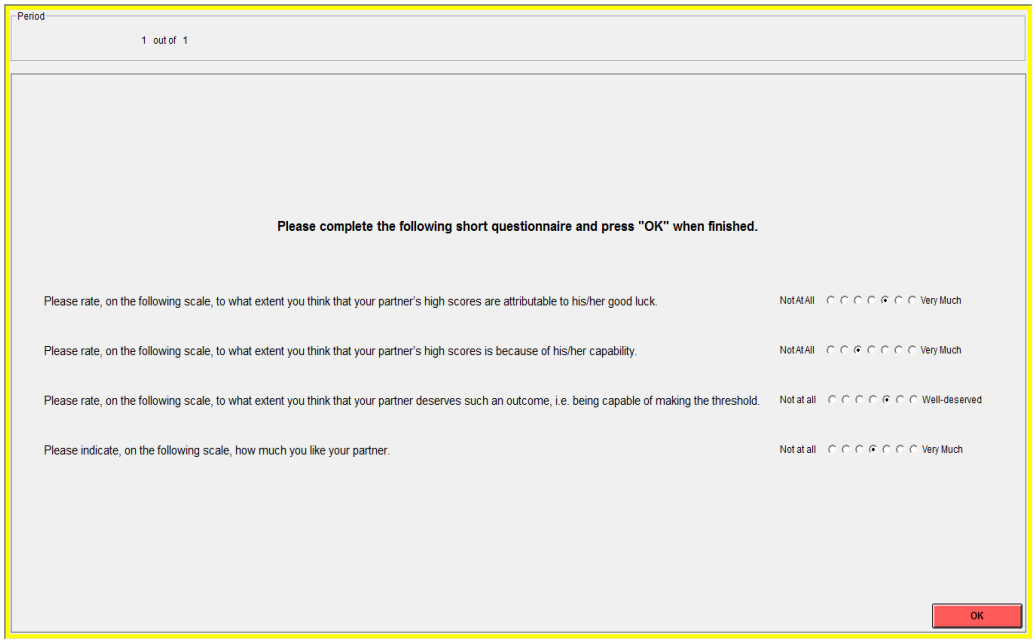
Player 1s: **Partners'** Die-rolling Score (and Payoff) Display

Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

Player 2s: **Partners'** Die-rolling Score (and Payoff) Display



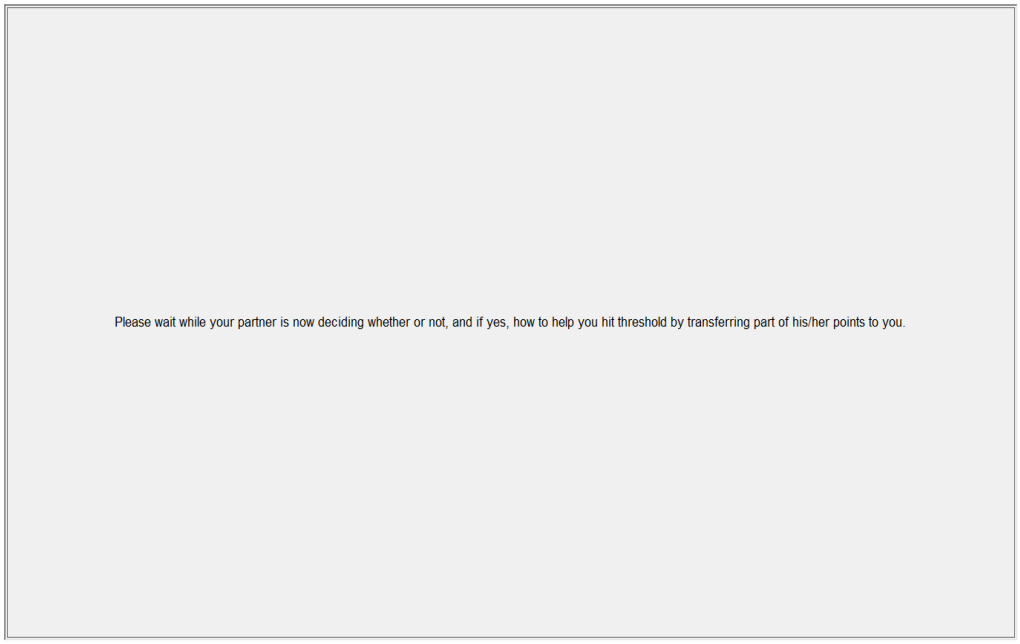
IV. Pre-offer Ratings: Attributions of partners' Die-rolling (e.g. Player 2s)



Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

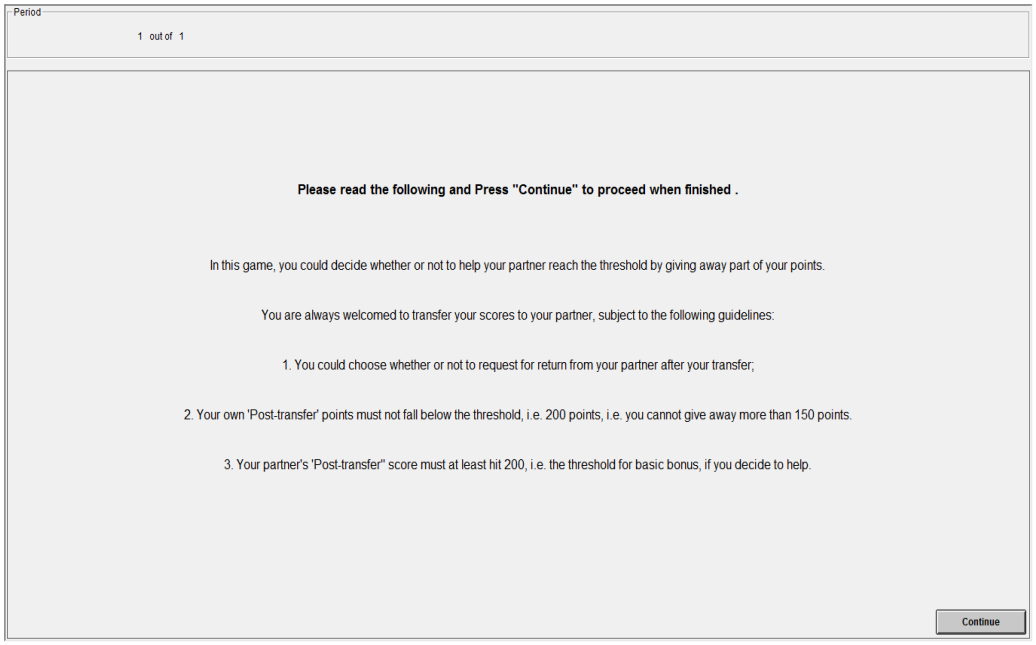
V. Waiting Screen (Player 2s) and Helping decisions (Player 1s)

a. Player 2s' Waiting Screen (while P1s made helping decisions)



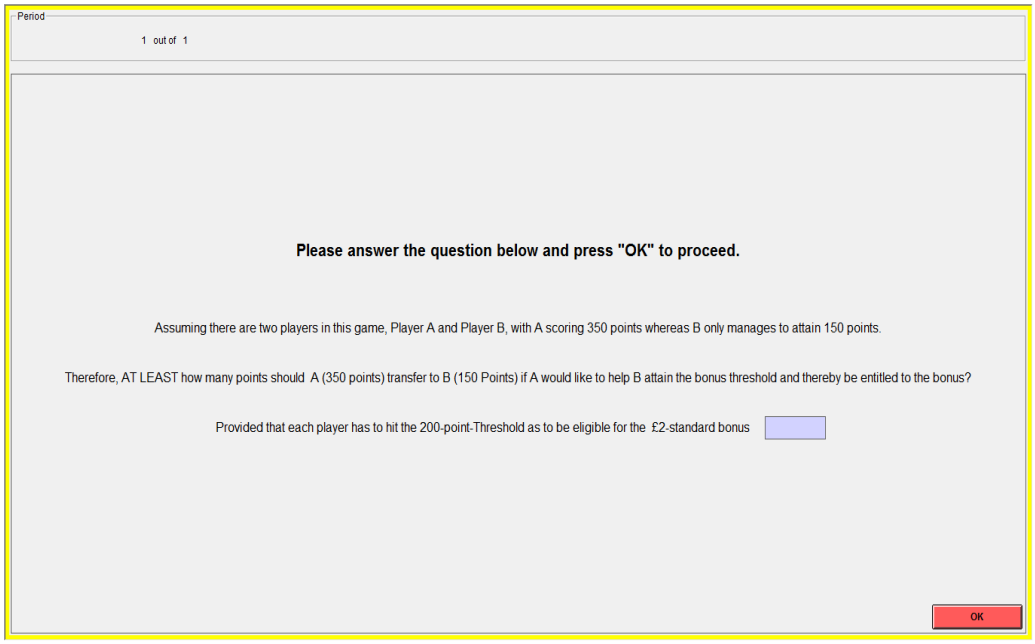
b. Helping Decisions by Player 1s

bi. Rules of Making an offer (Player 1s)



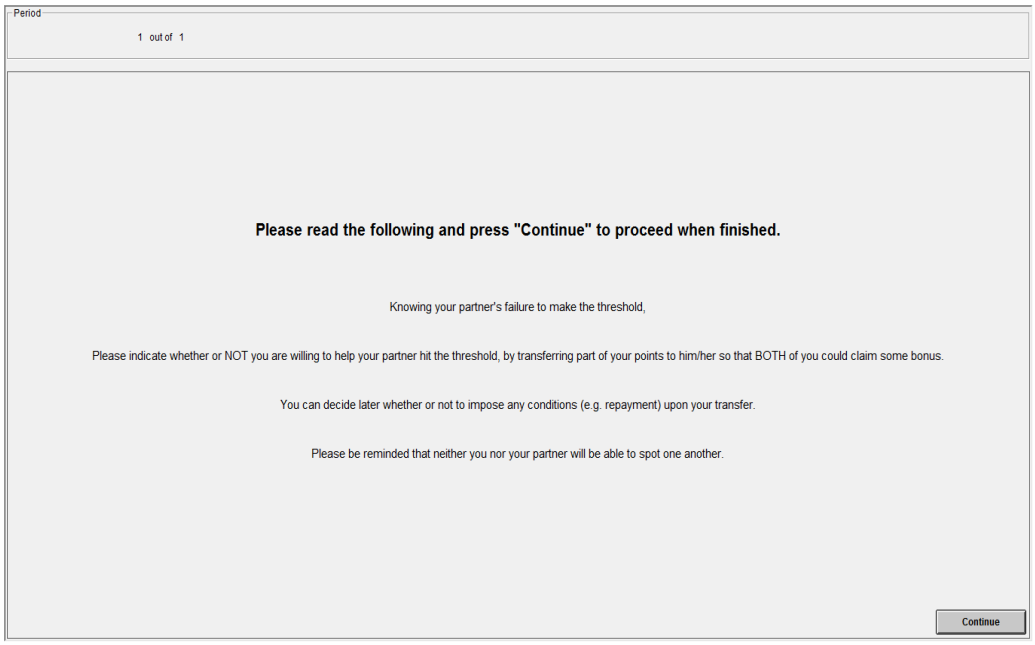
Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

bii. Understanding Check for the Rules of Transfer



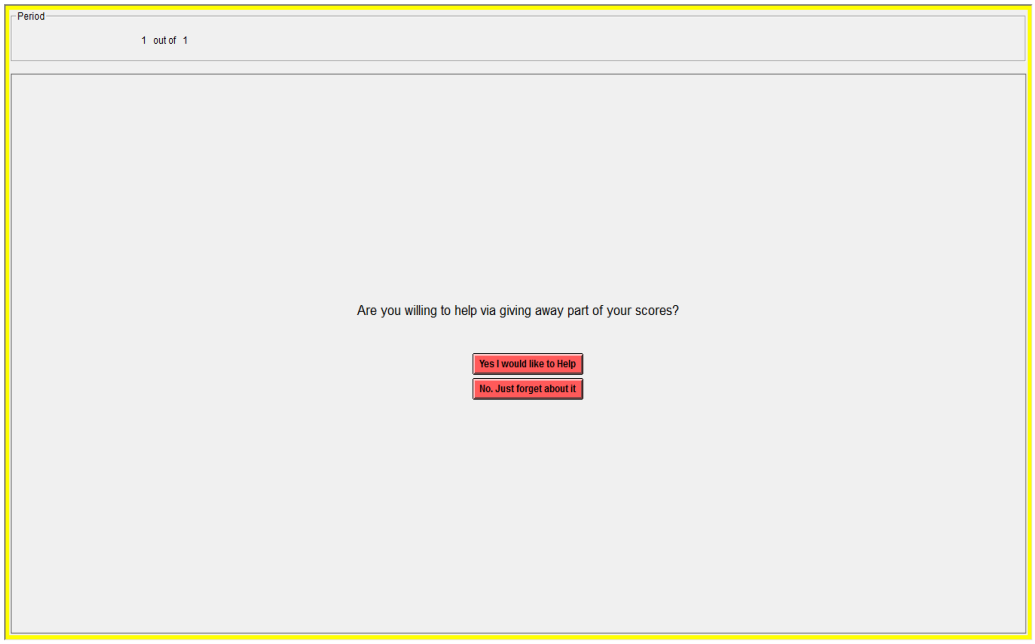
biii. Player 1s' Decision to Make an Offer (I to III)

