

# Punishment strategies in repeated games: Evidence from experimental markets

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May 2010: First draft

## Abstract

An experiment is designed to test the implications of the equilibria typically studied in the repeated game literature (i.e. those based on Nash reversion and optimal symmetric two-phase punishment strategies). One hundred pairs of subjects repeatedly set prices in a differentiated demand duopoly setting. Unlike existing experiments there was no fixed end date or time constraint (some pairs played the game for over six months). Subjects were occasionally asked to enter prices through one-period ahead pricing strategies. The equilibrium conditions predicted by theory are not satisfied in any of the one hundred markets. Less than 5% of elicited strategies have properties similar to those typically studied in the literature and almost 95% of strategies are less harsh (in their immediate response to undercutting) than implied by Nash reversion. Future earnings are highest for subjects adopting matching strategies, even after controlling for a subject's past earnings.

Keywords: Repeated games, cooperation, disproportionate punishment strategies

JEL : L11, L12, L13, L41

## 1 Introduction

When decision makers interact repeatedly through time, we know from experiments that in certain circumstances they can achieve cooperative outcomes. However, we know much less about the strategies they use to sustain these cooperative outcomes.

The folk theorems of infinitely repeated games imply a wide range of equilibrium strategies are possible. The theoretical and applied literature has predominantly focused on decision makers which adopt *disproportionate punishment strategies*. According to these strategies, the same severe punishment applies regardless of the nature of the deviation. The prime examples of disproportionate punishment strategies are the grim trigger strategy following Friedman (1971) in which agents cooperate initially but revert to the one-shot Nash equilibrium forever following any agent defecting from the cooperative phase

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(so-called Nash reversion) and the optimal symmetric two-phase punishment strategies following Abreu (1986, 1988) in which the initial punishment can be even more severe than Nash reversion but is enforced by the prospect of returning to the cooperative phase if there is no deviation from the punishment phase. The applied literature of cooperation in repeated games is almost entirely based on these two types of equilibria.<sup>1</sup>

This paper aims to shed light on the types of strategies actually adopted in an experimental setting by making use of a variation of Selten's (1967) strategy method. It addresses whether experimental evidence provides any support for the disproportionate punishment strategies typically considered in the literature, or whether any other norm is adopted. It also addresses which type of strategy does best.

In the experiment, 200 university students are randomly assigned into 100 duopoly markets. Each subject faces a payoff table which comes from a symmetric Bertrand duopoly with differentiated products and is the same for all markets. Subjects are paired with one other subject throughout the experiment. Compared to most existing repeated game experiments, the experiment in this paper is novel in two respects.

First, while normally each subject in a market is asked for their price after observing the other subject's previous price, occasionally (as determined by a random draw) a subject does not see the other subject's previous price. Instead, subjects are asked for their pricing strategy (the price they wish to set for each possible price the other subject might have set in the previous round). This is a variant of the "strategy method" first proposed by Selten (1967). A subject's given pricing strategy, together with the price the other subject actually set in the previous round, determines their price for the round. By only asking for subjects' pricing strategy occasionally, we allow subjects to first experience setting prices in the normal fashion. This design also allows us to establish that asking for strategies rather than prices does not distort decisions, thereby validating the strategy method for the present experiment.

Second, the experiment does not have any explicit or implicit final round. Rather, after 12 rounds, each market has a 5% chance of ending each round. After an introductory lab session, subjects set their prices through a custom designed program run over the Internet. This has several important benefits. It means I adhere more closely to the theoretical setting (and real world settings) in which there is typically no fixed time period or number of rounds after which subjects know the game will end.<sup>2</sup> It means subjects have a more realistic amount of time to reflect on their choices (2-3 days per round compared to 1-2 minutes in a lab session). It also ensures that there are sufficient rounds (at least in the longer lasting markets) for subjects' actions to stabilize so that their "equilibrium" behavior can be observed.

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<sup>1</sup>A small literature (Kalai and Stanford, 1985, and Friedman and Samuelson, 1990, 1994; see also the strategies in Aumann and Shapley, 1994 and Rubinstein, 1979) has studied cooperation in repeated games with punishments that are more proportionate in the sense the punishment is tailored to fit the crime. A recent example is provided by Lu and Wright (2010) who study equilibrium price-matching punishment strategies.

<sup>2</sup>Dal Bó (2005) establishes the importance of using a random continuation rule to simulate infinitely repeated games in lab experiments by comparing treatments with different ending rules.

The evidence from the experiment is not consistent with the equilibrium predictions of the existing theory based on Nash reversion or optimal symmetric two-phase punishment strategies. The equilibrium strategies implied by these theories are not adopted in any market for which strategies are elicited. In general, less than 5% of elicited strategies involve disproportionate punishments similar to those typically studied in the literature. Instead, subjects tend to use proportionate punishments. Indeed, almost 95% of elicited strategies are less harsh (in their immediate response to undercutting) than implied by Nash reversion. Commonly used one-period ahead strategies include setting a constant price regardless of the other subject's previous round price, setting the myopic best-response to the other subject's previous round price, and matching the other subject's previous round price.

The adoption of one-period ahead strategies other than disproportionate punishment strategies does not seem to come at any cost to the subjects involved. Subjects that adopt disproportionate punishment strategies do not enjoy significantly higher present discounted value of earnings compared to other strategies, suggesting the use of these other strategies is not irrational or suboptimal. In contrast, subjects that adopt one-period ahead matching strategies do enjoy significantly higher future earnings, even after controlling for past earnings, with the increase in the present discounted value of earnings ranging from about US\$15 higher than for disproportionate punishment strategies to about US\$27 higher than for myopic best-response strategies.

The present paper is related to the large number of previous experimental studies focusing on repeated competition games and collusion (see Haan *et al.*, 2009 and Engel, 2007 for recent surveys). In these experimental studies, actions (e.g. prices or outputs) rather than strategies are elicited from subjects. The one exception is Selten *et al.* (1997). They have each subject supply a strategy which is played against every other subject's strategy (one-by-one) in a series of 20-period supergames through a computerized tournament, with the goal being to supply a strategy which does the best against all others. They find subjects achieve cooperation best by a "measure-for-measure policy," which reciprocates movements towards and away from the ideal point by similar movements. Thus, they provide a generalization of the finding of Axelrod and Hamilton (1981) that tit-for-tat does best in computerized tournaments of 2x2 prisoner dilemma games.

Compared to Selten *et al.*, my setting is more standard in that subjects are matched into fixed symmetric duopoly markets rather than in tournaments, with subjects paid depending on their earnings in the experiment. I am also less ambitious in the strategies elicited. Selten *et al.* obtain subjects' full strategies (for all possible histories) by having subjects program their strategies in advance on a computer. Given the complexity of defining a full strategy (e.g. for a duopoly with  $N$  price choices, there are  $N^{2T}$  possible histories after  $T$  rounds), requiring full strategies be described from the start could severely distort or artificially simplify subjects' chosen strategies compared to what they would choose if they chose their actions sequentially. In comparison, I only occasionally elicit subjects' one-period

ahead strategies during the course of the experiment. Despite these differences, I also find a measure-for-measure policy does best, suggesting the principle it embodies may apply even to standard repeated game settings.<sup>3</sup>

The approach I take is complementary to some other recent experimental work which attempts to infer repeated-game strategies from subjects' actions — either by estimating a model from experimental evidence (Mason and Phillips, 2002 use a comparative static approach while Aoyagi and Fréchette, 2009 make use of an environment with imperfect monitoring and noise) or through the use of deterministic finite automata to back out the strategies that best fit the observed actions (Engle-Warnick *et al.*, 2004 and Engle-Warnick and Slonim, 2006). By directly eliciting subjects' strategies, some of the difficult inference problems associated with these approaches is avoided.<sup>4</sup> The obvious limitation is that I only observe a small part of a subject's full strategy (what they will do in the subsequent round, and this, only occasionally). Nevertheless, doing so is enough to test whether some of the necessary conditions for existing theories.<sup>5</sup>

The rest of the paper proceeds as follows. Section 2 outlines the experimental procedure. Section 3 briefly summarizes the testable implications of the theories considered. Section 4 details the results from the experiment, while Section 5 concludes.

## 2 Experimental design

The experiment was conducted at the National University of Singapore (NUS) and through the Internet from August 2007 to March 2008. 219 subjects were recruited from third and fourth year undergraduate classes across NUS. The experiment was conducted in English, the national language of Singapore and the medium of instruction at NUS.

Students were handed a sign-up flyer after entering or exiting an identified class, from which they received a unique code which allowed them to sign-up through the Internet to a briefing session. Signed-up subjects attended one of 13 separate briefing sessions. The briefing sessions, which lasted about 45 minutes, involved giving out instructions (which were read out aloud) as well as going through some demonstration rounds on computers in the lab and a short “test” designed to see whether they understood the instructions. The experiment (including the briefing session) was run through a custom-built website. Figure 1 provides a screen shot from a typical round (the name and numbers are fictitious). Subjects

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<sup>3</sup>In the symmetric setting of my experiment, the measure-for-measure policy Selten *et al.* propose implies the price matching strategy I find does best.

<sup>4</sup>Even more difficult inference problems arise in drawing conclusions on strategies from observed actions in non-experimental settings. Lu and Wright (2010) discuss some of the anecdotal and empirical support for the view firms in selected industries use price-matching type strategies to punish deviations from cooperative outcomes; see also Bhaskar *et al.* (1991, footnote 4) and Levinstein (1997). In the context of imperfect monitoring environments, Porter (1983), Ellison (1994) and others have investigated the extent to which the 1880s railroad cartel provides evidence in favor of the Green and Porter (1984) equilibrium theory of collusion.

<sup>5</sup>Eliciting one-period ahead strategies is sufficient to capture subjects' full strategies if subjects instead use constant intertemporal-reaction functions as proposed in Kalai and Stanford (1985), and Slade (1987, 1992).

took away copies of the instructions (see Appendix A) and the payoff table (see Appendix B) to use for the actual experiment, which was carried out online, along with a payment of ten Singapore dollars (S\$10  $\simeq$  US\$7) in cash for attending the briefing session. Given there was excess demand for the 200 places in the experiment, subjects were selected to participate in the ongoing experiment on the basis of their test answers (in terms of giving the correct answers).

