The Impact of Announcing Future Punishment Opportunities in a Public Goods Game

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Abstract

We explore the effects of announcements of future punishment opportunities in public goods games. Announcements can influence subject behaviour, through changing expectations, before the institution is implemented (adjustment effect) or after implementation (adaptation effect). Our results indicate that announcements do not lead to significant adjustment effects, nor increased free-riding before implementation. Once punishment opportunities are implemented, those forewarned with announcements exhibit positive adaptation effects. The number of contributors to the public good increases relative to the no-announcement treatment; this is partly mediated by increased utilisation of punishment, but diminished anti-social use of the institution. Announcements can therefore increase the efficacy of institutional change.

JEL C9, H41, H30, Q58

1 Introduction

The question of whether institutional change should be announced prior to its implementation has received little attention in the field of economics. In the organisational psychology literature, Richardson and Denton (1996) argue that the key to successful organisational change is consistent and precise communication with the workforce about any change before it is instituted. However, the value of announcements is dependent on how they impact behaviour.

The present study aims to draw inferences for policy implementation from the results of an experimental study. This paper uses public goods games to

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examine the difference in effects between a straightforward transition from a no-punishment treatment to the implementation of a punishment institution, and that of making an announcement of the future punishment phase prior to its actual implementation. To the knowledge of the authors, no other experimental study has been conducted on the effects of announcements of future institutional reform on subject behaviour using economic games. This study therefore adds to the experimental economics literature a unique insight into the behavioural consequences of manipulating subjects' expectations of future institutional conditions.

The rational expectations hypothesis states that in aggregate, the public's expectations are equal to objective mathematical expectations and thus all relevant information is utilised in the formation of expectations (Muth, 1961). Furthermore, policy changes change the way in which the public's expectations are formed, with a central element being whether a given policy change is anticipated or not. If a policy change is anticipated, the policy ineffectiveness proposition implies that people will revise their expectations and behaviour accordingly, causing the policy change to have no effect (Sargent and Wallace, 1976; Holland, 1985). Rational expectations form the backbone of such theories like Ricardian equivalence, which argues that temporary tax cuts will not boost aggregate demand as consumers realise that today's tax cuts will have to be financed by tax increases sometime in the future (Barro 1989). Thus a central implication of rational expectations is that anticipated changes in policy can result in immediate behavioural responses before the policy change is implemented, causing a curved rather than a discontinuous adjustment path (Heijdra and van der Ploeg 2002).

Announcements of future policy change are the mechanism through which change becomes anticipated in the expectations of the public. Therefore the main purpose of this paper is to investigate the impact announcements of future policy intentions have on the present behaviour of agents (*adjustment effect)* as well as their future behaviour once the new institution has been introduced (*adaptation effect).* The term *adjustment effect* has been modified from authors who use the term adjustment path to represent the effect of announcements on behaviour before the implementation of institutional change (Adams et al., 2001; Heijdra and van der Ploeg, 2002). The term *adaptation effect* is modified from Blundell et al.'s (2001) use of the term implementation effect to model the effect announcements have on behaviour after the implementation of institutional change.

The results of the experiment presented here suggest that announcements of future policy change do influence adaptive behaviour. Announcements lead to increased numbers of contributors to the public good once punishment opportunities are allowed, and this is partly mediated by greater utilisation of the new punishment institution in terms of both participation and effort. A further adaptation effect of announcements is a decreased incidence of incorrect usage of the punishment institution, in the form of anti-social punishing. These positive adaptation effects imply that announcements increase the efficacy of policy change. However, out of line with the rational expectations hypothesis, the announcements are shown to *not* influence adjustment behaviour prior to the actual implementation of the policy change

Depending on the informational content of the announcement, the sign of the announcement effect has been shown to vary in the literature. Blundell et al. (2011) model the effect of announcements of in-work benefit reforms on female labour force decisions. It is found that the announcement of a future tax credit reform leads to an increase in current labour force participation: a positive adjustment effect. Adams et al. (2001) examine the effect of timing announcements on the implementation of policy reform to reduce the negative environmental impact caused by pig farming in the EU and Denmark. The authors find that announcements result in a smoother adjustment path, but lead to no long run adaptation advantages. In Becker et al. (1989), it was discovered that by introducing announcements about the impending ban on smoking in the hospital, the level of smoking reduced even before the ban came to be. Additionally, there was a relatively high level of compliance when the ban was actually introduced. This implies that the announcements had an adjustment effect of increased compliance before the ban was introduced *and* an adaptation effect of increased compliance when the ban came into being.

The announcement effect has been studied widely in the area of financial markets. Ferrero and Secchi (2007) emphasize that by providing timely credible information on the likely evolution of the interest rate, a central bank may be able to improve the efficacy and efficiency of monetary policy. The announcements of future policy intentions affect market expectations about the future development of monetary policy and allow the market to price financial assets more efficiently. It has been shown that announcements of changes in money supply and even of the central bank's general monetary policy stance have an immediate impact on financial markets (Urich and Wachtel, 1981; Cornell, 1983; Rosa and Verga, 2007). Unexpected earnings results are more likely to result in an announcement timing change and bad news is announced earlier so as to minimise the impact on asset prices (Chen and Moha, 1994). Brown and Han (1992) provide support that the mechanism through which earnings announcements affect behaviour is through impacting analysts' expectations.

The experimental economics literature does not contain many studies on announcements, and those studies which do look at the subject do not study announcements of future institutional change. This literature mainly looks at individuals' announcements of intended future behaviour, as a form of a non-binding signalling device meant to induce cooperation in public goods games (Wilson and Sell, 1997; Croson and Marks, 2001; Berlemann, 2003; Berlemann et al., 2004). The relevant implication from these studies is the finding that individual signalling can influence contribution behaviour of other group members by changing subjects' expectations or beliefs.

