

–The Announcement Effect: the impact of early warnings of future thresholds  
under different framing and risk contexts”

By

Alexander Child

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Supervisor: Martine Visser

Faculty of Humanities

University of Cape Town

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# THE ANNOUNCEMENT EFFECT: THE IMPACT OF EARLY WARNINGS OF FUTURE THRESHOLDS UNDER DIFFERENT FRAMING AND RISK CONTEXTS

## *Abstract*

*The effect of announcing future institutional change is investigated in three different contexts: a gains frame, a loss frame, and a loss frame with risk. The institutional change is the transition from a normal public goods game into a threshold public goods game. Announcements may change subject behaviour, through influencing their expectations, before the implementation of the new institution (adjustment effect) and/or after the implementation (adaptation effect). We find that announcements in the gains frame cause zero adjustment effects and negative adaptation effects; while announcements in the loss frame cause positive adjustment and adaptation effects. However, including risk into the threshold phase of the loss frame causes the announcements to have zero effects. These results have important implications for the climate change debate.*

*(JEL classification: C9; H41; H30; Q54)*

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## 1. Introduction

Humanity has, up until now, been freely contributing to the pollution of the atmosphere while the negative effects have only been felt marginally by the whole planet. However, in recent years, scientists have become aware that the climate may be reaching a tipping point due to anthropocentric interference, passed which irreversible and dangerous climate change will occur causing large, discontinuous harm to both natural and human systems (O'Neill & Oppenheimer 2002; Solomon et al. 2009). The 2009 Copenhagen Accord resulted in a non-binding agreement to stop catastrophic climate change by limiting the global temperature rise to below 2 degrees Celsius by requiring all nations to reduce their emissions of greenhouse gasses (Rogelj et al. 2010). The scientific findings of future climate thresholds are effectively announcements of a coming change in the implicit institutional environment of carbon emissions from a continuous public good game to that of a threshold public good game. The current paper situates itself within the framework of such institutional change. Our main aim is to investigate what impact an early-warning announcement of future institutional change, such as impending climate thresholds, has on countries' voluntary contributions to carbon emissions abatement

We build on the work done by Visser et al. (2014) by performing laboratory experiments testing the impact of announcements of future institutional change in three differently framed contexts. The institutional change is that of 20 periods of continuous public goods games transitioning to 10 periods of a threshold public goods game. The effect of announcements of the future institutional change are then tested in a gains frame, where contributions go towards yielding benefit; in a loss frame, where contributions go towards reducing a loss; and a loss-and-risk frame, where the negative impact from not reaching the threshold only exists with a 50% chance in the new threshold institution.

The behavioural changes that accompany the transition from one set of institutions to another is a topic that has received relatively little investigation in the field of economics. Those studies that do focus on institutional change mainly do so in light of endogenous versus exogenously imposed changes of institution (see for example: Janssen et al. 2008; Sutter et al. 2010). Virtually no attention has been given to the impact of early-warning announcements of impending exogenously imposed institutional change, apart from the recent paper by Visser et al. (2014)

who provide initial experimental evidence on the positive impact of announcements of future punishment opportunities. This is the first paper in the literature to model the impact of announcements of the future transition into a threshold public goods game structure. We also include a pre-experimental measure of subjects' risk attitudes using the Eckel & Grossman (2008b) method of gamble choices and include these as predictors of subject behaviour in the public goods games, which we believe has not been done before.

The theory of change that accounts for the potential impacts of announcements on behaviour follows closely the theory of rational expectations. The theory states that anticipation of policy change is a vital part of whether the change has an effect, as a population who anticipates change can revise their expectations, and thus their behaviour, immediately leading to zero overall policy effectiveness (Sargent & Wallace 1976; Holland 1985; Heijdra & van der Ploeg 2002). Therefore the main vehicle for behavioural change caused by announcements is their influence on the expectations of the population. In this paper we will look at two major avenues in which the announcement effect can occur. The announcements of a future institutional change can have an immediate effect, before the implementation of the new institution (*adjustment effect*) or it can have a later effect, after the implementation of the new institution (*adaptation effect*). The term *adjustment effect* has been adapted from the use of the term adjustment path which represents the effect of announcements on behaviour before the implementation of institutional change (Adams et al. 2001; Heijdra and van der Ploeg, 2002). While our *adaptation effect* term has been modified from Blundell et al.'s (2001) use of the term implementation effect to model the effect announcements have after the implementation of an institutional change

Our findings suggest that announcements can increase the effectiveness of eventual institutional change. While in the gains frame we see zero adjustment effects and negative adaptation effects of announcements, in the loss frame we find significant positive adjustment effects of increased contributions and significant positive adaptation effects of higher likelihoods of groups reaching the threshold. However, when the loss frame is combined with risky impact from not reaching the threshold, all significant effects previously found due to announcements are negated.

We also find strong evidence that the initial adjustment effect in the loss frame is mediated by the announcements' influence on the likelihood of subjects playing by certain strategies such as free-riding or full cooperation. However, the adaptation effects found in the gains and loss frame have only weak evidence for a similar announcement effect on player strategies. There is also evidence that among groups successful in reaching the threshold, the announcements cause stronger, earlier action relative to successful groups who do not receive the announcements.

The above results have important implications for the climate change debate, especially considering the 21<sup>st</sup> Conference of the Parties (COP) meeting that will be held in Paris in 2015 to implement legally binding emissions reduction pledges from nations. A natural application of this research is the efficacy of early warning systems in the climate change debate. An integral part of early warning systems is the dissemination of timely information in order to enable threatened individuals to prepare and act so as to reduce the possibility of harm (UNISDR 2010). This information dissemination can be seen as a series of announcements, such as those described in the current paper. Numerous studies have attested to the possibility and the benefits of early warning systems with regards to the negative impacts of tipping over dangerous climate thresholds, in particular the two degree Celsius warming threshold (Lenton 2011; Rogers & Tsirkunov 2011; Huntingford et al. 2012; Marvin et al. 2013; Ludescher et al. 2014). Alfieri et al. (2012) attribute a large degree of the benefits of early warning systems to increased population preparedness and coordination, as well as a necessary philosophical perspective change from post-disaster recovery to disaster prevention. However, what is lacking from the literature is a focus on how early warning initiatives can actually affect individual-level cooperative decisions; it has even been stated that 'the individual's willingness to act cannot be taken for granted' (Rogers & Tsirkunov 2011: 14). The current study is an initial probe into this void in the literature. The inclusion of a context in our experimental design that incorporates risk in the new institution is to mimic the inherently unpredictable nature of the future and, in particular, the various uncertainties implicit with the current scientific knowledge of climate thresholds (Hallegatte 2009; Dannenberg et al. 2011; Lenton 2011; Tavoni et al. 2011; Barrett & Dannenberg 2012).

## **2. Literature Review**

### **2.1 Public Goods Games & Threshold Public Goods Games**

The phenomenon of cooperation in social dilemmas where collective interests and individuals' private interests are at odds is an area that has been increasingly studied by the experimental economics tradition using public goods games (PGGs). A tenet of traditional economic game theory implies that humans act only out of narrow self-interest, which would result in zero cooperation in such dilemmas, yet this has been consistently violated in experimental findings where significantly more than zero cooperation is observed (Andreoni 1995a; Hoffman et al. 1998). The importance of using experimental methods to study cooperation stems from their ability to simulate real life social dilemmas, such as climate change, while isolating the effect of particular interventions in a controlled environment (Ledyard 1995; Zelmer 2003). Interventions that are found to have a significant impact on cooperation in the laboratory can then be applied to real life situations with the evidence that *ceteris paribus* they will have an effect.

Threshold public goods games (TPGGs) are an extension of the classic experimental public goods game in which each individual is placed into a group and asked to contribute to some public good yielding a return. In the classic PGG, the welfare-maximising solution is reached if everyone contributes to the public good, but each individual has the incentive to contribute nothing (free-ride) in order to maximise their own pay-off (Zelmer 2003). As such, there exists a social dilemma as there is always a conflict between individual and collective interests. However, with the addition of a threshold in TPGGs, the payoff structure of the public goods game is transformed into a coordination game, with the threshold as a focal point (Cadsby & Maynes 1999; Barrett & Dannenberg 2012). Herein lies the main difference between TPGGs and PGGs: the inclusion of a threshold turns a social dilemma into a social coordination game, where cooperation can benefit the collective as well as the individual if the threshold is met (Abele et al. 2010). In TPGGs, any combination of contributions that sum to the threshold exactly is both a Nash Equilibrium and Pareto efficient, whereas PGGs if linear only have one Nash Equilibrium (zero contribution) and one Pareto efficient solution (full contribution), which are at odds (Abele et al. 2010).

The placement of the threshold can vary in different TPGG designs. In the traditional model, the threshold exists after each contribution period, making it either a one-shot game or a series of one-shot games (Rapoport & Eshed-Levy 1989; Erev & Rapoport 1990; Rapoport & Suleiman 1993; Cadsby & Maynes 1999). In a more recent variation of the model the threshold is cumulative, in that contributions to the public good are summed over a number of periods, at the end of which a cumulative threshold must be met (Milinski et al. 2008; Dannenberg et al. 2011; Tavoni et al. 2011). This design nuance means that there is conflict between short-term and long-term interest (Milinski et al. 2008; Chen et al. 2012). This conflict element increases the social dilemma aspect which traditional TPGG designs minimize and allows for the modelling of inter-temporal social dilemmas, such as dangerous climate change. As such, the cumulative threshold design will be used in the experiment presented here.

The mechanics of the threshold work as such: if the sum of contributions from group members does not equal or exceed the threshold, then the group as a whole either fails to earn a reward or incurs a loss, depending on the framing of the game (Sonnemans et al. 1998; Iturbe-Ormaetxe et al. 2011). A traditional feature of TPGGs is the step-return of reward (loss) from reaching (not reaching) the threshold, which is lump-sum and constant, as these public goods ‘do not increase in quality or quantity after more contributions [over the threshold] are voluntarily made’ (Rapoport and Suleiman, 1993: 172). This element gives rise to threshold public goods also being known as lumpy, binary, discrete, or step-level goods.

Total contributions which fall short of the threshold level may be returned to contributors in some studies (McBride 2006). This would be an example of a refund policy or a money back guarantee, which have been shown to increase cooperation and public good provision, possibly by alleviating the incentive for non-cooperative behaviour of fear of others’ non-contribution (Rapoport & Eshed-Levy 1989; Cadsby & Maynes 1999; Menezes et al. 2001). In general, contributions which exceed the threshold level are not returned to contributors (Nitzan & Romano 1990; Marks & Croson 1998; Iturbe-Ormaetxe et al. 2011). However, such rebate policies have been shown to increase cooperation and public good provision, especially if a proportional rebate rule is used, this potentially works by diminishing the greed motivation for non-cooperation (Rapoport & Eshed-Levy 1989; Spencer et al. 2009). Including a rebate or refund rule might misrepresent a special element of modelling certain threshold public goods

which often entail the fear and greed incentives for not cooperating as vital strategic components. These rules basically amount to insurance against any other outcome but the efficient Nash Equilibrium of contributions meeting the threshold exactly. Such insurance is commonly not possible in real life scenarios due to the sunk cost characteristic of things like investments into climate change abatement or the physical contribution of hours of one's time volunteered for a project (Makris 2009) .

Various mechanisms to increase contributions and public good provision in TPGG have been explored. Continuous contributions have been shown to increase contributions and public good provision relative to binary contributions. High thresholds; unequal initial endowments and low rewards (step-returns) have been found to decrease contributions and provision, though pre-play communication and taxation helps to alleviate the impact of inequality and increase cooperation (Rapoport & Suleiman 1993; Cadsby & Maynes 1999; Croson & Marks 2000; Brick & Visser 2010; Tavoni et al. 2011; Van der Hoven et al. 2012) Allowing subjects to contribute sequentially rather than simultaneously has also been shown to increase contribution levels and public good provision (Erev & Rapoport 1990; Normann & Rau 2011). Increasing the identifiability of subjects' decisions has been found by Marks & Croson (1998) to increase contributions and provision, possibly through the motivation of reputation-seeking within groups.

### 2.1.1 Framing Effects and Loss-Aversion

The framing of Public Goods Games also affects subject behaviour. The quintessential element of framing is the presentation of logically equivalent options in superficially different ways (Krishnamurthy et al. 2001). Tversky & Kahneman (1986) describe how people systematically behave differently when faced with different presentational formats of equivalent options, a phenomenon known as framing effects. Such framing effects, the authors argue, violate the rational choice axiom of invariance and render normative models of choice unable to provide descriptive accounts of human behaviour. There are many ways that choices can be framed differently, but for this review we will focus only on valence framing: positive (gains) and negative (loss) presentations.

Andreoni (1995b) provides one of the earliest studies of framing in normal PGGs. The model he constructs are derived from the fact that a public good rendering non-excludable positive value is logically equivalent to a private good rendering non-excludable negative value. He finds that his positive frame results in higher cooperation than his negative frame. More recent work has supported this view. Cox (2014) performs experiments using more nuanced and decomposed definitions of framing effects and finds a similar pattern but reduced significance of loss framing in the context of contributions to public goods, while framing effects are almost entirely driven by responses from male subjects. Andreoni's (1995b) findings also found that negative framing has a stronger impact on subjects with more individualistic oriented values versus subjects with more cooperative orientations (Willinger & Ziegelmeyer 1999; Park 2000). The current paper will utilise the same template of Andreoni (1995b) in designing our loss framed PGG, but it will also heed the nuances proposed in Cox (2014) in terms of instructional simplicity. The motivations in our two PGG frames can be summarised as i) gains frame – benefit maximisation and ii) loss frame – loss minimisation.

The TPGG can also be framed either positively (gains frame), whereby contributions go towards the provision of a monetary reward; or negatively (loss frame), whereby contributions go towards preventing a monetary loss. The loss framed TPGG can be quite similar in structure to the Common Pool Resource (CPR) dilemma, in which a group of subjects must decide individually how much to extract from an exhaustible resource (joint account) for private gain, which yields negative externalities to other group members (Budescu et al. 1995). Sonnemans et al (1998) show that behaviour is similar between the two frames for early periods of repeated interactions, yet after a while contributions decay in the loss frame causing provision and cooperation to diminish relative to the positive frame. Bougherara et al (2007) also finds that a loss frame leads to lower cooperation, and also notes that framing effects seem to be most powerful under high thresholds.