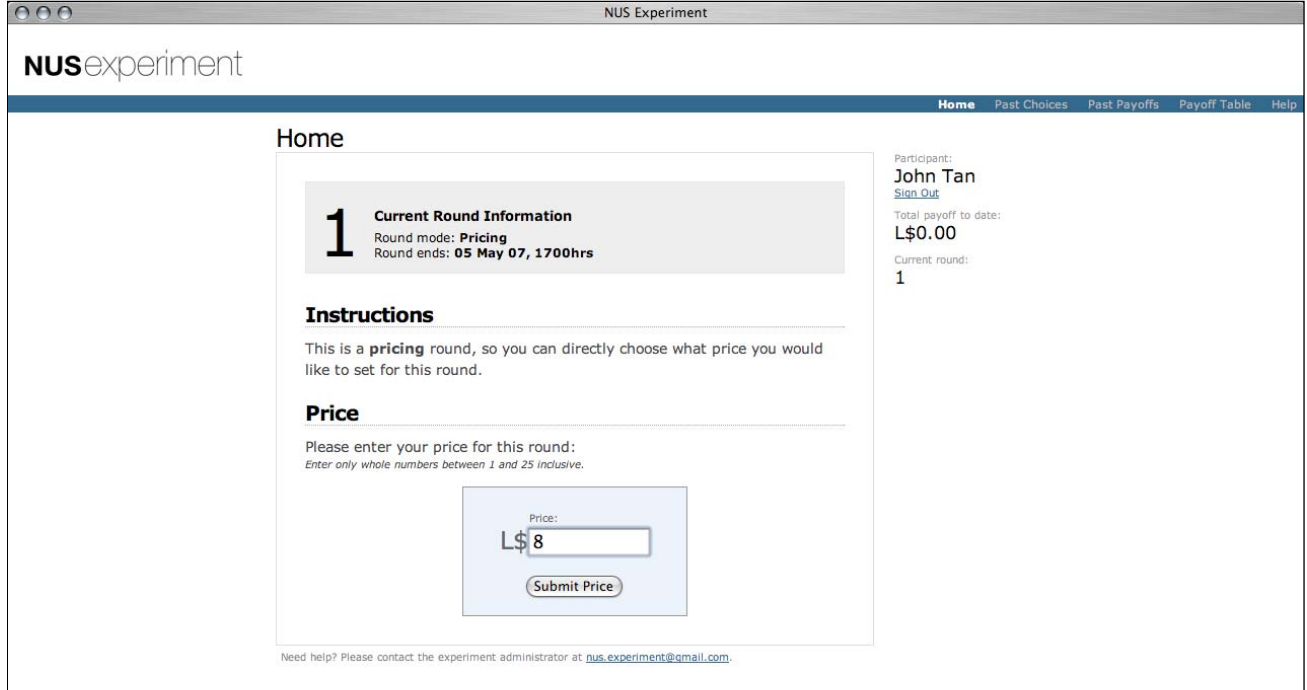


Figure 1: Screen shot of hypothetical price round

The 200 selected subjects were randomly paired into 100 markets subject to the constraint that no two subjects from the same major were matched. All subjects received the same payoff table (see Appendix B) which showed their payoff (in lab dollars L\$) for each combination of their own price and the other subject's price. The payoffs are derived from a standard one-shot symmetric Bertrand duopoly game with differentiated products and linear demands. Specifically, inverse demand functions are given by  $p_i = \alpha - \beta(q_i + \gamma q_j)$ , where  $0 < \gamma < 1$  serves as a measure of the degree of product substitutability. Inverting the inverse demand functions implies firm  $i$ 's demand function is (for price  $0 < p_i < \alpha$ )

$$\begin{aligned}
 q_i &= \frac{\alpha(1-\gamma) - p_i + \gamma p_j}{\beta(1+\gamma)(1-\gamma)} & \text{if} & \quad \frac{-\alpha(1-\gamma) + p_j}{\gamma} < p_i < \alpha(1-\gamma) + \gamma p_j \\
 &= \frac{\alpha - p_i}{\beta} & \text{if} & \quad 0 < p_i \leq \frac{-\alpha(1-\gamma) + p_j}{\gamma} \\
 &= 0 & \text{if} & \quad p_i \geq \alpha(1-\gamma) + \gamma p_j.
 \end{aligned}$$

The payoff for subject  $i$  is  $\pi_i(p_i, p_j) = p_i q_i$  (to the nearest lab cent), where  $\alpha = 46$ ,  $\beta = 1$ ,  $\gamma = 21/22$  and  $p_i$  is any whole number from 1 to 25. The one-shot Nash equilibrium price  $p^n = \alpha(1-\gamma)/(2-\gamma)$  is exactly L\$2, which is also the unique Nash equilibrium price of the discretized game. The corresponding

payoff is denoted  $\pi^n$ . The monopoly price  $p^m = \alpha/2$  is exactly L\$23. In addition to the S\$10 for participating in the lab session, subjects received S\$1 (about US\$0.70) for every 50 lab dollars (L\$) they obtained (paid into their bank account from the university after their market ended). Thus, at the one-shot Nash equilibrium price, their payoff is L\$45.02 or S\$0.90 per round, while at the monopoly price their payoff is L\$270.65 or S\$5.41 per round. By way of contrast, the standard hourly wage for undergraduate research assistants at NUS was S\$8.74 during the same period. Given that students could enter their prices at any time up to a fixed cutoff time for each round, and could do so whenever they were already logged onto the Internet, the experiment required minimal time (e.g. a minute to log-in and enter their price). On the other hand, unlike a lab session, subjects were free to spend as long as they wanted making their choices (up to the cut off time for each round, which was at 8pm on a Tuesday, Thursday and Sunday of each week).

In the instructions, subjects were told that they represent a “seller” of a product or service and that they have to decide what price to set. They were matched throughout with one other subject, known to them only as the “other seller”. In the first round and most subsequent rounds, subjects had to decide which price to set (from L\$1 to L\$25), as illustrated in figure 1. They were informed that after round 12, there was a 5% chance each round that a market would end, upon which both subjects would be paid their cumulative earnings. It was also possible that a subject did not enter their price by the cutoff time, despite reminders, in which case the affected market was closed. The other subject was paid according to their projected earnings (calculated assuming she continued to earn the market average earnings for the remaining rounds in which the market would have existed)

Subjects were also informed that sometimes a round may differ in that instead of entering a single price, they would have to enter a pricing strategy. This, together with the price the other subject actually set in the previous round, determined their actual price for the round. Figure 2 provides a screen shot from a typical strategy round (the name and numbers are fictitious). Unlike prices, a subject’s past strategy is not observed by the other subject they are matched to (only the price that is implied by their past strategy is observed in subsequent rounds).

In theory, it would make no difference to a subject whether a round is a strategy round or a price setting round. However, asking for strategies in every round would likely cause confusion among subjects since they would never actually see the prices set by their opponent in the preceding round. Indeed this was the experience from a smaller pilot experiment that was run two years earlier with a smaller group of business and economics students. Instead, strategies were only asked starting from round 10. Specifically, in rounds 10, 15, 20, ... each market which was still open had a 25% chance of being selected as a strategy round if the market was not going to end within the subsequent 5 rounds and a 75% chance of being selected as a strategy round if the market was going to end within the subsequent 5 rounds.<sup>6</sup> This design

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<sup>6</sup>Before the start of the experiment, the length of each market was fixed based on a simulation using the 5% chance for a market to end each round (after round 12).

ensured that subjects would not get asked for strategies too many times, especially during the earlier part of their market's existence, while at the same time increasing the chance of eliciting their strategies as late as possible during their market's existence (when prices are more likely to have stabilized at the cooperative level). Subjects were not informed of this particular formula; in the lab briefing they were only told that "sometimes" a round will be a strategy round. By only asking for strategies in round numbers which are multiples of 5, and keeping randomness in the decision to ask for strategies, subjects would not likely be able to work out any linkage between being asked for a strategy and the increased likelihood their market would end.<sup>7</sup>