We follow the experimental economics tradition by using a standard linear public goods game with the future introduction of a peer sanctioning regime as the institutional change. We then isolate the effects of making announcements about the future implementation of punishment by using several treatments.

The public goods game is the main tool economists use for studying cooperation. A large number of studies report contributions significantly larger than the theoretically-predicted zero in both repeated and one-shot public goods games and in dictator games with no possible strategic motivations (Andreoni, 1990; Ledyard, 1995 Batson and Powell, 2003; Zelmer, 2003 Murphy and Ackerman, 2013). Voluntary contributions to a public good have also been shown to be possible without external monitoring (Kollock, 1998; Bowles and Gintis, 2002; Andreoni et al., 2003; Kocher et al., 2009; Reuben and Riedl, 2013). The frequency of such findings has resulted in researchers re-examining the theory and looking more closely at cultural effects as well as the impact of altruistic motivations (Andreoni., 1995; Kocher et al., 2009).

The literature shows that contributions to the public good can be increased significantly when the opportunity to punish peers is permitted. With a credible threat of punishment near-complete levels of cooperation can be achieved and cooperation has been shown to increase as punishment effectiveness increases (Bowles and Gintis 2004; Nikiforakis and Normann, 2008). Free-riders are punished heavily even if punishment comes at a cost (Fehr and Gächter, 2000; Fehr et al., 2002; Visser and Burns, 2013). There is also evidence that punishment occurs as a result of deviations from accepted social norms or standards (Fehr and Fischbacher, 2004; Kocher et al., 2009; Akpalu and Johansson-Stenman, 2010; Nikiforakis, 2010; Chaudhuri, 2011). These findings are contrary to the notion that individuals act always in narrow self-interest and therefore do not contribute or punish because these actions are costly with uncertain and potentially small relative gains.

2 Experimental Design

2.1 *Basic Design*

The design of the game is similar to that used by Fehr and Gächter (2000) and Gächter and Herrmann. (2009). The experiment made use of a repeated linear public goods game with three separate treatments of 30 periods each. The first of these treatments was a standard game with no punishment throughout and will henceforth be referred to as experiment "N". Treatments two and three both included periods of no punishment as well as periods with a punishment condition.

Treatment two consists of twenty periods with no punishment and then a punishment condition is introduced for the last 10 periods of the experiment. The introduction of the punishment condition was made only after participants had finished with the first 20 periods of the no punishment condition[1]. Henceforth, treatment 2 will be referred to as experiment "P".

Treatment three includes the implementation of an announcement condition and is the primary focus for the paper. The design is the same as treatment P,

[1] Despite referring to the process as punishment in this paper, participants knew it only as "deducting points" from other group members' income. Throughout the experiment only neutral language was used so as not to influence the participant's strategies.

except that from Period 11 onwards, announcements are made in each round informing subjects that from Period 21 onwards the game structure will change to allow punishment opportunities. An explanation was given at the beginning of the announcement stage (period 11) as well as before the actual implementation of the punishment condition (end of period 20) as to how the punishment would work. Treatment 3 will be referred to as experiment "A".

The punishment condition used in this paper is similar to that used in Gächter and Herrmann. (2009). Group members simultaneously choose if and by how much to punish the other members of the group without ever knowing the punishment choices of others (Ostrom et al., 1992; Fehr and Gaechter, 2000; 2002, Sefton et al., 2007; Bochet et a., 2006; Masclet et al., 2003). Using this punishment technology avoids the development of punishment feuds, possible under sequential punishment decisions (Cinyabuguma et al., 2004; Nikiforakis and Engelmann, 2011).

In each of the treatments, participants were randomly divided into groups of 4 and remained in the same group throughout the experiment. This is similar to the "Partner" design, seen in Fehr and Gächter (2000) and Gächter and Herrmann. (2009), in which group members are involved in the same public good (group project) and develop reputations according to their contributions. The "partner" design best suits this paper as it replicates an environment in which agents tend to be well established and their behaviour and reputation are well known.

2.2 *Payoff structure*

In all three treatments, each of the n = 4 participants receives an endowment of 20 tokens (experimental currency units) at the beginning of each period. They then have to decide how much of the endowment to contribute towards a group project and how much to retain for themselves. This process occurs in every period regardless of the treatment and is called the "First Stage". In the punishment phase of the P and A treatments, participants have the opportunity to assign deduction points after seeing the contributions of the other group members from the "First Stage". The punishment opportunity is conducted in a "Second Stage".

First Stage.—As explained above, each of the n = 4 participants receives an endowment y of 20 tokens, from which they contribute g_i tokens ($0 \leq g_i \leq 20$) to a group project. This decision on how much to contribute was made simultaneously. At the end of the first stage, each participant i's monetary payoff (π_{Ii}) is given by

$$\pi_{Ii} = (y - g_i) + 0.4 \sum_{j=1}^{4} g_j \tag{1}$$

for each period, where 0.4 is the marginal per capita return (MPCR) from public good contributions and $\sum_{j=1}^{4} g_j$ is the total contribution to the project by the group.

Second Stage—At the second stage participants are shown their group members' contributions and are allowed to punish them if they choose. A group member j may punish group member i by assigning deduction points p_{ji} to i. However, deduction points are costly to assign and will cost 1 token per deduction point assigned. For each deduction point received by an individual, their income was reduced by 3 tokens. This factor of 3 was chosen in accord with Nikiforakis and Normann's (2008) finding that this is the minimum factor of punishment effectiveness that raises contributions over time. If a participant receives more than one deduction then their income was reduced by 3 times the number of points received. All punishment decisions were made simultaneously and participants were not informed about who punished them.