Iturbe-Ormaetxe et al (2011) also study framing effects, yet their experiments contrast Sonnemans et al (1998) and Bougherara et al's (2007) findings by showing that contributions are higher in the loss frame (when thresholds are high) compared to the gains frame due to loss aversion; and that the loss frame results in more overall efficiency. However, Iturbe-Ormaetxe et al's (2011) study differs in one critical way that might explain the different findings. The authors'

loss frame treatment does not work by subjects deciding how much to withdraw from a joint account, as in Sonnemans et al (1998) and Bougherara et al (2007). Rather, subjects are initially endowed with a higher amount, representing value already enjoyed from a public good and then they are asked to contribute to avoid the deterioration of the public good which would result in a loss. The authors argue that the gains and loss frames differ with respect to reference points. In the loss frame the reference point is the already enjoyed benefit from the public good, whereas in the gains frame the reference point is the cost of contributing out of their private endowment (Iturbe-Ormaetxe et al. 2011). It is this formulation of a loss frame that the current paper will utilise due to its contribution mechanism being more comparable to that of the gains frame and its congruence with other papers modelling dangerous climate change (Milinski et al. 2008; Brick & Visser 2010; Hasson et al. 2010; Tavoni et al. 2011; Hasson et al. 2012; Van der Hoven et al. 2012).

The finding from Iturbe-Ormaetxe et al. (2011) that the loss frame induces more cooperation is in line with the concept of loss-aversion. Championed by Tversky & Kahneman (1986,1991), loss-aversion implies that losses are weighted more heavily than equal quantity gains in peoples' subjective utility functions. An example of loss-aversion is a phenomenon known as the endowment effect, whereby people value the same good more if they own it than if they do not (Thaler 1980). This corresponds to the loss framed TPGG by predicting that people already enjoying the value stemming from a public good in their private endowment will contribute more to avoid losing this value than people who contribute only to win a reward of the same value (gains frame), a result which Iturbe-Ormaetxe et al. (2011) found. Recent work on loss-aversion has found that people exhibit strongly correlated levels of loss aversion across both risky and riskless choices; that loss aversion is positively related to age and wealth and decreases in education; and that loss-aversion may exist in varying levels in different life domains due to evolutionary pressures (Gächter et al. 2010; Li et al. 2012)

The literature on TPGGs mainly uses loss frames in order to model ecological systems such as rivers, forests and the global climate which entail dramatic losses of function after a threshold level of exploitation has been reached (Rapoport & Suleiman 1993; Milinski et al, 2008; Wang et al, 2009; Dannenberg et al. 2011; Horan et al. 2011; Laurance et al. 2011; Tavoni et al, 2011; Barrett & Dannenberg 2012; Hasson et al. 2012). Gains framed TPGGs can also model the

minimum contributions required to meet a public good which requires a minimum capital investment such as the building of a bridge or a wildlife reserve or the building of a community garden or a public library (Menezes et al. 2001; Abele et al. 2010; Carlsson et al. 2011). Although it has not been done before (to the best of the authors' knowledge), species conservation might also lend itself to modelling by loss framed TPGG. If a minimum amount of protection is not conveyed upon an endangered species or its habitat, the population numbers might cross a tipping point and lead to catastrophic collapse (Dai et al. 2012), which would entail a large, discontinuous loss of value spread equally among all people.

### 2.1.2 Uncertainty and Risk

The impacts of different forms of uncertainty and risk on contribution behaviour have also been explored in the public goods games literature. Although it is a contentious subject, we will distinguish between risk and uncertainty by a particular reading of the Knightian view which states that *risk* is a situation where a future event occurs with a known probability, whereas *uncertainty* is a situation where a future event occurs with unknown probability (Langlois & Cosgel 1993). In every form of public goods game there is social uncertainty, which describes strategic risk as other people's contribution behaviour cannot be known *ex ante* (Rapoport & Eshed-Levy 1989; Eckel & Grossman 2008a; McCarter et al. 2010; Murphy & Ackermann 2013). Environmental uncertainty or risk can also be incorporated into various positions in the threshold public goods games.

Threshold uncertainty is usually modelled by making the threshold lie somewhere within a known probability distribution (commonly a uniform distribution) between a range of values, and thus increasing the uncertainty is represented by a mean-preserving increase in the spread of the range (McBride 2010). It is consistently found that increasing threshold uncertainty diminishes cooperation and the likelihood of public good provision, especially when the value of the public good is relatively low (Nitzan & Romano 1990; Suleiman et al. 2001; McBride 2006; Barbieri & Malueg 2010; Barrett & Dannenberg 2012). Impact uncertainty has been modelled in a similar way: by increasing the range of possible severity that the impact of not reaching the threshold (in the loss frame) causes. This type of uncertainty has been shown to have no significant impact on contribution behaviour (Barret and Dannenberg, 2012). However,

McCarter et al (2010) study impact uncertainty of the same structure described above and find that only when uncertainty in the value of the return from reaching the threshold includes the prospects of a loss (i.e a range of  $\{-50;100\}$  instead of  $\{50;150\}$ ), does the uncertainty decrease contributions.

A form of impact risk is a case where (in a loss framed TPGG) if the threshold is not reached, a negative impact, of known severity occurs with some known probability. This type of risk has been used in TPGGs called Collective Risk Social Dilemmas or Climate Games and as such have been used frequently to model the dynamics of disastrous climate change. Findings on the effects of such risk have consistently shown that a higher risk of negative impact increases cooperation and disaster avoidance (Milinski et al. 2008; Wang et al. 2009; Santos & Pacheco 2011; Tavoni et al. 2011). Chakra & Traulsen (2012) study the evolutionary dynamics of contributions under this type of risk and find that the strategy of late contributing (not contributing initially and then compensating later) fares better than the fair-sharing strategy, and thus what is set as the maximum contribution per period determines the timing of procrastinated contributions. Risk in this form could also be called threshold existence risk, as the probabilistic impact occurring from not reaching the threshold is tantamount to the threshold having a probabilistic existence. The current paper will utilise risk in this form, as it maximises our comparability to other studies utilising similar design features.

## **2.2 Expectations & Announcements**

A well-documented element in the literature is the mediating role played by expectations on subjects' contribution behaviour. Authors have consistently found evidence that a significant proportion of the population can be described as conditional cooperators: people who are willing to cooperate if they expect that a significant number of other people will also cooperate (Sugden 1984; Sonnemans et al. 1998; Burlando & Guala 2005; Rege & Telle 2004; Milinski et al. 2008; Iturbe-Ormaetxe et al. 2011; Visser 2013; Gächter 2014). This means that higher expectations of others' contribution will increase the contribution of an individual. Including a threshold adds a coordination point and thus leads subjects to focus their expectations on whether or not their contribution will be pivotal in reaching the threshold level of contributions. A higher expected

probability of being pivotal in reaching the threshold increases an individual's contribution (McBride 2006).

With this in mind, an institutional mechanism which increases the coordination of expectations might result in increased contribution and public good provision (Tavoni et al. 2011). The mechanism of introducing announcements of the future implementation of thresholds in a public goods game might be able to play this expectation-coordinating role. Cavaliere (2001) illustrates how the coordination problem can be solved by subjects using random exogenous signals to correlate their strategies. Public announcements of future threshold implementation might thus be able to serve as such a coordinating device.

Thus, the hypothesised mechanism through which announcements affect behaviour is through their impact on the expectations of individuals. In standard decision theory new information is incorporated into people's subjective probabilities estimates (expectations), so that probabilities are updated as new information is revealed, consistent with Bayes Theorem. This entails that peoples' expectations move consistent with the information that is presented as the theory of rational expectations predicts (Muth 1961). Yet it seems that in practice people find it difficult to perform perfect Bayesian reasoning, due to lack of an adequate mental model and the use of intuitive system of the mind that relies on heuristics and biases for quick judgments (Cole 1989; Van Raaij 1989; Kahneman 2011). This implies that the direction of the announcement effect depends crucially on how the announcement affects expectations, which might be hard to predict *ex ante*. Heim (2007) examines the impact of announcements of future tax rebates and find no announcement effect on total spending. Adams et al. (2001) study the consequences of announcements of future policy reform in pig farming practices and find a positive adjustment effect, but no long run adaptation effects. Blundell et al. (2011) find a positive adjustment effect of increased current labour force participation for single mothers in response to announcements of future in-work tax credit reforms.

The financial markets literature has studied the announcement effect more than any other field. Several authors find evidence that announcements from central banks about monetary policy intentions, or even general policy stances induce widespread and quick reactions in financial markets (Urich & Wachtel 1981; Cornell 1983; Rosa & Verga 2008). The rapid effect of

announcements is facilitated through the impact on the higher-order expectations of analysts and expectations of the market in general (Brown & Han 1992; Kondor 2009). Demiralp & Jordá (2001) describe how the U.S Federal Reserve can control the federal funds rate just by announcing a new target, even without any open market operations, due to the announcement's effect on market expectations.

The current paper seeks to be the first study to isolate the effects of announcements of a future institutional change under different framing and risk contexts. As such, it will utilise the experimental methodology in order to determine the announcements' impact on cooperation. This follows the work done by Visser et al. (2014) which was the first paper to explicitly look at the announcement effect. In this paper the authors made announcements of an impending transition to a peer-punishment institution in a public goods game and found that the announcements cause no adjustment effect, but cause a double adaptation effect: increased numbers of contributors partly mediated by increased use of the punishment institution.

The paper will continue with section 3 giving a description of the methodology used for the experiments. After this, section 4 will provide an analysis of the main results found. Section 5 will then discuss the implications of the results, particularly with regards to the climate change problem and section 6 will conclude.

### **3. Methodology**

#### **3.1 Subjects and Design**

In total 309 students from the University of Cape Town (UCT) participated in the study over one week from the 7<sup>th</sup> of October to the 14<sup>th</sup> of October 2014. Subjects were seated at computer terminals using Z-tree Software (Fischbacher 2007), and randomly split into groups of 3 without knowing the identity their other group members. They were then asked to take part in anonymous choice experiments. It was made clear to subjects that they could earn money based on their choices during the experiment, which would last approximately 2 hours. Before the

experiment began subjects were given a show-up fee of R10 and after the experiment they were asked to complete a short demographic questionnaire<sup>1</sup>. Subjects earned on average R126.48.

Once informed consent was given by all subjects, they were asked to complete two 5 choice gamble tasks with real financial stakes, in order to elicit risk and loss attitudes. This simple procedure follows the methodology of Eckel & Grossman (2008b), wherein subjects were faced with five 50/50 gambles, increasing in expected value and risk, and are asked to choose which gamble they would like to play. This method was chosen as its simplicity makes for an ideal pre-experiment; even though this comes at the trade-off of cruder risk preference classification, this method has been shown to have low noise and good consistency with other measures (Dave et al. 2010; Charness et al. 2013; Crosetto & Filippin 2013). Table 1 illustrates the design of the

**Table 1 Gamble-Choice Task with Expected Payoffs and Risk**

Choice	Event	Probability (%)	Payoff		Expected payoff		Risk*	CRRA ranges**
			Loss	No-Loss	Loss	No-Loss		
1	A	50	14	24	14	24	0	r>1.6
	B	50	14	24				
2	A	50	24	34	16	26	8	0.53<r<1.6
	B	50	8	18				
3	A	50	34	44	18	28	16	0.30<r<0.53
	B	50	2	12				
4	A	50	44	54	20	30	24	0.15<r<0.30
	B	50	-4	6				
5	A	50	54	64	22	32	32	r<0.15
	B	50	-10	0				

\*Measured as standard deviation of expected payoff.

\*\*Calculated as the range of r in the function  $U=x(1-r)/(1-r)$  for which the subject chooses each gamble assuming constant relative risk aversion

gamble-choice task. The method includes two frames: gains frame, where the gambles only contain outcomes of greater than or equal to zero; and a loss frame, where two of the riskiest gambles include the prospect of losing some or all of a subject's show-up fee. A rough estimation of loss aversion can then be revealed by comparing subjects' choices in the different

<sup>1</sup> See Appendix A for demographic questionnaire

frames. Whereas Eckel & Grossman (2008b) utilised a between-subject design to measure the differences in loss and risk attitudes between men and women, we will utilise a within-subject design so that we have measures of risk and loss attitudes for each subject. In our experiment, the loss framed gamble-choice task was played first followed by the no-loss frame. Subjects were told that the choices they make in the gamble-choice experiments might result in the loss of some or all of the R10 show-up. Outcomes of the 50/50 gambles were then decided at the end of the session by the computers generating a random number for each subject. If that number was less than or equal to 0.5 then value A would be paid to the subject, otherwise value B was paid to the subject.

Subsequently, subjects were told that they would now play interactive games for 30 periods. The design of the experiment was that subjects play 20 periods of standard voluntary contribution mechanism Public Goods Games (PGGs) then in the 21<sup>st</sup> period the game changes into a voluntary contribution Threshold Public Goods Game (TPGG), which is to be played for 10 periods until period 30. The main intervention is to see what effect announcing the future institutional change has on subject behaviour. In treatments with announcements, the institutional change occurring in Period 21 is explained fully in Period 11 and then after Period 20 again. Each period following Period 11 then incorporates a short announcement of the impending rule change on subjects' computer screens before play begins. This same design was

**Table 2 3x2 Between-Subject Design**

	<b>Gains Frame</b>	<b>Loss Frame</b>	
	<b>Certain Impact</b>	<b>Certain Impact</b>	<b>Risky Impact</b>
<b>Announcement</b>	Treatment GCA ( <i>N</i> =48)	Treatment LCA ( <i>N</i> =54)	Treatment LRA ( <i>N</i> =57)
<b>No-announcement</b>	Treatment GCN ( <i>N</i> =45)	Treatment LCN ( <i>N</i> =51)	Treatment LRN ( <i>N</i> =54)

tested in a gains framed environment; a loss framed environment; and then a loss framed environment with risk of loss in the TPGG phase if groups do not reach the threshold. Thus, we utilised a 3 (gains frame; loss frame; loss frame + risky impact) x 2 (announcement; no announcement) between-subject design, illustrated in Table 2.