I) Basic Ground Rules and Notification of Partners' 'Need for Help'

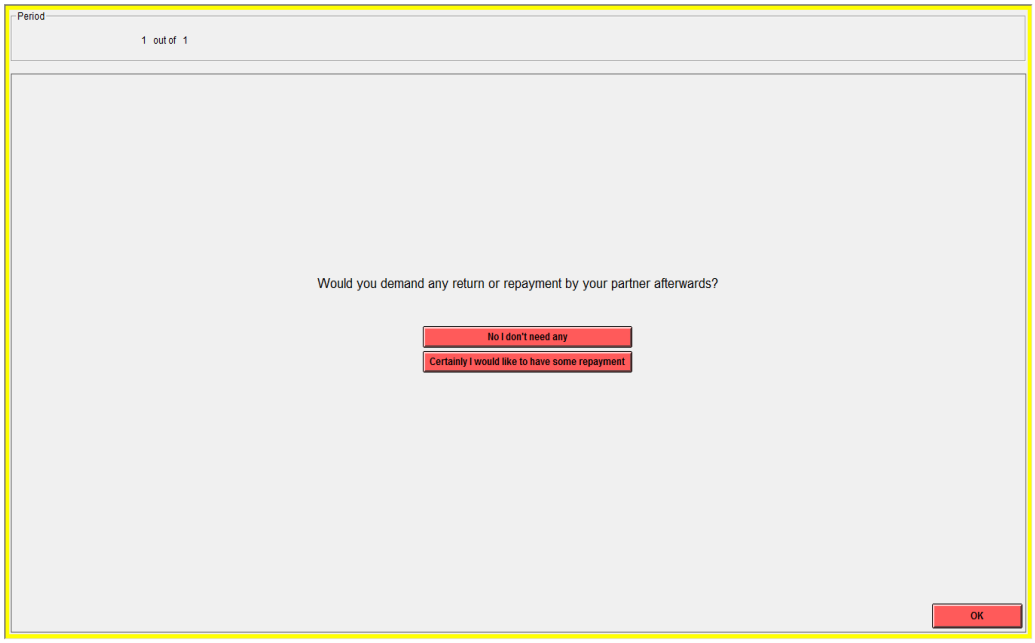


Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

II) **To Help or Not** to Help



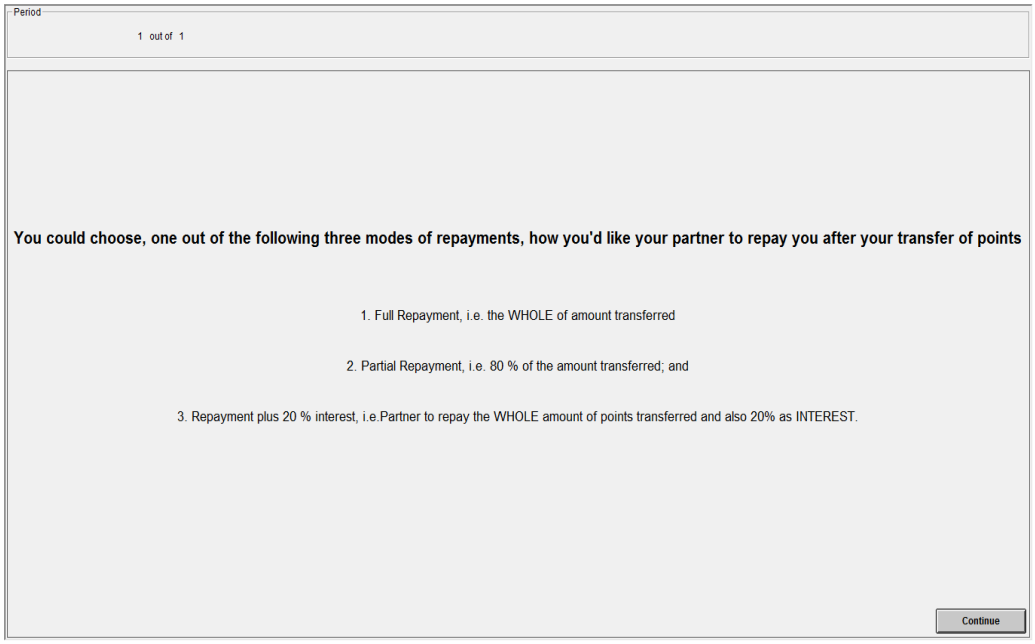
III) **Conditionality** of Offer: Whether to impose a *'repayment clause'*?



Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

biv. Player 1 after agreeing to make a Conditional Offer (I TO III)

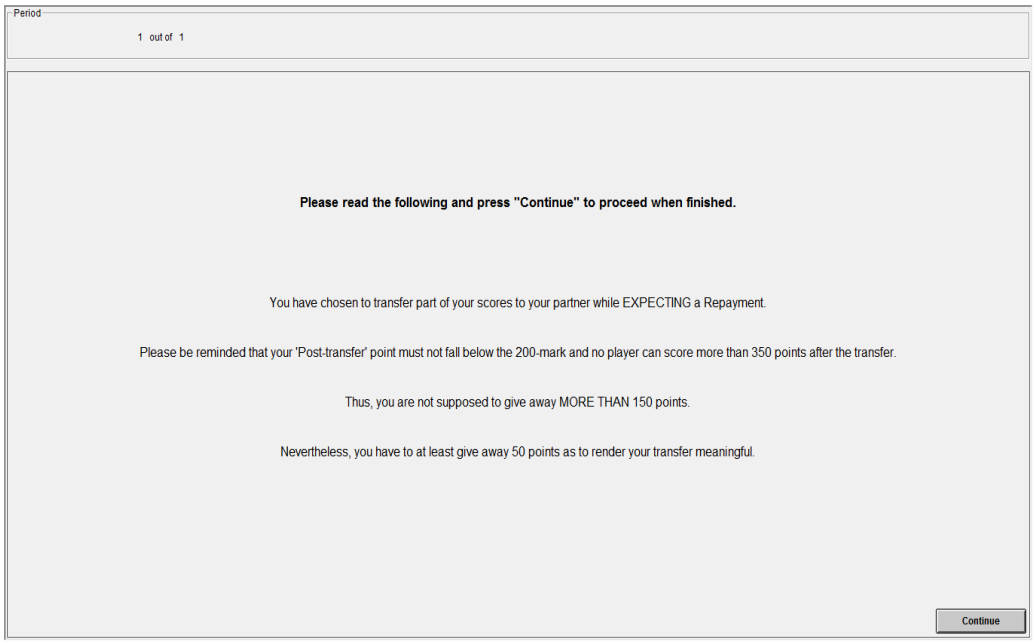
(I) The **list of Available** 'Repayment Clauses'



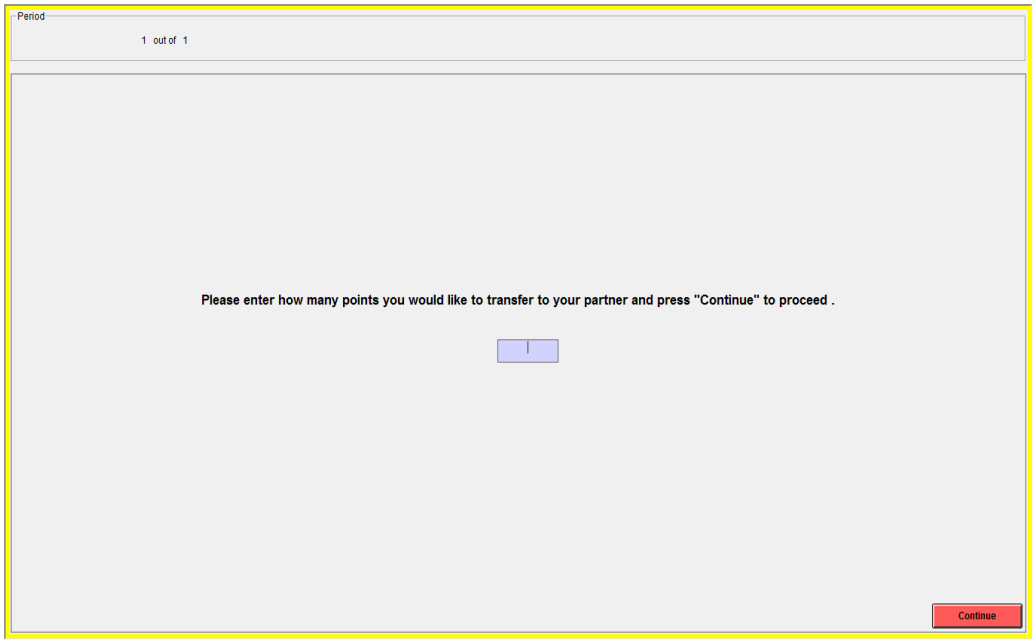
(II) Picking a Condition : *Full* vs. *Partial* vs. *Repayment* Plus *Interest*

Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

(III) A Reminder: Rules of Making a Transfer



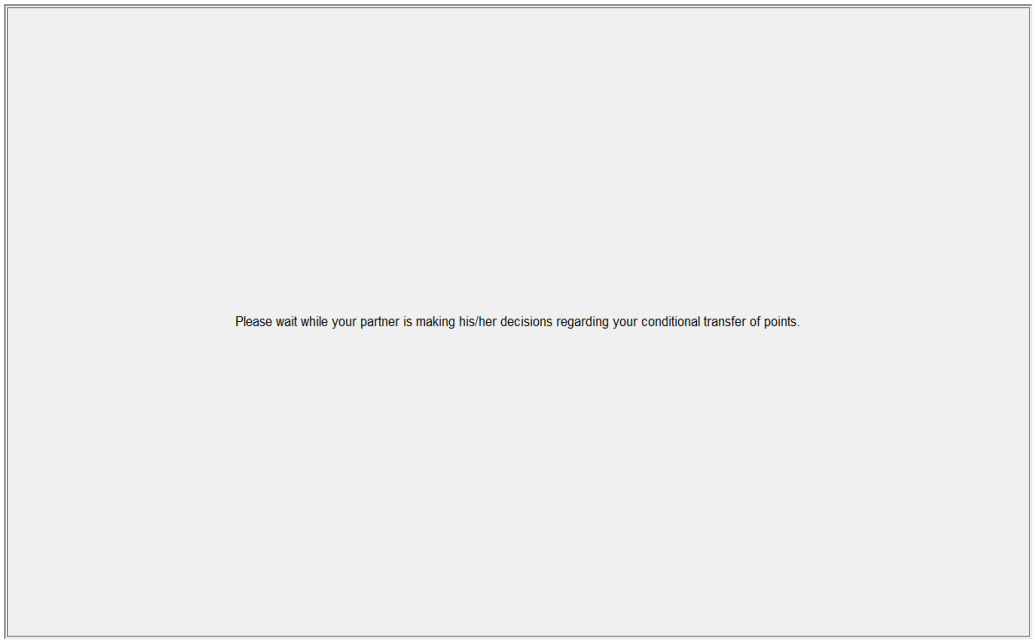
(IV) Deciding the *Magnitude* of Transfer



Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

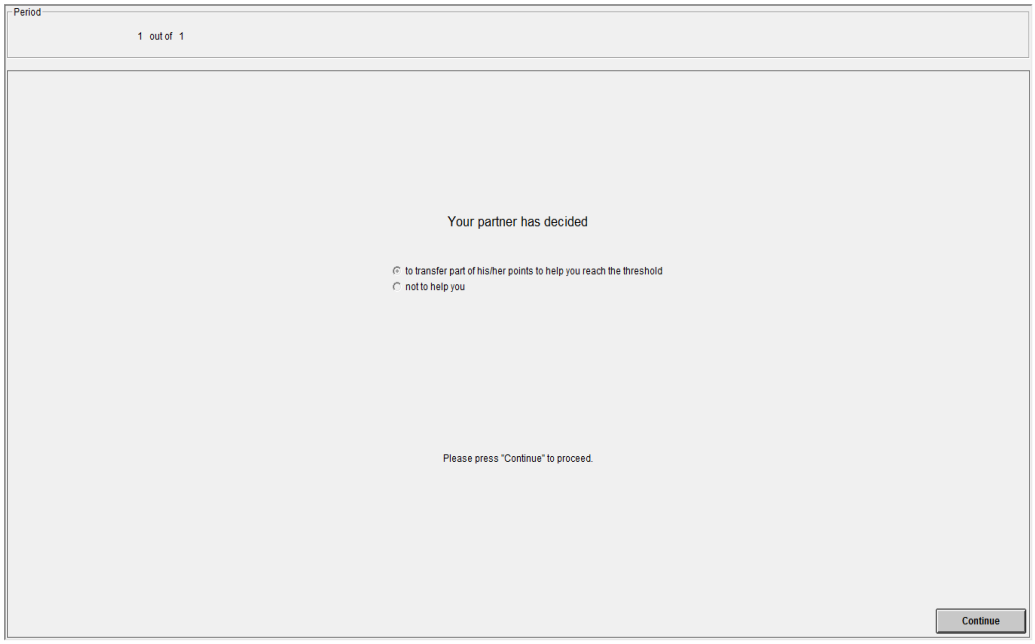
V. Offer Acceptance, Post-offer Ratings, and Repayment for Player 2s

a. Waiting Screen for Player 1s (while Player 2s made their decisions as regards their offers received)



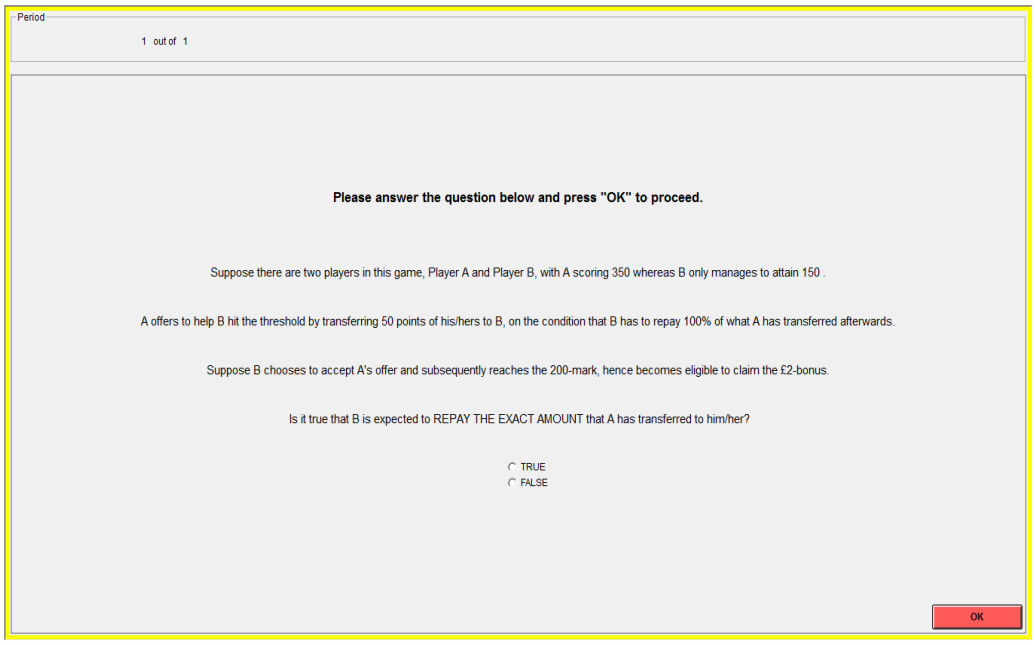
bi. Offer Display and Acceptance (or Rejection) for Player 2s (1 to 4)

(1) Partners' Decisions (*To Help or not to have helped*)

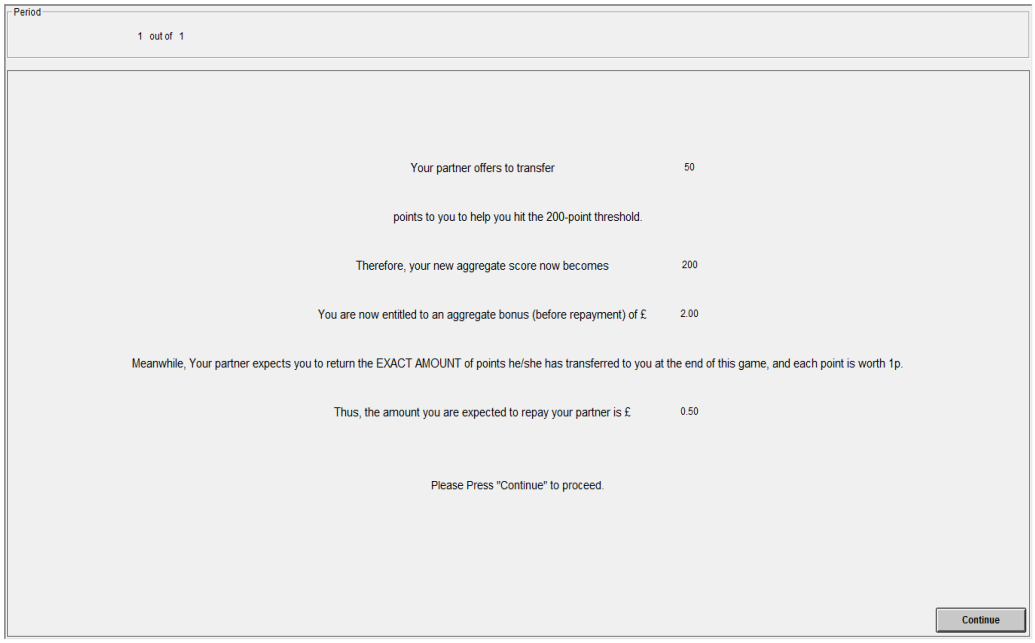


Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

(2) Understanding Check

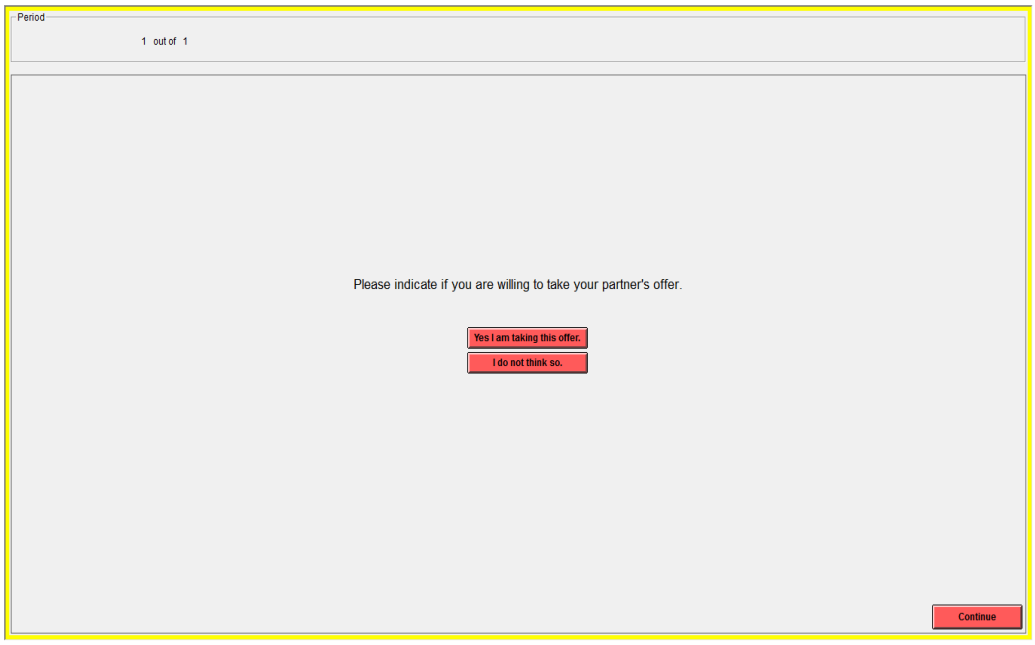


(3) Offer Display

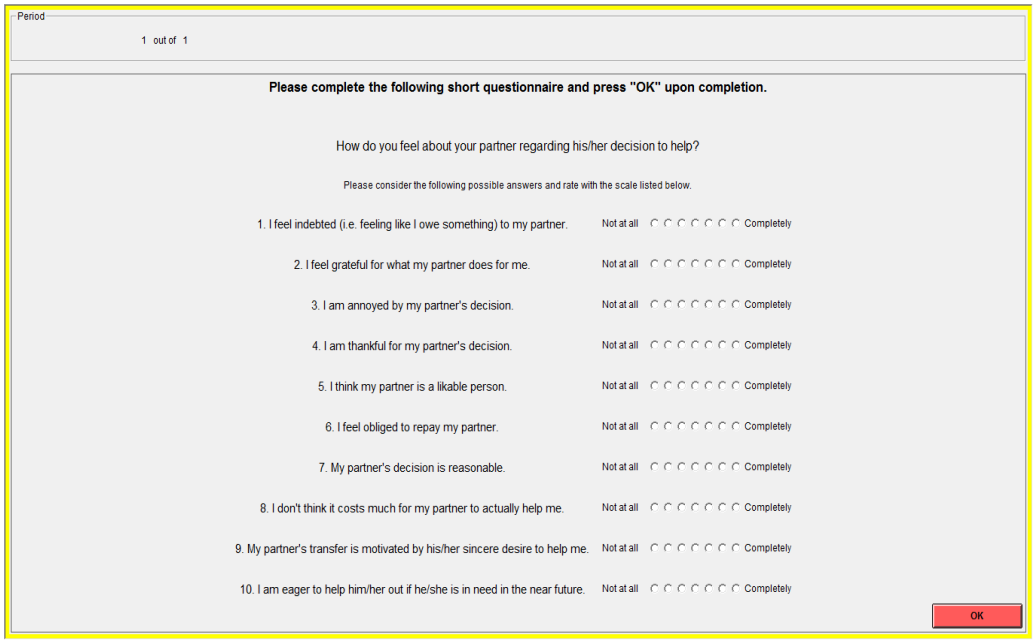


Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

(4) Acceptance or Rejection of the Offer



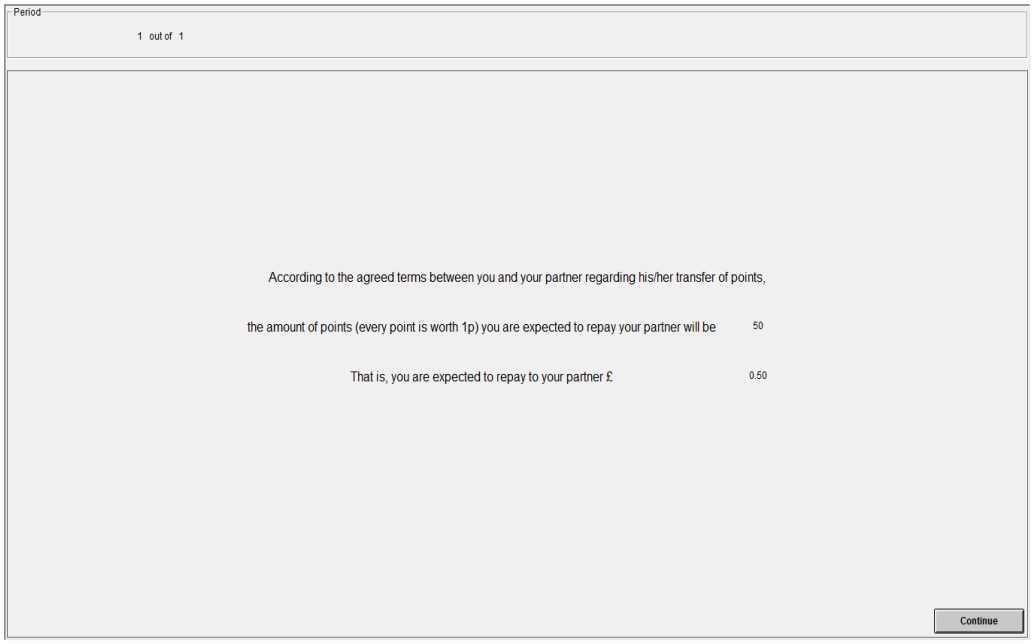
bii). Post-offer Ratings by Player 2s



Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

biii. Repayment Screens for Player 2s who **Accepted** their *Conditional* Offers