The profit to each individual, from the period, is the income earned from the first stage (π_{Ii}) less any deduction points received, less the cost of assigning deduction points to others. The payoff function is given by

$$\pi_{IIi} = \pi_{Ii} - 3\sum\nolimits_{j=1,j\neq i}^{3} p_{ji} - \sum\nolimits_{j=1,j\neq i}^{3} c_{ij}p_{ji} \tag{2}$$

Where p_{ji} represents the points i received from each other group members and $c_{ij}$is the cost to$i$ from assigning p_{ij} points to j(c_{ij}=1). Subjects knew their own punishment activity and the aggregate punishment imposed on them by other group members but the identity of the punishers was never revealed. Each participant was limited to assigning a maximum of 10 deduction points to each group member. This limitation was implemented so as to diminish the potential losses that a subject could incur due to overzealous punishing.

2.3 *Parameters, Conditions and Recruitment*

The experiments were conducted in a computerised laboratory[2] at the University of Cape Town (UCT) over three sessions. A total of 116 participants, split into groups of 4, took part anonymously in the experiments. Treatments N and A had 10 groups and treatment P had 9 groups and the composition of the groups remained the same throughout the experiment. Treatment P has only 9 groups due to low participant response on the day of the experiment, and should not impact results. Although it was impossible to ensure that subjects did not know each other, the identities of the group members were never revealed. No communication was permitted between subjects as soon as they entered the laboratories.

At the beginning of each session, participants were given a written set of instructions[3] about the first stage and told that they would play 30 periods of choice games. There was a set of control questions to test the participants understanding of how their payoffs were calculated. If any problems persisted it would be explained on a board in front of everyone to ensure full understanding. In the P and A treatments, a new set of instructions was handed out during the

[2] For the purposes of this experiment we used the experimental software "z-Tree" developed by Urs Fischbacher (1998).

[3] See appendix 9 for stage 1 instructions given to each participant

experiment describing how the stage 2 worked. A new set of control questions were answered. Each session lasted between 1 hour and 40 minutes and 2 hours and 15 minutes including the time needed for the instructions.

Only UCT undergraduate students were recruited and none of them had previously participated in a public good experiment. The recruitment process involved advertising the experiments on faculty and university residence websites as well as face to face recruitment on the various campuses. To avoid an experimenter effect, the same supervisor ran all the experiments. The exchange rate used was 1 token = R 0.10 and an additional show up bonus of R20 was also given, which was roughly equal to the value of a lunch voucher from the university's residence system.

3 Findings and Discussion

Result 1: The existence of punishment opportunities causes a large rise in the average contribution level. In the absence of punishment opportunities, average contributions converge over time close to full free-riding.

Figure 1 shows the average contributions of the three treatments over the 30 periods. All three treatments demonstrate a downward trend in contributions in the first 20 periods, while only the N treatment continues this movement in the last 10 periods. In experiments P and A, a punishment condition was introduced in period 21, and as a result average contributions over time increased substantially from the no punishment condition in the earlier periods.

Experiment N's downward trend over time is consistent with rational expectations and the prediction that, by applying the backward induction argument, participants have a dominant strategy to free ride. This is implied in **Figure 1** with the graph falling over time. A learning effect may be an explanation of the steady decline, as participants start to gradually realise their dominant and profit-maximising strategy is to free ride. **Table 1** contains some summary findings from the experiments, including average contributions per treatment per part and relative changes between parts, as well as average punishment for treatment P and A during part 3. This table also confirms result one, as it can be seen that treatments A and P both have more than double the average contribution per person in part 3 than treatment N.

A nonparametric Kruskal-Wallis equality-of-populations rank sum test shows that the difference in contributions between treatment N and treatments A and P, for periods 21 to 30, are significant at all conventional significance levels ($p < 0.0001$ and $p < 0.0001$ respectively)[4]. A nonparametric Wilcoxon rank sum test shows that the higher contributions in the punishment phase is significant at all conventional significance levels when comparing the punishment phase (periods 21 to 30) to the no-punishment phase (periods 11 to 20) within treatments A ($p < 0.0000$) and P ($p < 0.0000$)[5].

[4] See Appendix A for Kruskal-Wallis test results

[5] See Appendix C Wilcoxon rank sum test results

Result 2: There is no evidence that the introduction of announcements causes any adjustment effect before punishment is implemented.

An initial glance at the basic findings in **Table 1** might give one the impression that this result does not hold, as the relative decrease in average contributions from part 1 (periods 1-10) to part 2 (periods 11-20) is the least in treatment A compared to treatment P and N. So initially it seems as though announcements do lead to an adjustment effect of higher contributions. However, a more detailed analysis finds that this difference is not significant.

A Kruskal-Wallis test between treatment A and N isolating periods 11 to 20, indicates that there is no significant difference between the treatments (p= 0.6743)[6]. The same test between treatment A and P also reveals no significant difference between the treatments during these periods (p= 0.2542)[7]. This seems to suggest that the announcements are not causing any adjustment effect that would result in differential contribution behaviour in treatment A.

A series of two-stage hurdle model regressions further explore this finding in **Table 2**. Here two separate regressions are run for each part: periods 1 to 10; periods 11 to 20; and periods 21-30 separately. The first column in each part presents the coefficient estimates of a logit regression using the dependent variable *Contribution Decision,* which is a binary variable taking on the value 1 if an individual contributes anything more than zero. The second column presents the coefficients from a tobit regression where the dependent variable is the numeric variable of *Contribution Amount,* conditional on the individual making the decision to contribute. In all the regressions robust standard errors clustered at the individual level are used.

In both of the first two phases, both treatments N and P are counterfactuals to treatment A, as during these periods they are both merely normal public goods games. The regression analysis shows that periods 1 to 10 or periods 11 to 20, both in terms of the amount of contributors and the amount given by contributors. The regression reveals that the major determinant of the decision to contribute and the amount to contribute in both of these phases is one's contribution in the previous period. In periods 1-10 (11-20) 1 token of one's contribution in the previous period on average increases one's likelihood to contribute in this period by 1.66 percent (2.15 percent) , which is significant at the 1 percent level; and on average increases the amount one contributes by 0.640 (0.536) tokens, conditional on one contributing anything. This supports the earlier Kruskal-Wallis test that showed no significant difference between A and N and P for these periods. The non-parametric Kruskal-Wallis tests may be more accurate than the regression in this case due to the small sample size.