### 3.1.1 PGG phase: Gains and Loss frames

The gains-framed PGG works such that each group member ( $n=3$ ) receives an endowment ( $E$ ) of 20 tokens each period and must then decide how much to allocate to a private account ( $x_i$ ) yielding a return of 1 and how much to allocate to a group account ( $g_i$ ) ( $x_i + g_i = 20$ ), which yields a marginal per capita return (MPCR) of 0.5 for oneself and each other group member. To ease the explanation of the group account to subjects we used a similar method of instructions<sup>2</sup> as Cox (2014), in which it is explained that the group account starts off with zero tokens each period, and each token allocated to the group account will be multiplied by 1.5 and then split equally among all three group members. At the end of each period in the gains-framed PGG, the payoff for an individual is represented by:

$$\pi_i = (E - g_i) + \frac{1.5 \times \sum_j^3 g_j}{3} \quad (1)$$

The loss-framed PGG is modelled similarly to the Public Bad games used by Andreoni (1995b) and Willinger & Ziegelmeyer (1999), except with different parameters and keeping contribution to the group account as the cooperative option and the focal point. Similarly to Cox's (2014) instructions we explained the loss-framed PGG to subjects by saying that the group account starts off with a 90 token deficit, and each token contributed to the group account is multiplied by 1.5 and then subtracted from the deficit. Any deficit remaining in the group account at the end of the period must then be paid for equally out of each group member's remaining private account. For this frame each group member ( $n=3$ ) receives an extra 30 tokens of non-usable endowment each period, to ensure logical equivalence to the gains frame and to mimic an already endowed, untradeable benefit deriving from a public good – such as clean air to breathe. Subjects must still allocate their 20 tokens of usable endowment ( $E$ ) between a private ( $x_i$ ) and a group account ( $g_i$ ). At the end of each period in the loss-framed PGG, the payoff for an individual is represented by:

$$\pi_i = 30 + (E - g_i) + \frac{((1.5 \times \sum_j^3 g_j) - 90)}{3} \quad (2)$$

This equation is derived from manipulating equation (1) and by including a positive 30 and a negative 30 which cancel out. Investing into the group account ( $g_i$ ), reduces the loss faced by the

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<sup>2</sup> Instructions available upon request from the Authors

group by 0.5, while allocating tokens to the private account yields a positive return of 1 for oneself only.

In both the gains-framed and the loss-framed PGG the dominant strategy is to contribute nothing to the public good, yet the welfare maximising solution is achieved only by full contribution from all. Therefore, these two frames illustrate the quintessential element of framing effects, which is the presentation of logically equivalent options in semantically different ways (Krishnamurthy et al. 2001).

### 3.1.2 TPGG phase: Gains and Loss frame

The general design of the TPGG phase of the games combines a number of elements found consistently in the prevailing literature. The thresholds are cumulative in nature, meaning that the sum of total contributions to the public good needs to be at or above the threshold level by the end of the 30<sup>th</sup> period to avoid loss or gain reward (see for example: Milinski et al. 2008; Dannenberg et al. 2011; Tavoni et al. 2011). We also utilise a high threshold level of 75% of total contributable group endowment ( $T=450$ ), which has been shown to decrease cooperation and make public good provision more difficult (Cadsby & Maynes 1999). Similar threshold levels are also found in Rapoport & Suleiman (1993), Coats & Neilson (2005) and McCarter et al. (2010). A high threshold is used in order to examine whether announcements can improve cooperation in a contextual situation which is known for its difficulty. Furthermore, the reward (loss) for reaching the threshold (not reaching the threshold) ( $v$ ) is set at 300 tokens for each group member, in order to align the step return value ( $S.R = \frac{nxv}{T}$ ) to that which Croson & Marks (2000) find to be average in the literature and use in their experiments as the medium condition ( $S.R=2$ ). We do not use any refund or rebate rules, so cumulative contributions below or above the threshold level are wasted.

The gains-framed TPGG rewards groups ( $v$ ) whose total contributions are greater than or equal to the threshold level after 10 periods. Each period, every group member ( $n=3$ ) receives an endowment ( $E$ ) of 20 tokens and must decide how much to allocate to a private account ( $x_i$ ) and how much to a joint account ( $g_i$ ). At the end of the 10 periods of the gains-framed TPGG, the payoff for an individual is shown by:

$$\pi_i = \sum_{t=21}^{30} E_{it} - \sum_{t=21}^{30} g_{it} + v, \quad \text{if } \sum_{t=21}^{30} g_{1t} + g_{2t} + g_{3t} \geq T \quad (3)$$

$$\pi_i = \sum_{t=21}^{30} E_{it} - \sum_{t=21}^{30} g_{it}, \quad \text{if } \sum_{t=21}^{30} g_{1t} + g_{2t} + g_{3t} < T \quad (4)$$

The loss-framed TPGG works in a similar way to the loss-framed PGG in that subjects receive an extra, non-usable 30 tokens each period and a usable endowment (E) of 20 tokens that are investable into either  $x_i$  or  $g_i$ . However, in the loss-frame, groups who do not reach the threshold after 10 periods incur a loss (-v). The payoff for an individual in the loss-framed TPGG after 10 periods is as follows:

$$\pi_i = \sum_{t=21}^{30} 30 + \sum_{t=21}^{30} E_{it} - \sum_{t=21}^{30} g_{it}, \quad \text{if } \sum_{t=21}^{30} g_{1t} + g_{2t} + g_{3t} \geq T \quad (5)$$

$$\pi_i = \sum_{t=21}^{30} 30 + \sum_{t=21}^{30} E_{it} - \sum_{t=21}^{30} g_{it} - v, \quad \text{if } \sum_{t=21}^{30} g_{1t} + g_{2t} + g_{3t} < T \quad (6)$$

Both the loss-framed and the gains-framed TPGG are logically identical and entail dominant strategies of zero contribution if others' contributions are low enough, but also dominant strategies to cooperate such that the threshold is reached exactly. This entails multiple Nash Equilibria, as discussed earlier. In both of these TPGGs, expectations thus play an important role, as one will only contribute as much as one thinks will be pivotal in reaching the threshold given their expectations of the other group members contributions (McBride 2006). With such coordination possibilities inherent to TPGGs, we wish to examine what effect announcements of the future implementation of a threshold have on cooperative behaviour both before the change and after the change, and whether this effect is different depending on framing.

### 3.1.3 Loss & Risk Framed TPGG

The final variation in the treatments is the transition from a loss-framed PGG for 20 periods, into a loss-framed TPGG for 10 periods, where the negative impact from not reaching the threshold only exists with a certain probability ( $p$ ). The decision to only use a *loss-framed* risk treatment is to maximise comparability with other studies utilising the same form of risk and also to more realistically model the prospects of a risk of dangerous climate change from tipping over a threshold, which incorporates elements of scientific uncertainty and environmental resilience uncertainty (Milinski et al. 2008; Hasson et al. 2010; Santos & Pacheco 2011; Tavoni et al. 2011). The expected payoff for an individual after 10 periods of the risky-impact threshold TPGG is:

$$E(\pi_i) = \sum_{t=21}^{30} 30 + \sum_{t=21}^{30} E_{it} - \sum_{t=21}^{30} g_{it}, \quad \text{if } \sum_{t=21}^{30} g_{1t} + g_{2t} + g_{3t} \geq T \quad (5)$$

$$E(\pi_i) = \sum_{t=21}^{30} 30 + \sum_{t=21}^{30} E_{it} - \sum_{t=21}^{30} g_{it} - pv, \quad \text{if } \sum_{t=21}^{30} g_{1t} + g_{2t} + g_{3t} < T \quad (6)$$

The expected payoff function must be used as the situation involves risk. If there is no risk of a negative impact from not reaching the threshold ( $p=0$ ) then there is a dominant strategy to contribute zero, but as the risk increases ( $p>0$ ) there exist ever more Nash Equilibria at the total group account contribution level of the threshold. When the negative impact from not reaching the threshold exists with certainty ( $p=1$ ) the strategy domain becomes that of the certain-impact loss-framed TPGG described in 3.1.2. In this treatment the risk of negative impact from not reaching the threshold is set at 50%, which is a risk rate used in Tavoni et al. (2011) and Milinski et al. (2008) and corresponds to a relatively low risk of impact and has been shown to increase the difficulty of public good provision. Whether the impact occurred for groups with cumulative contributions lower than the threshold level in period 30 was decided by the computer generating a random number for each group. If this random number was less than or equal to 0.5 then the negative impact existed and each group member lost 300 tokens; if the number was greater than 0.5 then the negative impact did not occur and group members only lost the contributions they had invested into the group account over the 10 periods.

## **4. Results**

### **4.1 Overview**

Table 3 provides some summary statistics for each treatment based on the demographic data from the survey. The different number of subjects in each treatment is due to varying participation rates on each treatment day. However, for each frame there is only a small, three-person difference between the announcement and no-announcement treatments, so the results should be unaffected. The table provides evidence that there are no systematic variations between treatments with regards to demographics, yet in all subsequent individual-level regressions demographic control variables are included for further guarantee against spurious effects.

**Table 3 Summary Statistics**

Treatments	GCN	GCA	LCN	LCA	LRN	LRA	All
Number of Subjects	45	48	51	54	54	57	<b>309</b>
Male	0.533 [0.499]	0.438 [0.496]	0.588 [0.492]	0.519 [0.500]	0.426 [0.495]	0.474 [0.499]	<b>0.495</b> <b>[0.500]</b>
Black	0.556 [0.497]	0.625 [0.484]	0.706 [0.456]	0.685 [.465]	0.815 [0.389]	0.684 [0.465]	<b>0.683</b> <b>[0.465]</b>
White	0.178 [0.382]	0.146 [0.353]	0.059 [0.235]	0.093 [0.290]	0.093 [0.290]	0.14 [0.347]	<b>0.117</b> <b>[0.321]</b>
Coloured	0.089 [0.285]	0.125 [0.330]	0.098 [0.297]	0.111 [0.314]	0.019 [0.135]	0.053 [0.223]	<b>0.081</b> <b>[0.273]</b>
Other Race	0.178 [0.382]	0.104 [0.306]	0.137 [0.344]	0.111 [0.314]	0.074 [0.262]	0.123 [0.328]	<b>0.12</b> <b>[0.325]</b>
Below 24 years old	0.822 [0.382]	0.896 [0.306]	0.941 [0.235]	0.963 [0.189]	0.944 [0.229]	0.912 [0.283]	<b>0.916</b> <b>[0.278]</b>
25 years old and older	0.178 [0.382]	0.104 [0.306]	0.059 [0.235]	0.037 [0.189]	0.056 [0.229]	0.088 [0.283]	<b>0.085</b> <b>[0.278]</b>
Humanities	0.378 [0.485]	0.313 [0.464]	0.196 [0.397]	0.093 [0.290]	0.13 [0.336]	0.404 [0.491]	<b>0.25</b> <b>[0.433]</b>
Commerce	0.333 [0.472]	0.375 [0.484]	0.353 [0.478]	0.593 [0.492]	0.5 [0.500]	0.281 [0.449]	<b>0.408</b> <b>[0.491]</b>
Other Faculty	0.289 [0.453]	0.313 [0.464]	0.451 [0.498]	0.315 [0.465]	0.37 [0.483]	0.316 [0.465]	<b>0.343</b> <b>[0.475]</b>
High Income	0.044 [0.206]	0.042 [0.200]	0.098 [0.297]	0.056 [0.229]	0 [0]	0.018 [0.131]	<b>0.042</b> <b>[0.201]</b>
Middle Income	0.6 [0.490]	0.542 [0.498]	0.51 [0.500]	0.611 [0.488]	0.611 [0.488]	0.579 [0.494]	<b>0.576</b> <b>[0.494]</b>
Low Income	0.356 [0.479]	0.417 [0.493]	0.392 [0.488]	0.333 [0.472]	0.389 [0.488]	0.404 [0.491]	<b>0.382</b> <b>[0.486]</b>
Smoker	0.133 [0.340]	0.146 [0.353]	0.137 [0.344]	0.148 [0.355]	0.093 [0.290]	0.105 [0.307]	<b>0.126</b> <b>[0.332]</b>

Standard Deviations in square brackets

The results from this experiment are divided into two parts. Section 4.2 looks at the findings from the two choices made in the gamble-choice tasks before the interactive public goods games. This will be followed by section 4.3 which discusses the announcement effect results from the interactive public goods games in different framing and risk contexts.

## **4.2 Gamble-choice Task Results**

*Result 1: Males are less loss-averse than females. Furthermore, the subject pool in general is slightly risk-averse and loss-averse*

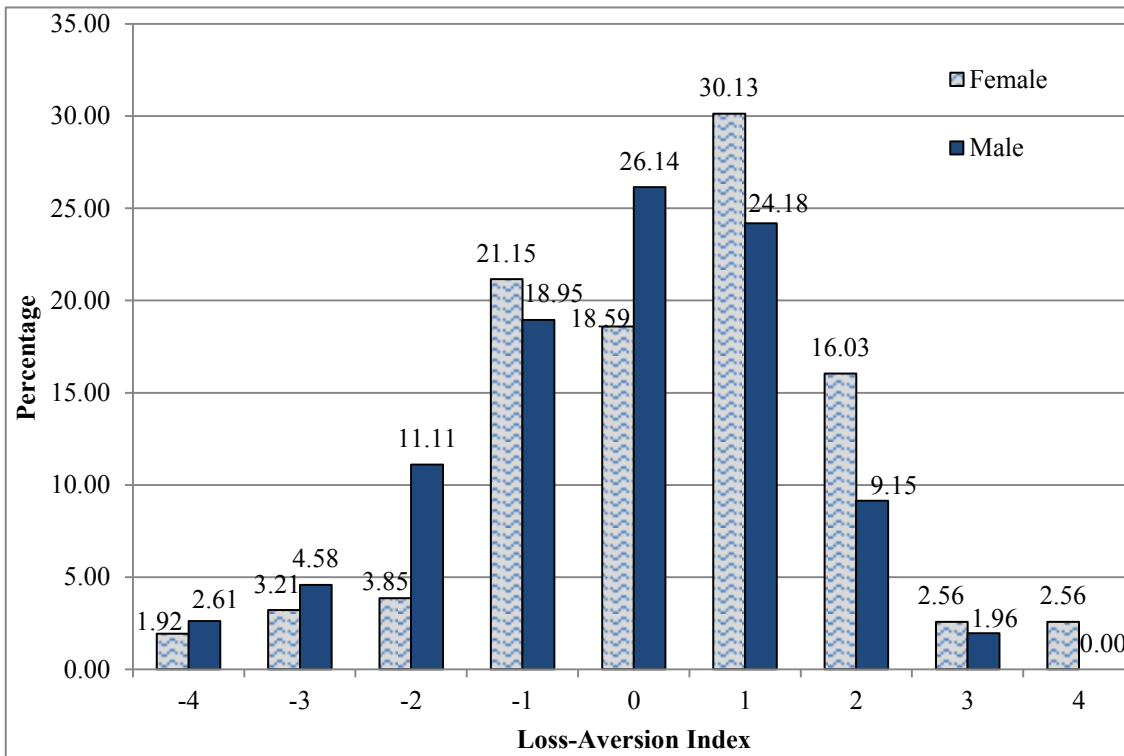
Appendix B shows histograms for the percent of subjects who choose each gamble-choice in both the loss framed and gains framed tasks. From these graphs it seems as though the prospect of losses in choice 4 and 5 in the loss framed task slightly increases subjects' risk-aversion. However, a two-sample means test cannot reject the null hypothesis that the mean choice from the two choice tasks are equal ( $p=0.3054$ ) and an Epps-Singleton test cannot reject the null hypothesis that the choices in the two tasks have the same distribution ( $p=0.2503$ ).

