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# Imperfect Tacit Collusion and Asymmetric Price Transmission

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## Abstract

We investigate the role tacit collusion plays in Asymmetric Price Transmission (APT), the tendency of prices to respond more rapidly to positive than to negative cost shocks. Using a laboratory experiment that isolates the effects of tacit collusion, we observe APT pricing behavior in markets with 3, 4, 6, and 10 sellers, but not in duopolies. Furthermore, we find that sellers accurately forecast others' prices, but nevertheless consistently set their own prices above the profit-maximizing response, particularly in the periods immediately following negative shocks. Overall, our findings support theories in which tacit collusion plays a central role in APT.

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The phenomenon of Asymmetric Price Transmission (APT), that is, that supplier prices rise quickly after positive input cost shocks, but fall relatively more slowly after similarly-sized negative cost shocks, has been repeatedly documented in the literature such that we can rightly describe it as a stylized fact.<sup>1</sup> However, while empirical evidence for the APT phenomenon is ample, identification of its causal forces is not settled. Many theoretical explanations have been proposed, but the empirical literature has yet to conclusively determine which of these are valid or are most influential.

Empirical studies of APT predominantly examine aggregate-level variables (e.g. inflation, concentration) proposed to be relevant in the theory literature. The focus on such variables occurs because firm-level determinants are either not directly observable, or are not adequately measurable in panel data form. This approach yields helpful correlations between such variables, but the effort to identify causal relationships has met with only limited success, most notably in the context of firm-level underpinnings of the phenomenon. While the search for accurate firm-level data should certainly be continued, and where discovered used to further inform our understanding of pricing behavior, experimental methods offer a comparative advantage: testing theories that involve variables which are unobservable in the field (e.g. agents' information sets) lie outside the reach of empirical methods;<sup>2</sup> if however these same variables can be controlled through experimental design, we can overcome this obstacle to testing theory.

A question of primary interest is whether tacit collusion drives APT-like pricing behavior.<sup>3</sup> The field data does not convincingly exclude the possibility that market competitors secretly communicate, given the strong legal and even criminal incentives for firms to conceal – or avoid engaging in – such activities.

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<sup>1</sup>See Section 1.1 for an overview of the evidence.

<sup>2</sup>Meyer and von Cramon-Taubadel (2004) and Frey and Manera (2007) provide extensive discussions of methodological issues in econometric tests of APT.

<sup>3</sup>In this paper we will use the term “tacit collusion” to mean the phenomenon in which suppliers coordinate on prices above the competitive equilibrium level, through the channel of publicly visible pricing alone. Tacit collusion can also take the form of coordination on quantities below competitive equilibrium levels, but in this paper we will focus strictly on the role of coordination on prices.

This provides an obvious challenge for identification and motivates turning to the controlled setting of the laboratory, where we can directly observe competitor behavior and credibly prevent communication between sellers.<sup>4</sup>

An argument put forth by Borenstein et al. (1997) is that a variation of the “trigger price” model of oligopolistic coordination, originally introduced by Green and Porter (1984), may explain the emergence of APT-type dynamics through tacit collusion. In their model, when positive shocks occur firms immediately raise prices in order to preserve profit margins; however, when negative shocks occur firms react adaptively, holding prices at pre-shock levels until they see convincing evidence that a rival has cut their prices. Rapidly lowering prices in response to a downward cost shock could be perceived as defection from a mutually beneficial regime of tacit collusion, thus inviting retaliation from other firms. In contrast, rapidly raising prices in response to an upward cost shock poses no such threat to one’s competitors, and therefore incurs no corresponding risk of retaliation. Although their arguments are sound, and consistent with a deep empirical literature finding correlations suggestive of tacit collusion, Borenstein et al. (1997) conclude that they are unable to conclusively draw support for this hypothesis from their data. As no other empirical study of which we are aware has accomplished this either, we thus find motivation to turn to the laboratory to examine the role of tacit collusion in driving APT dynamics.<sup>5</sup>

A second question of interest is whether the number of competing sellers in a given market plays a significant role in the realization of the APT phenomenon. Notably, in his broad study of U.S. wholesale and retail markets, Peltzman (2000) finds a negative relation between the number of competitors in a market and the magnitude of APT observed. As with any empirical study,

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<sup>4</sup>Furthermore, the laboratory may be the only environment in which we can reliably detect collusion, since the non-collusive prices or profits are unavailable without imposition of strong structural assumptions.

<sup>5</sup>There are some studies that regress the estimated asymmetry with measures of market concentration as Loy et al. (2016). Counter-intuitively, the authors find that asymmetry decreases with higher concentration in German milk market. However, it is difficult to associate this estimate with the causal impact of collusion on APT as higher concentration index may stem from higher efficiency or product differentiation rather than conduct.

however, this study does not exclude the possibility that explicit (but unobserved) communication between firms lies behind this result. Several (non-APT focused) studies of experimental oligopoly markets find that there is an inverse relation between the number of sellers in a market and the size of deviations from the Nash equilibrium (NE) outcomes (for example, see Huck et al. (2004), Dufwenberg and Gneezy (2000), and Fonseca and Normann (2012)). However, we are unaware of any experimental study that specifically studies the role of the number of sellers in driving the APT phenomenon. We therefore incorporate the number of sellers in our markets as a treatment variable in our experimental design.