(1). A Reminder: 'Terms' of the Repayment

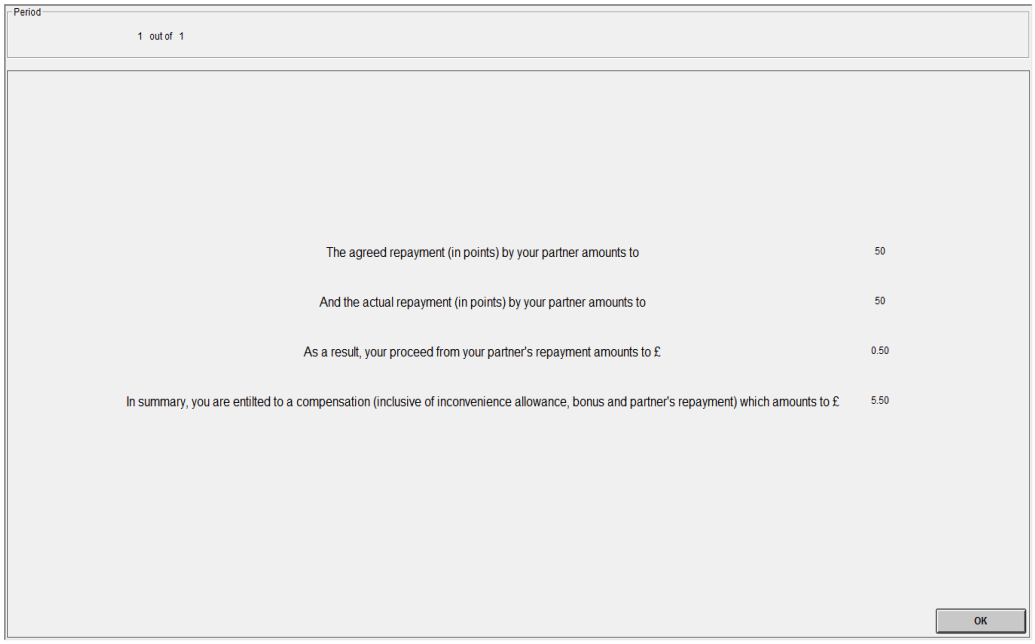


(2) Repayment Decisions: P2s to **Freely Decide** their *Magnitude of Repayment*

Appendix 5. On-screen Instructions for Experiment 1 (Cont.)

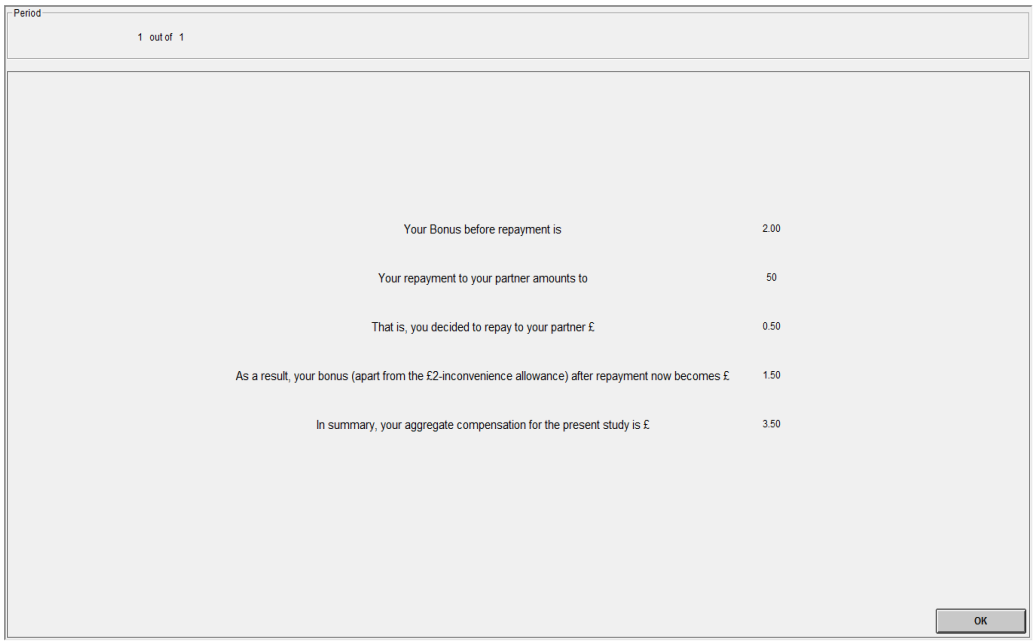
VI. Compensations Display

Player 1s- CONDITIONAL Helpers whose recipients *did not defect*



Player 2s - Recipients of *Conditional* offers and **Repaid Fully**

(i.e. *NO* 'breaching of contract')



Appendix 6. On-screen Instructions for Experiment 2

Example: A Participant who *Accepted* an 'Interest-Free' offer and *Partially Repaid* (i.e. Cheap-ride)

I. Opening (All participants)

Each of you is given 150 points at the beginning of this game. In this game there is a bonus threshold of 200 points, i.e. you will be entitled to a bonus (in addition to your £2-inconvenience allowance) only if you manage to hit 200 points (or above). Each point you get will be equivalent to 1p, if and only if your final score is at or above the threshold 200 points. For example, if your final score is 300 points, the total bonus for you will be 300 Pence, i.e. £3, as you manage to make the threshold. However, a final score of 150 (or any scores below 200) would yield you ZERO Bonus as you fail to reach the threshold.

The maximum points you can attain is 350, meaning that you could at most end up with a Bonus of 350 Pence, i.e. £3.50. In this game you will be asked to roll a fair die once, and afterwards enter the number you get to the computer. The system will then generate a CHANGE in your score based on the number you get out of the die-rolling. Dependent on your die-rolling results, there are SIX possible changes could be brought to your final scores, namely: i) -50, ii) Zero change, iii) +50, iv) +100, v) +150, vi) +200. Please note that a large number rolled DOES NOT NECESSARILY GUARANTEE a more favourable change and vice versa. Instead, the level of your final bonus is the outcome of the system's RANDOM assignment. The system would assign at random ONE of the SIX abovementioned outcomes to ONE of the SIX numbers on the die. That is, there is an equal opportunity for you to achieve either of the six abovementioned possible changes.

In this game, each of you would play alongside a partner. Please be reminded that NEITHER 1. Joint Effort, NOR 2. Competition, with partners will be necessary for each of you to secure the bonus. You and your partner are NOT IDENTIFIABLE to one another and will remain anonymous throughout this study. Throughout this gaming session you and your partner would take turn to make decisions that might influence each other's Final Payoffs.

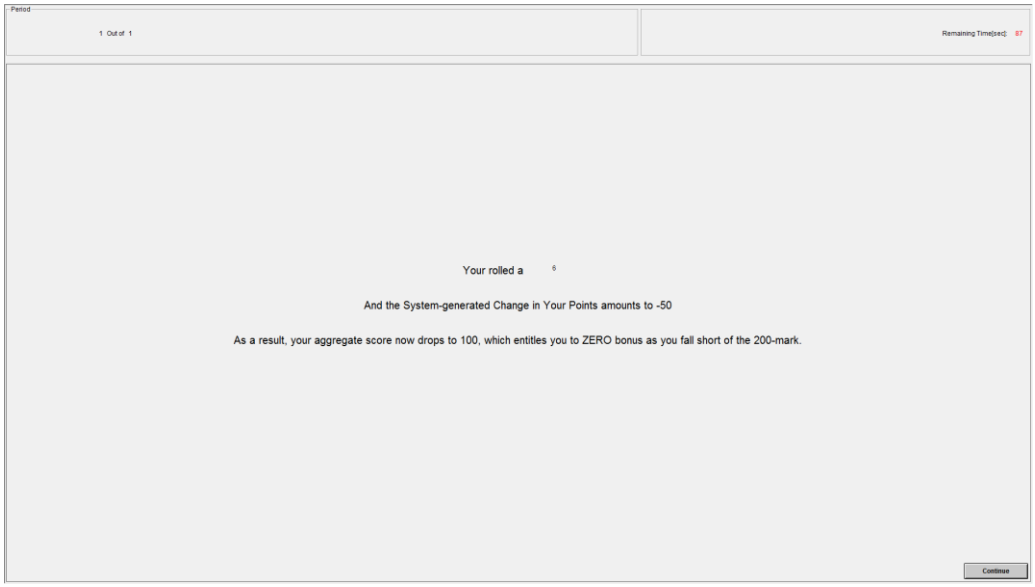
Owing to the interactive nature of this game, you will see several waiting screens as your partner is making his/her decision. Certain waiting screens could at most last for FIVE to SIX minutes, depending on how many decisions your partner has to make. Likewise, while you are asked to make a series of decisions, it will be your partner's turn to be shown the waiting screens.

Appendix 6. On-screen Instructions for Experiment 2 (Cont.)

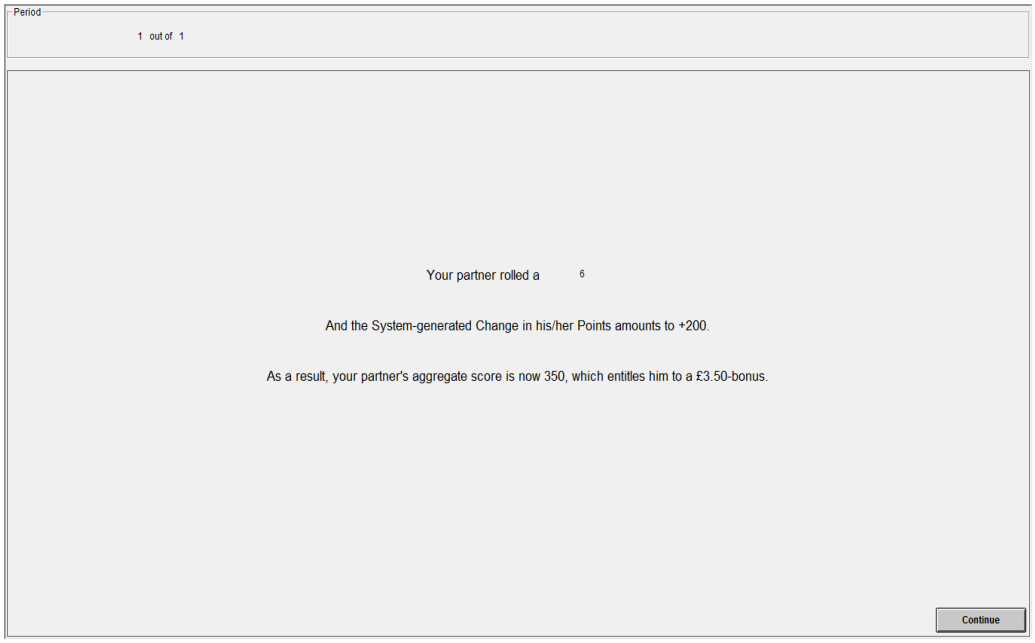
II. Die Rolling and Entry of Results – Same as Experiment 1

III. Display of Initial Payoffs-

(1) Own Payoff Display

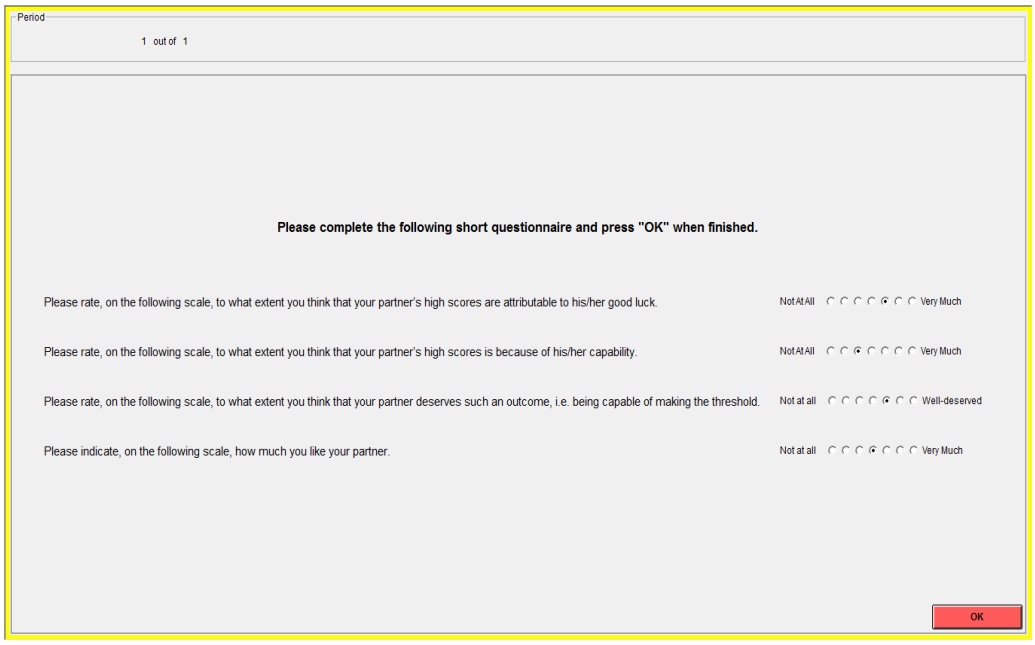


(2) Partner's Payoff Display



Appendix 6. On-screen Instructions for Experiment 2 (Cont.)