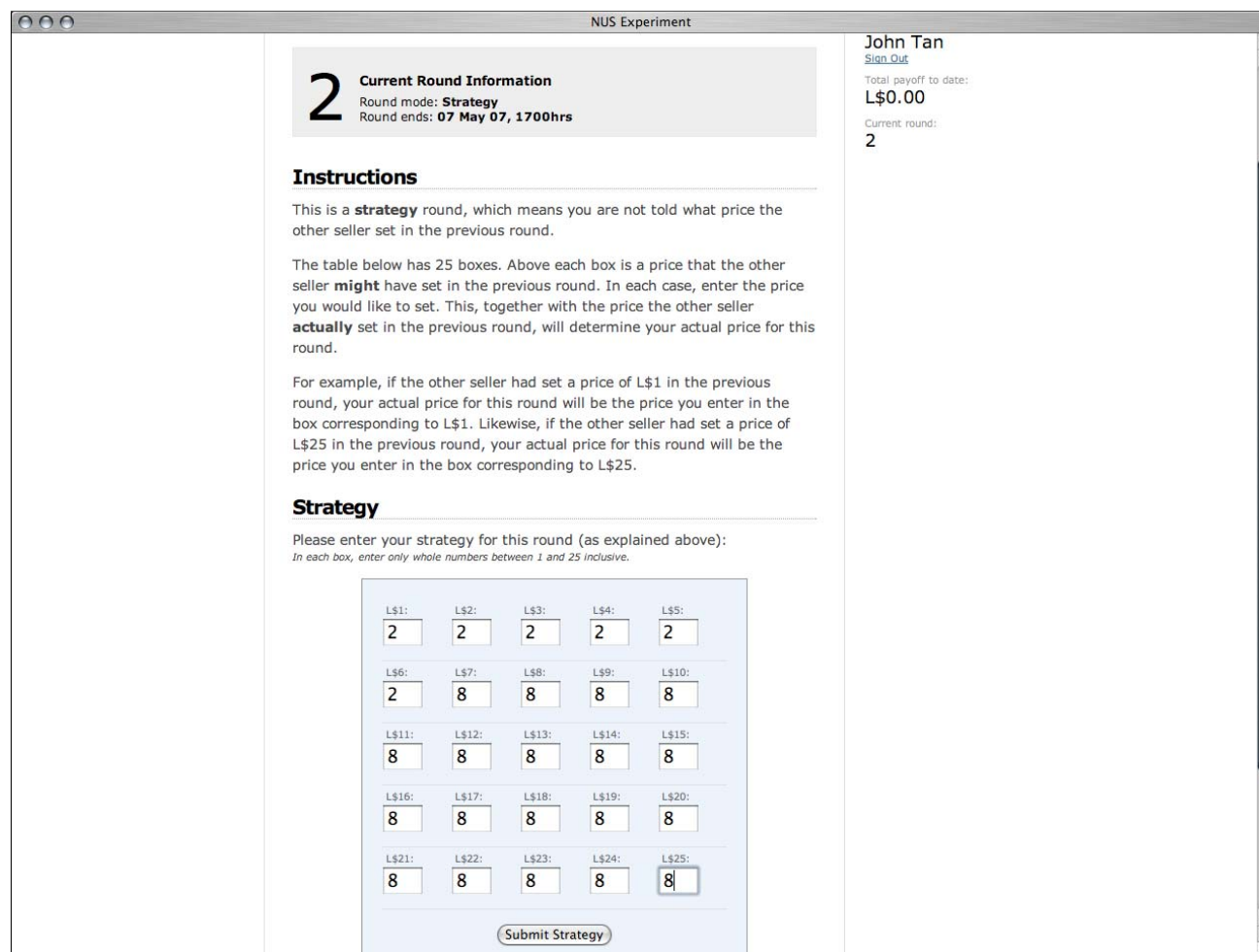


Figure 2: Screen shot of hypothetical strategy round

Communication between matched subjects was controlled. Subjects in one quarter of the markets (randomly selected) were informed that they could post text messages to each other through the website (one per round in each of the rounds). Since messages in round  $t$  are observed at the same time as prices from round  $t$  (i.e. when round  $t + 1$  prices are being set) communication is limited and there is no scope for simultaneous two-way communication. Messages were screened to make sure identities were

<sup>7</sup>Indeed, as discussed in Section 4, I do not find any significant last-round effect or strategy-round effect on prices.

not revealed in any way. For the remaining markets, communication between matched subjects seems highly unlikely given subjects from the same major were not matched and given subjects would have to find the one other person at NUS they were randomly matched to out of a total student population of around 29,000. Subjects were not shown any outcomes from other markets.

### 3 Theoretical predictions

In this section I outline the predictions of the standard equilibrium theory of cooperation in infinitely repeated games to be tested in Section 4 for the experimental setting I consider.

For the prices  $(p_1^c, p_2^c)$  of matched subjects 1 and 2 to be cooperative I require  $\pi_1(p_1^c, p_2^c) \geq \pi^n$  and  $\pi_2(p_2^c, p_1^c) \geq \pi^n$  with one of the inequalities strict. Given the payoffs in the experiment, this implies  $p_i^c > p^n$  and  $\pi_i(p_i, p_j) > \pi^n$  for  $i = 1, 2$  and  $j \neq i$ . According to Friedman's (1971) theory based on Nash reversion, both subjects in a market should set cooperative prices in every round unless either subject defects, in which case all subjects should set the one-shot Nash equilibrium price  $p^n$  thereafter. Optimal symmetric two-phase punishment strategies involve both subjects starting in the cooperative phase in which each subject sets the monopoly price in each round. Following any subject defecting, subjects should enter a punishment phase. The punishment phase involves the subjects pricing at the lowest level possible (in the experiment  $L\$1$ ), for at least one round after which the subjects should return to the cooperative phase but only if there has been no deviation from the punishment phase.<sup>8</sup>

It is standard in applied settings to focus on  $p_i^c = p^m$  when this outcome is also one of the equilibrium outcomes of the supergame. Indeed, according to optimal symmetric punishment strategies, this is required if it is an equilibrium outcome. Given the likelihood of the game ending each round is 5% (starting after round 12), it is straightforward to confirm that in theory the best outcome (the monopoly price) is indeed supported by the Nash reversion strategy and therefore also by optimal symmetric two-phase punishment strategies. Lower cooperative prices are also supported by Nash reversion, which is why I allow for them. For generality, the conditions given below also capture two-phase punishments in which the (possibly unequal) cooperative prices are less than monopoly, in which the punishment phase involves the one-shot Nash equilibrium price rather than the lowest possible price, or any other trigger strategies which start with constant cooperative prices (possibly unequal) being set each round and which respond immediately to any defection by having both subjects price at  $L\$1$  or  $L\$2$  for at least one round.

From the experiment, prices and (occasionally) one-period ahead strategies are observed for each pair of subjects. Denote the price chosen by subject  $i$  in round  $t$  as  $p_{it}$  and the one-period ahead strategy submitted by subject  $i$  in round  $t$  as  $s_{it}(p)$  (which is a mapping from each possible price  $p$  that the other

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<sup>8</sup>In the game being considered, it makes no difference to the set of equilibrium outcomes whether a defection from the cooperative phase for either of these strategies is defined as any price different from the cooperative price (the usual definition) or just that a subject sets a price lower than  $p_i^c$ . I will assume either definition of a defection is consistent with the theory.

subject  $j$  ( $\neq i$ ) may have set in round  $t - 1$  to subject  $i$ 's corresponding intended price in round  $t$ ). The last round prior to a market ending (as determined by the random stopping rule) is denoted  $T$ .

With these definitions, the standard equilibrium theory implies:

$$p_{it} = p_i^c \quad \text{if } t_0 \leq t \leq T \quad (1)$$

$$s_{it}(p) = \left\{ \begin{array}{l} p_i^c \quad \text{if } t = t_0 \\ p_i^c \quad \text{if } p = p_j^c, t_0 < t \leq T \\ p^p \quad \text{if } p < p_j^c, t_0 < t \leq T \\ p^p \text{ or } p_i^c \text{ if } p > p_j^c, t_0 < t \leq T \end{array} \right\}, \quad (2)$$

for  $i = 1, 2$  and  $j$  ( $\neq i$ ), where  $t_0 = 1$  is the initial round of cooperation,  $p^p = p^n = 2$  under Nash reversion and  $p^p = 1$  under the optimal punishment strategy. Given there are no shocks in the experimental setup, either type of equilibrium strategy implies prices should be set at the cooperative level in every round. The corresponding one-period ahead strategy (observed at the earliest from round 10) should involve each subject pricing at  $p_i^c$  if the other subject set a price of  $p_j^c$  last round, and to price at  $p^p$  if the other subject set a lower price in the previous round. In case the other subject set a price above  $p_j^c$  last round, either  $p^p$  or  $p_i^c$  is possible depending on whether a price above  $p_j^c$  is considered a defection or not.

In practice it may take subjects some time to reach such an equilibrium. As such, it may be more reasonable to only require the equilibrium conditions apply from some later round at which point both subjects in a market have coordinated on the cooperative prices. In this case  $t_0 > 1$ . If strategies happen to be elicited in the first round in which cooperation takes place, then they should involve subject  $i$  setting the constant price  $p_i^c$  regardless of the price set by the other subject in the previous round. Aside from this difference, the implications of the theory for observed prices and one-period ahead strategies from rounds following  $t_0$  remain the same.

## 4 Experimental results

In this section I present the results from the experiment. Before turning to the analysis of strategies, I first describe the data and some tests of the validity of the experimental design.

### 4.1 Descriptive statistics

The 200 subjects generated a total of 5,362 rounds of price observations (subject – rounds). 204 of these rounds are strategy rounds in which one-period ahead strategies were elicited. Excluding 12 markets for which subjects dropped out during the experiment, the remaining 176 subjects generated 4,950 rounds of observations. This corresponds to markets lasting 28.1 rounds on average.

Subjects were well compensated for their efforts. The average earnings are S\$3.30 per round. As noted in Section 2, for the amount of time required to enter a price (or strategy) for a round, this compares favorably with the S\$8.74 hourly wage for undergraduate research assistants. There is also considerable

variation in earnings across subjects. The standard deviation of earnings is S\$2.82 per round. In 20.3% of rounds, subjects obtained no payoff, while in 23.5% of rounds, subjects obtained S\$5.41 per round (corresponding to the monopoly payoff when both subjects set a price of L\$23). In 137 cases, subjects obtained more than S\$10 in a round. In total, including the S\$10 for attending the lab briefing, earnings varied from S\$16.10 to S\$483.82 excluding the 12 subjects who dropped out (and who did not obtain anything beyond the initial S\$10).



Figure 3: Average price of all subjects by round

Figure 3 plots the average price per round. The average price across all 5,362 observations is L\$12.83. For the first 43 rounds (14 weeks of the experiment), the average price across subjects is quite close to L\$12, even as the number of subjects drops from 200 to 24 over the same period. Average prices rise sharply in subsequent rounds reflecting that more markets stabilize at monopoly prices in later rounds.

Defining stable prices as three consecutive rounds in which both subjects in the market set the same constant price, 71.1% of the 1,736 stable price observations involve the monopoly price of L\$23, 19.2% are less than or equal to the one-shot Nash price of L\$2, and only 9.7% involve prices between the one-shot Nash price and monopoly. (Not surprisingly, there are no stable prices above the monopoly price.) Once both subjects in a market set the monopoly price, they tend to stick with it. Indeed, starting from rounds when both subjects in a market first set their prices at the monopoly price, the average price for all subsequent rounds is L\$21.7, with only 10.2% of the 1,395 subsequent observations falling below the monopoly price.

## 4.2 No last-round or strategy-round effects

Unlike many existing experiments that look at repeated games, I avoid having a known last round or implicit time limit. Markets had a fixed 5% chance of ending (after round 12) in every round. Indeed one market lasted 89 rounds or about 30 weeks. This suggests there should not be any last-round effect.

I consider two different tests of the hypothesis that there is no last-round effect — (i) that individual subjects set the same prices in their last-rounds as in their other rounds; and (ii) that subjects who are

in their last round set the same prices as other subjects who are not in their last round. Since prices tend to increase in later rounds, both tests control for the round number in which the last round occurs.

To test (i) I estimate the following fixed effects model:

$$p_{it} = \alpha_i + \lambda_t + \beta I_{it} + \varepsilon_{it}, \quad (3)$$

where  $p_{it}$  is the price of subject  $i$  in round  $t$  and  $I_{it}$  is an indicator variable which equals 1 if round  $t$  is the last-round for a participant  $i$  and 0 otherwise ( $\alpha_i$  captures subject-specific effects and  $\lambda_t$  captures round-specific effects). Excluding the 12 markets where subjects dropped out, the estimate of  $\beta$  is  $-0.079$  with a standard error of 0.442, so I cannot reject (for any reasonable significance level) that subjects set the same price in their last round as in other rounds.<sup>9</sup>

To test (ii) I conduct a Mann Whitney test of the null hypothesis that the prices set by subjects who are in their last round are the same as the prices set by subjects who are not in their last round. To adjust for prices varying with rounds, I first de-mean each subject's price in each round by subtracting from it the average price across all subjects for the round. Based on this test I cannot reject the null hypothesis that prices for the two groups are the same (p-value is 0.925).<sup>10</sup> Thus, I do not find any evidence of a systematic last-round effect.