To our knowledge, Bayer and Ke (2018) is the only experimental study that directly targets the topic of APT. The authors' study employs a Bertrand duopoly setting in which sellers' costs either increase, decrease or stay constant at the halfway point of the experiment. With two extensions of this baseline condition, they further test the impacts of search costs and asymmetric information on APT. They find APT across all treatments, even in the absence of search and information frictions. They argue that the asymmetry can be explained with a backward-looking learning model: If a seller fails (manages) to sell the good in the period prior to the shock, it is more (less) likely that she will adjust her price downwards (upwards) in the following period. The authors' results support this regularity when the shock is negative, but not when it is positive. Hence, although this learning model may account for the downward rigidity, it falls short of explaining the asymmetry.<sup>6</sup>

While Bayer and Ke (2018)'s study provides a useful benchmark to our own, our design choices differ substantially from theirs, as we pursue different research questions. Whereas we aim to assess the roles of cooperative behavior and tacit collusion on pricing asymmetries, they deliberately try to attenuate

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<sup>6</sup>Bayer and Ke (2018) also reason that following positive cost shocks sellers will reason that other sellers will all immediately raise their prices, and so they do the same, while following a negative cost shock sellers do not see any reason to cut their prices unless and until they subsequently lose sales. They cite factors such as bounded rationality as explanations for this behavior, but do not offer a more precise explanation of the channels through which the observed behavior emerges.

their impacts to isolate the role of learning.<sup>7</sup> In particular, in their experiment sellers whose stores are not visited by a buyer receive only limited information on the market price, due to the feedback structure. In our experiment, we inform sellers of the average market price of the other sellers, as we want to create the conditions in which price signalling can be studied more explicitly.

In our experimental setting, subjects play the role of sellers and a computer plays the role of buyers. Each seller faces demand that linearly decreases with one's own price and linearly increases with the average price of others. We vary the size of groups across sessions as 2, 3, 4, 6, and 10, while calibrating the demand function to hold the best-response functions of each seller identical, across all group sizes. This approach allows us to isolate and study the impact of group size on the realization of APT through the coordination channel. Throughout our experiment, sellers experience a series of input price shocks – either large or small – that shift the NE price either up or down. Through this design, we are able (i) to test whether APT emerges despite the absence of market frictions and information asymmetries that are often theorized to be the causal forces behind pricing asymmetries; and, (ii) if APT does occur, to assess the impact of number of sellers on the magnitude of the resulting asymmetries. To our knowledge, ours is the first experiment that study the role of number of sellers in shaping APT.

Our contributions to the literature are two-fold: First, we document the prevalence of the APT phenomenon through experiments in which we possess strict control over the environment. In particular, our results indicate that the APT may emerge even in the absence of market frictions and information asymmetries that are often theorized to be the causes of pricing asymmetries.

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<sup>7</sup>Although Bayer and Ke (2018) exert effort to minimize the role played by tacit collusion with their study, their typed-stranger matching protocol significantly reduces but does not completely eliminate the possibility that subjects might repeatedly interact, and thus have the opportunity to establish reputation over time. By contrast, the perfect-stranger matching protocol, in which a subject is assured they will be matched with another only once in a session, does eliminate this possibility. Moreover, the duopoly setting of their study makes collusion presumably more reachable, since coordination is easier when there is only one other market participant. As a result, it is hard to assess the extent of the role to which cooperative behavior played in their study.

This suggests that in markets with three or more sellers, the presence of agents who attempt to coordinate on prices via price signaling may suffice for APT pricing dynamics to emerge. In our duopoly markets, however, our results suggest that coordination on prices can be so successful that rather than the APT phenomenon, persistent pricing at near-monopoly levels may instead emerge. Second, by calibrating demand based on the number of sellers in a *ceteris paribus* manner, we are able to isolate and perform hypothesis tests on the effects of increased group size on APT. For markets with three or more sellers, we find no significant difference in either the magnitude of observed APT, or the rate at which post-shock price behavior converges to NE-implied prices. Together, the results of our study support theories that highlight the role of tacit collusion on APT. We conclude that APT may be the product environments in which collusion is significant, but imperfect.

## 1 Related Literature

### 1.1 Field Evidence

Bacon (1991) provides an early empirical study suggesting that retail gasoline prices in the United Kingdom experience faster and more concentrated responses to crude oil price increases, than they do to similar crude oil price decreases. Bacon termed this phenomenon “Rockets and Feathers,” and since this paper was published dozens of other researchers have detected the presence of this sort of asymmetry in a variety of consumer and intermediate goods markets.

Peltzman (2000) provides one of the most comprehensive empirical examinations of APT. He conducts a broad study of pricing behavior of 77 consumer and 165 producer goods markets in the U.S., and he concludes that in more than two-thirds of these markets prices rise faster than they fall, in response to input cost changes. Peltzman also seeks correlations between various features of markets and industries, and the degree to which evidence of APT is present. Most notably, he finds that markets with fewer competitors tend to exhibit

more pricing asymmetry, while on the other hand markets with higher levels of concentration tend to be less likely to exhibit pricing asymmetry, as in Loy et al. (2016). Peltzman’s study, however, does not provide an explanation for these correlations.

In an early survey of field evidence, Meyer and von Cramon-Taubadel (2004) find that (excluding Peltzman (2000)’s study), symmetry in price response is rejected in almost one-half of all cases in the literature. Their survey also shows that different test methods yield highly varying rejection rates (between 6% and 80%). Frey and Manera (2007) and Perdiguero-García (2013) provide meta-regression analyses with more comprehensive and recent data sets. Both studies confirm that APT is very likely to occur but also emphasize the variation of reported outcomes. Their results show that this heterogeneity can be explained with several factors as characteristics of data (e.g., data frequency) and of the employed econometric model. Most notably, Perdiguero-García (2013) reports that the asymmetry tends to decrease in more competitive segments of the industry.