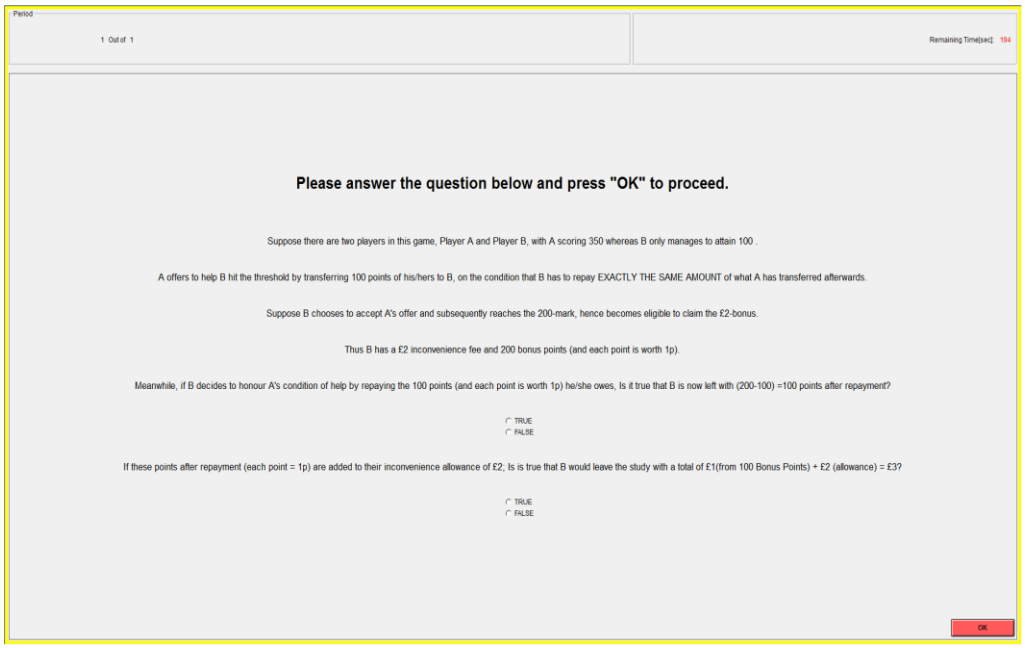
IV. Pre-offer Ratings: Attributions of partners' Die-rolling (e.g. Player 2s)



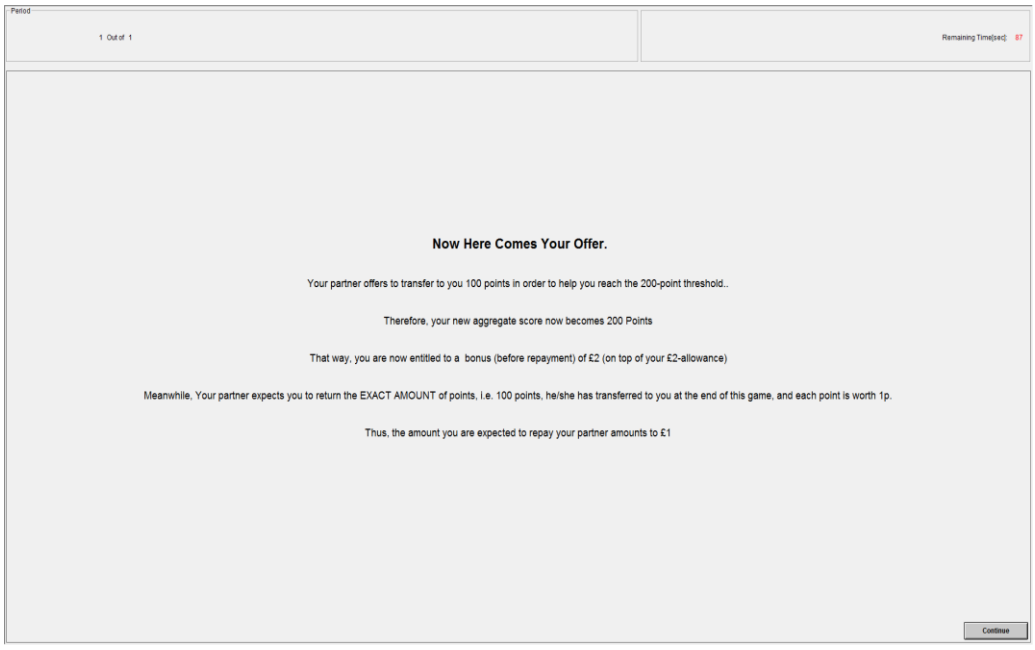
V. Waiting Screen

Appendix 6. On-screen Instructions for Experiment 2 (Cont.)

VI. Understanding Check

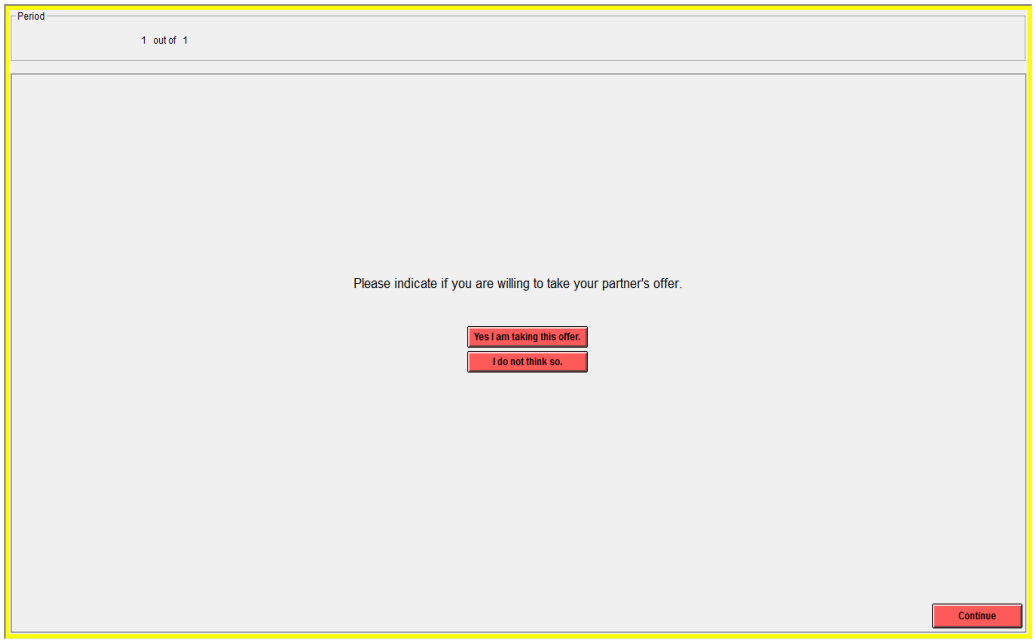


VII. Offer Display (Point Transfer)

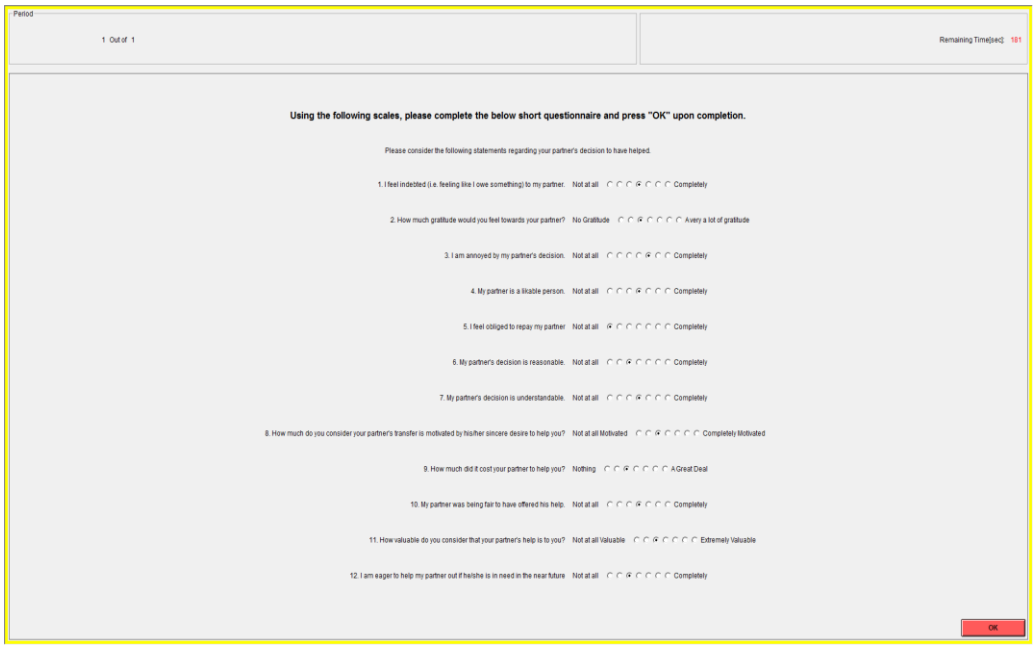


Appendix 6. On-screen Instructions for Experiment 2 (Cont.)

VIII. Offer Acceptance (or Rejections)

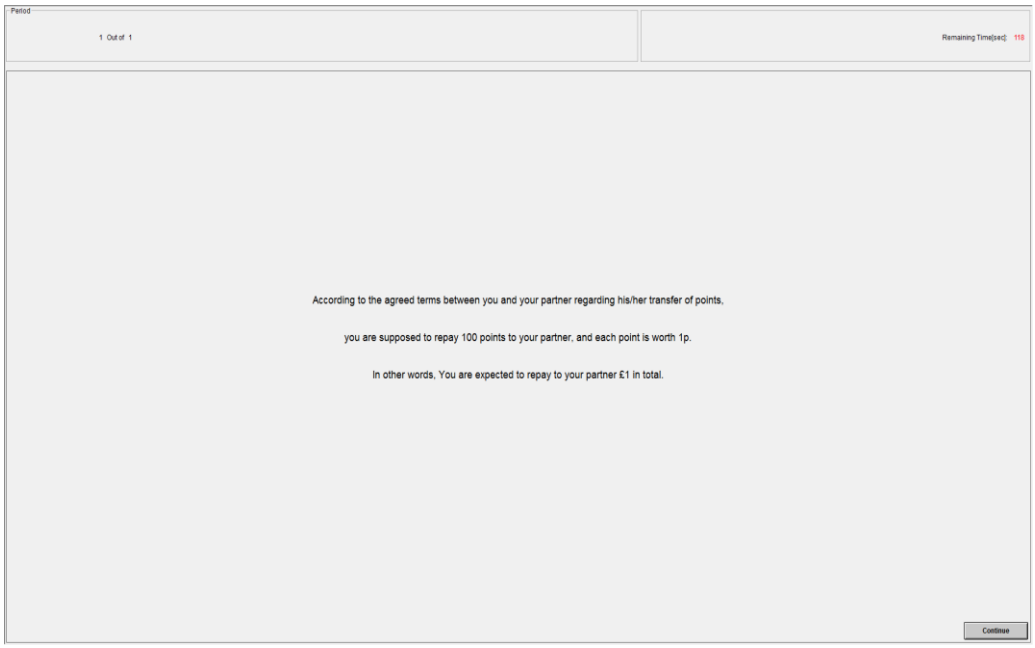


IX. Pre-repayment Ratings (See Chapter 4 for items)

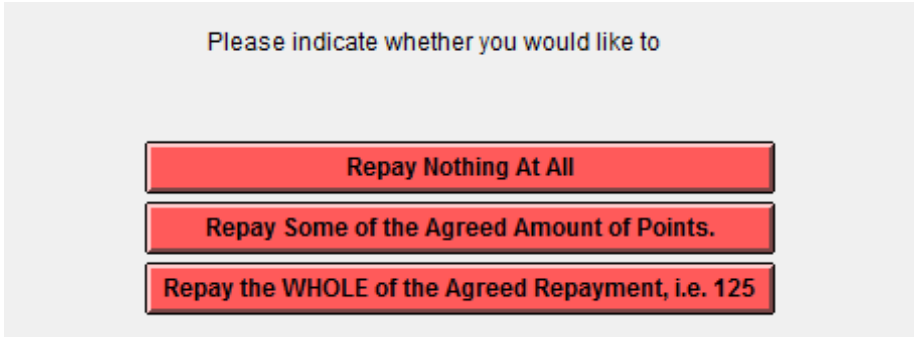


Appendix 6. On-screen Instructions for Experiment 2 (Cont.)