The prices set in strategy rounds also do not appear to differ significantly from the prices set in other rounds. The same approach is taken as for last rounds — I test whether (i) individual subjects set the same prices in their strategy rounds as in their other rounds; and (ii) that subjects who are asked for strategies set the same prices as other subjects who are not asked for strategies in the same round. Since strategy rounds are more likely to arise within five rounds from a market ending, they are more likely to arise in later rounds, and it is therefore again important to control for the round in which they arise in order to see whether prices are significantly different in strategy rounds.

To test (i) I redefine  $I_{it}$  in (3) to equal 1 if round  $t$  is a strategy round for participant  $i$  and 0 otherwise. The estimate of  $\beta$  is  $-0.274$  with a standard error of 0.460, so the null hypothesis that individual subjects set the same price in strategy rounds as in other rounds cannot be rejected (for any reasonable significance level). To test (ii) I conduct the same Mann Whitney test as above but only for rounds for which strategies are sometimes elicited (i.e. rounds 10, 15, 20 ...). Based on this test I cannot reject the null hypothesis

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<sup>9</sup>Subjects that dropped out may have done so due to low earnings. This would be the case for subjects (or those they are matched to) that had particularly low prices just before dropping out. Including these 12 markets implies an estimate of  $\beta$  in (3) of  $-0.388$  with a standard error of 0.408, so does not change our conclusions. Estimating (3) for the 12 dropped markets separately gives an estimate of  $\beta$  of  $-1.870$  with a standard error of 0.984, which is marginally significant (p-value of 0.058), indicating there may indeed be a last-round effect for these markets but with the usual causation reversed (i.e. for these markets, low prices may have caused the game to end by leading one of the subjects to drop out).

<sup>10</sup>Alternatively, conducting the test one round at a time on the original prices, I cannot reject (at the 5% significance level) that the prices set by subjects who are in their last round are the same as the prices set by subjects who are not in their last round for 31 out of 33 rounds for which there are sufficient observations to run the test (after excluding the 12 markets where subjects dropped out). In one case (round 23) prices are higher for the group in their last round, while in the other case (round 68) prices are higher for the group not in their last round.

that prices for the two groups are the same (p-value is 0.156).<sup>11</sup> Overall the evidence does not point to any systematic difference in pricing as a result of subjects being asked for their strategies.<sup>12</sup>

### 4.3 Do subjects adopt disproportionate punishment strategies?

In this section, I consider whether subjects adopt the equilibrium disproportionate punishment strategies assumed in most of the existing literature (i.e. either based on Nash reversion or optimal symmetric two-phase punishment strategies). A strict interpretation of the theory requires that both subjects in a market should adopt the equilibrium strategies from round 1 onwards (i.e.  $t_0 = 1$ ). Given the lack of any shocks, this implies each subject should maintain a constant price (equal to their initial cooperative price) throughout the experiment. This possibility can be immediately rejected given there are no markets in which both subjects set constant prices in all rounds. Subjects seem to take some rounds to reach an equilibrium although this need not take very long (e.g. in two markets, subjects coordinated on the monopoly price in all rounds starting from round 2).

Allowing for the fact subjects take some time to settle on an equilibrium, (1) implies both subjects in a market should set constant prices starting from some round until the last round in which the market existed. Since subjects could start cooperating in the last round (i.e.  $t_0 = T$ ), all markets with both subjects earning above  $\pi^n$  in the last round automatically satisfy (1) by construction. This accounts for 41 out of the 100 markets. 7 of the remaining markets involve prices that stabilized (as defined earlier) at L\$1 or L\$2, contradicting (1). Prices in the remaining markets did not stabilize and involved either one subject earning strictly less than  $\pi^n$  in the final round (47 markets) or both earning no more than  $\pi^n$  in the final round (5 markets). These markets therefore provide no support for (1).

For the 41 markets which ended with prices that are cooperative, the theory implies (2) should hold for any rounds in which strategies are observed between  $t_0$  and  $T$ , where  $t_0$  is determined by the first round for which both prices in a market remain constant through to the last round  $T$ . For 32 of the 41 markets, no strategies are observed during the rounds  $[t_0, \dots, T]$  reflecting that strategies are only elicited occasionally. This means (2) cannot be tested for these 32 markets. For the remaining 9 markets, a total of 32 one-period ahead strategies are observed during the rounds  $[t_0, \dots, T]$ . The price paths for subjects in these 9 markets are shown in figure 4. Dotted vertical lines indicate strategy rounds, with those occurring during the rounds  $[t_0, \dots, T]$  labelled. As can be seen from the figure, the cooperative prices are common and equal to the monopoly price in all cases (i.e.  $p_1^c = p_2^c = p^m$ ).

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<sup>11</sup>Alternatively, conducting the test on rounds 10, 15, 20, ... one by one, I cannot reject (at the 5% significance level) that the prices set by subjects who are asked for their strategies are the same as the prices set by subjects who are not asked for their strategies in rounds 10, 15, 20, 25, 30, 40, 55, or 60. Prices are significantly lower for subjects who are asked their strategy in round 35, and prices are significantly higher for subjects who are asked their strategy in rounds 45 and 50.

<sup>12</sup>The broad similarity in outcomes between the direct approach and strategy approach is consistent with findings from some experiments based on the ultimatum bargaining game, such as those run by Brandts and Charness (2000) and Oxoby and McLeish (2004), although they also note other studies that find significant differences across the two approaches.

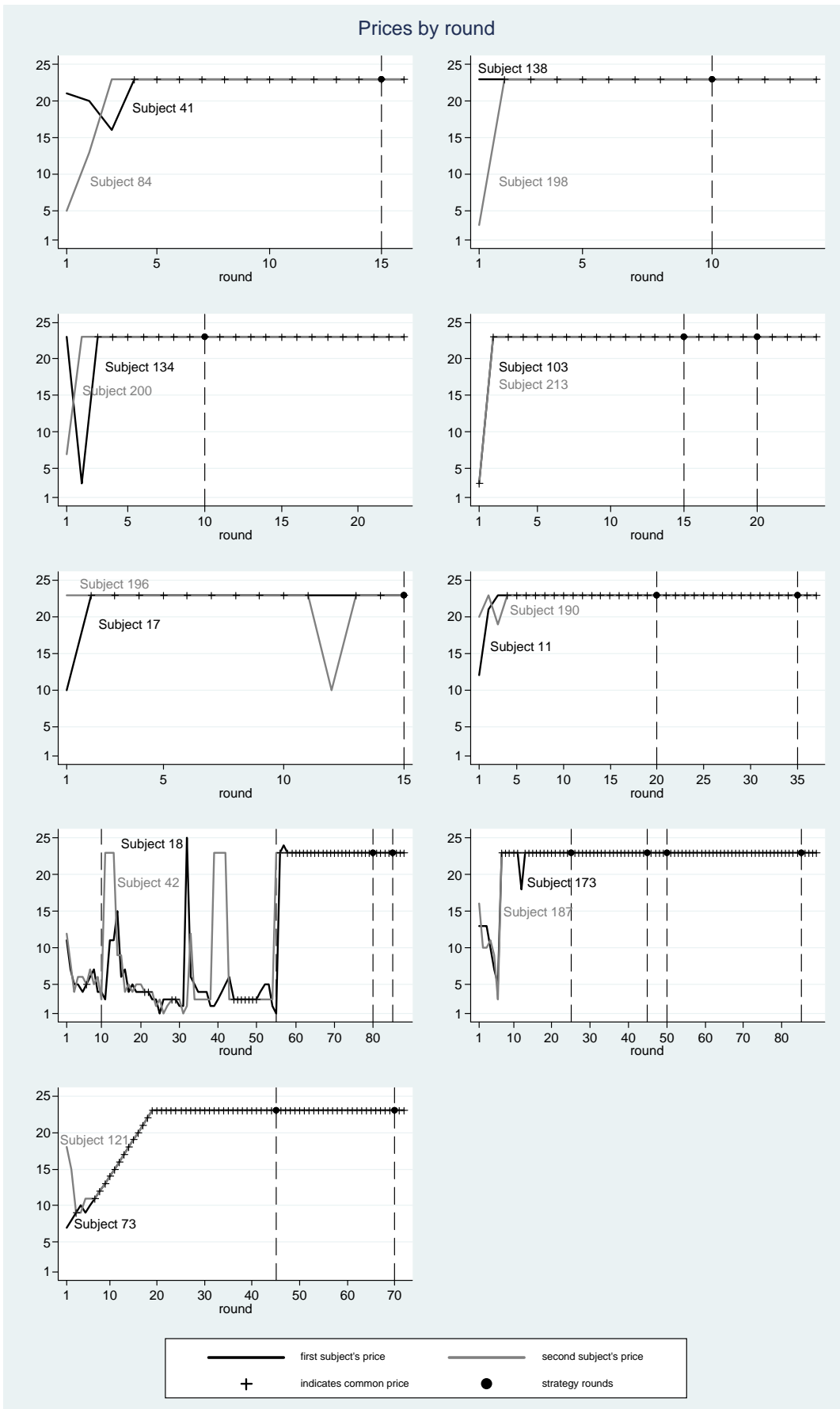


Figure 4: Time path of prices for participants settling on constant cooperative prices

According to (2), one-period ahead strategies for both subjects in a market are suppose to satisfy  $s_{it}(p) = p^m$  if  $p = p^m$  and  $p^p$  if  $p < p^m$  given that the monopoly price is reached prior to the strategy round in each of the 9 markets (as can be seen in figure 4). However, only 3 observed strategies satisfy these conditions individually (subject 103, rounds 15 and 20, and subject 138 round 10) and these two subjects are from different markets so that (2) is not satisfied for any pair of subjects. The equilibrium conditions in (1)-(2) are therefore rejected for each of the markets for which they can be tested. Instead, aside from the 3 disproportionate punishment strategies, 12 one-period ahead strategies are constant-price strategies, 8 are matching strategies, 1 is a close approximation to a matching strategy, 4 are close approximations to myopic best-response strategies, while a further 4 defy any obvious description.<sup>13</sup>

A limitation of the above test is the lack of markets for which strategies are observed during rounds in which prices remained constant till the market ended. An alternative test of the theory which uses all information from the 204 strategy rounds is just to see whether the conditions on prices and strategies in (1)-(2) are satisfied for the round  $t_s$  in which strategies are elicited. This can be done by setting  $T = t_s$  in (1)-(2) thereby removing the requirement that they hold for all subsequent rounds till the market ends. This is a very weak version of the theory since it does not require the equilibrium strategies be actually followed in subsequent rounds (i.e. that prices remain constant at the cooperative price, or in case they do not, that punishments be carried out). However, even with this version of the theory, the equilibrium requirements in (1)-(2) are never met. In short, the evidence from the experiment does not support the standard theories of repeated games based on Nash reversion, optimal symmetric two-phase punishment strategies or the other disproportionate punishment strategies noted in Section 3.