This result has important policy implications. Unlike the findings in previous literature (Urich and Wachtel, 1981; Becker et al., 1989; Adams et al., 2001; Heijdra and van der Ploeg 2002), this result would imply that making announcements of future policy will not help to smooth the adjustment path as individuals, households or firms do not incorporate the future policy changes

[6] See Appendix A for Kruskal-Wallis test results

[7] Ibid

into their present day strategies.

Result 3: Incorporating announcements of future punishment opportunities into a treatment does not seem to induce a greater tendency to free-ride just before the institutional change.

A Kruskal-Wallis test reveals that there is no significant difference (p = 0.8501) in the relative decrease in contributions between treatments A and P comparing period 19 to 20, and the same for treatments A and N (p= 0.2186)[8]. This provides further evidence that people do not change their present behaviour when faced with the prospect of future punishment opportunities. However, although the differences between A and N and P are not significant here, each test shows that treatment A has a slightly higher rank-sum, which could indicate that if there is any difference it would be that treatment A results in *less* free-riding just before the policy change.

This could have policy implications in situations where authorities want to limit a particular behaviour before the implementation of a policy. Free-riding in treatment A is not significantly more before the policy change compared to treatments P and N; if anything there may be slightly more free-riding in the uninformed treatments. Adams et al. (2001) mentions that environmentalists might argue for a surprise implementation of policy change so as not to incentivise agents to further pollute the environment prior to a policy change. However, this result implies that agents informed of future policy change do not actually take advantage of their knowledge by free-riding just before the policy implementation, and may in fact cooperate more just before the change.

Result 4: Incorporating announcements into a treatment may result in a more efficient adaptation to a punishment condition from a no-punishment condition.

As can be seen from **Figure 1**, average contributions rise to a higher level in treatment A compared to treatment P for the punishment phase. This result is corroborated by a Kruskal-Wallis test that shows contributions in treatment A are significantly higher than contributions in treatment P for the periods 21-30 (p= 0.0051)[9]. This implies that announcements lead to greater adaptation to the newly implemented institution, as higher contributions represent a more socially desirable outcome.

Columns five and six of **Table 2** unpack this adaptation effect. It is found that what is driving the higher average contribution in treatment A is a significant increase in subjects' propensity to contribute (p= 0.073) rather than an increase in the amount a contributor gives. Column five shows that being in treatment P results in a 10.99 percent lower likelihood of contributing anything for the average subject relative to being in treatment P, ceteris paribus. However, column six suggests that conditional on a subject deciding to contribute, there is no significant difference in the amount of contribution given between those in treatment A and those in P, holding all else fixed. This finding is corroborated by a Kruskal-Wallis test for periods 21-30 that shows the number of

[8] See Appendix B for Kruskal-Wallis test results

[9] See Appendix A for Kruskal-Wallis test results

contributors are significantly higher in treatment A than in treatment P (p= 0.0001)[10], while the amount given by those contributing is not significantly different between treatments A and P (p= 0.9882)[11]. This result has important policy implications for predicting behavioural change when a policy change is implemented. Investigating the degree of agents' adaptation when policy change is implemented is important, as it determines whether the policy change will be worthwhile. This result implies that greater adaptation to a new policy regime in terms of greater numbers of cooperators can be induced by investing in announcements and advertising of impending future policy changes.

Result 5: The adaptation effect of increased numbers of contributors in treatment A in periods 21-30 is partly driven by greater utilisation of the punishment institution by subjects in treatment A.

In trying to uncover what is driving treatment A to have systematically more contributors than treatment P in part 3, the most natural mechanism to investigate is if the use of punishment varies between these two treatments. **Table 3** presents the same hurdle model for contribution in periods 21-30 as presented in **Table 2**, but includes a variable of individuals' received punishment points in the previous period as a predictor of current contribution. The intuition behind including this variable is the assumption that the more one is punished in the previous period for not contributing, the more likely one will contribute in the next round in order to avoid punishment. The regression seems to suggest that the amount of punishment received in the previous period does not influence the current decision to contribute, but it does significantly increase the amount contributed if the decision to contribute has been made. On average, one extra point of punishment received in the previous period results in a 0.355 increase in contribution in the current period.

The evidence that the amount of punishment one receives in the previous period does not influence one's current decision to contribute, yet being in treatment A still results in a significantly higher likelihood of contributing might indicate that the announcement is influencing subjects' expectations. Unfortunately, the design of this experiment did not include any direct elicitation of the expectations of individuals. However, it might be that the announcement increases the expectation that punishment is a credible threat and non-cooperation will actually result in punishment.

Such, punishment resulting in increased contributions has been evidenced before in Fehr and Gächter (2000); Kocher et al. (2009); Nikiforakis (2010) and Visser and Burns (2013). Furthermore, this new variable slightly decreases the statistical significance of the treatment P dummy variable (compared to column five of **Table 2**). The larger standard errors for treatment P dummy in Table 3 might be due to multicollinearity if the treatment P dummy variable is systematically correlated to the amount of punishment one receives although any direct comparisons between the models is not possible as the inclusion of the punishment variable excludes Treatment N as a predictor and reduces sample

[10] Ibid

[11] Ibid

size.

In order to examine whether the two treatments use punishment in different ways and thus to see whether the treatment P dummy is correlated to the punishment variable, **Table 4** provides the results of a two-stage hurdle model for the determinants of receiving punishment. This model follows the same design as the hurdle model specified in Nikiforakis and Engelmann (2011). In the first column, the dependent variable is *Punishment Decision*, which is a binary variable that takes the value 1 if an individual has been punished and 0 if not. The second column uses the dependent variable *Punishment Severity* which is a numeric variable of how many punishment points one has received conditional on punishment being given.