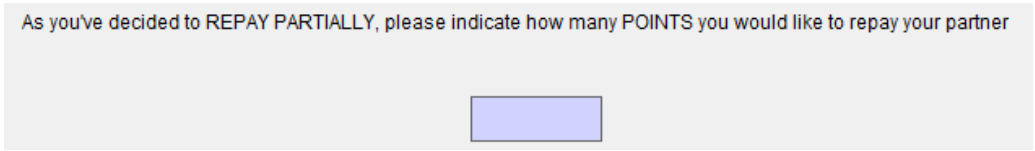
X. A Reminder: 'Terms' of the Repayment



XI. Repayment Stage: Options



XII. Cheap-riders' Repayment Entries

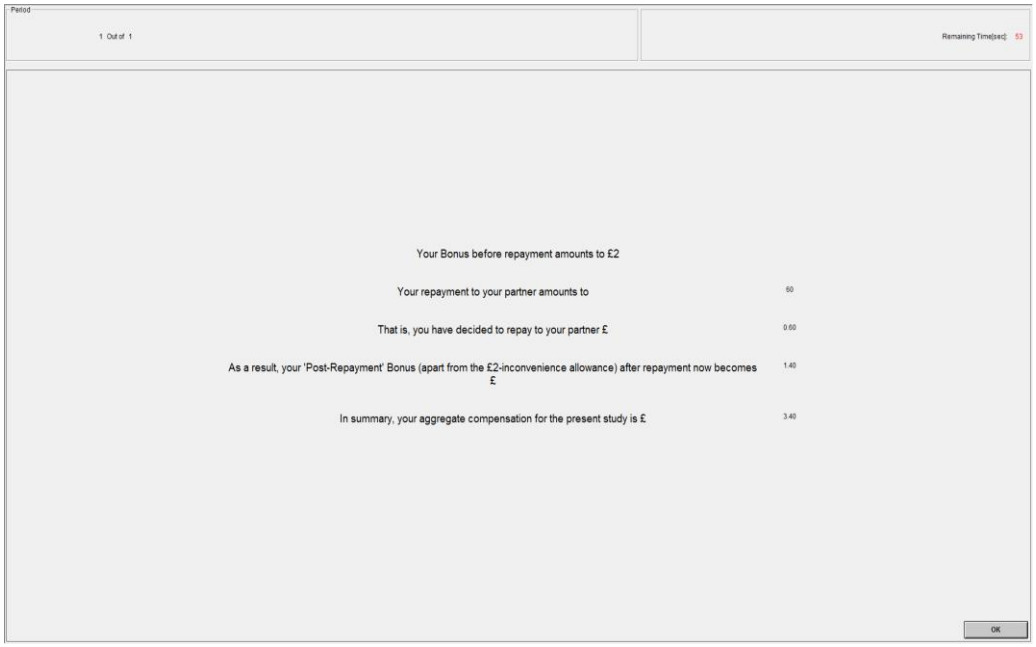


Appendix 6. On-screen Instructions for Experiment 2 (Cont.)

XII. Post-repayment Ratings (Reasons for Repayment or Non-repayment)

1. The fact that I repaid/did not repay made me feel good about myself.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
2. It made me feel emotionally positive about myself to have repaid/have avoided the repayment.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
3. I feel irritated by my partner's request for a repayment.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
4. My repayment/non-repayment left me with a feeling of 'warm glow' inside.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
5. I see my Repayment/Non-repayment as a means to punish my partner for his/her unreasonable request.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
6. I would feel guilty if i didn't stick with my repayment decision.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
7. The way I responded to my partner's offer is the most appropriate way to restore fairness between myself and my partner .	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
8. I see my Repayment/Non-repayment as a retaliatory act against my partner's offer .	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
9. I see my repayment/non-repayment best signifies how grateful I feel towards my partner.	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
10. By repaying/not repaying , I manage to offer my partner what he/she truly deserves .	Not at all ○ ○ ○ ○ ○ ○ ○ Completely
11. I repaid/ did not repay because I endeavoured to attain a relatively more equal payoff distribution between me and my partner .	Not at all ○ ○ ○ ○ ○ ○ ○ Completely

XIII. Compensations Display



Appendix 7. On-screen Instructions for Experiment 3

Example: A Participant who i) was 'Cheap-rode' (Repayment Manipulation) in Trial 1 (First Helper Trial) and ii) had *accepted* and *cheap-rode* on his/her '25%-Interest' offer in Trial 2 (1st Recipient Trial).

I. Opening (All participants)

Aim of the Game

1) Try to win as many 'bonus points' as you can in each trial. There are multiple trials in this experiment;

2) There will be a lottery draw at the end of the experiment, and one of you will have a chance to win a cash prize (at most £35);

3) One of you will be randomly picked as the winner, and for that person one trial they played will be randomly chosen and they will be paid on the basis of that trial;

4) How big the prize money is will depend on the person's performance on that randomly chosen trial, i.e. the 'Jackpot Trial';

5) Therefore, treat all trials as though they are real.

Rules of the Game

1) To begin with each of you will be given 150 'Bonus Points'. There is a bonus threshold of 200 points you have to reach to stand a chance to win a cash prize. Only if you can attain 200 points or above will you have the chance to be entered into the lottery to win. Each point you get is worth 10p, only if your final bonus points are at or above the threshold 200 points will you have the chance to be entered into the lottery. For instance, if you win the lottery draw, and your final score at the 'Jackpot Trial' amounts to 350 points, then you will receive a cash prize of £35. Nevertheless, a final score below 200 (e.g. 150 points) at the 'Jackpot Trial' would mean that your cash prize would be ZERO even if you win the lottery draw, since you fall short of the threshold.

2) The maximum Bonus Points you can get in each trial is 350, meaning that you can potentially win a cash prize of £35. You will start with 150 points.

3) In this game you will be then asked to roll a fair die once in every round (there will be multiple rounds), you will be asked to enter the number you rolled on the dice. The system will then generate a change in your score (the number of extra points you'll receive) based on the number you rolled on the dice.

Dependent on your die-rolling, there are 6 possible changes that could be brought to your final points scores, namely: i. -50 (resulting in you have 100 points); ii. Zero change (resulting in 150 points: no change); iii. +50 (resulting in 200 points: you just meet the threshold to be entered into the lottery and you could win £20); iv. +100 (resulting in 250 points: you exceed the threshold to be entered into the lottery and you could win £25); v. +150 (resulting in 300 points: you exceed the threshold to be entered into the lottery and you could win £30); vi. +200 (resulting in 350 points: you exceed the threshold to be entered into the lottery and you could win £35.

A large number rolled does not necessarily guarantee a more favourable change, and vice versa. Instead, the level of your final bonus is the outcome of the system's RANDOM assignment. The system would assign at random 1 of the 6 abovementioned outcomes to 1 of the 6 numbers on the die. That is, there is an equal opportunity for you to achieve either of the six abovementioned possible changes.

Structure of the Game

In this game each player will play alongside a different partner at each round. You and your partners will not be identifiable to one another, and will remain anonymous throughout the whole experiment. Throughout this experiment you and your partners will take turns to make decisions that might affect each other's possible final payoffs at each round.

Interactions with your Partner

In this game we allow interactions among the anonymous partners in each round following your initial die-rolling. Depending on your (or your partner's) final score at each trial, you could be in a position to either **help your partner** (by giving some of your points) or **to receive help** (i.e. bonus points) from your partner.

Helper

In case your post die-rolling score amounts to 250 or more, you can decide whether or not to help by transferring some of your bonus points to your partner in case he/she fails to hit the threshold. That way your partner could be eligible to earn a cash prize. There are certain basic guidelines, though, if you agree to help:

A. You could decide whether to request a repayment from your partner;
B. Your own Post-transfer must not fall below the 200-point threshold. For instance, if your post die-rolling point is 350, you cannot give more than 150 to your partner;
C. Your partner's Post-transfer score **must be at least 200** to render your transfer meaningful

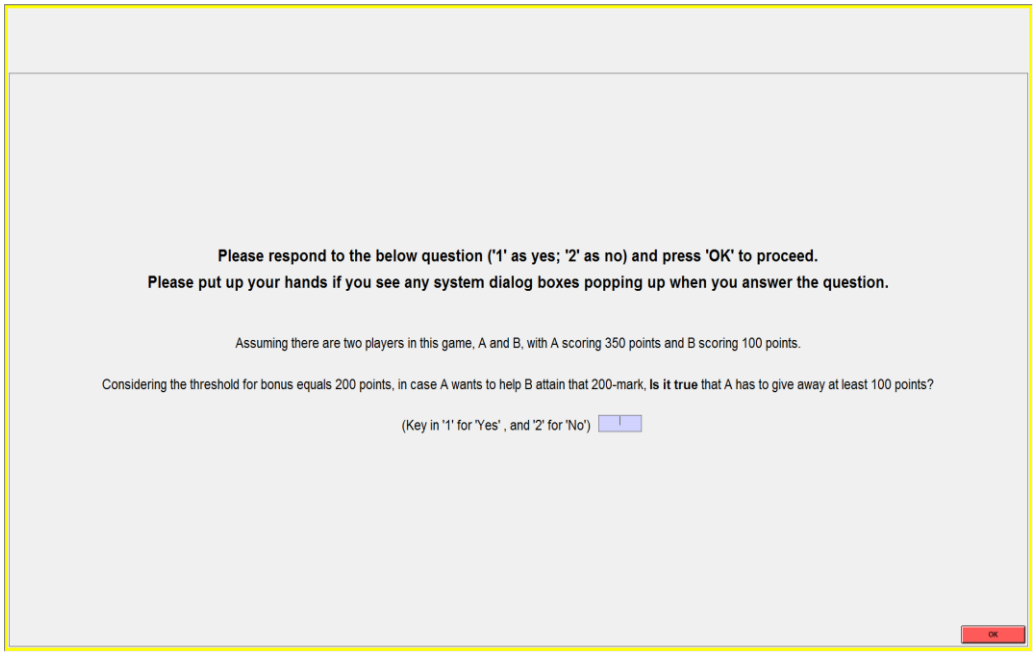
Recipient

You could potentially receive help (in the form of a point transfer) from your partner, in case your Post die-rolling score fall short of the 200-point threshold in any trials That way you are still eligible to win a cash prize in case you win the lottery draw, and this trial is selected as your 'Jackpot Trial'. Your partner could decide whether to transfer some of his/her extra points to you to help you reach the bonus threshold. Your partner may decide to impose a repayment condition on his/her transfer. You reserve the rights to agree to or to decline your partner's offer at each round. Please be reminded that any rejection of partner's transfer will yield you ZERO payoff, if you win the lottery draw and that trial being your 'Jackpot Trial'.