#### 4.4 Characterizing adopted strategies

Having shown subjects do not adopt the equilibrium strategies used in the existing theory of repeated games, in this section I investigate which types of one-period ahead strategies subjects do adopt. In Section 4.3, aside from disproportionate punishment strategies, three other distinct types of one-period ahead strategies were found to be used by individual subjects in rounds in which they set stable cooperative prices — matching strategies, (myopic) best-response strategies and constant-price strategies. This suggests the following four theoretical one-period strategies.

- *Disproportionate* punishment strategy. This involves some cooperative price  $p^c \geq 3$  and some punishment price  $p^p \in (1, 2)$  such that either (i)  $s_{it}^D(p) = p^c$  if  $p = p^c$  and  $s_{it}^D(p) = p^p$  if  $p \neq p^c$ , or (ii)  $s_{it}^D(p) = p^c$  if  $p \geq p^c$  and  $s_{it}^D(p) = p^p$  if  $p < p^c$ . These are implied by (2) in the case that a subject expects the other subject to adopt the same cooperative price as himself when deciding

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<sup>13</sup>The formal definitions of these different strategies is given in Section 4.4, along with the meaning of “close approximation”. Only one market involved both subjects (17 and 196) adopting the same one-period ahead strategy, that being the constant-price strategy. As can be seen from figure 4, this arises despite the fact both subjects had already coordinated on the monopoly price.

which prices to punish<sup>14</sup>, but without the requirement in (1) on prices.

- *Matching* strategy. This involves setting a price equal to the other subject’s previous price, implying  $s_{it}^M(p) = p$ . This type of reciprocity is consistent with the “measure-to-measure” strategy that Selten *et al.* (1997) find. Wright and Yu (2010) call this “price-matching”.
- *Best-response* strategy. This involves setting a price equal to the myopic best-response to the other subject’s previous price, implying  $s_{it}^B(p) = BR(p)$ , where  $BR(p) = p - 1$  if  $p < 5$  and  $BR(p) = p - 2$  if  $p \geq 5$  is the one-period (i.e. myopic) best-response function.
- *Constant-price* strategy. This involves setting the same price regardless of the other subject’s previous price, implying  $s_{it}^C(p) = p_{it}$ . In case  $p_{it} = 2$  it is just the one-shot Nash equilibrium strategy. For higher (i.e. cooperative) prices it is consistent with sticking to a cooperative price for (at least) one round regardless of the other subject’s previous price (i.e. a *forgiving* strategy).

Each of the 204 observed strategy is assigned to one of these four types, provided the fit is close enough. The closeness of fit between the actual strategy of subject  $i$  in round  $t$  and the theoretical strategy  $k$  is measured by the root mean square error between the two:

$$RMSE_{it}^k = \sqrt{\frac{\sum_{p=1}^{25} (s_{it}(p) - s_{it}^k(p))^2}{25}}. \quad (4)$$

Specifically, the theoretical strategy  $k$  that has the lowest value of  $RMSE_{it}^k$  is selected provided  $RMSE_{it}^k \leq \tau$ , where  $\tau$  is a threshold value to ensure that strategies are only assigned if they are close fits to one of the four types. I consider both the case  $\tau = 0$ , which requires an exact fit, and the case  $\tau = 1$ , which requires that a strategy be no more than L\$1 away on “average” (i.e. across  $p$ ) from the closest theoretical strategy (i.e. a close fit). In figure 5 the 13 disproportionate punishment strategies assigned based on  $\tau = 1$  are plotted. Those which are also assigned to be disproportionate punishment strategies when  $\tau = 0$  (i.e. exact fits) appear in the first row of figure 5.

Table 1 presents the number of strategies assigned to be of each type  $k$  according to the two values of  $\tau$ , along with the corresponding average of  $RMSE_{it}^k$  across  $i$  and  $t$  for each  $k$ . Best-response, constant-price and matching strategies account for more than 90% of the assigned strategies for either value of  $\tau$ . These strategies have properties that are in stark contract to disproportionate punishment strategies — subjects may not respond immediately to a rival’s price change (constant-price strategy), or where they do react, the intended price change is proportional to the rival’s price change (best-response and matching strategies). Only 5 strategies out of 204 are classified as disproportionate punishment strategies when  $\tau = 0$  and 13 strategies out of 204 are classified as disproportionate punishment strategies when  $\tau = 1$ . In other words, the vast majority of subjects do not adopt disproportionate punishment strategies.<sup>15</sup>

<sup>14</sup>This seems like a reasonable requirement given the symmetric setup of the experiment and that subjects never settle on constant but *unequal* cooperative prices (i.e.  $p_{iT-1} = p_{iT} > p^n$  and  $p_{jT-1} = p_{jT} > p^n$  imply  $p_{iT} = p_{jT}$ ).

<sup>15</sup>This finding does not seem to be explained by subjects lacking sufficient experience. In a probit regression, the adoption of disproportionate punishment strategies is not significantly related to the number of rounds at which subjects are asked

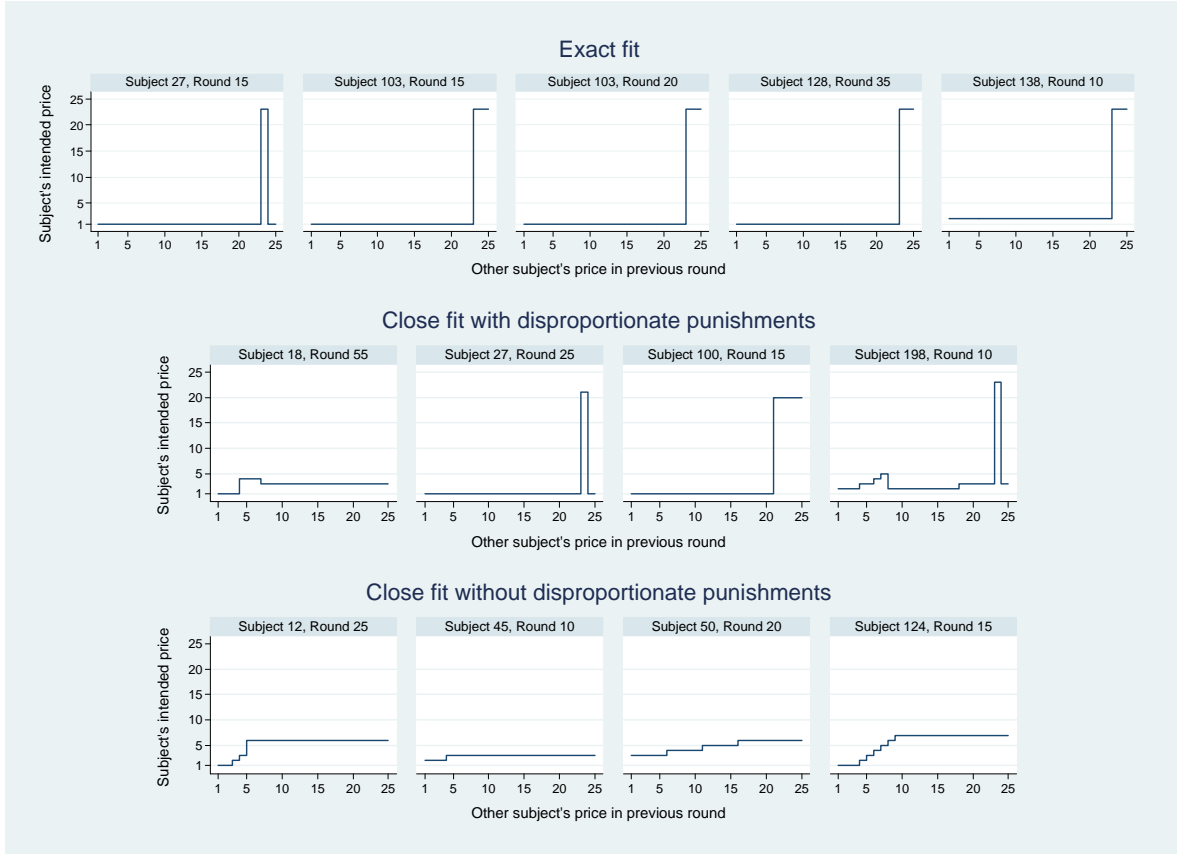


Figure 5: Disproportional punishment strategies (assigned based on  $\tau = 1$ )

Table 1 also considers the extent to which the key properties of disproportionate punishment strategies are satisfied. Such strategies should involve a severe punishment for small amounts of undercutting (i.e. at the trigger point) and should be constant for most changes in price (i.e. except at the trigger point). To quantify these properties it helps to conceive of one-period ahead strategies  $s_{it}(p)$  as a series of steps corresponding to the change in intended price for each increase in  $p$  of L\$1. Define the height of a step at some price  $p$  as  $\Delta s_{it}(p) = s_{it}(p+1) - s_{it}(p)$ . The height of the biggest step of the strategy of subject  $i$  in round  $t$  is defined as

$$\Delta_{it} = \max_{1 \leq p \leq 24} \Delta s_{it}(p),$$

which should be large for disproportionate punishment strategies given  $\max_{1 \leq p \leq 24} \Delta s_{it}^D(p) = p^c - p^p$  but small for strategies with proportionate punishments. Let  $\Delta_{it}^+(p) = 1$  if  $\Delta s_{it}(p) > 0$  and  $\Delta_{it}^-(p) = 1$  if  $\Delta s_{it}(p) < 0$ . The number of upward steps of the strategy of subject  $i$  in round  $t$  is defined as

$$\Sigma_{it}^+ = \sum_{p=1}^{24} \Delta_{it}^+(p),$$

for their strategies, regardless of whether strategies are assigned based on  $\tau = 0$  or  $\tau = 1$ . The sign of the coefficient on the number of rounds is negative in both cases.

which equals 1 for disproportionate punishment strategies but is large for strategies with proportionate punishments. The number of downward steps of the strategy of subject  $i$  in round  $t$  is defined as

$$\Sigma_{it}^- = \Sigma_{p=1}^{24} \Delta_{it}^-(p),$$

which is either 0 or 1 for disproportionate punishment strategies (depending on whether prices above  $p^c$  trigger punishment or not).