Similarly to Nikiforakis and Engelmann's (2011) and Fehr et al.'s (2002) results, we find that contributions which are less than the group's average are punished more frequently and more severely as the magnitude of the deviation increases. However, the results of **Table 4** also highlight some implications unique to the current study. The treatment P dummy variable affects the likelihood of the decision to punish *and* the actual punishment severity. Holding all else constant, the marginal effect of being in treatment P results in a 30.73 percent lower likelihood of punishment relative to being in treatment A (p=0.000). Additionally, conditional on the decision to punish being made, and all else equal, the average marginal effect of being in treatment P lowers the severity of punishment given by 0.640 punishment points(p=0.001) compared to being in treatment A. This suggests that if the announcement's impact of increasing the number of contributors is working through influencing the expectation of a higher utilization of punishment, then the expectation is proved correct, as subjects in treatment P punish more frequently and punish more severelywhen they do punish. Furthermore, the strong correlation between the treatment P dummy variable and the punishment variables lend support to the claim that some of the significance of the treatment P variable in Tables 2 and 3 is working through the mechanism of differential usage of punishment.

Table 4 also show that contributions in treatment A that are positive deviations from the group's average are significantly less likely to be punished, whereas the interaction term of treatment P and positive deviations indicate the exact opposite for treatment P. Being in treatment P and contributing 1 token above the group mean increases one's probability of being punished by 6.36 percent (p=0.004) relative to being in treatment A and contributing 1 token above the group mean. This phenomenon of punishing people after they behave pro-socially, known as anti-social punishment, has been consistently evidenced and found to sometimes be just as prominent as free-rider punishment (Cinyabuguma et al., 2004; Gächter and Herrmann, 2009; Sutter et al., 2010; Visser and Burns, 2013). The announcements therefore seem to be reducing the likelihood of anti-social punishment in treatment A relative to treatment P when the punishment is introduced. If one looks at anti-social punishment as the incorrect use of the new institution, the announcements cause people to take the new institution more seriously; which means greater efficacy of the institution once implemented. Speculatively, the continual announcements of future punishment opportunities might be causing learning effects before the institution is even introduced, so that as soon as punishment is allowed people know how it is meant to be used. The finding of increased utilisation of punishment due to the announcements in treatment A is supported by **Figure 2**. From panels A and B, it is clear that both the proportion of people punished and the average amount of punishment meted out per person is greater in treatment A than in treatment P.

Treatment A starts the punishment period with over 82.5 percent of subjects being punished and 146 points of punishment being utilised, compared to treatment P's 52.78 percent of subjects being punished and 56 points of punishment being utilised. A Kruskal-Wallis test confirms the significance of the differences between the two treatments with respect to the amount of punishment received (p=0.0004) and the number of punished subjects (p=0.002) per round for the punishment phase[12].

The higher utilisation of punishment in treatment A seems to taper off towards the end of the punishment phase, as both panels in **Figure 2** show. If one can attribute this to the announcement effect decaying with time, then it would have worrisome implications for the long term effectiveness of announcements. However, we suggest that this is not the case. Another explanation for this tapering off could be that early, heavy use of punishment achieves its desired effect and thus its actual use is no longer necessary, as a credible threat has been established. The evolution of the proportion of free-riders in each treatment, illustrated in **Figure 3**, supports the above explanation. Free-riders here are defined as subjects who contribute zero tokens to the group project. In this graph it can be seen that the proportion of free-riders in each treatment starts off relatively similar and then as the punishment phase continues, the proportion of free-riders in treatment A diminishes rapidly and then stabilises relative to treatment P. A Kruskal-Wallis test confirms that there are significantly less free-riders in treatment A during this period (p=0.0001)[13]. This trend suggests that the abundance of early punishment in treatment A succeeds in incentivising less free-riding, and thus is testament to the greater efficacy of the punishment institution when coupled with prior announcements.

The adaptation effect that results from announcements therefore seems to, at least partly, originate from increased utilisation of the punishment institution. Moreover, from the two-stage hurdle model analysis and the evolution of punishment and free-riding it is shown that the increased utilisation is derived from two sources: more people participate in the new institution and those participating exert more effort and are more effective. The policy implication is that if one wants better participation and interaction when institutional change is implemented, then one must keep the population well informed of the proposed changes.

[12] See Appendix C for Kruskal-Wallis test results

[13] See Appendix C for Kruskal-Wallis test results

4 Concluding Remarks

Our study makes a novel contribution to understanding the impact of announcements on subsequent behaviour. No previous paper has directly looked at the effects of announcing future institutional changes on individual behaviour in an experimental setting. Furthermore, our paper is the first in the experimental economics literature to not use announcements for individual behaviour signalling (as seen in Wilson and Sell (1997) and Berlemann (2003) and others), but rather as an exogenous signalling device of institutional change. The novelty of this paper's design also means it is the first study on the announcement effect to be able to accurately distinguish between two avenues of potential impact – the adjustment effect and the adaptation effect.

Announcements are shown to not affect the current behaviour of individuals; there is no adjustment effect. This implies that if policy-makers wish to use announcements to change behaviour immediately, they might be faced with some difficulty. On the other hand, individuals are shown to not increase free-riding just before the policy change when forewarned about the change; which would be a negative adjustment effect. This casts doubt on the arguments for surprise policy changes exhorted by environmentalists in Adams et al. (2001).

The results also suggest positive adaptation effects resulting from announcements of future policy intentions. Evidence was found that making announcements about punishment opportunities increases the levels of contributors once punishment is allowed. This is partly mediated by increased and more effective use of punishment and diminished anti-social punishment by individuals. Announcements therefore cause better adaptation on multiple behavioural levels: more subjects contribute; subjects punish more; and use punishment more effectively and less anti-socially.

Our main finding that announcements cause zero adjustment, but significant adaptation poses an important lesson for the phasing in of future institutions. In contrast to the theory of rational expectations' predictions of anticipated change resulting in overall ineffectiveness, our study reveals that anticipated institutional change actually *increases* the effectiveness of the new institution. Keeping the population informed of future changes through announcements can thus be seen as a pragmatic and valuable policy tool.