## 1.2 Theoretical Explanations

There is a growing body of literature on the theoretical accounts of APT, an unsurprising fact given that pricing asymmetries are not predicted by standard price competition models.<sup>8</sup> These studies propose explanations of APT mainly by introducing market frictions, information asymmetries or boundedly

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<sup>8</sup>A notable exception is the case of Markov-perfect equilibria, and in particular the case of the Edgeworth cycle. In this phenomenon, firms undercut each others’ prices successively until prices approach marginal cost; at this point, one of the firms decides with some positive probability to spike its price, and once this occurs the cycle is repeated, yielding each firm positive economic profits. Maskin and Tirole (1988) further show that these cycles provide a case where asymmetric pricing can be sustained in equilibrium. However, the Edgeworth cycle model requires that firms make price decisions alternately; the model does not support an equilibrium when price decisions are made simultaneously or continuously. Moreover, the emergence of the phenomenon seems in practice to be limited to environments in which competitors rapidly and publicly change prices (see for example Byrne and De Roos (2019) for an interesting case in Perth, Australia petrol markets, in which a government mandate for retail suppliers to publish their prices daily seems to have facilitated the emergence of a weekly cycle of Edgeworth-like pricing dynamics that persisted for many years.). The Edgeworth cycle model therefore applies to a relatively narrow range of market contexts.



rational agents into the underlying models. One reason there is such a variety in the way different studies explain the APT is because these studies typically focus on specific market structures (e.g., wholesale petroleum markets) and their idiosyncrasies. In this subsection, we review some of these studies in an attempt to categorize as well as to highlight discrepancies.<sup>9</sup>

Borenstein et al. (1997) consider the role of search costs in facilitating APT. They hypothesize that negative cost shocks in the presence of costly search provide firms temporary pricing power, which they then use to delay reductions in prices, yielding temporarily superior profits. Benabou and Gertner (1993) and Yang and Ye (2008) also develop explanations based on consumer search costs, but also on the volatility of input costs. They reason that volatility should reduce search incentives for consumers; producers, realizing that short-term demand elasticity is increased as a result, thereby yielding them temporarily increased pricing power, respond by reducing prices more slowly. Reagan and Weitzman (1982) and Borenstein and Shepard (1996) propose explanations based on inventory costs, reasoning that it is relatively more costly for manufacturers and suppliers facing capacity constraints or sharply rising short-term production costs to deal with unanticipated increases in demand resulting from price drops, than it is to respond to corresponding drops in demand due to price increases. Ball and Mankiw (1994) consider a menu-cost model in conjunction with positive trend inflation as an explanation of APT. In another study, Ahrens et al. (2017) show that the presence of consumers with loss aversion may explain why prices are more sluggish to adjust downwards than upwards in response to permanent demand shocks.

The various explanations and models described above provide differing implications for government policy: if APT occurs due to collusion, there may be room for regulation to improve economic efficiency; if however APT is primarily caused by the presence of inventory costs, asymmetric menu costs, or search costs, then regulation that controls pricing behavior may actually induce inefficiency rather than attenuate it. Given the robust evidence of

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<sup>9</sup>For more exhaustive surveys of theoretical explanations, see Meyer and von Cramon-Taubadel (2004) and Brown and Yucel (2000).

the widespread existence of APT and its non-trivial magnitude and impact on consumer outcomes, identifying which theories describe the asymmetric pricing behavior is key to informing effective public policy.

### 1.3 APT and Experiments

Despite the many possible explanations that have been proposed, the empirical literature yields only mixed evidence that is often inconclusive due to identification issues. This suggests there is room for further research to shed light on the phenomenon. We consider the advantages of experimental methods in isolating and studying causal determinants of APT.<sup>10</sup> In this subsection, we summarize the most relevant literature to our study.

There are two studies of which we are aware – in addition to Bayer and Ke (2018) – that conduct market experiments with APT-related results. Deck and Wilson (2008) investigate gasoline markets and find that retail prices adjust asymmetrically to changes in station costs in zones with clustered stations, but not in zones with stations that are relatively isolated from competitors. Cason and Friedman (2002) find weak evidence of APT in posted offer markets where customers incur switching costs. While these studies examine their findings on APT, their experimental designs are optimized to investigate questions regarding the structure of gasoline markets (e.g., zone pricing, divorcement) and of consumer markets (e.g., switching costs), not to identify causes of APT. In particular, sellers’ costs in both experiments follow random-walk shocks, which may not be salient enough to detect APT. Our study distinguishes itself from this string of literature by examining APT with larger, persistent shocks.<sup>11</sup>

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<sup>10</sup>The usage of experimental methods in macroeconomic research is becoming more and more prevalent. See Duffy (2016) and Cornand and Heinemann (2019) for recent surveys.

<sup>11</sup>Fehr and Tyran (2001) also employ large positive and negative shocks and report APT-like behavior in a price-setting game. However, the authors do not analyze the phenomenon, nor do they probe its implications. In another related experimental study, Duersch and Eife (2019) consider Bertrand duopolies with zero marginal cost in either inflationary, deflationary or constant price environments. They find that real prices are significantly lower in the inflationary environment compared to non-inflationary environments.

Apart from studies that directly target APT, price competition experiments that study the impact of group size on tacit collusion are also relevant to the current paper. Dufwenberg and Gneezy (2000) provide an early evidence for such a relation through an oligopoly game that corresponds to a discrete version of the Bertrand model. They find that winning prices tend to converge to NE levels in groups of three or four competitors, but stay consistently high in duopolies. Morgan et al. (2006) find that increasing the number of sellers from 2 to 4 decreases the prices paid by some consumers (the ones informed about the entire distribution of prices) but not for others (the ones who buy with motives other than prices). Abbink and Brandts (2008) also find that there is a negative relationship between the number of competing firms and price levels.<sup>12</sup> Nevertheless, as in Dufwenberg and Gneezy (2000), they find that collusive pricing is the modal outcome in duopolies. Fonseca and Normann (2012), Orzen (2008), Davis (2009) and Horstmann et al. (2018) provide further evidence that collusive prices are very likely to be observed in duopolies. Average prices approach considerably close to the NE in the baseline condition of these studies (fixed matching, no communication, symmetric sellers etc.) when the number of sellers is 3 or greater.

The main conclusion of these studies is that persistent coordination over collusive prices is unlikely in markets other than duopolies. This, however, does not preclude the possibility that players might manage to coordinate temporarily on high prices, following negative shocks. Experiments also indicate that increasing the number of sellers often leads to more competitive outcomes (in terms of price and output), which in turn should make APT less likely. Although, the meta-analyses of Fiala and Suetens (2017) and Horstmann et al. (2018) on oligopoly experiments indicate that there may not be a linear relationship between the number of competing firms and the degree of tacit collusion. Horstmann et al. (2018) argue that this result may stem from the relatively small number of studies that provide pairwise comparisons and the lack of statistical power in these studies. Our study contributes to the litera-

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<sup>12</sup>Their results are particularly interesting since in their price competition setting, there exist multiple equilibria.

ture through improvements of these axis.