Table 1: Assignment of 204 strategies and properties for each assignment

		Number assigned	RMSE	Number of upward steps $\Sigma_{it}^+$	Number of downward steps $\Sigma_{it}^-$	Greatest upward step $\Delta_{it}$	Harshness index $H_{it}$
Based on exact fit ( $\tau = 0$ )							
Disproportionate	$p^p = 1$	4	0	1	0.25	22	1
	$p^p = 2$	1	0	1	0	21	0.95
Matching		15	0	24	0	1	0.52
Best-response		33	0	21	0	1	0.55
Constant-price	$p_{it} \leq 2$	3	0	0	0	0	0
	$p_{it} > 2$	34	0	0	0	0	0
Unassigned		114	1.84	12.54	1.16	4.57	0.54
Based on close fit ( $\tau = 1$ )							
Disproportionate	$p^p = 1$	7	0.2	2.29	0.43	9.86	1
	$p^p = 2$	6	0.62	3.17	0.33	7.83	0.72
Matching		17	0.11	23.53	0	1.12	0.52
Best-response		72	0.28	20.86	0.14	1.42	0.56
Constant-price	$p_{it} \leq 2$	4	0.15	0.25	0	0.25	0.13
	$p_{it} > 2$	37	0.05	0.27	0.22	0.16	0.01
Unassigned		61	2.95	9	1.8	6.11	0.52

Notes: Aside from column 1, all other entries are averages of the respective measure across strategies of the corresponding type. The RMSE for non-assigned strategies corresponds to the average of the RMSE across these strategies, calculated based on the closest fit strategy in each case.

Another property of disproportionate punishment strategies is that their punishments are maximal (or close to maximal in the case of Nash reversion). This is reflected in the rotated-L shape of the disproportionate punishment strategies up to  $p^c$  (as shown in the first row of figure 5). The extent to which a one-period ahead strategy has this shape can be measured by

$$H_{it} = \begin{pmatrix} \frac{\sum_{p=1}^{\bar{p}_{it}-1} (\bar{s}_{it} - s_{it}(p))}{\sum_{p=1}^{\bar{p}_{it}-1} (\bar{s}_{it} - 1)} & \text{if } \bar{p}_{it} > 1 \\ 0 & \text{if } \bar{p}_{it} = 1 \end{pmatrix},$$

where  $\bar{s}_{it} = \arg \max_p s_{it}(p)$  is the maximum price subject  $i$  intends to set in round  $t$  and  $\bar{p}_{it} = \min[\arg \max_p s_{it}(p)]$  is the lowest price the rival could set in round  $t - 1$  that would lead subject  $i$

to set this maximum price in round  $t$ . This index of the harshness of punishment captures the extent of a subject's intended punishment as a proportion of the maximum punishment possible. The maximum punishment possible is measured relative to the maximum price in  $s_{it}(p)$ , which is taken as the cooperative price  $p^c$  that subject  $i$  intends to set.<sup>16</sup> This is consistent with the definition of equilibrium strategies in (2) if subjects are in a cooperative state and is consistent with the definition of disproportionate punishment strategies noted previously. This approach allows me to identify prices below  $\bar{p}_{it}$  as deviations by subject  $j$  from the perspective of subject  $i$  and to measure subject  $i$ 's response to these deviations.

The measure  $H_{it}$  varies between 0 and 1. For optimal punishment strategies it equals 1 since  $s_{it}(p) = 1$  for  $p < \bar{p}_{it}$  and  $\bar{p}_{it} > 2$ . For the Nash reversion punishment strategy with cooperative price  $p^c$  it equals  $(p^c - 2) / (p^c - 1)$  which is close to 1 provided  $p^c$  is sufficiently high (e.g. it equals 0.889 for  $p^c = 10$  and it equals 0.955 for  $p^c = 23$ ). In comparison, for matching strategies  $H_{it} \approx 0.521$  and for best-response strategies  $H_{it} \approx 0.555$ .<sup>17</sup> Whenever  $\bar{p}_{it} = 1$  (this happens for strategies where the intended price never increases in  $p$ , such as for constant-price strategies),  $H_{it} = 0$  reflecting that there is no one-period punishment in this case, even in response to the lowest price set by the rival.

As can be seen from Table 1, which reports the average values of  $\Sigma_{it}^+$ ,  $\Sigma_{it}^-$ ,  $\Delta_{it}$  and  $H_{it}$  across  $i$  and  $t$  for each  $k$ , strategies assigned to be constant-price have low values of  $\Sigma_{it}^+$ ,  $\Sigma_{it}^-$ ,  $\Delta_{it}$  and  $H_{it}$ , while strategies assigned to be matching or best-response have high values of  $\Sigma_{it}^+$ , moderate values of  $H_{it}$ , and low values of  $\Sigma_{it}^-$  and  $\Delta_{it}$ . This is consistent with these strategies not involving disproportionate punishments. In contrast, the 13 disproportionate punishment strategies assigned based on  $\tau = 1$  have, on average, low values of  $\Sigma_{it}^+$  and  $\Sigma_{it}^-$  and high values of  $\Delta_{it}$  and  $H_{it}$ . However, these properties do not hold for all of the 13 assigned strategies. The strategies corresponding to subjects 12, 45, 50 and 124 have values of  $\Sigma_{it}^+$  which are greater than or equal to the value of  $\Delta_{it}$ , suggesting these do not actually involve disproportionate punishments. A visual inspection of these 4 strategies, shown in the third row of figure 5, confirms this view. Thus, of the 13 disproportionate punishment strategies assigned based on  $\tau = 1$ , only 9 can really be said to involve disproportionate punishments, corresponding to the strategies shown in the first two rows in figure 5.

Figure 6 plots a histogram of the index of harshness  $H_{it}$ . The median value of  $H_{it}$  is 0.552 (the mean is 0.456), which indicates the typical punishment is similar in harshness to matching and best-response strategies. Out of the 197 strategies that involve a maximum intended price above the one-shot Nash equilibrium price, 94.4% of punishment strategies are less harsh (in their immediate response) than implied by Nash reversion for the same maximum intended price. This further supports the conclusion that only a small minority of subjects adopt disproportionate punishment strategies.

<sup>16</sup>The results using  $H_{it}$  are almost identical if in the calculation of  $H_{it}$ ,  $\bar{s}_{it}$  is constrained to be less than or equal to 23 to take into account that the intended cooperative price is unlikely to be ever above 23.

<sup>17</sup>Even when the myopic best-response to a rival's deviation price leaves the rival with zero profit, this is not a very severe one-period punishment since the rival anticipating such a punishment can either match or further lower its price to ensure a higher payoff compared to under Nash reversion (except in the case  $p^c = 3$ ).

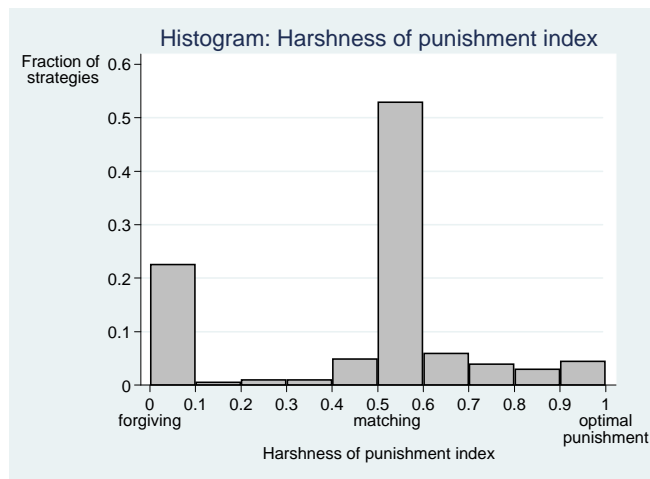


Figure 6: Proportion of strategies with different degrees of harshness

#### 4.5 Which strategies do better?

The focus thus far has been on characterizing the different one-period ahead strategies chosen by subjects. Having shown the vast majority of subjects do not adopt strategies that are as harsh as disproportionate punishment strategies in their one-period response, it is natural to ask whether this choice comes at a cost to the subjects concerned.

Table 2 shows estimates from regressions of the present discounted value (PDV) of earnings on subjects' choice of strategy across all 204 strategy rounds. The PDV of earnings is constructed by calculating the present discounted value of all earnings from the current round (in which the strategy is elicited) till the last round for a subject, where discounting is based on the likelihood the market ends each round (0% up to round 12 and 5% thereafter).<sup>18</sup> Then for rounds beyond the last actual round, the subject's earnings are fixed at their level in their last actual round and the PDV of subsequent earnings (out to infinity) is calculated using the 5% likelihood of markets ending each round. The PDV of earnings takes into account that future earnings are worth less since there is a chance the market will end before they can be realized, and therefore enables me to compare the earnings impact of strategy choices across markets that end in different rounds.

The first two columns of Table 2 differ depending on whether strategies are assigned only if they fit their theoretical counterpart precisely ( $\tau = 0$ ) or whether a close fit is sufficient ( $\tau = 1$ ). Based on either approach, matching strategies are associated with the highest PDV of earnings and best-response strategies with the lowest PDV of earnings. Table 2 also reveals that disproportionate punishment strategies do best when they are adopted precisely while best-response strategies do worst when they are adopted precisely. Wald tests are carried out to test the hypothesis that each strategy has the same

<sup>18</sup>Almost identical results to those shown in Table 2 are obtained if the PDV of earnings is based entirely on future earnings (i.e. calculated from the round after the strategy round) and if past earnings include earnings from the strategy round.

effect on the PDV of earnings as each other strategy, with  $p$ -values reported for each comparison in Table 2. The results imply subjects adopting different types of strategies from disproportionate punishment strategies do not obtain significantly different earnings, with the exception that matching strategies do better based on the classification of strategies when  $\tau = 1$  and best-response strategies do worse based on the classification of strategies when  $\tau = 0$ . In contrast, subjects adopting matching strategies obtain significantly higher earnings than each of the other types of strategies when  $\tau = 1$  (and for all cases except disproportionate punishment strategies when  $\tau = 0$ ).