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Table 1: Basic Findings by part

	Treatment N	Treatment P	Treatment A
Number of Subjects	40	36	40
Part 1 (Periods 1-10)			
Average Contributions	8.47	7.71	6.68
Part 2 (Period 11-20)			
Average Contributions	5.51	6.39	5.63
Relative Change between Part 1 & 2 (percent)	-34.95	-17.12	-15.72
Part 3 (Periods 21-30)			
Average Contributions	4.58	10.61	12.09
Relative Change between Part 2 & 3 (percent)	-16.88	66.04	114.74
Average Punishment points received	-	1.17	2.1

Table 2: Hurdle Model Regressions of Individual Contributions

	Periods 1-10		Periods 11-20		Periods 21-30	
Independent Variables	Contribution Decision (0/1)	Contribution Amount (1;20)	Contribution Decision (0/1)	Contribution Amount (1;20)	Contribution Decision (0/1)	Contribution Amount (1;20)
Treatment N	-0.0429	0.880	0.00499	0.224	-1.152***	-1.852**
	(0.289)	(0.697)	(0.246)	(0.659)	(0.366)	(0.770)
Treatment P	-0.133	0.717	-0.0230	0.930	-0.731*	0.218
	(0.308)	(0.722)	(0.275)	(0.779)	(0.407)	(0.826)
Lagged Contribution	0.0937***	0.640***	0.0977***	0.536***	0.184***	0.847***
	(0.0204)	(0.0578)	(0.0183)	(0.0499)	(0.0252)	(0.0466)
Period	-0.0965***	-0.0924	-0.0890***	-0.160**	-0.119***	0.102
	(0.0238)	(0.0629)	(0.0191)	(0.0740)	(0.0228)	(0.0743)
Constant	1.109***	4.954***	1.530***	7.424***	3.706***	2.632
	(0.294)	(0.755)	(0.378)	(1.285)	(0.651)	(1.648)
Log Pseudo Likelihood	-548.395	-2194.245	-696.165	-2221.262	-491.205	-2069.941
Observations	1,044	780	1,160	761	1,160	859
Wald chi^2 (4)	56.67	-	56.88	-	127.09	-
Prob > chi^2	0	-	0	-	0	-
F-stat	-	34.61	-	31.64	-	147.16
Prob > F	-	0	-	0	-	0
Pseudo R^2	0.0711	0.0709	0.0676	0.0503	0.2604	0.1398

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

Table 3: Hurdle Model Regressions for part 3 including punishment

	Periods 21-30	
Independent Variables	Contribution Decision	Contribution amount
Treatment P	-0.828*	0.570
	(0.477)	(0.664)
Lagged own contribution	0.265***	1.083***
	(0.0511)	(0.0450)
Lagged own received punishment points	0.0723	0.355***
	(0.0575)	(0.115)
Period	-0.129***	0.219**
	(0.0435)	(0.101)
Constant	3.506**	-4.249*
	(1.463)	(2.494)
Log Pseudo-Likelihood	-190.222	-1184.580
Observations	684	577
Wald chi^2 (4)	69.62	-
Prob > chi^2	0	-
F-stat	-	164.43
Prob > F	-	0

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

Table 4: Hurdle-model estimation for determinants of punishment[1]

	Punishment Phase: Periods 21-30	
Independent Variables	Punishment Decision	Punishment Severity
Absolute Negative deviation from Group's Average	0.600***	0.110***
	(0.192)	(0.0198)
Positive deviation from Group's Average	-0.128**	-0.0222
	(0.0637)	(0.0504)
Period	-0.200***	-0.0146
	(0.0401)	(0.0182)
Treatment P	-1.282***	-0.640***
	(0.315)	(0.185)
P x Absolute Negative deviation from Group's Average	0.0597	0.0196
	(0.230)	(0.0305)
P x Positive deviation from Group's Average	0.256***	0.0173
	(0.0893)	(0.0559)
Constant	4.671***	1.510***
	(1.071)	(0.463)
Log Pseudo-Likelihood	-381.719	-740.735
Observations	760	323
Wald chi^2 (6)	70.24	-
Model chi^2 (6)	-	269.3
Prob > chi^2	0	0
Pseudo R^2	0.2634	0.1538

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

[1] A logit regression was run for the punishment decision binary variable, and a zero-truncated Poisson regression was run for punishment severity regression. In both columns the reported statistics are the coefficient estimates and standard errors are robust and clustered at the individual level.