Yet, because of the gamble-choices task within-subject design, looking at the average behaviour between subjects is of little help. What is more interesting is to see how the behaviour of individual subjects changed across the frames. In order to investigate this, we created a variable named the *Loss-Aversion (LA) Index* which is defined as a subject's gamble choice in the gains frame minus a subject's gamble choice in the loss frame and gives each subject a value between -4 and 4. A high LA index means an individual is more risk averse in the presence of losses; and a low LA index means an individual is less risk averse in the presence of losses. For the purposes of this paper loss aversion will be defined as increasingly risk-averse behaviour in the presence of loss prospects.

$$\text{Loss Aversion Index (LA)} = \text{Choice}_{\text{gains}} - \text{Choice}_{\text{loss}} \quad (7)$$

The intuition behind this variable follows the logic of (Eckel & Grossman, 2008b) who state that a crude measure of loss aversion can be created by observing the difference in risk choice between the two frames. Both choices are logically equivalent and differ only in the prospect of potential losses in the final two choices in the loss frame, thus if choices are the same in the [gains] treatment but differ in the loss treatment, then that would imply difference in loss aversion rather than risk aversion' (Eckel & Grossman, 2008b: 4). The structure of our within-subject design may have caused ordering effects which can bias responses. However, because all subjects faced the same ordering effects we can still investigate relative differences in risk and loss attitudes between subjects.

Appendix C shows the results of Tobit regressions testing if any of the demographic variables are significant predictors of the two gamble choices and the LA index. All regressions contain robust standard errors clustered at the individual level. These regressions show that *male* is the most significant of the demographic variables, and therefore columns 4-6 include this as the only regressor to improve the fit of the models. The male variable is significantly and positively related to the loss framed gamble choice; insignificant in the gains framed gamble choice; and significantly and negatively related to the LA index. This implies that females are more risk averse in the presence of loss-prospects relative to males but have the similar risk preferences in the gains framed task, which equates to females being significantly more loss averse with respect to the constructed LA index. The average LA index value for a male is -0.13 while that for a



**Figure 1 Loss Aversion Index by Gender**

female is 0.34 and a two sample means test confirms the difference is significant ( $p= 0.0081$ ). Figure 1 presents a percentage histogram of the distribution of LA index values by gender. It can be seen that the most likely LA index value for a male is 0 while for a female it is 1, implying lower loss aversion amongst males. An Epps-Singleton test rejects the null hypothesis that the LA index values of males and females have the same distribution ( $p= 0.07798$ ).

The finding that females are more risk averse than males seems to be quite widespread (Eckel & Grossman 2008b; Dave et al. 2010). However, Eckel & Grossman (2008b) find that females are significantly more risk averse in both the loss *and* gains framed tasks, which they interpret as the gender differences in choices not being driven by loss aversion alone. Yet Crosetto & Filippin (2013) argue that gender differences in risk attitudes are not as prevalent as many claim, and that behavioural differences across genders in situations of uncertainty are due to females being more loss averse rather than more risk averse. Indeed, these gender effects in loss aversion are also found in Schmidt & Traub (2002). Crosetto & Filippin (2013) attribute the misinterpretations as due to certain aspects of some elicitation methods, so it could be that changing Eckel & Grossman's (2008b) model into a within-subject design, as we have done here, increases its predictive accuracy, at least in ordinal terms. We also find that the mean LA index value for the subject population is 0.106 and the mean average gamble choice ( $(\sum_i^{309} \frac{\text{Sum of Gamble Choices}_i}{2})/309$ ) for the subject population is 2.6. These numbers imply that the subject pool is slightly loss averse (LA index > 0) and risk averse with a CRRA coefficient of between 0.53 and 1.6 (refer to Table 1). Evidence for similar patterns of overall risk aversion in a sample population is also found in Holt & Laury (2002) and measured evidence for loss aversion is found in Abdellaoui et al. (2008), although the coefficients found in the latter are not comparable to the *LA index* coefficient here due to the ordering effects present in this study which render the variable mainly useful for relative comparisons.

Our measures of risk preferences and our estimated LA index seem to tie in well with previous findings and thus may be seen as largely credible. We therefore feel confident to include them as estimators for subject behaviour in the subsequent analysis of the interactive games. This paper will be the first, to the authors' knowledge, to include measures of risk preferences *and* loss aversion as predictors of subject behaviour in differently framed public goods games.

### **4.3 Announcement Effect in Interactive Public Goods Games**

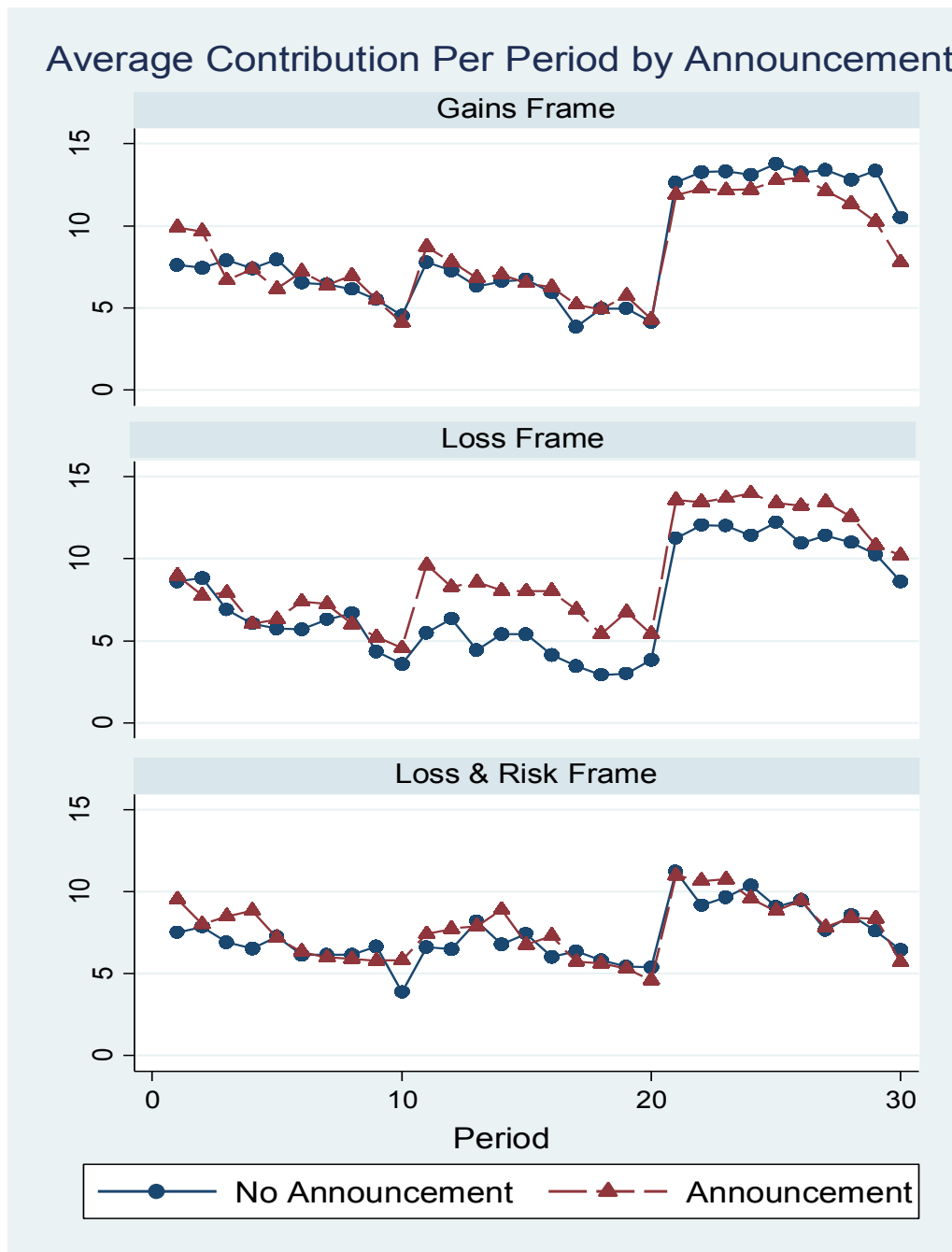
The analysis of the announcement effect is split into two sections relating to the adjustment effect, before the implementation of the new institution, and the adaptation effect, after the implementation of the new institution. Section 4.3.1 will discuss the adjustment effect on current

subject behaviour in different framing and risk contexts. Section 4.3.2 will then explain the results of the adaptation effect, post-implementation of the TPGG rule change, in different framing and risk contexts. For each of the following results, the between-subject design of the interactive games will become apparent as the analysis will be conducted for each different frame separately between the announcement treatment and no-announcement treatment (refer to table 2).

#### **4.3.1 Adjustment Effect: Periods 11-20**

*Result 2: There is no contribution adjustment effect in the gains frame or the loss-and-risk frame, but there is a significant and positive contribution adjustment effect in the loss frame.*

In order to examine whether the announcements of a future rule change impact subject contribution behaviour before the actual implementation of the rule change, we initially ran some non-parametric tests on contribution levels. In both the gains frame (GCN/GCA) and the loss-and-risk (LRN/LRA) framed public goods games, Kruskal-Wallis tests showed no significant differences between the contribution levels in the announcement and no-announcement treatments for the periods 11-20 (GCN/GCA:  $p=0.2530$ ; LRN/LRA:  $p=0.3350$ ). However, in the loss frame (LCN/LCA) the same Kruskal-Wallis test shows contribution to be significantly higher in LCA relative to LCN ( $p=0.0001$ ). Figure 2, showing the average contribution per period in each treatment, also supports this finding as the average contribution per period in the loss frame with announcement seems to be considerably higher than that of the loss frame with no-announcement, while the other two frames do not seem to vary much across the announcement.



**Figure 2 Average contribution per period in each treatment**

To further unpack these findings we ran tobit regressions for each framing context in tables 4, 5 and 6. In each regression the dependent variable is contribution and demographic controls are included. All standard errors are robust and clustered at the individual level. The variable *Sum of Gamble choices* is a measure of an individual's overall risk preferences. The higher one's

gamble choice in each of the tasks the lower one's estimated risk aversion coefficient, therefore the addition of each subject's two gamble choices can be seen as a rough measure of how much risk they are willing to accept. The main result from Tables 4, 5 and 6 is found in the dummy variable *announcement*. The announcements are made in the announcement treatments from period 11 onwards, therefore we do not expect the announcement and no-announcement treatments to be significantly different in periods 1-10 and this is included in the tables as a baseline. The adjustment effect would therefore be evidenced by significant differences in contribution behaviour during periods 11-20.

In both the gains (Table 4) and the loss-and-risk frame (Table 6), the announcement dummy is insignificant in both periods 1-10 and 11-20, implying that the announcement does not cause any contribution adjustment effects. However, in the loss frame (Table 5) we see that the announcement causes a large, positive and very significant ( $p= 0.000$ ) impact on contribution. In the loss frame, receiving the announcement increases the average contribution amount by 4.693

**Table 4 Gains Frame: Contribution Adjustment Effects<sup>3</sup>**

Independent Variables	Dependent Variable: Contribution	
	Periods 1-10	Periods 11-20
Lagged sum of others' contribution	0.230*** (0.0402)	0.323*** (0.0480)
Announcement (GCA)	0.753 (1.133)	0.821 (1.234)
Sum of Gamble choices	-0.369 (0.339)	-0.196 (0.332)
Loss Aversion index (LA)	0.704 (0.428)	0.276 (0.479)
Period	-0.525*** (0.135)	-0.653*** (0.142)
Constant	4.814** (2.414)	11.79*** (3.194)
Demographic Control Variables	Yes	Yes
Observations	837	930
Prob > F	0.0000	0.0000
Pseudo R <sup>2</sup>	0.0233	0.0367

Robust standard errors in parentheses  
\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

<sup>3</sup> The significance of results from all the regressions described in this paper are robust to the exclusion of the Loss Aversion index variable and the Sum of Gamble choices variable.

**Table 5 Loss Frame: Contribution Adjustment Effects**

Independent Variables	Dependent Variable: Contribution	
	Periods 1-10	Periods 11-20
Lagged sum of others' contribution	0.286*** (0.0436)	0.478*** (0.0584)
Announcement (LCA)	1.744 (1.269)	4.693*** (1.318)
Sum of Gamble choices	-0.523 (0.324)	-0.342 (0.361)
Loss Aversion index (LA)	-0.311 (0.378)	-0.582 (0.432)
Period	-0.519*** (0.147)	-0.555*** (0.167)
Constant	4.271 (2.920)	6.030 (4.520)
Demographic Control Variables	Yes	Yes
Observations	945	1,050
Prob > F	0.0000	0.0000
Pseudo R <sup>2</sup>	0.0303	0.0633

Robust standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 6 Loss & Risk Frame: Contribution Adjustment Effects**

Independent Variables	Dependent Variable: Contribution	
	Periods 1-10	Periods 11-20
Lagged sum of others' contribution	0.226*** (0.0387)	0.278*** (0.0522)
Announcement (LRA)	-0.279 (1.124)	-0.809 (1.526)
Sum of Gamble choices	-0.225 (0.276)	0.0842 (0.397)
Loss Aversion index (LA)	-0.312 (0.333)	-0.657 (0.597)
Period	-0.464*** (0.113)	-0.476*** (0.115)
Constant	7.549*** (2.672)	7.321* (3.887)
Demographic Control Variables	Yes	Yes
Observations	999	1,110
Prob > F	0.0000	0.0000
Pseudo R <sup>2</sup>	0.0238	0.0186

Robust standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

in periods 11-20, *ceteris paribus*. It is also important to see that the announcement variable is not significant for periods 1-10 in the loss frame, as this was before the announcements began in period 11, indicating that without the announcement these two treatments have no significant differences in contributions.