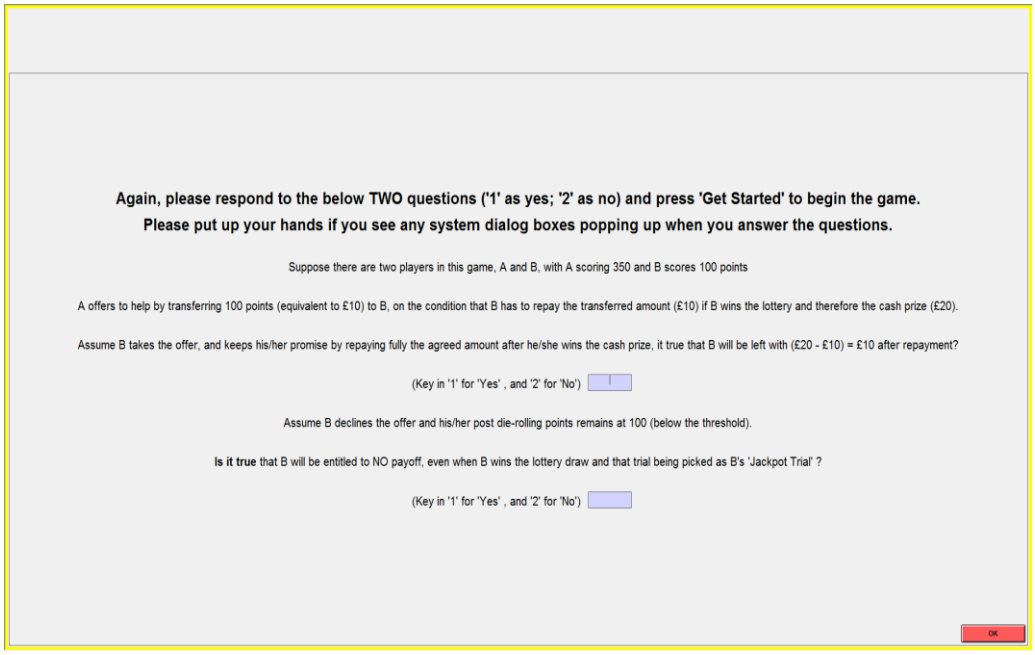
Owing to this interactive nature of the present experiment, you would therefore see several waiting screens (at every round) as your partner is making his/her decisions. Certain waiting screens can at most last for 5-6 minutes, depending on how many decisions your partners have to make. Likewise, your partners will be shown the same waiting screens when you are asked to make your decisions. Please once again be reminded that you and your different partners will not be identifiable to one another throughout the entire experiment

Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

I. Understanding Check (Helper Trials)



II. Understanding Check (Recipient Trials)

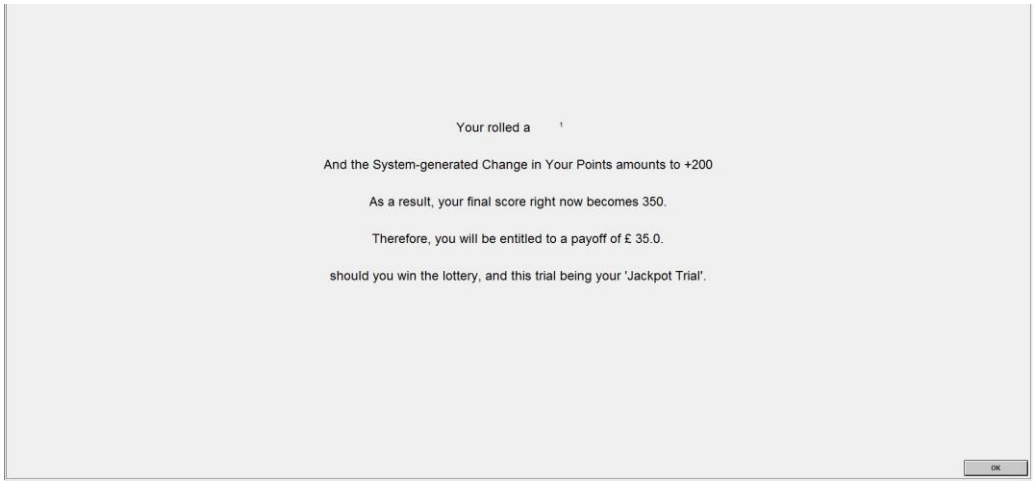


III. Trial 1: Die Rolling and Entry of Results – Same as Experiment 1

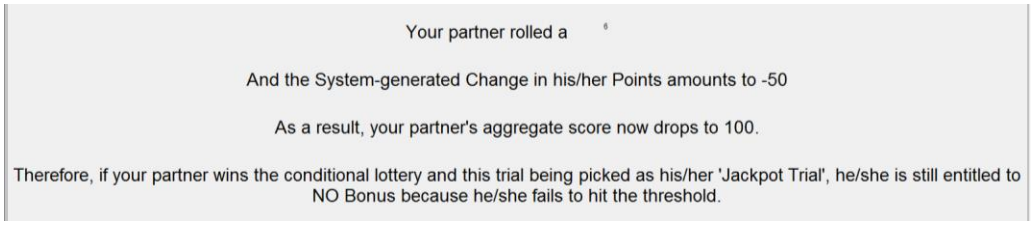
Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

IV. Trial 1: Display of Initial Payoffs-

(1) Own Payoff Display



(2) Partner's Payoffs



VI. Trial 1: Helping Rules (Information Screen A)

'In this game we allow high-scorers to decide **whether or not to help** their low-scoring partner(s) reach threshold by giving away part of their points'

'That is, you can choose to transfer some of your points to your partner, subject to the following guidelines if you decide to help'

1. You can decide whether to request a repayment from your partner following your transfer;
2. Your own 'Post-Transfer' points must not fall below the threshold, i.e. 200 points, meaning that you cannot give more than 150 points;
3. Your partner's 'Post-Transfer' scores must at least hit 200, i.e. the threshold for being eligible to win a cash prize in case he/she wins the lottery.'

Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

VII. Trial 1: Helping Rules (Information Screen B)

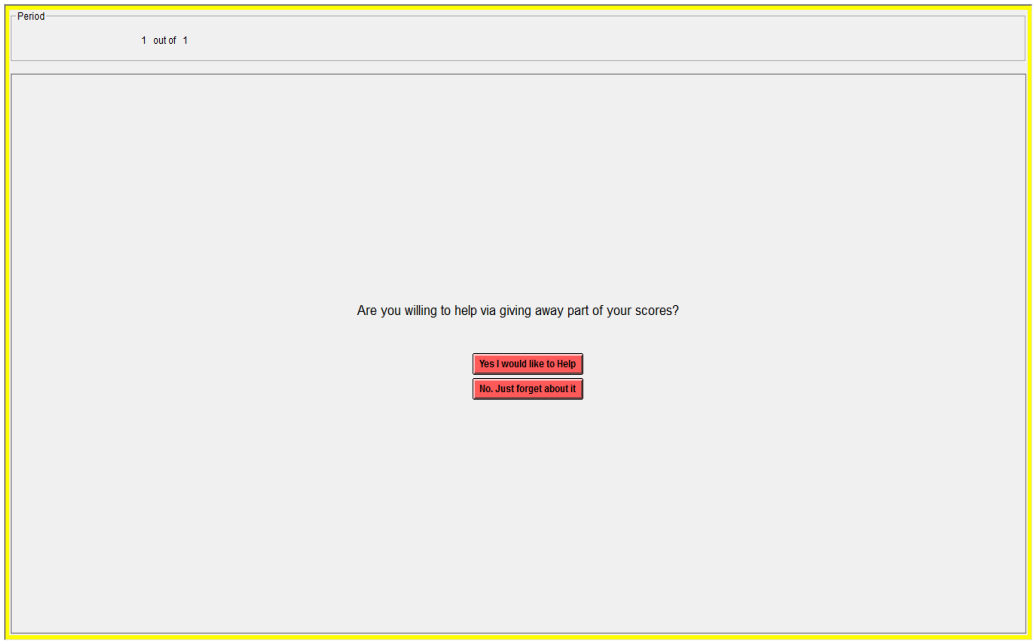
‘Knowing your partner's failure to make the threshold,

Please indicate whether or not you want to help your partner, by transferring part of your points to him/her, so that he/she could **claim some bonus** in case he/she wins the lottery.

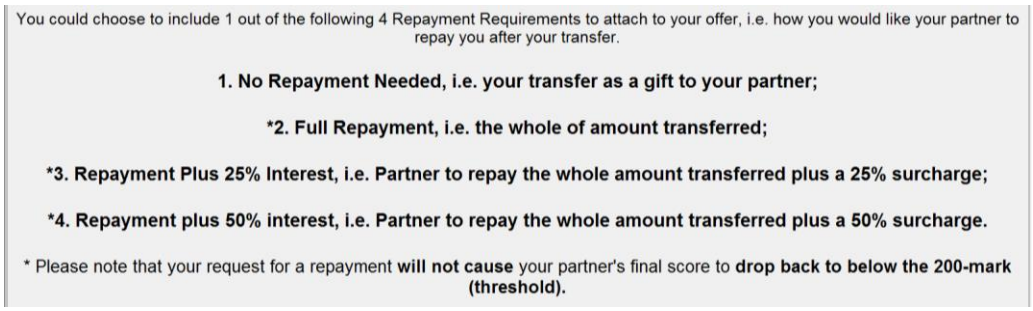
You can decide later whether or not to impose any repayment requests on your transfer.

Please be reminded that neither you nor your partner will be able to spot one another.’

VIII. Trial 1: Helping Decision: To Help or Not to Help



IX. Trial 1: Range of Helping Options (For Helpers, Figure 5.4)



Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

X. Trial 1: Helping Decisions, Conditionality

'In the light of the above alternatives 1 to 4, how would you like your partner to repay you for your transfer?

1. NO Repayment;
2. Full Repayment;
3. Repayment Plus 25% Interest;
4. Repayment Plus 50% Interest'

XI. Trial 1: Helping Rules: Transfer Magnitude

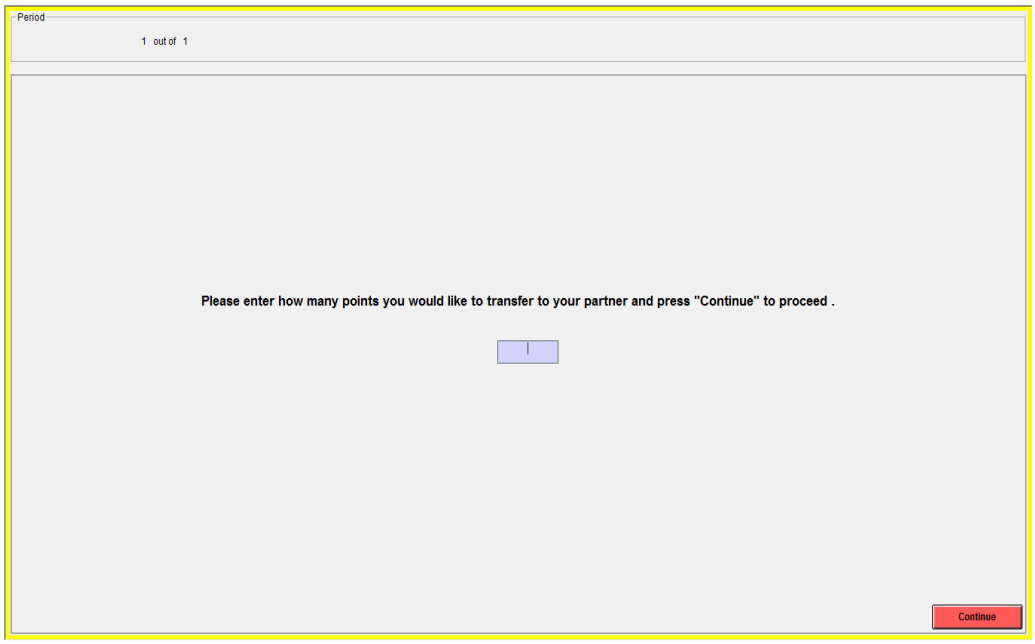
'You have chosen to transfer part of your points to your partner either unconditionally or expecting a repayment.

Please be reminded that your own 'Post-Transfer final score must not be under the 200- Point Mark.

Thus you are not expected to give away more than 150 Points.

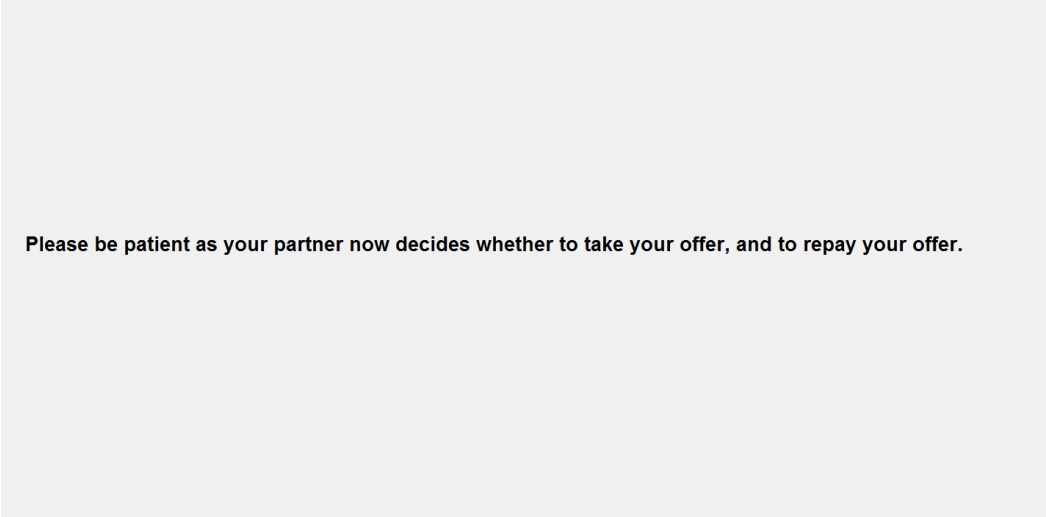
However, you have to at least give away 100 Points to render your transfer meaningful.'