Figure 1: Average contribution over time

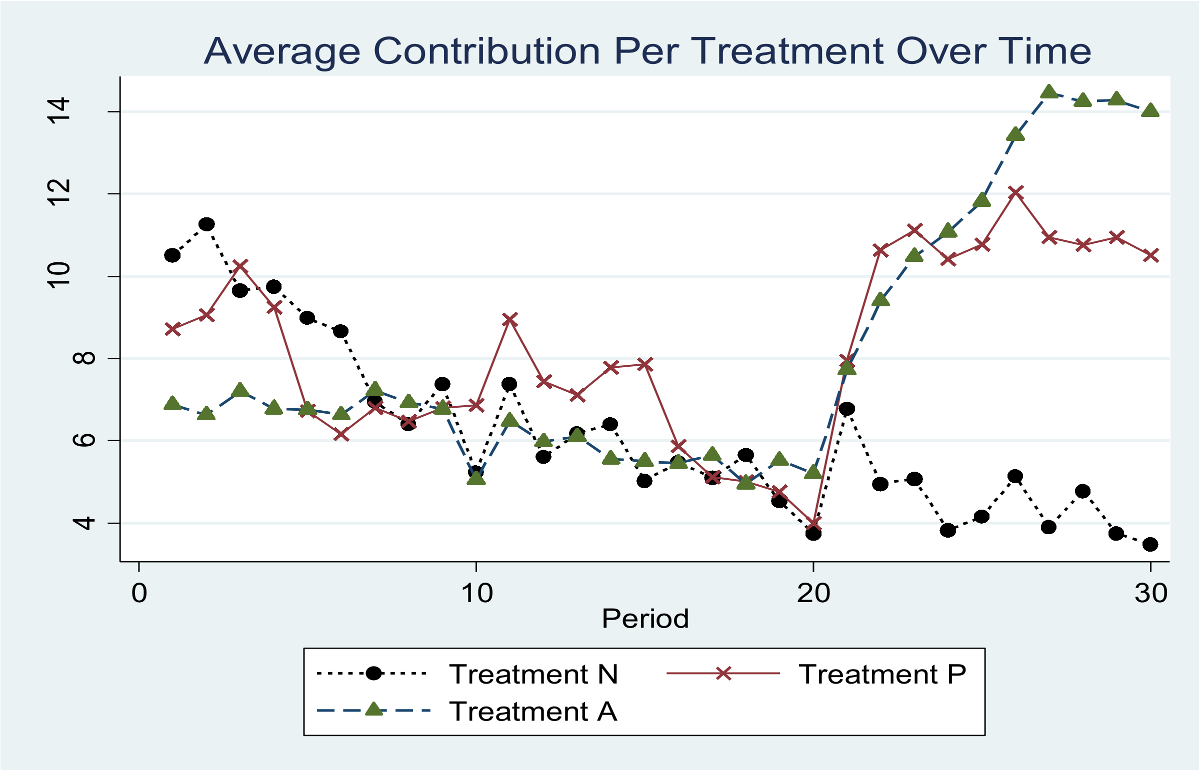


Figure 2: Dynamics of punishment use by Treatment

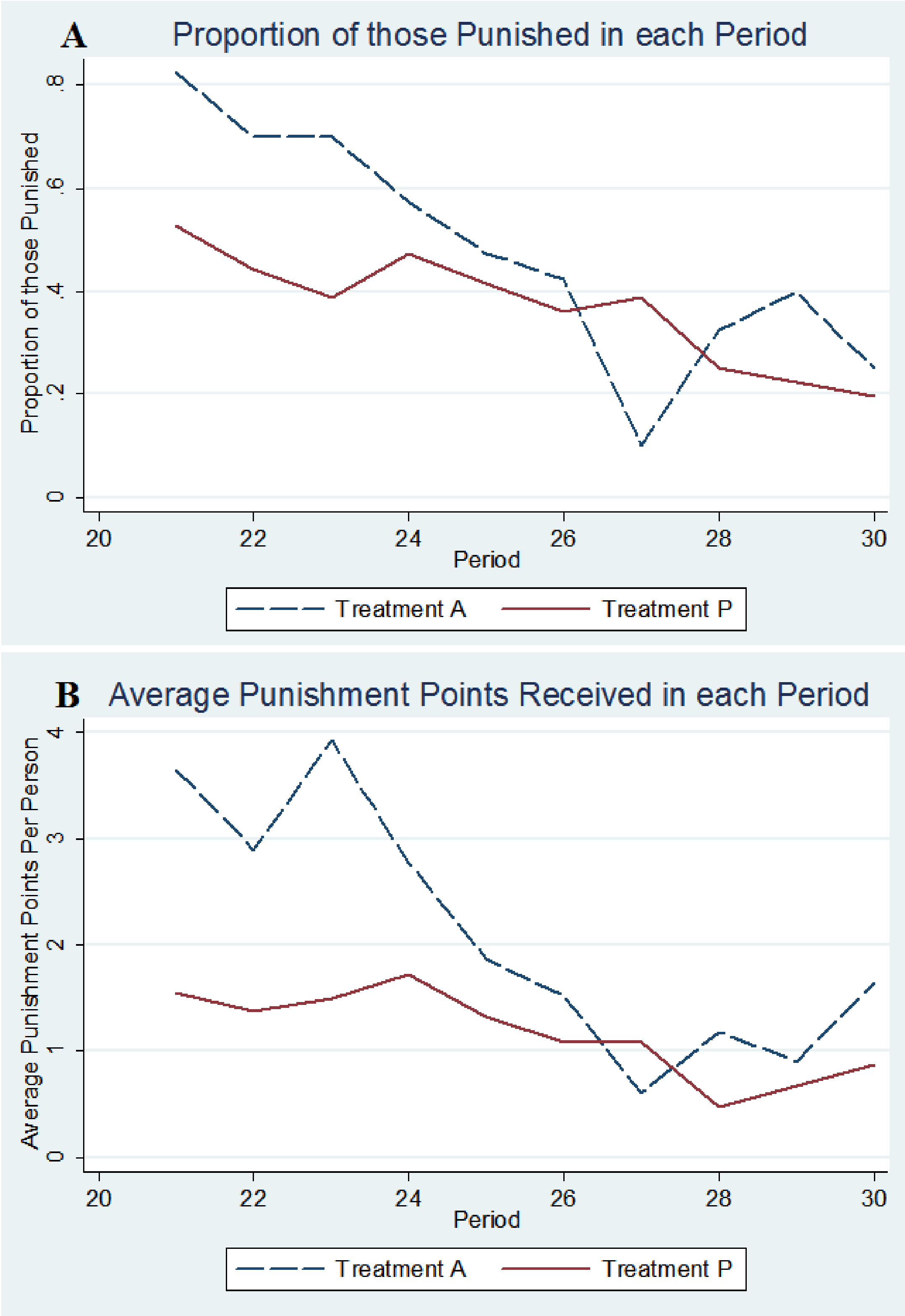
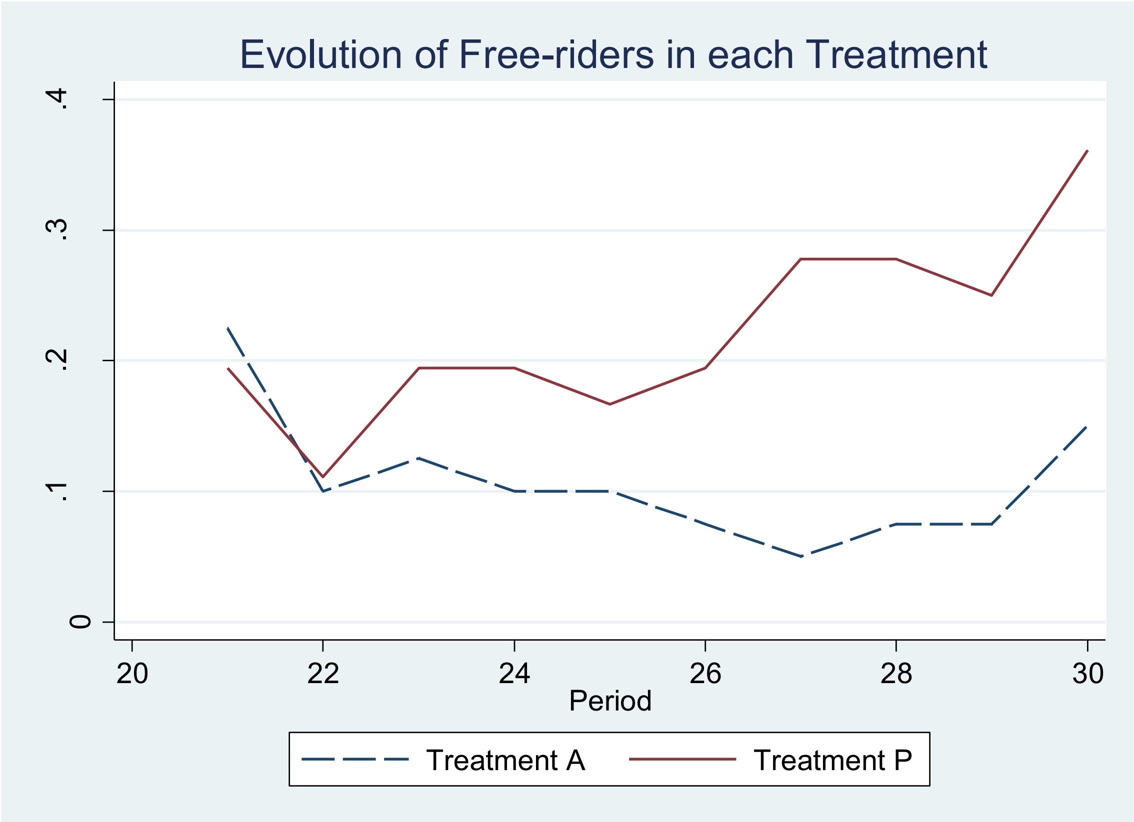