Interesting to note is that for all three tables the variable *lagged sum of others' contribution*, which is how much the rest of one's group contributed in the previous period, is positive and significant at the 1% level for periods 1-10 and 11-20. This implies that a highly important predictor of one's current contribution in all frames is how much the rest of one's group contributed in the previous period. Neither the sum of gamble choices nor the LA index variables show any real significance in any of the tables. However, it is interesting to see that the sign of the LA index is positive in the gains frame (Table 4) and negative in the two loss frames (Tables 5 & 6). This suggests that loss aversion might work differently depending on the frame, and might be a part of the explanation of framing effects.

*Result 3: In the loss frame, the announcement significantly increases the likelihood of a subject being a full cooperater and significantly decreases the likelihood of a subject being a free rider. However, in the gains and loss-and-risk frame there is no evidence of any such player strategy adjustment effects.*

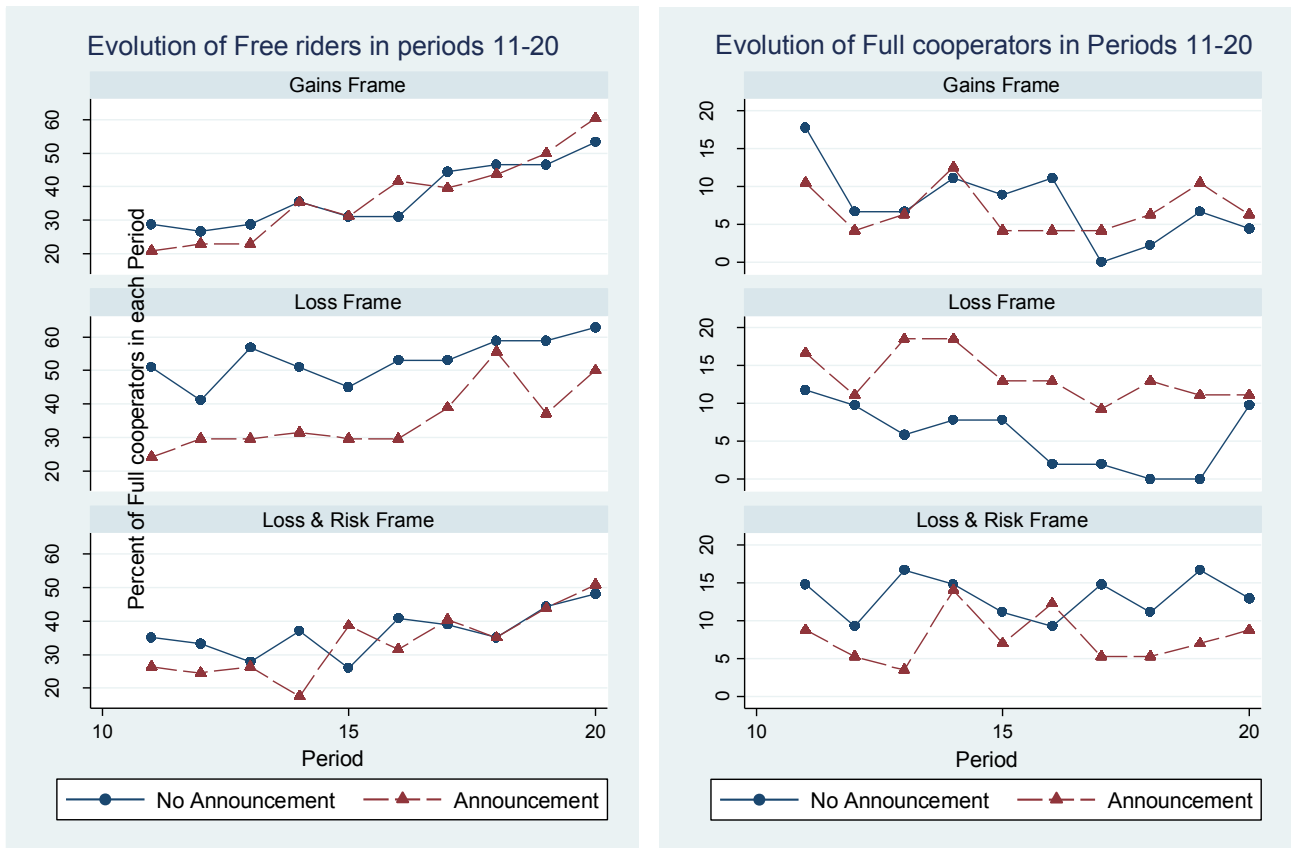
In order to be a full cooperater a subject must have contributed their full endowment of 20 tokens to the group account for the period; and in order to be a free rider a subject must have contributed 0 tokens to the group account. We created a dummy variable for each of these two player types, equal to 1 if their definition is met and equal to 0 if not. To see if the announcement changes the likelihood of a subject being either a full cooperater or a free rider in each of the three different framing contexts we employed Kruskal-Wallis non-parametric tests and a series of logit regressions in Appendices D, E and F. Four regressions were run for each frame: periods 1-10 and 11-20 with the dependent variable the dummy for being a full cooperater; and periods 1-10 and 11-30 with the dependent variable the dummy for being a free rider. All regressions report logit coefficients and robust standard errors clustered at the individual level. In the gains frame Kruskal-Wallis tests find no significant differences between the GCN and GCA treatment with regards to the numbers of full cooperaters ( $p=0.6886$ ) and the numbers of free riders

( $p=0.8848$ ) for periods 11-20. This is confirmed by Appendix D which shows the announcement dummy to be an insignificant predictor on the likelihood of an average subject being either a full cooperators or a free rider.

In the context of the loss frame, initial Kruskal-Wallis tests for the periods 11-20 indicate that the announcement has a significant and positive influence on the numbers of full cooperators ( $p=0.0001$ ) and a significant and negative impact on the numbers of free riders ( $p=0.0001$ ). This finding is supported by Appendix E, which also shows that the announcement dummy is only significant in each regression for periods 11-20, after the announcements have been made. The marginal effects of the announcement in the loss frame increase the likelihood that the average subject is a full cooperators by 4.03% ( $p= 0.032$ ) and decrease the likelihood that the average subject is a free rider by 14.25% ( $p= 0.012$ ).

Kruskal-Wallis tests for the loss-and-risk frame suggest no significant differences in the number of free riders ( $p= 0.2706$ ) between LRN and LRA, but significantly less full cooperators in LRA ( $p= 0.0030$ ) in periods 11-20. The logit regressions in Appendix F corroborate the insignificant impact of the announcement with regards to the likelihood of free riders, but do not support the finding of the announcement having any significant effect on the likelihood of full cooperators. This shows that once other variables are controlled for, the announcement does not have any significant adjustment effect on the likelihood of player-types in the loss-and-risk frame.

Figure 3 provides an illustration of the percentage of subjects playing as a free rider or full cooperators in each period for periods 11-20. The patterns shown in this graph support the initial Kruskal-Wallis tests, especially with regards to the largest effect of the announcements on player strategies being in the loss framed context. These findings imply that the announcement's positive adjustment effect of higher contributions found in the loss frame in *Result 2* is driven by the announcement causing significantly more people to fully cooperate and significantly fewer people to completely free-ride. One might see this as the announcement causing greater overall cooperation in the loss frame. This interpretation is also strengthened by the fact that there are no contribution adjustment effects for the announcements in the gains and loss-and-risk frames and, correspondingly, no player-type adjustment effects for either of these two frames.



**Figure 3 Evolution of Player Strategies in Periods 11-20**

In terms of climate change, results 1 and 2 imply that announcements of future climate thresholds can influence country emission abatement behaviour positively. However, it is important that the announcements are framed in the loss context, emphasising how cooperation can reduce the losses faced by all. Perhaps the positive adjustment effects on contribution are the result of group members using the announcement as information of the necessity of future coordination, and thus they use Period 11-20 as a mock-trial: increasing their cooperation to test whether their group will be able to reach the threshold by the end of ten periods when it counts. In other words, maybe people reason to themselves: I will use the next ten periods as a test to see whether, if I cooperate fully, my group members will join me and we can reach the amount of required cooperation before the rule change, thus signalling our coordination intentions for when the threshold game actually begins". In this way, announcements of future climate

thresholds are vital for society to test how much each country is willing to cooperate to signal their commitment to avoiding devastation when the time comes

### 4.3.2 Adaptation Effect – Periods 21-30

*Result 4: The announcement significantly increases the likelihood of groups reaching the threshold after the implementation of the new rules in the loss frame. However, the announcement significantly decreases the likelihood of groups reaching the threshold in the gains frame; and the announcement has no significant impact on the probability of groups reaching the threshold in the loss-and-risk frame.*

Figure 2 shows that the implementation of the threshold rules, beginning in period 21, results in significantly increasing the average contribution per period in all treatments. This rise in contribution is to be expected, as the threshold game reduces the conflict between individual and collective interests inherent to the normal public goods game; and implies increased benefit to the individual for coordinated cooperation (Cadsby & Maynes 1999; Abele et al. 2010). However, the trends between the announcement and no-announcement treatments in Figure 2 also seem to suggest that the announcement is causing increased contribution in the loss frame, decreased contribution in the gains frame, and no real impact in the loss-and-risk frame. Kruskal-Wallis tests confirm the significance of these announcement effects on contribution in the gains frame ( $p=0.0166$ ) and the loss frame ( $p= 0.0001$ ) and the insignificant effects in the loss-and-risk frame ( $p= 0.8239$ ).

**Table 7 Groups that reached the threshold by treatment and announcement**

	<u>No Announcement</u>			<u>Announcement</u>		
	<b>Total Groups in Treatment</b>	<b>Total Groups Reached Threshold</b>	<b>Percentage of Groups Reached Threshold</b>	<b>Total Groups in Treatment</b>	<b>Total Groups Reached Threshold</b>	<b>Percentage of Groups Reached Threshold</b>
Gains Frame	15	8	53.33%	16	5	31.25%
Loss Frame	17	7	41.18%	18	11	61.11%
Loss & Risk Frame	18	4	22.22%	19	6	31.58%

Table 7 and Figure 4 show that these announcement effects on contribution in the gains and loss frames correspond to similar patterns in the number of groups who reach or exceed the threshold in period 30. It can be seen that in the announcement treatment 22.08% fewer groups reaching the threshold relative to the no-announcement treatment in the gains frame. Whereas the announcement treatments in the loss and the loss-and-risk frames results 19.93% and 9.36%, respectively, more groups reaching the threshold compared to the no-announcement treatments.

These adaptation effects of announcements on the likelihood of reaching the threshold in period 30 are further explored in logit regressions reporting coefficients with robust standard errors clustered at the individual level (Appendix G). Announcements in the gains frame seem to result in a significantly lower likelihood ( $p = 0.011$ ) of a group reaching the threshold on average, while in a loss frame they seem to result in a significantly higher likelihood ( $p = 0.045$ ) of a group reaching the threshold on average. In the loss-and-risk frame, the slightly higher percentage of groups reaching the threshold seen in the announcement treatment in Figure 4 is shown to not be significant ( $p = 0.181$ ).

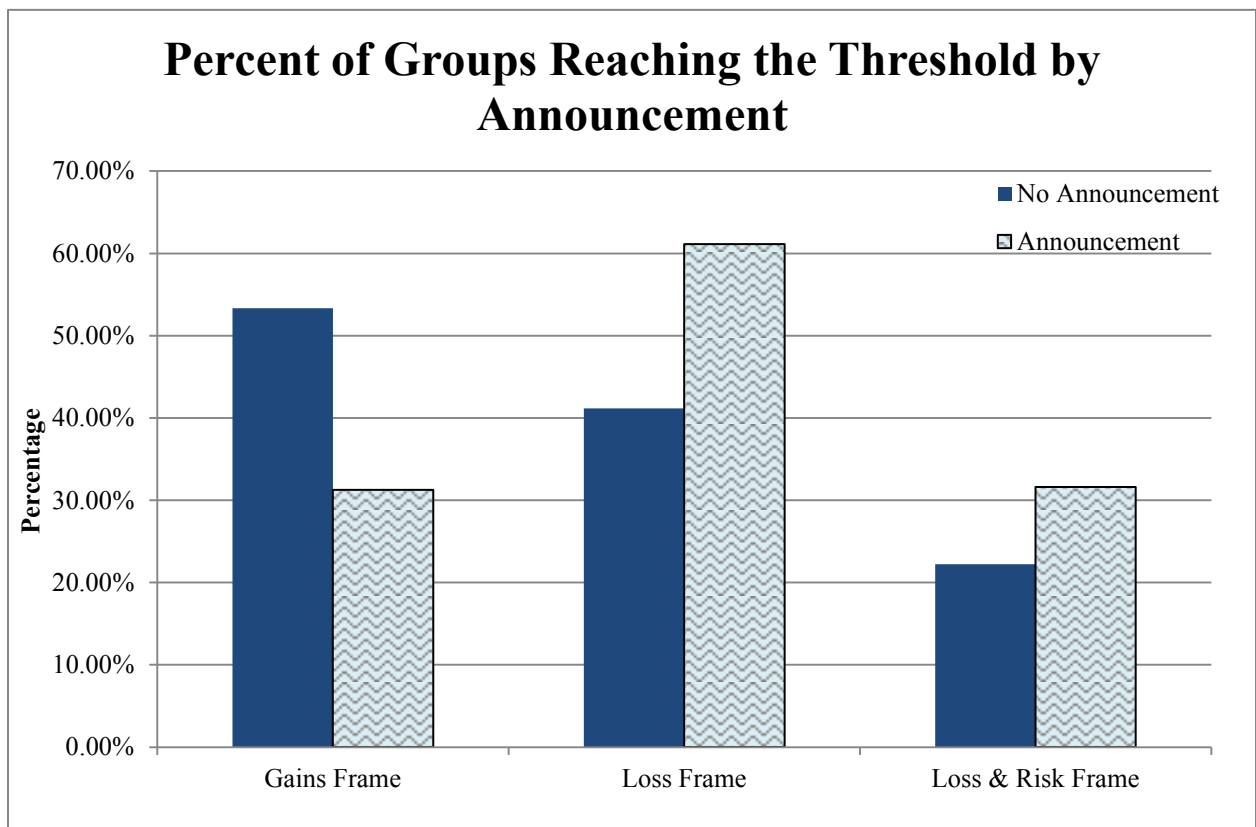


Figure 4 Percentage of Groups reaching the threshold in each treatment

### *Average Threshold Required per Period*

However, the models in appendix G all suffer from a rather small sample size, ranging from 93 to 111 subjects, because the sample has been limited to only looking at period 30 of the threshold public goods game. Consequently, the Wald  $\chi^2$  statistics for all of these regressions is small, meaning that we cannot reject the null hypothesis that all of the independent variables have no effect on the likelihood of reaching the threshold in period 30. Therefore, in order to be more confident about the results from appendix G, we ran new logit regressions in Table 8 using a larger sample size.