Table 2: Estimated effect of strategy choice on PDV of earnings

	Model 1		Model 2	
	$\tau = 0$	$\tau = 1$	$\tau = 0$	$\tau = 1$
disproportionate punishment strategy	89.527*** (19.135)	71.756*** (13.661)	20.111 (14.836)	14.869 (11.630)
matching strategy	108.989*** (8.494)	102.576*** (7.920)	41.044*** (15.283)	36.764*** (13.486)
best-response strategy	45.238*** (9.719)	62.441*** (7.153)	2.213 (9.366)	13.554 (7.997)
constant-price strategy	66.793*** (10.328)	64.818*** (10.043)	15.703 (10.303)	10.261 (9.714)
unassigned strategy	67.279*** (5.586)	63.714*** (7.644)	17.591** (8.342)	10.421 (10.069)
past earnings per round			15.790*** (2.326)	16.591*** (2.164)
p-values (Wald tests of equal coefficients)				
Ho: disproportionate = matching	0.354	0.053*	0.135	0.080*
Ho: disproportionate = best-response	0.041**	0.540	0.203	0.897
Ho: disproportionate = constant-price	0.980	0.683	0.717	0.662
Ho: disproportionate = unassigned	0.256	0.593	0.830	0.671
Ho: matching = best-response	0.000***	0.000***	0.006***	0.045**
Ho: matching = constant-price	0.002***	0.004***	0.043**	0.025**
Ho: matching = unassigned	0.000***	0.001***	0.052*	0.037**
Ho: best-response = constant-price	0.132	0.848	0.202	0.712
Ho: best-response = unassigned	0.047**	0.901	0.112	0.741
Ho: constant-price = unassigned	0.967	0.930	0.819	0.987

Notes: 204 observations in each regression. Dependent variable is PDV of earnings as measured in Singapore dollars ( $\text{S\$}1 \approx \text{US\$}0.70$ ). Robust standard errors, clustered on subjects, are reported in parentheses.

\*\*\* Significant at 1%.

\*\* Significant at 5%.

\* Significant at 10%.

One reason certain strategies could do better than others is not because they lead to higher earnings, but that higher past earnings could lead subjects to adopt a particular strategy and, at the same time, to higher future earnings. If this is the case, then strategy choices should no longer affect the PDV of earnings once past earnings are controlled for. Columns 3 and 4 repeat the estimation and tests in columns 1 and 2 once a control for past earning is added (i.e. the average of a subject’s past earnings per round). The estimates in columns 3 and 4 show that even controlling for past earnings, the PDV of earnings is significantly higher when subjects adopt matching strategies but this is not true for other assigned strategies. Moreover, qualitatively, the test results remain virtually unchanged after controlling for past earnings — broadly, they imply disproportionate punishment strategies do not lead to higher earnings than other strategies while matching strategies do (with the increase in earnings ranging from S\$20.93 or about US\$15 higher than disproportionate punishment strategies to S\$38.83 or about US\$27 higher than for best-response strategies). Thus, the evidence suggests individual subjects are not behaving suboptimally or irrationally by adopting strategies different from those normally prescribed in the literature.

When prices are above the one-shot Nash equilibrium, constant-price strategies which ignore any price cut and maintain a constant price regardless of what the rival did in the previous round, would seem to be particularly vulnerable to an opportunistic rival. However, this does not seem to be the case. As can be seen from Table 2, these strategies do not lead to significantly lower earnings compared with best-response strategies, disproportionate punishment strategies or unassigned strategies. This could be because these forgiving strategies may not extend beyond one round in case a subject undercuts them for more than one round, and the subsequent punishment may make up for the initial forgiving approach.<sup>19</sup> In the case of subjects that have already coordinated on monopoly prices, the purpose of doing so may be to avoid reacting to an accidental price cut of the rival. For example, in one case a subject confirmed in his message that he put the constant price of L\$23 for the strategy round in round 10 since he assumed any other price of the other subject “was probably a mistake”. In another case, the subject writes “Hey I put 22 for this round cos you put 22 for the last? I’ll assume its a typo cos you could have got more if u put 21”, and the other subject from the same market writes “sorry, last round I was too excited about the number 22, so accidently entered 22 for the price”. These two subjects maintained a price of L\$23 in all rounds from 2 to 46 except for the round in which one set the price of L\$22 and the subsequent round when the other set a price of L\$22 in response. More generally, forgiving strategies may only be maintained if the rival also adopts a strategy which is compatible with cooperation.<sup>20</sup>

<sup>19</sup>It is worth noting however that even in this case, such strategies are not equilibria in standard repeated game models, as the rival will always want to undercut for at least one round given there are no consequences from doing so.

<sup>20</sup>Subjects are less likely to use a constant-price strategy if their rival uses a best-response strategy. The estimated coefficient from a probit of the adoption of constant-price strategies on the adoption of best-response strategies by their rival (classified when  $\tau = 1$ ) and a constant is  $-0.452$  (standard error of 0.224) which is significant at the 5% level.

## 4.6 The role of communication

Given subjects in 25 out of the 100 markets were allowed to send a message to each other to be read before the subsequent round, one might expect cooperation to be easier for these subjects. A natural question to ask is whether this is because communication allows subjects to adopt certain types of strategies.

As with many earlier studies (e.g. see the survey in Haan *et al.*, 2006), I do find subjects set significantly higher prices when allowed to communicate. To show this I run two-group mean-comparison tests (allowing for unequal variances using Welch’s approximation) with the null hypothesis being that prices set by subjects where no communication is allowed and those where communication is allowed are equal. Figure 7 shows a 95% confidence interval around the difference in the mean prices of the two groups by round. Not surprisingly, there is no significant difference in the mean prices between the two groups of subjects in round 1 (when no communication is possible prior to prices being set), while from round 2 onwards prices are, on average, significantly higher for markets where communication is allowed. Only when the sample becomes small (from round 29, in which there are only 16 communicating subjects and 42 non-communicating subjects), does the difference become insignificant in some rounds. Indeed from round 47 onwards there are no active markets left with communicating subjects.

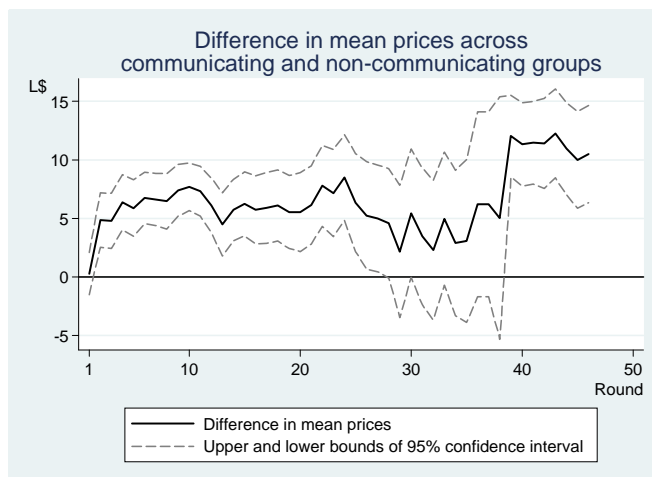


Figure 7: Prices are significantly higher with communication

The main role of communication seems to be to facilitate coordination on a high price. There is very little communication about subjects’ intentions to punish deviations in the 239 messages sent between subjects that are allowed to communicate. The majority of messages are either proposals to set higher prices (most commonly the monopoly price of L\$23), affirmations of the intention to stick to a particular high price, or general comments noting that their profits will be higher if they both set higher prices. Indeed, 110 of the 239 messages contain the number “23”. Threats of punishment in case high prices are not adopted are only mentioned in eight of the 239 messages, with explicit punishment threats detailed in five of these cases. In these cases, the stated threat is to set very low prices.

Consistent with these threats, subjects that could communicate were significantly more likely to adopt disproportionate punishment strategies. A probit of the adoption of disproportionate punishment strategies (assigned based on  $\tau = 0$ ) on whether messaging is allowed and a constant has a coefficient of 1.053 (standard error of 0.438) which is significant at the 5% level. Allowing messaging increases the probability of adopting disproportionate punishment strategies from 0.007 to 0.077. (The increase in probability is almost identical although only significant at the 10% level when strategies are assigned instead based on  $\tau = 1$ .) One interpretation of this result is that disproportionate punishment strategies are easier to adopt when subjects can communicate with each other about their intentions, suggesting traditional theories of punishment strategies may be more relevant for overt collusion (rather than tacit collusion) in which communication is possible.

## 5 Conclusion

The economic theory of cooperation in repeated games has largely focused on equilibrium strategies which involve disproportionate punishments, in which the same severe punishment applies regardless of the nature of the deviation. In this paper I designed an experiment to see the extent to which decision makers actually adopt such strategies. Rather, than trying to make inferences from prices alone, I provide direct evidence by also eliciting subjects' one-period ahead strategies.

The experimental evidence suggests there is minimal adoption of disproportionate punishment strategies. In the few cases where subjects do adopt such strategies, their rivals adopted different types of strategies. Thus, the evidence is inconsistent with the standard equilibrium theories used in applied works (i.e. based on Nash reversion or optimal symmetric two-phase punishment strategies). This is despite the fact that subjects faced a relatively simple environment in the experiment — they faced symmetric choices and payoffs with the monopoly price being the obvious cooperative outcome to coordinate on, there were no shocks during the experiment, and subjects faced no monitoring difficulties. In more realistic environments, such as those faced by competing firms in real world markets, coordinating on these types of equilibria would seem to be even more challenging. A possible exception is if decision makers can communicate extensively with each other, as could be the case in overt collusion cases involving cartels. Indeed, I find that disproportionate punishment strategies are significantly more likely to arise when subjects are allowed to communicate, suggesting traditional theories may be more relevant for overt collusion.