## 2 Method

### 2.1 Pricing Game

We develop a variant of classical price competition related to the “Linear city” model of Hotelling (1929) and the “Circular city” model of Salop (1979), and employ this in our experimental environment. In this setting, the demand facing seller  $i \in N$  in period  $t \in T$  is equal to

$$q_{i,t}(p_{i,t}, p_{-i,t}; \delta, \gamma) = \begin{cases} \delta - \gamma(p_{i,t} - p_{-i,t}), & p_{i,t} \in [p^{min}, p^{max}] \\ 0, & \text{otherwise} \end{cases} \quad (1)$$

where  $\delta$  and  $\gamma$  are parameters of demand,  $p_{i,t}$  is the price set by seller  $i$  and  $p_{-i,t}$  is the average price chosen by the rival sellers in the same market (i.e.  $p_{-i,t} \equiv \frac{1}{N-1} \sum_{j \neq i} p_{j,t}$ ) at period  $t$ .  $p^{min}$  represents the price floor and  $p^{max}$  is the representative consumer’s valuation of the good.<sup>13,14</sup> Given the own-demand specification in (1), seller profits are calculated as

$$\pi_{i,t} = (p_{i,t} - mc_t)q_{i,t} - f, \quad (2)$$

where  $q_{i,t}$  is quantity demanded from seller  $i$  as defined in (1),  $mc_t$  is marginal cost that shifts every  $\bar{T}$  periods that comprise a round (denoted  $r \in R$ ) and

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<sup>13</sup>This reduced form demand function can be represented by either Hotelling’s or Salop’s address models for  $N = 2$ , and by Salop’s model for  $N = 3$ , by setting  $\delta = \frac{L}{N}$  and  $\gamma = \frac{L}{c} \frac{(N-1)}{N}$ . Here,  $L$  corresponds to the number consumers that are equidistantly located along a linear (circular) city, and  $c$  is their travel cost. For  $N \geq 4$  we depart from Salop’s model in two ways. First, we do not assume that sellers directly compete only with the two adjacent sellers along the circle. We instead assume symmetric cross-price demand elasticities for all market participants, regardless of their “address”. Second, we calibrate the values of parameters  $L$  and  $c$  by group size  $N$  in such a way that  $\delta$  and  $\gamma$  remain constant for all markets. The former makes the strategic space easier for subjects to understand and the latter allows us to make *ceteris paribus* comparison across treatment groups.

<sup>14</sup>Note that a linear specification of demand is the direct result of an assumption of quadratic utility (see Amir et al. (2017) for a proof).

$f$  is fixed cost. Sellers set their prices in each period simultaneously from a discrete set that is bounded as  $p_{i,t} \in [mc_t, p^{max}]$ , such that the price floor is equal to the marginal cost of that round.

In the described game, there is a unique symmetric stage-game NE which can be retrieved from the first-order condition of the profit-maximization problem. This NE corresponds to the unique subgame-perfect Nash equilibrium (SPNE) of the finitely-repeated game by backward induction. In this NE, all sellers set their prices equal to

$$p_t^{NE} = mc_t + \frac{\delta}{\gamma}, \quad (3)$$

with each seller achieving (current-period) profits of  $\pi^{NE} = \frac{\delta^2}{\gamma} - f$ . Sellers may achieve the joint profit maximum (JPM) if they each set their prices to the maximum price  $p^{max}$ . In addition to the NE price, the maximum price constitutes a second likely focal point upon which sellers may attempt to coordinate.

The best-response function derived from the first-order condition can be expressed as:

$$p_{i,t}^{BR} = \frac{1}{2} \left( mc_t + \frac{\delta}{\gamma} + \mathbb{E}_{i,t-1}[p_{-i,t}] \right) \quad (4)$$

where  $p_{i,t}^{BR}$  and  $\mathbb{E}_{i,t-1}[p_{-i,t}]$  represent the best-response action and the conditional expectation of seller  $i$  for the average price of others, respectively.

In this pricing game, neither own-demand nor own-profit depend on the number of sellers. These only depend on own-price and the average price of rival sellers. The best-response action is also independent of  $N$  for a wide range of expectation models, including rational expectations. This feature assures that the incentives given to the sellers of different group sizes are matched and the market power of each seller is *ex-ante* equal. We consider this as necessary for ensuring a *ceteris paribus* comparison between the treatment conditions.

Table 1: Experimental design parameters

General parameters	
Number of periods per round	$\bar{T} = 15$
Number of rounds per session	$R = 5$
Demand parameters	$\delta = 8.50, \gamma = 7.275$
Fixed cost	$f = 1$
Maximal/reservation price	$p^{max} = 3$
Varying parameters	
Group size across treatments	$N \in \{2, 3, 4, 6, 10\}$
Marginal cost across rounds	$mc : (0.90, 0.50, 1.30, 0.50, 0.90)$
Cost shock sequence	$\Delta mc \equiv \eta : (-0.40, +0.80, -0.80, +0.40)$
NE price across rounds	$p^{NE} : (2.07, 1.67, 2.47, 1.67, 2.07)$

## 2.2 Experimental Design

Sellers interact repeatedly in the described pricing game for  $R$  rounds, which are each composed of  $\bar{T}$  periods. Marginal cost  $mc_t$  fluctuates at the beginning of each round, modeling large exogenous cost shocks, but remains invariant throughout the remainder of each round. Our experimental manipulations consist of varying the size of markets across sessions in a between-subjects design, and of varying the size and direction of shocks across rounds in a within-subjects design. We implement a fixed-matching protocol during a session.

The calibration of the experimental game is summarized in Table 1. The experiment consists of 5 rounds of 15 periods each, with a new marginal cost announced at the beginning of each round. The sequence of shocks is identical across all treatments: Marginal cost starts at \$0.90 in Round 1, drops to \$0.50 in Round 2, rises to \$1.30 in Round 3, falls again to \$0.50 in Round 4, then rises to \$0.90 for Round 5.