XII. Trial 1: Helping Decisions: Transfer Magnitude

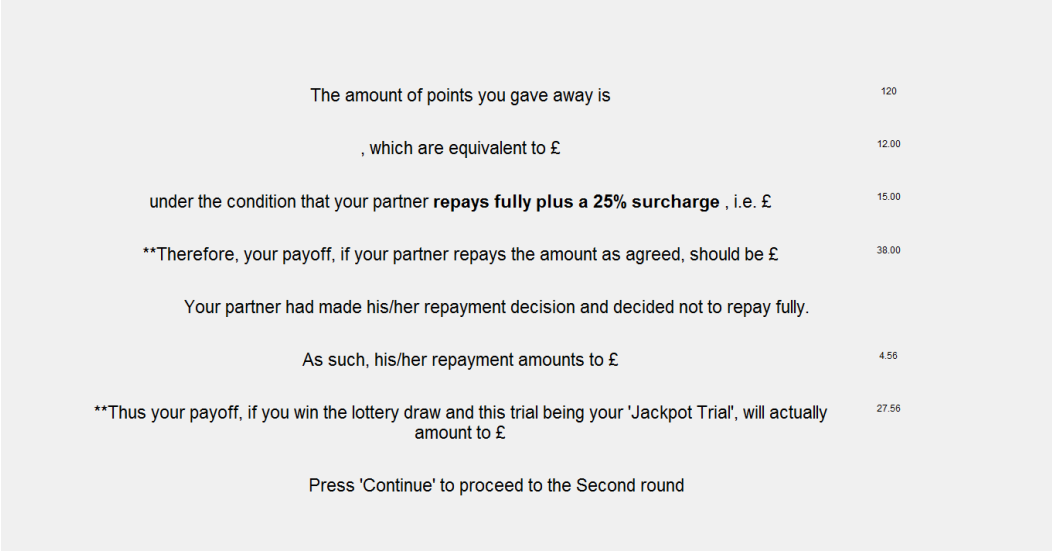


Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

XIII. Trial 1: Waiting Screen for Helpers



XIV. Trial 1: Notification of Partner's Repayment (Cheap-riding)

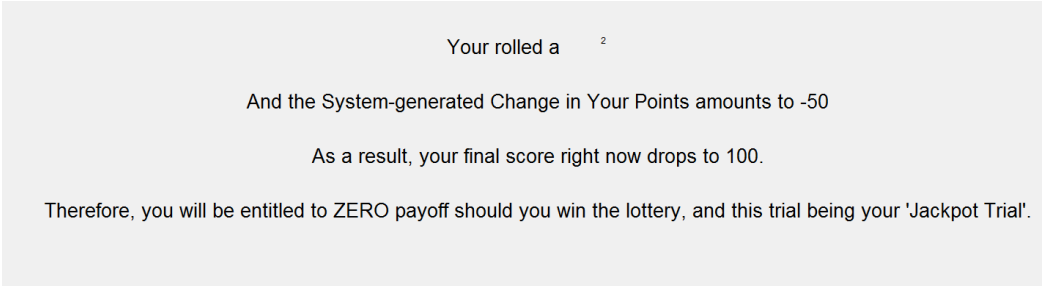


XV. Trial 2 (1st Recipient Trial): Die Rolling and Entry– Same as Trial 1

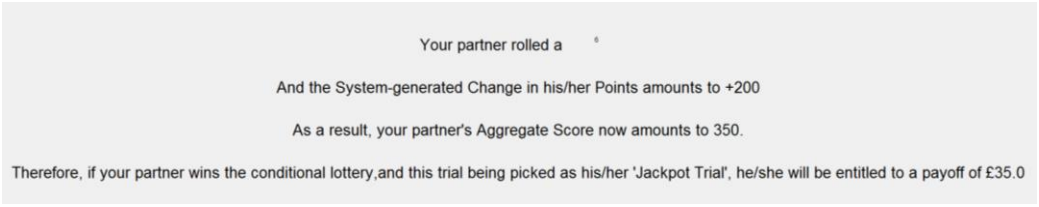
Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

XVI. Trial 2: Display of Initial Payoffs

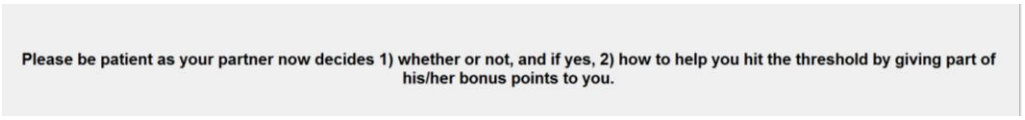
(1) Own Payoff Display



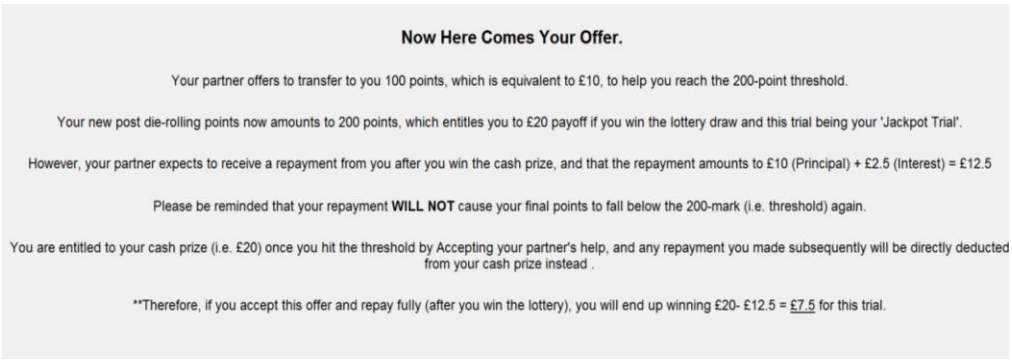
(2) Partner's Payoff Display



XVII. Trial 2: Waiting Screen

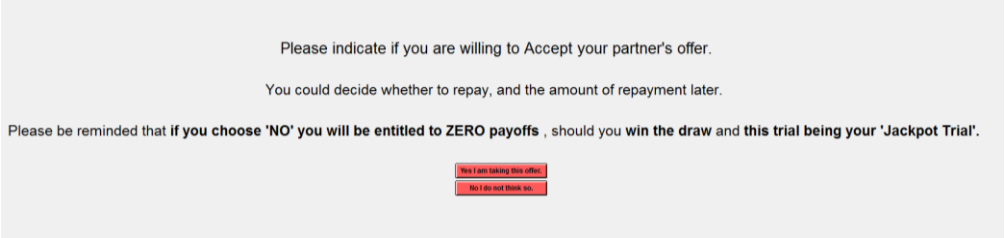


XVIII. Trial 2: Offer Screen (25%-Interest Offer Given)



Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

XIX. Trial 2: Offer Acceptance



XX. Trial 2: Pre-repayment Ratings

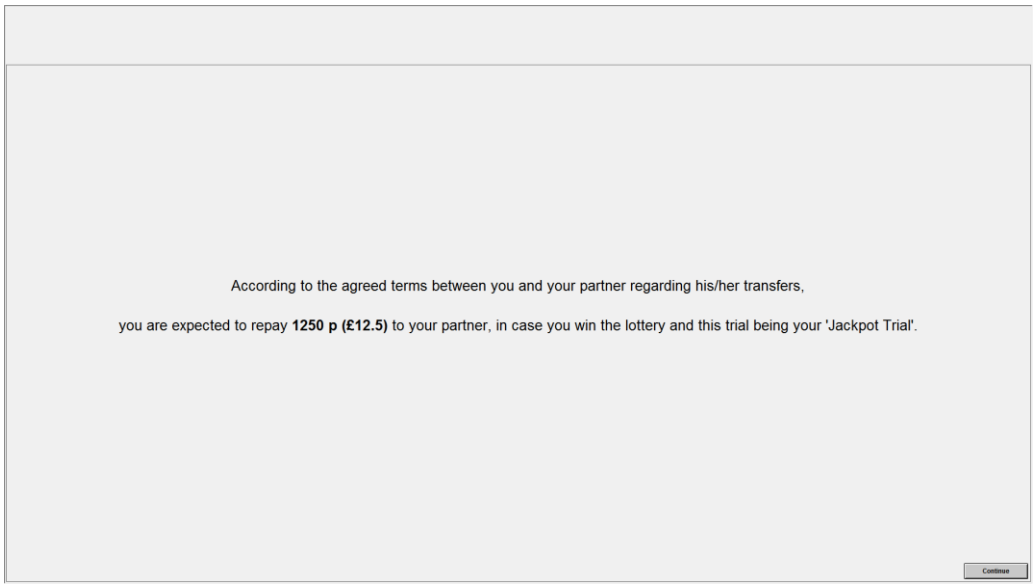
'Please consider the following statements, using the following scales, regarding your partner's decision to have given an offer to you.

1. I feel indebted (i.e. feeling like I owe something) to my partner*.
2. How much gratitude would you feel towards your partner? ('1': No Gratitude; '7': A very lot of Gratitude)
3. I am annoyed by my partner's decision*.
4. I would have done the same thing were I in my partner's shoes*.
5. I feel obliged to repay my partner*.
6. How much do you consider your partner's transfer is motivated by a sincere desire to help? ('1': Not at all motivated, '7': Completely Motivated)
7. My partner could have been more generous to me*.
8. How much do you think it cost your partner to have offered you help? ('1': Nothing, '7': A Great Deal)
9. I am eager to help my partner out if he/she is in need in the near future*.
10. How valuable do you consider that your partner's help is to you? ('1': Not at all Valuable; '7' Extremely Valuable)'

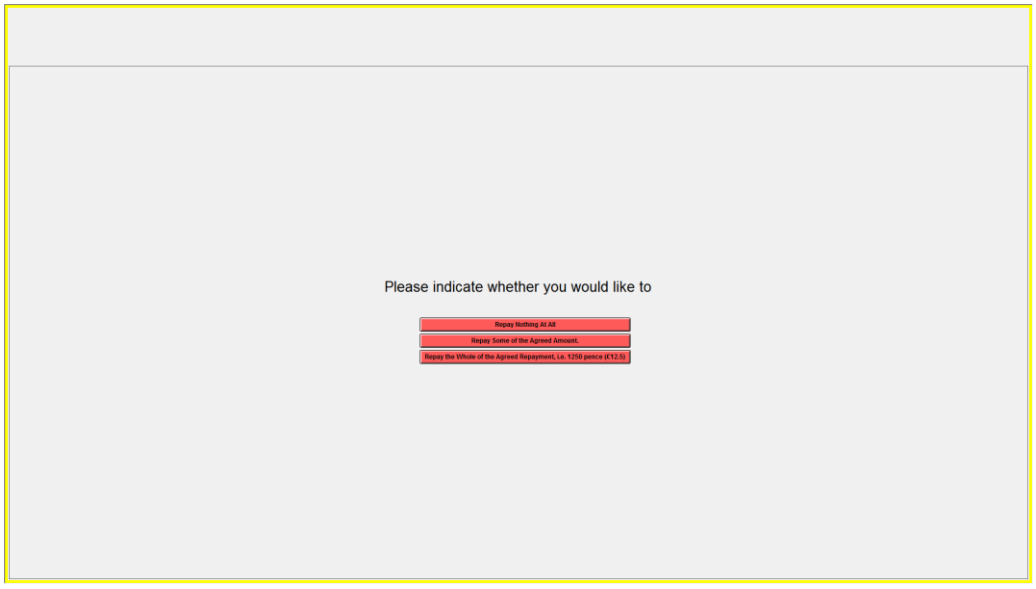
*All items were rated on a 1 ('Not At All') to 7 (Completely), seven-point Likert scale unless otherwise specified.

Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

XXI. Trial 2: Repayment: Reminder



XXII. Trial 2: Repayment Decisions –Categorical



Appendix 7. On-screen Instructions for Experiment 3 (Cont.)

XXIII. Trial 2: Cheap-rider's Entry of Repayment

XXIV. Trial 2: Compensations Display