This was done by changing the dependent variable to a dummy variable (*Average Threshold Required*) that equals 1 if a group reached or exceeded the average required group account contributions per period (AT) to reach the threshold in period 30 (AT = 45 tokens). This number is what a group must contribute each period on average in order to reach the threshold level of 450 cumulative group account contributions by the end of the 10 periods of the threshold public goods game. With the use of this dependent variable, the regressions could then be run over the periods 21-30 which considerably increases the sample size. Table 8 reports logit coefficients with robust standard errors clustered at the individual level and each regression includes demographic control variables.

The results in Table 8 corroborate the findings in appendix G. The announcements in a loss frame context result in a significantly higher likelihood of an average individual's group reaching the average required threshold per period. Whereas the announcements in the gains frame context results in a significantly lower likelihood of an average individual's group reaching the average required threshold per period. Finally, in the loss-and-risk frame the announcement has no significant impact. The marginal effects for the announcement in the loss frame are such that being in the announcement treatment causes the average individual to be 12.03% more likely to be in a group that reaches or exceeds the 45 token average required group account contribution per period; while being in the announcement treatment in the gains frame decreases the average individuals probability of being in a group that exceeds the 45 token required average by 16.57% per period. These adaptation effects in both the loss and gains frames are significant at the 5% level. The models in Table 8 also exhibit better overall

prediction for the variation in the dependent variable, with each regression having a high Wald  $\chi^2$  statistic.

**Table 8 Likelihood of Reaching the Average Threshold per Period in Different Frames**

Independent Variables	Dependent Variable: Average Threshold Required for periods 21-30 (0/1)		
	Gains Frame	Loss Frame	Loss and Risk Frame
Lagged Sum of Contributions	0.0873*** (0.00950)	0.0943*** (0.00711)	0.118*** (0.00888)
Announcement	-0.670** (0.270)	0.509** (0.227)	0.370 (0.256)
Loss Aversion (LA) Index	-0.211** (0.0874)	-0.0147 (0.0703)	0.134* (0.0775)
Sum of Gamble Choices	0.145** (0.0727)	-0.0537 (0.0603)	0.0795 (0.0665)
Period	-0.141*** (0.0336)	-0.148*** (0.0253)	-0.108*** (0.0300)
Constant	-0.259 (1.005)	-0.456 (0.893)	-3.022*** (0.961)
Demographic Control Variables	Yes	Yes	Yes
Log pseudo-likelihood	-458.454	-517.575	-396.725
Observations	930	1,050	1110
Wald $\chi^2$	117.79	314.94	243.41
Prob > $\chi^2$	0.0000	0.0000	0.0000
Pseudo R <sup>2</sup>	0.2886	0.2851	0.3957

Robust Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The effects of the loss aversion and risk preference variables are not consistent throughout the frames. In the gains frame, higher levels of loss aversion and risk aversion appear to diminish the probability of an average individual being in a group that reaches the average group account contribution required per period, both significant at the 5% level. While in the loss frame both the variables have little of both practical and statistical significance. Then in the loss-and-risk frame only the loss aversion variable is significant (at the 10% level) and suggests that higher levels of loss aversion relate to a higher likelihood of an average individual being in a group that reaches the average required group account contributions per period, which is the opposite of the effect found in the gains frame.

As a brief aside, one might wonder whether there is any difference between the groups that were successful in reaching the threshold in Period 30 without announcements and those that did it after being forewarned with announcements. Appendix H shows the graphs of the average group account contribution per period for groups that succeeded in reaching the threshold across frames and whether or not they received the announcements. The trends from the graphs seem to show that in the loss frame and the loss-and-risk frame, successful groups that received announcements made stronger, earlier cooperative action relative to successful groups that did not receive the announcement. This is reinforced by Kruskal-Wallis tests on total group account contributions per period between no-announcement treatments and announcement treatments for the first half of the threshold phase (periods 21- 25) in both the loss frame ( $p=0.0023$ ) and the loss-and-risk frame ( $p=0.0011$ ). This effect of strong, early action in successful groups is not present in the gains frame ( $p=0.2368$ ). However, this finding must be tempered by the observation that in both the loss and the loss-and-risk frames, the announcement treatments have more successful groups and this might be biasing the Kruskal-Wallis tests. Nevertheless, such adaptation effects of strong, early action due to announcements have been found in Visser et al. (2014) who find that announcements of future punishment opportunities result in the increased and more effective early-utilisation of the punishment institution, once it is implemented.

Result 3 implies that early warnings of climate change, when made in a loss frame, can significantly improve society's chances of avoiding a dangerous future climate threshold, relative to an unannounced discovery of future climate thresholds, by incentivising increased cooperation and coordination. Possibly this increased cooperation is due to the announcements increasing people's estimation of *their* contribution being pivotal and necessary to make the difference. The important role of subjective expectations of being pivotal have been well noted (e.g. (Rapoport & Eshed-Levy 1989; McBride 2006; Makris 2009; McBride 2010)). This feeling of being pivotal might also explain the observation of stronger, early action by groups forewarned with announcements. This would have major implications for climate change policy, especially considering the importance that *The Stern Review* places on strong, early action in order to solve global warming (Tol & Yohe 2006) and the positive effects that early signals of cooperative intent have on the successful provision of threshold public goods in sequential games (Dannenberget al. 2011; Normann & Rau 2011).

*Result 5: The adaptation effects of the announcement on the likelihood of groups reaching the threshold in the gains and loss frames may be driven by the announcement's impact on particular player strategies. The announcement in the gains frame seems to diminish the amount of subjects contributing according to a fair-share principle. Whereas the announcement in the loss frame seems to increase the number of subjects playing as altruists and diminish the numbers of those playing as free-riders.*

In trying to uncover the announcements' adaptation effects on player types, we constructed three different player type variables: altruist, fair-sharer and free-rider. A free-rider is defined as a subject who contributes zero tokens to the public account for the period. A fair-sharer is defined as a subject who contributes their fair share of the average required group account contribution per period, which is 45 tokens shared equally amongst all three group members ( $AT/3 = 15$  tokens). An altruist is defined as a subject that contributes anything more than their fair share; anything greater than 15 tokens.

Initial Kruskal-Wallis tests for the periods 21-30 suggested that the announcement in the loss frame lead to significantly more instances of subjects being altruists ( $p=0.0104$ ) and significantly fewer instances of subjects free-riding ( $p=0.0001$ ). While Kruskal-Wallis tests for the same periods in the gains frame suggested that the announcement causes significantly more instances of subjects being free-riders ( $p=0.0093$ ) and significantly fewer instances of subjects being fair-sharers ( $p=0.0002$ ). Looking at graphs of the evolution of the percentage of these player types in each frame by announcement seems to support the findings of the Kruskal-Wallis tests (Appendices I and J).

However, logit regressions on different player type for the gains and loss frames in tables 9 and 10 provide less conclusive evidence for the importance of announcements as predictors of player types in periods 21-30. In the loss frame the announcement's positive impact on the likelihood of altruists and negative impact on the likelihood of free-riders is only significant at the 14% level ( $p=0.133$  and  $p=0.130$  respectively). While in the gains frame the announcement's negative impact on the likelihood of fair-sharers is only significant at the 11% level ( $p=0.104$ ),

**Table 9 Loss Frame: Likelihood of Player Type in periods 21-30**

Independent Variables	(1) Free-rider (0/1)	(2) Fair-sharer (0/1)	(3) Altruist (0/1)
Lagged Sum of Contributions	-0.117*** (0.0149)	0.0368*** (0.00772)	0.0610*** (0.0115)
Announcement	-0.585 (0.386)	0.0893 (0.299)	0.466 (0.310)
Loss Aversion Index (LA)	-0.0982 (0.125)	-0.0490 (0.0935)	-0.105 (0.0957)
Sum of Individual's Gamble Choices	0.0170 (0.0861)	-0.163** (0.0698)	0.0271 (0.0678)
Period	0.293*** (0.0415)	-0.179*** (0.0243)	0.0243 (0.0320)
Constant	-4.283*** (1.386)	2.964*** (0.880)	-3.742*** (1.076)
Demographic Control Variables	Yes	Yes	Yes
Log pseudo-likelihood	-295.469	-612.065	-525.095
Observations	1050	1050	1050
Wald chi <sup>2</sup>	122.50	81.02	57.96
Prob > chi <sup>2</sup>	0.0000	0.0000	0.0000
Pseudo R <sup>2</sup>	0.4049	0.1025	0.1227

Robust standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

and its positive impact on the likelihood of free-riders is insignificant ( $p= 0.479$ ). The lower significance of the announcement dummies found in these regressions implies that once other variables are controlled for, announcements do not seem to have as much impact on the likelihood of an average individual being a particular player type.

However, (apart from free-riders in the gains frame) the announcement variables still retain *some* significance, which might be an indication that some of the adaptation effects on the likelihood of reaching thresholds found in the gains and loss frame are driven by the announcements having a slight impact on player strategies. These patterns in player strategy and probability of reaching the threshold are quite harmonious. In the gains frame, the announcements lead to lower likelihoods of reaching the threshold, and this might be partly mediated by the announcements causing less people to play the strategy of fair-sharing. While in the loss frame, the announcements increase the likelihood of reaching the threshold, and this

could be somewhat due to the announcement decreasing the likelihood of players free-riding and increasing the likelihood of players acting as altruists.

**Table 10 Gains Frame: Likelihood of Player Type in periods 21-30**

Independent Variables	(1) Free-rider (0/1)	(2) Fair-sharer (0/1)	(3) Altruist (0/1)
Lagged Sum of Contributions	-0.0848*** (0.0145)	0.0420*** (0.0111)	0.0253*** (0.00770)
Announcement	0.277 (0.391)	-0.527 (0.324)	0.000995 (0.290)
Loss Aversion Index (LA)	0.133 (0.152)	-0.138 (0.116)	0.0758 (0.106)
Sum of Individual's Gamble Choices	-0.166 (0.152)	0.0396 (0.0778)	0.143** (0.0667)
Period	0.223*** (0.0453)	-0.171*** (0.0284)	0.0439 (0.0345)
Constant	-5.006*** (1.493)	3.608*** (0.952)	-4.204*** (0.954)
Demographic Control Variables	Yes	Yes	Yes
Log pseudo-likelihood	-316.567	-554.463	-492.506
Observations	930	930	930
Wald chi <sup>2</sup>	75.33	55.70	31.01
Prob > chi <sup>2</sup>	0.0000	0.0000	0.0055
Pseudo R <sup>2</sup>	0.2295	0.1074	0.0651

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## **5. Discussion**

The results as a whole from the experiment presented in this paper suggest that early warning announcements can have a significant impact on cooperation and the efficiency of behaviour in light of future institutional change. Important insights for the climate policy debate with respect to how announcements should be framed are also found.

The lack of consistent significance of the loss aversion (*LA index*) and risk preference variables (*sum of gamble choices*) throughout the results is an interesting aside. This could indicate that these individual-level variables are not important predictors of individual

contribution behaviour in a group context or the probability of a group reaching the threshold. Perhaps what might be more fruitful for further research would be the use of group level variables for aggregate risk preferences and loss aversion seeing as decisions are made within the context of a continuing group identity. One would especially think that in the loss-and-risk treatment there should be more significance for the risk preference variable than found here. Conversely, it could be the case that the Eckel & Grossman (2008b) elicitation method, as used here, is not an accurate measure of these risk and loss attitudes. Future research could test this hypothesis by using different elicitation methods. However, this caveat is separate from the focus of the current paper which is to determine the effect of announcements of future institutional change.

It appears as though the context in which announcements of future institutional change are made significantly affects their impact on people's behaviour. When the situation is framed in the gains domain, the announcements do not have any impact on the cooperative behaviour of individuals before the new institution is implemented (adjustment effect), but has the negative impact of significantly reducing the likelihood that groups will meet the required threshold after the new institution has begun (adaptation effect). There is weak evidence that this is partly working through announcements reducing the number of subjects playing the fair-share strategy and increasing the levels of free-riders.

On the other hand, when the context is framed in the loss domain, the announcements cause positive adjustment effects in the form of significantly higher contributions, mediated through increasing the instances of full-cooperators and diminishing the instances of free-riders. Additionally, once the new institution is implemented, those groups previously warned through announcements face positive adaptation effects of significantly higher likelihoods of reaching the required threshold level of contributions. These adaptation effects may be working through the announcements' impact on extreme player types: causing greater numbers of altruists and fewer free-riders, but the inconclusive nature of this evidence means we cannot properly reject the alternative explanation of the announcements' adaptation effects being due to increased general overall cooperativeness and coordination.

Our third context supports the findings in the previous literature on collective-risk social dilemmas by showing that the inclusion of impact risk in the loss frame reduces contributions and the numbers of groups reaching the threshold, from 41.18% to 22.22%. Our results also show the negative impact of risk on the effectiveness of announcements in the loss frame. The inclusion of the 50% probability of a negative impact if group contributions do not meet the threshold in the 30<sup>th</sup> period causes the announcements to have no significant adjustment or adaptation effects. This result has implications for future research to tease out what psychological element about announcements containing uncertain information causes the negation of the adjustment and adaptation effects.

A natural application of this research is the efficacy of early warning systems in the climate change debate. An integral part of early warning systems is the dissemination of timely information in order to enable threatened individuals to prepare and act so as to reduce the possibility of harm (UNISDR 2010). This information dissemination can be seen as a series of announcements, such as those described in the current paper. Numerous studies have attested to the possibility and the benefits of early warning systems with regards to the negative impacts of tipping over dangerous climate thresholds, in particular the two degree Celsius warming threshold (Lenton 2011; Rogers & Tsirkunov 2011; Huntingford et al. 2012; Marvin et al. 2013; Ludescher et al. 2014). Alfieri et al. (2012) attribute a large degree of the benefits of early warning systems to increased population preparedness and coordination, as well as a necessary philosophical perspective change from post-disaster recovery to disaster prevention. However, what is lacking from the literature is a focus on how early warning initiatives can actually affect individual-level cooperative decisions; it has even been stated that ‘the individual’s willingness to act cannot be taken for granted’ (Rogers & Tsirkunov 2011: 14). The current study is an initial probe into this void in the literature. The climate change debate and announcements of future tipping points have naturally been spoken of in the loss frame, and our results show that this is the most advantageous way to frame announcements of future thresholds: with positive adjustment and adaptation effects resulting. However, less optimistic is the diluting effect of risk. It seems that if we cannot convince people of the certainty of disaster if the climate temperature threshold is exceeded, then announcements will have no impact. This is particularly worrisome in a debate that is already fraught with much ambiguity, scientific uncertainty and impact

asymmetry; never mind the ethical issues of unequal historical emissions responsibilities (Lorenzoni et al. 2005; Lobell & Burke 2008; Tavoni et al. 2011; Marvin et al. 2013).