## 2.3 Procedures

Experimental sessions were conducted at the University of California, Santa Barbara’s Experimental and Behavioral Economics Laboratory (EBEL) using

the z-Tree platform (Fischbacher, 2007), between September and December of 2018. A total of 245 subjects were recruited from the experimental economics subject pool of the same university, using the ORSEE tool (Greiner, 2015). Subjects were allocated to markets of size 2, 3, 4, 6 and 10, with a total of 36, 39, 52, 48 and 70 subjects assigned to each group size condition, respectively. This setup yields 59 independent markets for the analysis.<sup>15</sup>

At the beginning of each experiment, subjects are provided written instructions which are also read to them aloud by an experimenter. Subjects then proceed to take a short comprehension quiz.<sup>16</sup> In the main part of the experiment, each subject plays the role of sellers and makes a series of 75 pricing decisions, whereas consumer behavior is simulated by computer. We also elicit subjects' one-period-ahead expectations about the average price chosen by rival sellers (i.e.,  $\mathbb{E}_{t-1}[p_{-i,t}]$ ). These expectations are not rewarded separately, to avoid creating hedging issues. Subjects are able to set a price between the marginal cost and the maximum price (of \$3.00), in increments of \$0.01. Once all subjects set their prices and expectations, they are individually notified by the computer of the average price established by the others in their market, reminded of their own price, and shown their own resulting payoff for that period. Subjects are able to track the previous values of these outcomes through a history box that is available in their screen (see Online Appendix 1.4).

We notify subjects that a new cost shock will occur at the beginning of each new round, either an increase or decrease, of either \$0.40 or \$0.80. We reveal the magnitude and direction of each shock immediately prior to the first period of each respective round. At that time, we also hand out copies of a printed payoff table corresponding to the new marginal cost. These tables assist subjects in estimating the profits they will receive, conditional on the hypothetical prices they and others may set in each period of that round (see Online Appendix 1.3).

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<sup>15</sup>In one session (20 subjects), the data from the final period (of 60 periods) is lost due to technical reasons. All the analysis in the results section is performed based on all the available data.

<sup>16</sup>We reviewed answers for each subject and provided explanations where needed. See Online Appendix 1 for all the experimental material.

Sessions lasted a total of 90 to 125 minutes. Subjects were paid \$18.66 on average (a minimum of \$10.89 and a maximum of \$28.50), which includes the \$5.00 show-up fee and \$3.00 for the completion of the optional survey (no subject declined this offer). The remaining payoff is determined as the average payoff of a randomly chosen round of the game.

### 3 Hypothesis

This experimental setup allows us to test the following hypotheses:

**Hypothesis 1:** *Prices respond symmetrically to (equally sized) positive and negative shocks.*

As the experimental design specifically avoids any of the features outlined in Section 1.2 (e.g., frictions, information asymmetries), theory suggests prices should react symmetrically. This can further be verified by reviewing the linearity of best-response function with respect to its arguments. In a directed counter-hypothesis, we predict prices to react asymmetrically to shocks. In particular, we expect downward rigidity in line with the arguments of Borenstein et al. (1997). We can test this hypothesis by exploiting the exogenous within-subjects treatment variations in marginal cost.

Our second hypothesis concerns tacit collusion and coordination:

**Hypothesis 2:** *Sellers' market power is invariant to the number of sellers in their market, and is unaffected by the existence of periodic shocks.*

Market power, that is the ability of sellers to raise prices over marginal cost, should be invariant to the number of rival sellers, given that we have calibrated demand in such a way that both the profit and best-response functions are independent of group size. Moreover, in the absence of frictions and the ability of competitors to communicate, the theory predicts a constant markup for all levels of marginal cost. However, if tacit collusion occurs, we expect to observe



higher market power (i) in smaller markets, and (ii) in the periods occurring soon after shocks. For (i), we expect to observe effective coordination more often in smaller markets, where there are fewer sellers to dampen the strength of price signals. For (ii), we expect that shocks may boost the market power of sellers (at least temporarily), as such shocks may play the role of a coordination device. We can test this hypothesis by using the between-subjects treatment variations in group size, and within-subject treatment variations in marginal cost.

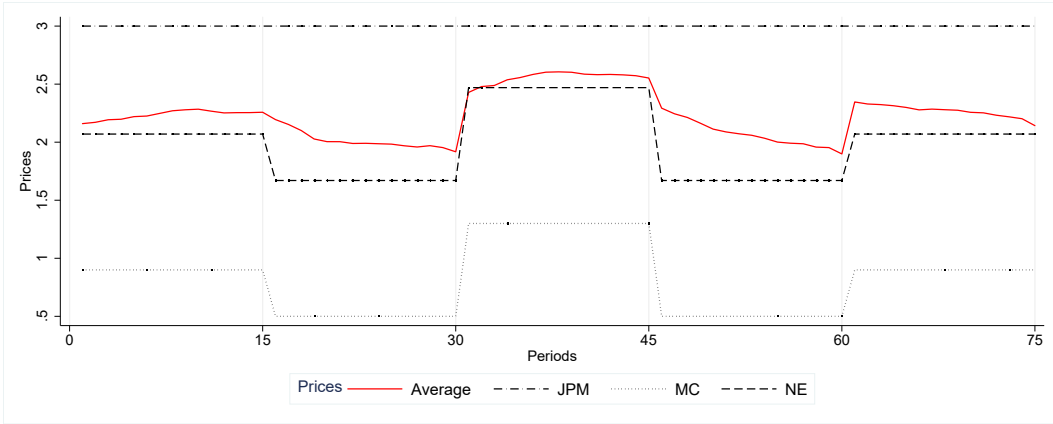
Finally, our third hypothesis concerns individual pricing strategies:

**Hypothesis 3:** *Conditional on expectations, pricing behavior follows the best-response function.*

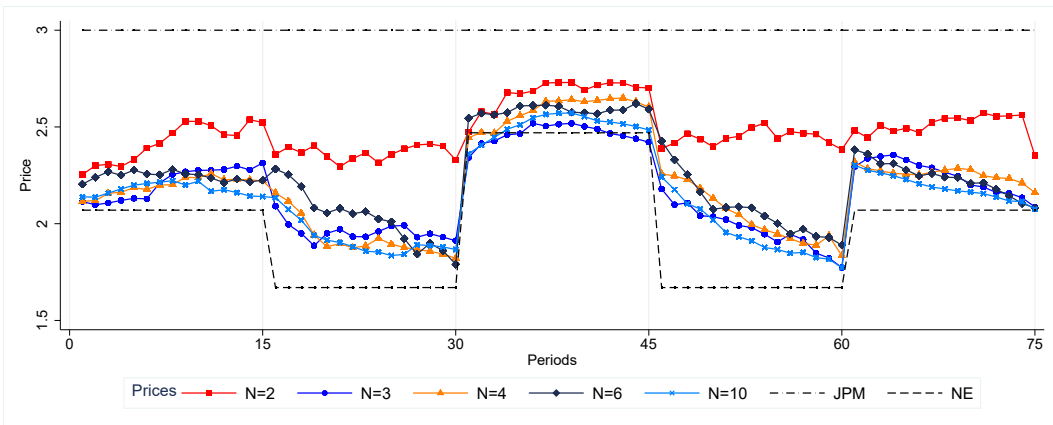
This hypothesis is built on the following rationale: The Rational Expectations Hypothesis (REH) of Muth (1961) admits the possibility of expectation errors at the individual level, but which should tend to cancel out in aggregate. Also, after observing  $t - 1$  periods of price history, a seller may learn that the others do not behave consistently with the predictions of REH. Nevertheless, conditional on expectations, sellers should select the best-response action as this maximizes their profit. As we elicit subjects' guesses on the average price set by others, we can test this hypothesis without assuming a specific expectation model.

## 4 Results

Figure 1 provides a depiction of the average price per period, as the average of all market prices and as broken out by group size. Here, market price refers to the average of all prices in market  $m$  (i.e.,  $p_{m,t} = \frac{1}{N} \sum_{i=1}^N p_{i,t}$ ). The reader can readily discern that for groups of size 3 and greater, average prices rise rapidly after positive cost shocks, while they fall more slowly after negative cost shocks. By contrast, for groups of size 2, it is not immediately obvious whether average pricing behavior is affected by cost shocks. A second observation



(a) Average of all market prices



(b) Average market prices by group size

Figure 1: Average pricing behavior across periods and group sizes.

that is immediately clear is that average prices are generally above the NE price, with deviations being higher following negative shocks compared to the positive ones. Overall, the visual inspection of the data suggests the presence of market behavior consistent with APT.

#### 4.1 Estimation of Asymmetry

We follow Peltzman (2000) and estimate the coefficients of the distributed lag model (DLM) to measure the magnitude of APT. This model can be ex-

pressed as:

$$\Delta p_{i,t} = \sum_{k=0}^K b_{t-k} \cdot \Delta mc_{t-k} + \sum_{k=0}^K c_{t-k} \cdot (\mathbb{1}[\Delta mc_{t-k} > 0] \cdot \Delta mc_{t-k}) + \epsilon_{i,t} \quad (5)$$

where the change in output price (i.e.,  $\Delta p_{i,t} = p_{i,t} - p_{i,t-1}$ ) is modelled as a function of the lagged changes in marginal cost (i.e.,  $\Delta mc_{t-k}$ ). The indicator variable  $\mathbb{1}[\Delta mc_{t-k} > 0]$  takes the value 1 if the change in marginal cost in period  $t - k$  is positive and equal to 0 otherwise. The sum of interaction coefficients  $\sum_{k=0}^K c_{t-k}$  reflects the magnitude of asymmetry and its persistence over  $K$  periods.

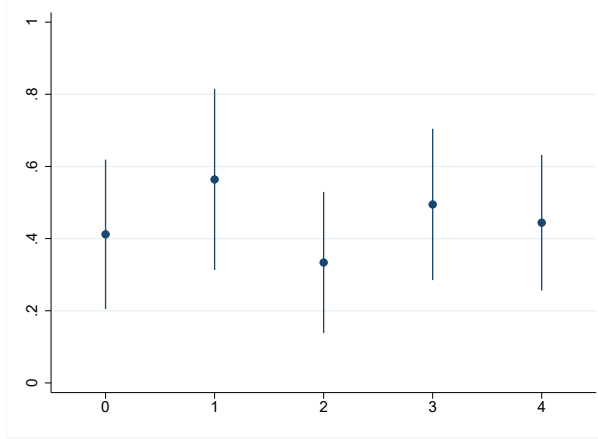


Figure 2: Cumulative response after  $K$  periods. Dots refer to  $\sum_{k=0}^K c_{t-k}$ . Lines represent 95% confidence intervals.

We estimate model (5) with Ordinary Least Squares (OLS) regressions in a step-wise manner. Figure 2 reports the estimated asymmetry for  $K = 4$ .<sup>17</sup> Estimates indicate that the APT is both strong and persistent. Immediate price

<sup>17</sup>We report the full set of results in Online Appendix 2.1. All estimations employ robust standard errors that are clustered at market level. We also include a set of indicator variables that are specific to each group size (i.e.,  $\mathbb{1}[N = s]$ ), the lagged change in the average price of rival sellers (i.e.,  $\Delta p_{-i,t-1}$ ), a three-way interaction term (between  $\mathbb{1}[\Delta mc_{t-k} > 0]$ ,  $\Delta mc_{t-k}$  and group size specific indicator variables) and autoregressive terms amongst the set of regressors to check the robustness of estimates. The significance of asymmetry coefficients as well as their magnitude are robust to the inclusion of these variables.

reactions are 32.9 cents greater in magnitude for positive than for negative 80-cent shocks.