To date the literature has not provided any theory to explain why particular equilibrium strategies should be selected in repeated games other than that provided by optimality (i.e. to justify optimal punishment strategies). As Harrington (1991, p.1089) has noted “*it is quite natural to think of a punishment strategy as being an industry norm with respect to firm conduct ... Thus, even though the norm might not be the best in some sense (for example, it might not be a most severe punishment strategy), firms*

might choose to maintain it if it seems to work.” In the experimental setting I consider, price matching strategies seem to work the best. As discussed in the introduction, this finding is consistent with the evidence from some other studies based on tournament approaches which show that measure-for-measure type strategies do best. A possible explanation for this is that such strategies are less sensitive to accidental deviations or misunderstandings. When reaching (or returning to) the cooperative outcome is difficult, say due to limitations on communication, subjects may be better off using a softer punishment initially to avoid getting stuck at very bad outcomes. This suggests future research should be directed towards developing a theory in which the “best” strategy is not always based on the harshest possible punishment in the first round following a defection from a cooperative agreement.

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## 7 Appendix A: Instructions

The following are the main instructions that were read in the briefing session. Some minor administrative procedures relating to payment and emailing that were read out at the end are left out here for brevity.

### **Instructions for NUS Experiment**

Welcome to this briefing session. Please read the following instructions carefully together with me. Before explaining the detailed rules of the experiment, let me emphasize a few important points.

- It is important you remain quiet during this session. If you have a question, please raise your hand and somebody will answer your question discretely.
- This experiment will involve you receiving actual money. To receive this money you must complete the experiment. This means you must enter the required information in the given website by the stated deadlines. Failure to do so will likely involve removal from the experiment.
- This experiment does not involve us deceiving you in any way.
- You will be matched with one other participant throughout the experiment. Your identity will not be revealed to this or any other participant. Your responses will only be used in an anonymous way.
- These instructions and all other information you see (including the payoff table you have been given) are identical for all participants.

I will now explain the detailed rules of the experiment.

## Rules

Over the course of the coming weeks and months, you will be expected to make some decisions which will earn you real money. In this experiment, you represent a firm that sells a product or a service. We will call you a “seller”. You will have to decide what price to set. You will be anonymously matched with one other participant, who we will call the “other seller”. The other seller you are matched with will remain the same throughout the experiment.

In the first round (and most subsequent rounds) you will have to decide a price for the round. This involves logging into a website and entering a price between 1 and 25. Your payoff will be determined by your own price and that chosen by the other seller. All prices and payoffs will be in a fictitious currency, which we will call lab dollars or L\$ for short. Note 50 lab dollars equals 1 Singapore dollar.

There will be 3 such rounds per week. After 12 rounds (4 weeks), we will introduce a 5% chance each round that the experiment will end for you (as well as the other seller you are matched with). The amount of Singapore dollars we will pay you is determined by the sum of all the lab dollars you obtain in the experiment divided by 50. We will pay this to you after you have finished the experiment. An additional S\$10 will be paid to you in cash after this briefing session for your participation.

If you withdraw from the experiment or fail to meet one of the deadlines, this will also constitute the end of the experiment for you (as well as the other seller you are matched with). In this case you will not receive any payment from the experiment other than the participation fee of S\$10. The other affected participant will receive their earnings to date (calculated as above) plus an additional payment designed so that the other participant will neither be particularly advantaged or disadvantaged due to this event.

## Payoff table

Please now look at the payoff table you have been given (we have given you two copies). This payoff table is important. It shows you how to determine your payoff for a round. You should keep this payoff table and use it throughout the experiment. It will also be loaded on the website for your convenience.

Different rows correspond to different prices you might choose. Different columns correspond to different prices the other seller might choose. The intersection of a row and column tells you how much you will earn given your price and that of the other seller. The other seller is given exactly the same payoff table as you.

We will now go through a few example rounds so you can see how the experiment will work. Keep in mind this is a practice session, so your decisions here are purely for practice and will not be used in the actual experiment in any way. Moreover, the other seller you are matched to in the actual experiment will be different from the one you are matched to today.

## Example round 1 (Pricing round)

Now please go to the website [www.NUSexperiment.sg/experiment](http://www.NUSexperiment.sg/experiment) and login using your username and password. After considering the payoff table, please choose a price for round 1 as instructed on the screen. This should be a whole number between 1 and 25 inclusive. The participant you are randomly matched to will be doing the same

thing. In the actual experiment you will be free to change your mind and enter a different price up until the stated deadline.

After entering your price, please wait for further instructions (do not use the computer while you wait).

#### **Example round 2 (Pricing round)**

Round 1 is now closed. Click on “Refresh” in Internet Explorer.

On the screen you can see the price you chose in round 1, the price the other seller chose in round 1, and your earnings from round 1 in lab dollars. Check you can calculate your earnings from the two prices set by comparing with the relevant cell in the payoff table we have provided you.

Now, as instructed, enter the price you would like to set in round 2 in the given box. After entering your price, please wait until further instructions.

#### **Example round 3 (Strategy round)**

Round 2 is now closed. Click on “Refresh” in Internet Explorer.

Sometimes a round may differ in that instead of entering a single price, you enter a pricing strategy. We will call this a strategy round. This is just a different way for you to specify your price for the round. It does not differ in any other aspect.

Let me read from the screen which should appear in front of you.

This is a strategy round, which means you are not told what price the other seller set in the previous round.

The table below has 25 boxes. Above each box is a price the other seller might have set in the previous round (*i.e. in this case round 2*). In each case, enter the price you would like to set for this round. This, together with the price the other seller actually set in the previous round, will determine your actual price for this round.

For example, if the other seller had set a price of L\$1 in the previous round, your actual price for this round will be the price you enter in the box corresponding to L\$1. Likewise, if the other seller had set a price of L\$25 in the previous round, your actual price for this round will be the price you enter in the box corresponding to L\$25.

Please enter your strategy for this round now. You must fill in each of the 25 boxes with a whole number between 1 and 25. Use the tab key to shift between boxes. After entering your pricing strategy, please wait until further instructions.

#### **Round 4 (Pricing round)**

Round 3 is now closed. Click on “Refresh” in Internet Explorer.

In this example, round 4 is a pricing round, so you will observe the prices chosen by the other seller in the previous round. This is actually the price implied by his pricing strategy in round 3. For example, his price for round 3 is determined by applying his strategy to your round 2 price. Likewise, your price for round 3 is determined by applying your strategy to his round 2 price. Please check you can work out where your price in round 3 comes from.

### **Further rounds**

We will stop here (at round 4) for this practice session. In the actual experiment, further rounds would continue in the same way. They will either be pricing rounds or strategy rounds. When you enter the website for a new round you will see whether it is a pricing or a strategy round. It should now be clear that asking for your strategy is just another way of having you specify which price you would like to set for a round.

Remember also that after 12 rounds, we will introduce a 5% chance each round that the experiment will end for you (as well as the other seller you are matched with).

We will also email you to remind you when a round is near to ending if you have not already entered the required information. Remember, failure to complete a round by the stated deadline can result in your removal from the experiment with no payment. Do not leave it to the last minute. You should email us if you ever encounter any problems so we can rectify the problem quickly.

The deadlines for logging in to enter the required information will be kept standard. Unless otherwise specified, they will be every Tuesday, Thursday and Sunday at 8pm. However, you cannot login to a new round until the last round is finalized and a new round started. Typically this will be by no later than the next morning following the closure of each round. We will email you as soon as each round is started. (We will also give you an option to unsubscribe from this service if you wish).

### **Communication**

Unless otherwise specified, you are not to communicate with others about the experiment. Some selected pairs of participants will be allowed to send messages to each other in each of the rounds. If you are a selected participant, then from the first round onwards, the following extra information will appear on your screen.

“Clicking on the highlighted link will allow you to post a message to the other seller. This is optional. If you do so, your message will be observed by the other seller from the start of the subsequent round. Note any message that could be used by the other seller to infer your identify (either directly or indirectly) will be banned, as will any defamatory, obscene or abusive message. We will check all messages for such information before a subsequent round is started. Otherwise, you are free to send any text based message.”

As with entering prices or strategies, when sending a message you can change your entry up until the deadline for each round. In that case, only your final entry will be used for the subsequent round (and so seen by the other seller).

## 8 Appendix B: Payoff table

		The other seller's price																										
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25		
1	23.02	33.77	44.51	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45	45		
2	23.53	45.02	66.51	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88	88		
3	1.53	33.77	66	98.23	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129	129		
4	0	0	42.98	85.95	128.93	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168	168		
5	0	0	0	51.16	104.88	158.6	205	205	205	205	205	205	205	205	205	205	205	205	205	205	205	205	205	205	205	205		
6	0	0	0	0	58.33	122.79	187.26	240	240	240	240	240	240	240	240	240	240	240	240	240	240	240	240	240	240	240		
7	0	0	0	0	0	64.47	139.67	214.88	273	273	273	273	273	273	273	273	273	273	273	273	273	273	273	273	273	273		
8	0	0	0	0	0	0	69.58	155.53	241.49	304	304	304	304	304	304	304	304	304	304	304	304	304	304	304	304	304		
9	0	0	0	0	0	0	0	73.67	170.37	267.07	333	333	333	333	333	333	333	333	333	333	333	333	333	333	333	333		
10	0	0	0	0	0	0	0	0	76.74	184.19	291.63	360	360	360	360	360	360	360	360	360	360	360	360	360	360	360		
11	0	0	0	0	0	0	0	0	0	78.79	196.98	315.16	385	385	385	385	385	385	385	385	385	385	385	385	385	385		
12	0	0	0	0	0	0	0	0	0	0	79.81	208.74	337.67	408	408	408	408	408	408	408	408	408	408	408	408	408		
13	0	0	0	0	0	0	0	0	0	0	0	79.81	219.49	359.16	429	429	429	429	429	429	429	429	429	429	429	429		
14	0	0	0	0	0	0	0	0	0	0	0	0	78.79	229.21	379.63	448	448	448	448	448	448	448	448	448	448	448		
15	0	0	0	0	0	0	0	0	0	0	0	0	0	76.74	237.91	399.07	465	465	465	465	465	465	465	465	465	465		
16	0	0	0	0	0	0	0	0	0	0	0	0	0	0	73.67	245.58	417.49	480	480	480	480	480	480	480	480	480		
17	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	69.58	252.23	434.88	493	493	493	493	493	493	493	493		
18	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	64.47	257.86	451.26	504	504	504	504	504	504	504		
19	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	58.33	262.47	466.6	513	513	513	513	513	513		
20	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	51.16	266.05	480.93	520	520	520	520	520		
21	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	42.98	268.6	494.23	525	525	525	525		
22	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	33.77	270.14	506.51	528	528		
23	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	23.53	270.65	517.77	529	
24	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	12.28	270.14	528
25	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	268.6

Your price