2015's 21st Conference of the Parties (COP 21) to the United Nations Framework Convention on Climate Change (UNFCCC) meeting in Paris might serve as an ideal arena to implement the insights of this study. The conference will attempt to be the first climate meeting to place legally binding obligations for countries to reduce their emissions so as to avoid global temperature warming by more than 2 degrees Celsius. Fair and equitable emissions targets are to be devised in the 2015 conference and come into full force by 2020 (European Commission 2013). This institutional context almost perfectly mimics the experiment performed in this paper. 2015 will act as period 11 in the experiment, where an announcement of the future implementation of binding emissions targets in five years' time is made. The year 2020 will then, correspondingly, be like period 21 in the experiment, when the new institution is implemented. It is our belief that the legally binding nature of the agreements could overcome the impact uncertainty (of actual climate change) that we have shown to be problematic for the efficacy of announcements. This is because they might be formulated so that if countries do not reach their emission-abatement targets, severe sanctions can be applied from an international body in order to increase the certainty of negative impacts. The preceding experimental results suggest that if we use the agreed targets that will be made in 2015 as consistent and pervasive announcements of future change, we will reap the benefits of increased cooperation both before and after 2020.

While the discussion has been framed in terms of how announcements could benefit the climate change problem, we have so far just assumed that our results are externally valid in such a real-world context. Although it would be widen the scope of the analysis contained in this paper too much to include, it is worth considering what type of empirical data could help justify the inference of our results onto the international climate dilemma. There are a number of potential sources for such data. The number of countries who originally pledged emissions reductions at the Kyoto Protocol or the point at which they actually made signed obligations could be investigated. Additionally, country specific rates of renewable energy adoption or the introductions of national policies to foster CO<sub>2</sub> reductions might also serve as proxies for cooperation levels with regards to the climate change dilemma. These cooperation proxies could

then be graphed on a time scale in relation to clear climate change threshold announcements, such as the Kyoto Protocol or the COP 21 or various widely publicized scientific reports.

## **6. Conclusion**

This paper makes a unique contribution to the economics literature by being only the second study on the impact of announcements of future institutional changes, and the first using the transition to a threshold public goods game structure. This represents a shift towards the use of announcements as exogenously given, credible informational interventions. As such it provides necessary evidence on the influence that such announcements have on changing peoples' expectations and consequently their behaviour. This study is also the first, to the authors' knowledge, that utilizes a pre-experiment to elicit risk attitudes and subsequently construct crude loss aversion measures which are then used as predictors of subject behaviour in public goods games.

Our results show that announcements of future institutional change can increase the effectiveness of institutional change if the situation is framed in the correct way. If the context of the situation is framed in the loss domain, announcements cause positive adjustment and adaptation effects. Whereas if the situation is framed in the gains frame then announcements cause zero adjustment effects and negative adaptation effects; and if the loss framed context includes impact risk in the threshold institution the announcements have no adjustment or adaptation effects. We also find evidence that within groups that are successful in reaching the threshold in the loss and loss-and-risk framed contexts announcements lead to stronger, earlier cooperative action.

Future research could delve deeper into testing whether forewarning people with announcements truly does result in stronger, earlier action. Additionally, there seems to be scope for future research to test the announcement effect in ever widening contextual situations, such as different classifications of institutional change or testing slight nuances in the wording of the announcements. This indeed seems to be a research gap in the literature, wherein expectations changing rationally on the basis of relevant information is assumed. Yet exactly how

expectations change and under what circumstances and the differing consequences thereof has been largely neglected.

The findings of this paper have important implications for the climate change debate; providing optimism but also a caution. We find that announcements have the most positive impact on the effectiveness of institutional change in the loss frame, and this bodes well for the current scientific announcements of future climate thresholds as they have been, and are most naturally, explained in terms of loss minimization. However, we also see that introducing risk of loss, rather than certain loss, from not reaching the threshold negates the positive announcement effects. The lesson that climate policy-makers must heed from this is that impact certainty needs to be made explicit if the current announcements of dangerous climate tipping points are to have any effect. Otherwise, perhaps a global international body with a large amount of sanctioning power can be created in order to increase the certainty of negative impacts for those nations who do not reach their emissions abatement targets. This information is particularly valuable in light of the COP 21 in Paris in 2015 where nations will, for the first time, sign legally binding obligations to reduce their greenhouse gas emissions.

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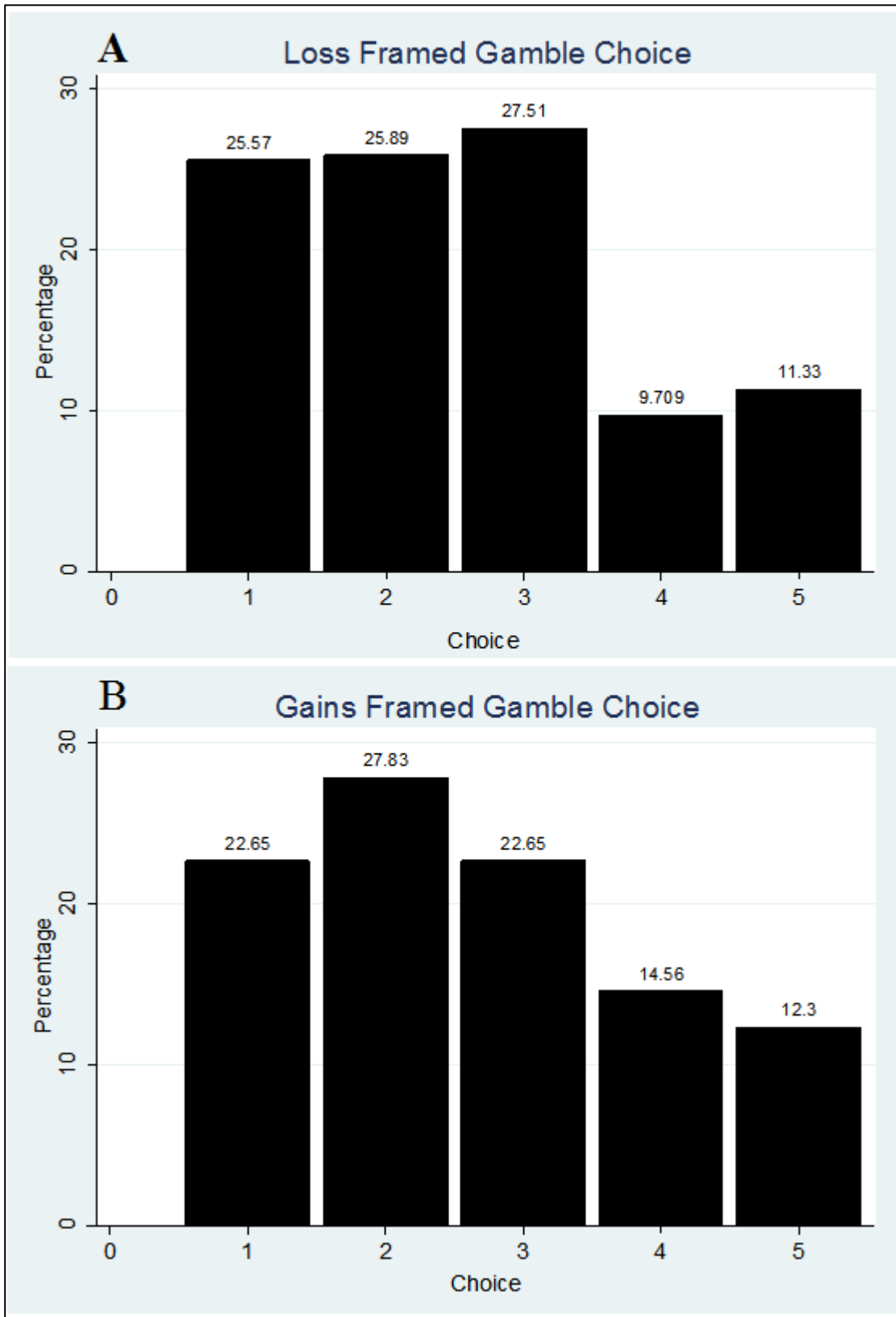
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## Appendix A – Demographics Survey

### Demographic Questionnaire

1. **Experiment Number:** \_\_\_\_\_
  
2. **What is your Gender?**
  - Female
  - Male
  
3. **How would you classify yourself?**
  - Black
  - Coloured
  - Indian
  - White
  - Prefer not to answerOther: \_\_\_\_\_
  
4. **What is your age?**
  - 18-24 years old
  - 25-30 years old
  - 31-35 years old
  - 36 years and older
  
5. **In which faculty are you registered?**
  - Commerce
  - Engineering and the Built Environment (EBE)
  - Health Sciences
  - Humanities
  - Law
  - Science
  
6. **Which income group do you belong to?**
  - Low income
  - Middle income
  - High income
  
7. **Do you smoke?**
  - Yes
  - No

**Appendix B – Gains and Loss Framed Gamble Choice Histogram**



### Appendix C - Tobit Regressions for Gamble Choices and LA index

	(1) Gamble Choice Loss Frame (1,5)	(2) Gamble Choice Gains Frame (1,5)	(3) Loss Aversion Index (-4,4)	(4) Gamble Choice Loss Frame (1,5)	(5) Gamble Choice Gains Frame (1,5)	(6) Loss Aversion Index (-4,4)
Male	0.620*** (0.227)	-0.0946 (0.237)	-0.501** (0.196)	0.596*** (0.227)	-0.0980 (0.226)	-0.489*** (0.182)
Black	-0.679* (0.352)	-1.078*** (0.323)	-0.253 (0.304)	-	-	-
White	-0.834* (0.504)	-0.564 (0.467)	0.145 (0.418)	-	-	-
Coloured	-0.671 (0.509)	-1.216** (0.501)	-0.353 (0.400)	-	-	-
Below 24 years old	0.168 (0.447)	-0.0493 (0.409)	-0.155 (0.334)	-	-	-
Commerce	0.223 (0.273)	0.151 (0.257)	-0.0733 (0.241)	-	-	-
Humanities	-0.110 (0.299)	0.150 (0.308)	0.239 (0.235)	-	-	-
High income group	1.375* (0.818)	0.528 (0.915)	-0.521 (0.623)	-	-	-
Low Income group	0.181 (0.224)	0.159 (0.237)	-0.00758 (0.189)	-	-	-
Smoker	0.137 (0.309)	-0.117 (0.326)	-0.207 (0.244)	-	-	-
Constant	2.316*** (0.581)	3.353*** (0.531)	0.698 (0.482)	2.070*** (0.161)	2.574*** (0.162)	0.344*** (0.130)
Log Likelihood	-514.059	-524.064	-576.210	-519.389	-530.444	-579.446
Observations	309	309	309	309	309	309
F Stat	1.50	1.43	1.35	6.89	0.19	7.18
Prob > F	0.1392	0.1672	0.2032	0.0091	0.665	0.0078
Pseudo R <sup>2</sup>	0.0169	0.0122	0.0117	0.0067	0.0002	0.0061

Robust Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix D - Gains Frame: Player Types Adjustment Effects**

Independent Variables	Full Cooperator (0/1)		Free Rider (0/1)	
	Periods 1-10	Periods 11-20	Periods 1-10	Periods 11-20
Lagged others' contributions	0.0172 (0.0148)	0.0420*** (0.0137)	-0.0418*** (0.00941)	-0.0664*** (0.0117)
Announcement (GCA)	0.451 (0.340)	-0.0922 (0.364)	-0.270 (0.235)	-0.0802 (0.276)
Loss Aversion Index (LA)	0.0789 (0.166)	0.0436 (0.155)	-0.153* (0.0828)	-0.0882 (0.0992)
Sum of Gamble choices	-0.0887 (0.0694)	0.0795 (0.0929)	0.0911 (0.0732)	0.0877 (0.0760)
Period	-0.110** (0.0518)	-0.0778 (0.0567)	0.106*** (0.0294)	0.145*** (0.0281)
Constant	-2.343*** (0.813)	-2.392*** (0.924)	-1.422*** (0.518)	-2.905*** (0.708)
Demographic Control Variables	Yes	Yes	Yes	Yes
Observations	837	930	837	930
Wald chi <sup>2</sup>	40.66	24.84	47.28	73.22
Prob > chi <sup>2</sup>	0.0002	0.0362	0.0000	0.0000
Pseudo R <sup>2</sup>	0.0780	0.0612	0.0671	0.1211

Robust standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## Appendix E - Loss Frame: Player Types Adjustment Effects

Independent Variables	Full Cooperator (0/1)		Free Rider (0/1)	
	Periods 1-10	Periods 11-20	Periods 1-10	Periods 11-20
Lagged others' contributions	0.0179 (0.0121)	0.0630*** (0.0142)	-0.0443*** (0.00882)	-0.0691*** (0.00961)
Announcement (LCA)	0.477 (0.359)	0.805** (0.379)	-0.251 (0.231)	-0.588** (0.237)
Loss Aversion Index (LA)	-0.0970 (0.109)	-0.0369 (0.122)	0.0463 (0.0631)	0.107 (0.0732)
Sum of Gamble Choices	0.000154 (0.0768)	0.223** (0.0964)	0.0836 (0.0624)	0.111* (0.0636)
Period	-0.0914* (0.0468)	-0.105* (0.0565)	0.0877*** (0.0273)	0.0759*** (0.0251)
Constant	-2.131*** (0.765)	-2.438* (1.259)	-0.584 (0.579)	-0.868 (0.699)
Demographic Control Variables	Yes	Yes	Yes	Yes
Observations	945	1,050	945	1,050
Wald chi <sup>2</sup>	23.08	53.75	69.28	83.35
Prob > chi <sup>2</sup>	0.0589	0.0000	0.0000	0.0000
Pseudo R <sup>2</sup>	0.0721	0.2174	0.0833	0.1466