We now assess the reaction of prices to equally sized shocks between our treatment groups with non-parametric tests. We compare immediate pass-through rates of shocks that are  $\beta_0^+$  and  $\beta_0^-$  calculated as:

$$\begin{aligned} p_{i,t+\tau}^+ &= p_{i,t-1}^+ + \beta_\tau^+ \eta^+ \\ p_{i,t+\tau}^- &= p_{i,t-1}^- + \beta_\tau^- \eta^- \end{aligned} \tag{6}$$

where  $\eta^+$  ( $\eta^-$ ) reflects either the large or small positive (negative) shock and  $t - 1$  corresponds to the period just before the shock. Note that the demand function we describe in (1) is perfectly inelastic, so that shifts in the NE price following cost shocks are exactly equal to the magnitude of the cost shock itself. In accordance, we can denote the cases  $\beta_0^+ = 1$  and/or  $\beta_0^- = 1$  as incidences of “full pass-through” of cost shocks. The ratio of  $\beta_0^+$  and  $\beta_0^-$  thus conveys information on the degree of APT in immediate cost-shock responses. A ratio of 1 would indicate the absence of APT.

Table 2 provides average value of pass-through rates for different aggregation levels. First, we note that the hypothesis of full pass-through can generally be rejected.<sup>18</sup> Second, we test APT in the immediate post-shock responses by testing the equality of immediate pass-through rates for equally sized shocks as  $H_0 : \beta_0^+ = \beta_0^-$  via Wilcoxon signed-rank tests. The pooled data and the data for groups of size greater than 2 suggest rejecting the null. For groups of size 3, we reject symmetry for the smaller but not for the larger shock. For duopolies, we see that the asymmetry is reversed; the average price response following the larger cost shock is significantly greater for the negative than for the positive cost shock. We do not attempt to reconcile this puzzling result, but simply note that the data for duopolies do not suggest pricing behavior in line with the “Rockets and Feathers” phenomenon. Taken together with the estimates of the DLM, we reach the first two results of our paper:

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<sup>18</sup>Exceptions consist of the small positive shock (i.e.,  $\eta^+ = 0.40$ ) and  $N = 6$  for the large positive shock.

Table 2: Asymmetry in the immediate pass-through rates

	All	$N > 2$	$N = 2$	$N = 3$	$N = 4$	$N = 6$	$N = 10$
Small shocks							
$\beta_0^-$	0.159 (0.0676)	0.115 (0.0739)	0.411 (0.162)	0.558 (0.247)	0.158 (0.138)	-0.144 (0.130)	0.0154 (0.0985)
$\beta_0^+$	1.119 (0.0672)	1.270 (0.0659)	0.244 (0.196)	1.305 (0.172)	1.209 (0.121)	1.233 (0.121)	1.322 (0.123)
$p$ -value	0.000	0.000	0.411	0.028	0.000	0.000	0.000
Large shocks							
$\beta_0^-$	0.324 (0.0362)	0.313 (0.0405)	0.391 (0.0734)	0.303 (0.107)	0.432 (0.0811)	0.206 (0.0706)	0.304 (0.0712)
$\beta_0^+$	0.639 (0.0396)	0.718 (0.0400)	0.181 (0.111)	0.537 (0.116)	0.779 (0.0636)	0.945 (0.0796)	0.619 (0.0643)
$p$ -value	0.000	0.000	0.035	0.202	0.013	0.000	0.006
Observations	245	209	36	39	52	48	70

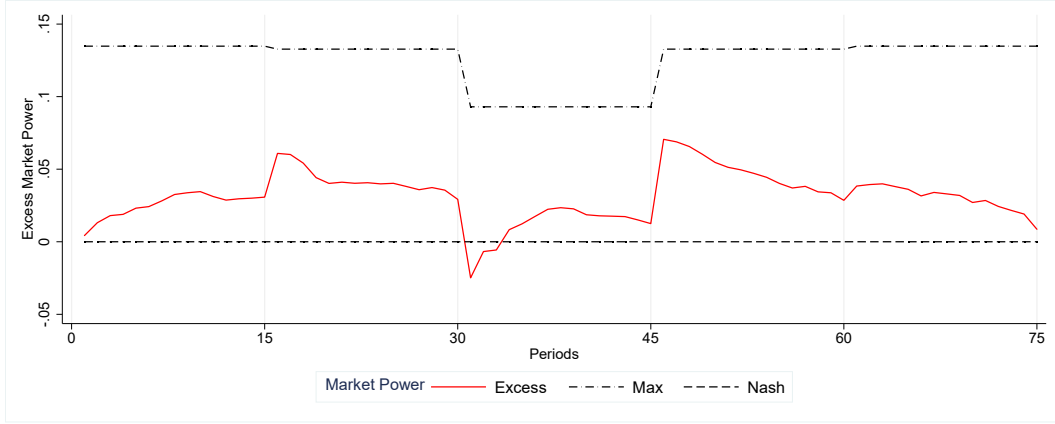
The averages of pass-through rates by differing group sizes are reported. Below averages, standard errors are reported in parentheses.  $p$ -values correspond to the result of the Wilcoxon signed-rank test on the equality of pass-through rates for small or large shocks (i.e.  $H_0 : \beta_0^+ = \beta_0^-$ ).