Figure 3: Proportion of Free-riders per Period



Appendix – Nonparametric Test Tables

A. *Kruskal-Wallis Tests between treatments for each Phase*

	Period 1-10		Period 11-20		Period 21-30	
	Ranksums	p-value	Ranksums	p-value	Ranksums	p-value
Variable: Contribution						
Between A and N	A=150275.00 N= 170125.00	0.0022	A=161542.50 N= 158857.50	0.6743	A=204635.00 N=115765.00	0.0001
Between A and P	A=146149.00 P=143031.00	0.0431	A=148829.00 P=140351.00	0.2542	A=160488.50 P=128691.50	0.0051
Between N and P	N= 155724.00 P= 133456.00	0.2385	N= 147685.00 P= 141495.00	0.1266	N= 122436.00 P= 166744.00	0.0001
Variable: Number of Contributors						
Between A and N	- -	-	- -	-	A=187200.00 N=133200.00	0.0001
Between A and P	- -	-	- -	-	A=160460.00 P=128720.00	0.0001
Between N and P	- -	-	- -	-	N=136160.00 P=153020.00	0.0001
Variable: Contribution made if a contributor						
Between A and N	- -	-	- -	-	A=120965.00 N= 46945.00	0.0001
Between A and P	- -	-	- -	-	A= 113911.50 P=89291.50	0.9882
Between N and P	- -	-	- -	-	N=42109.00 P=84144.00	0.0001

B. *Kruskal-Wallis Tests for Relative Change in Contribution Before Punishment Institution*

	Period 19-20	
	Ranksums	p-value
Variable: Relative Change in Contribution		
Between A and N	A=1744.00 N= 1496.00	0.2186
Between A and P	A=1557.50 P=1368.50	0.8501
Between N and P	N= 1457.00 P= 1469.00	0.3757

C. Kruskal-Wallis Tests for the Punishment Phase

	Punishment Phase: Period 21-30	
	Ranksums	p-value
Variable: Punishment Received		
Between A and P	A=161858.50 P=127321.50	0.0004
Variable: Number of Subjects Punished		
Between A and P	A=160180.00 P= 129000.00	0.002
Variable: Number of Free-riders		
Between A and P	A= 143940.00 P= 145240.00	0.0001

D. Wilcoxon Rank-sum Tests of Changes in Contributions between Phases

	Treatment N		Treatment P		Treatment A	
	Ranksums	p-value for Ho of equality	Ranksums	p-value for Ho of equality	Ranksums	p-value for Ho of equality
Between Part 1 (Periods 1-10) and 2 (Periods 11-20)	Part 1 =178681.5 Part 2 =141718.5	0	Part 1 =138021 Part 2 =121539	0.0027	Part 1 =168599 Part 2 =151801	0.0091
Between Part 2 (Periods 11-20) and 3 (Periods 21-30)	Part 2 = 168699.5 Part 3 = 151700.5	0.0072	Part 2 = 111341.5 Part 3 = 148218.5	0	Part 2 = 121461.5 Part 3 = 198938.5	0