Robust Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix F - Loss & Risk Frame: Player Types Adjustment Effects**

Independent Variables	Full Cooperator (0/1)		Free Rider (0/1)	
	Periods 1-10	Periods 11-20	Periods 1-10	Periods 11-20
Lagged others' contributions	0.0138 (0.0146)	0.0171 (0.0142)	-0.0517*** (0.00933)	-0.0454*** (0.0102)
Announcement (LRA)	-0.252 (0.326)	-0.652 (0.424)	-0.0565 (0.222)	0.0469 (0.266)
Loss Aversion Index (LA)	0.0268 (0.104)	-0.0970 (0.134)	0.0202 (0.0627)	0.0881 (0.0844)
Sum of Gamble choices	0.0958 (0.0813)	0.190 (0.140)	0.0832 (0.0521)	0.0471 (0.0641)
Period	-0.0505 (0.0489)	0.00490 (0.0340)	0.104*** (0.0266)	0.0939*** (0.0208)
Constant	-3.062*** (0.833)	-4.678*** (1.251)	-1.778*** (0.517)	-2.169*** (0.673)
Demographic Control Variables	Yes	Yes	Yes	Yes
Observations	999	1,110	999	1,110
Wald chi <sup>2</sup>	18.85	11.28	98.43	58.10
Prob > chi <sup>2</sup>	0.1277	0.5873	0.0000	0.0000
Pseudo R <sup>2</sup>	0.0385	0.0581	0.0985	0.0692

Robust Standard errors in parentheses

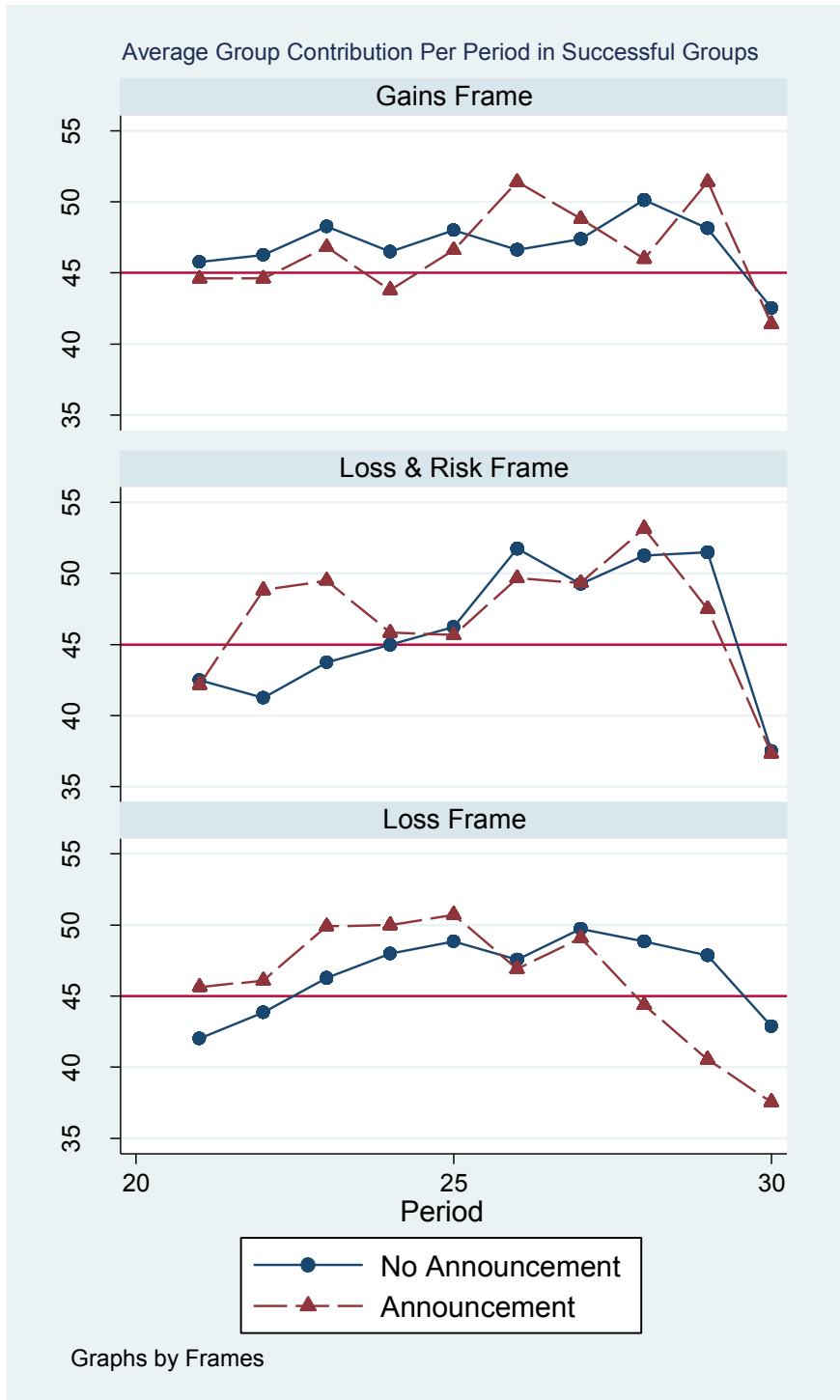
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## Appendix G - Likelihood of Reaching the Threshold in Period 30

Independent Variables	Dependent Variable: : Reached Threshold in Period 30 (0/1)		
	Gains Frame	Loss Frame	Loss and Risk Frame
Announcement	-1.180** (0.462)	0.888** (0.443)	0.658 (0.491)
Loss Aversion (LA) Index	-0.167 (0.148)	-0.152 (0.125)	0.187 (0.182)
Sum of Individual's Gamble Choices	0.207* (0.116)	-0.129 (0.116)	0.0408 (0.117)
Constant	-1.350 (0.935)	-0.555 (0.991)	-2.898** (1.405)
Demographic Control Variables	Yes	Yes	Yes
Log pseudo-likelihood	--57.088	-66.107	-58.437
Observations	93	105	111
Wald chi <sup>2</sup>	13.05	11.15	11.37
Prob > chi <sup>2</sup>	0.221	0.346	0.329
Pseudo R <sup>2</sup>	0.0974	0.0912	0.0978

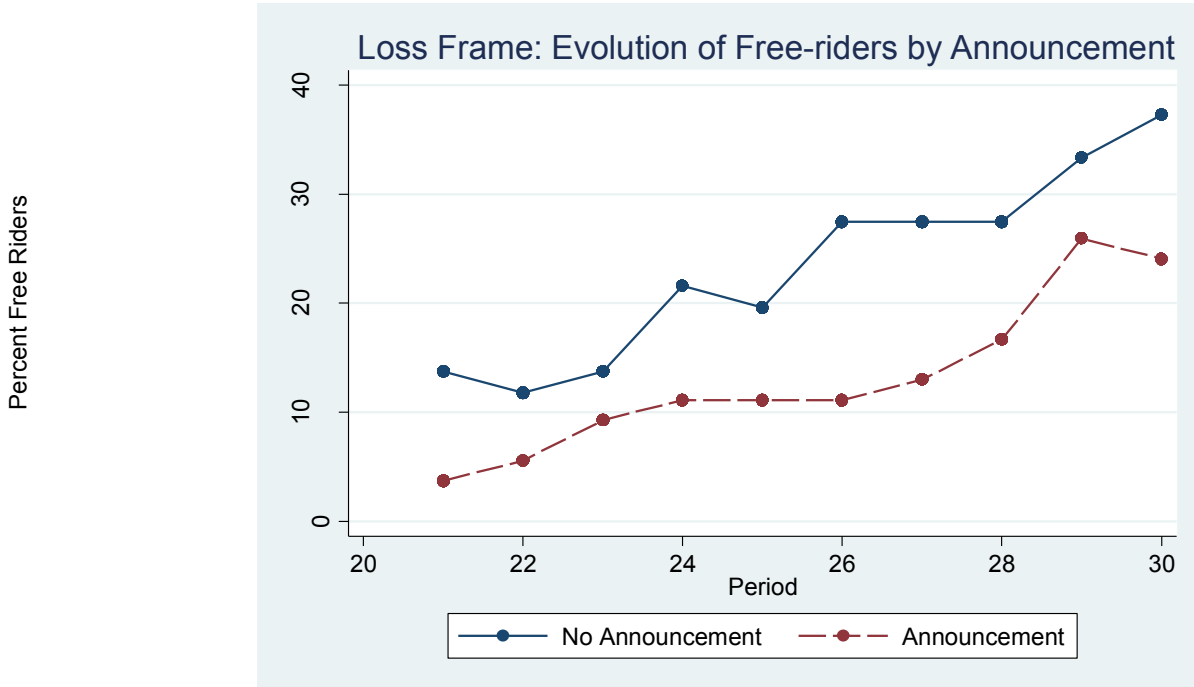
Robust Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Appendix H – Average Group Contribution per period for Groups Successful in Reaching the Threshold, by Frame.**

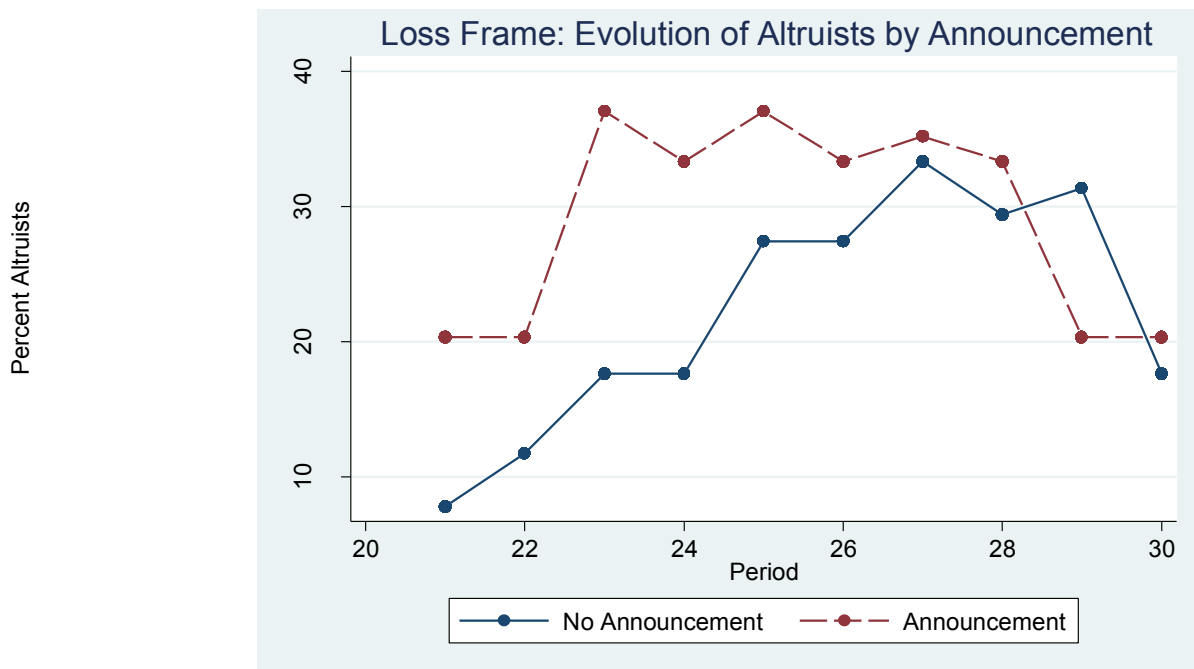


**Appendix I – Loss Frame: Evolution of Player Types in periods 21-30**

**A) Free-riders**

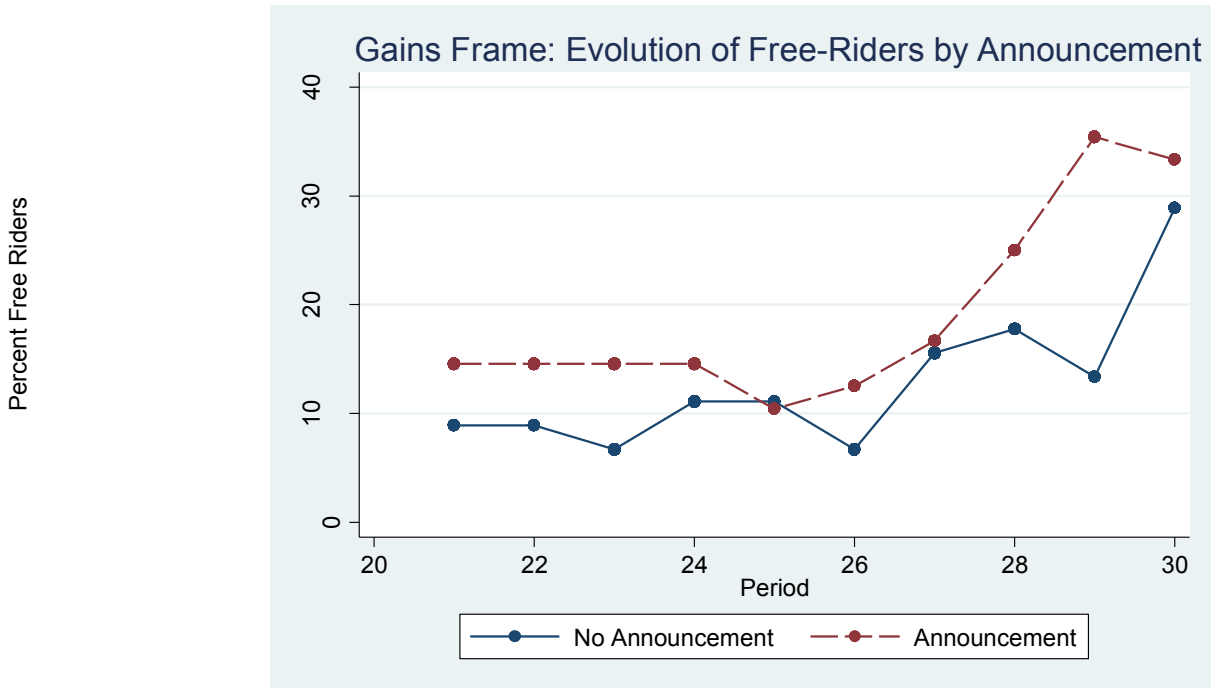


**B) Altruists**



**Appendix J – Gains Frame: Evolution of Player Types in periods 21-30**

**A) Free-riders**



**B) Fair-sharers**