**Result 1.1:** *Prices do not react symmetrically to equally sized positive and negative shocks.*

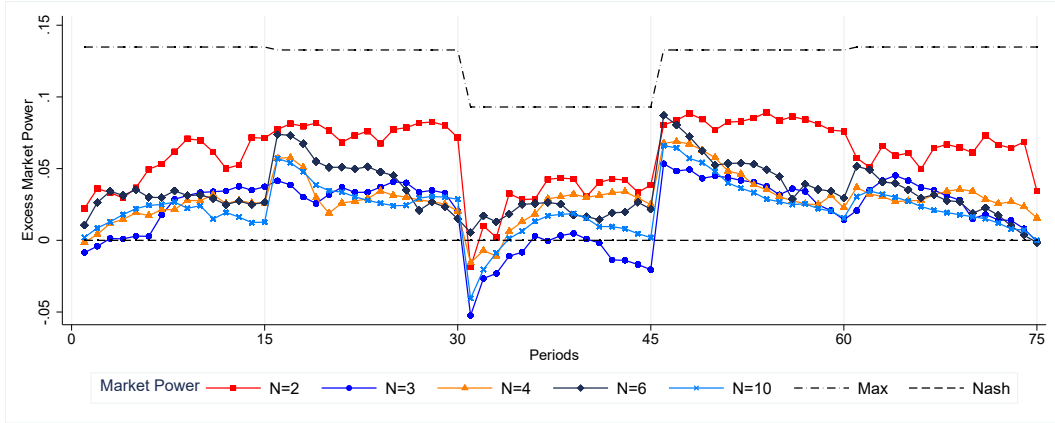
**Result 1.2:** *Price reactions in duopoly markets are not consistent with APT.*

## 4.2 Market Power

We now turn to our second hypothesis. We follow the literature in applying the Lerner index as the relevant measure of market power:  $L_{i,t} = \frac{p_{i,t} - mc_t}{p_{i,t}}$  (Lerner, 1934). We propose that the difference between the observed Lerner index (i.e.,  $L_{i,t}$ ) and the “theoretical” Lerner index, that is the index that would be relevant if behavior was consistent with NE predictions (i.e.,  $L_t^{NE} = \frac{p_t^{NE} - mc_t}{p_t^{NE}}$ ), provides a measure of “excess” market power due to collusion. We further propose this as an appropriate measure of tacit collusion,



(a) Average of excess market power



(b) Excess market power by group size

Figure 3: Excess market power across periods and group sizes. In both subfigures, “Max” refers to the maximum excess market power that can be observed (i.e., when  $L_{i,t} = L_t^{max} = \frac{p^{max} - MC_t}{p^{max}}$ ) and “Nash” refers to the case  $L_{i,t} = L_t^{NE}$ .

as our price competition structure incorporates homogeneous goods, and we control marginal costs. Thus, we do not suffer the identification problem of observational studies. Our measure of excess market power can then be expressed as:

$$L_{i,t}^x = L_{i,t} - L_t^{NE} = mc_t \left( \frac{1}{p_t^{NE}} - \frac{1}{p_{i,t}} \right). \quad (7)$$

Figure 3 depicts the average of our measure of excess market power, by

period and treatment. Upon visual examination, one can immediately see that excess market power generally lies above the theoretical “Nash” level, consistent with an environment in which tacit collusion exists. Also, this average measure reaches its highest levels during the second and fourth rounds, the two rounds that immediately follow negative shocks. Following the large positive shock at the beginning of the third round, excess market power falls so much that it turns negative for several periods. Following the small positive shock at the beginning of the fifth round, excess market power does not react notably.

We test veracity of these observations by performing OLS regressions.<sup>19</sup> We consider the following specification:

$$L_{i,t}^x = \alpha + \sum_{s \neq 2} \delta_s \cdot \mathbb{1}[N = s] + \sum_{e \neq 1} \gamma_e \cdot \mathbb{1}[r = e] + \epsilon_{i,t}, \quad (8)$$

where the excess market power of seller  $i$  in period  $t$  is modeled as a function of group size- and round-specific indicator variables. Our main hypothesis consists of the joint nullity of all coefficients.

Table 3 reports the estimates in a step-wise manner. In model (5), we truncate the data to the periods where shocks shift the marginal cost (i.e., periods 16, 31, 46 and 61) and replace the dependent variable with the change in excess market power as  $\Delta L_{i,t}^x$ . This allows us to interpret the estimates of round specific indicator variables as the immediate effect of cost shocks on the tacit collusion in model (5).

First, we reject the joint nullity of all coefficients in all models except model (2) at a confidence level of 0.01. The fact that the constant  $\alpha$  is positive and significant in model (1) indicate the overall presence of tacit collusion. Second, the coefficients of round-specific indicator variables in model (3) indicate that tacit collusion is higher during the second and fourth rounds, and lower during the third round relative to the first round. In model (5) where we truncate the data, the coefficient of rounds 3 and 5 are negative and that of round 4 is positive. Furthermore, we reject the hypothesis  $H_0 : \alpha + \delta_s + \gamma_e = 0$

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<sup>19</sup>The non-parametric counterpart of this test is available in Online Appendix 2.2

Table 3: Excess market power

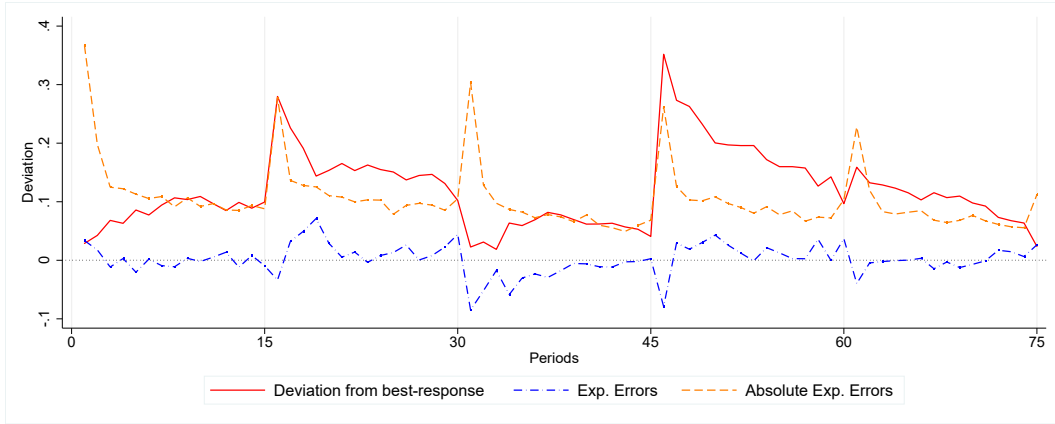
	(1)	(2)	(3)	(4)	(5)
Constant	0.032*** (0.004)	0.060*** (0.011)	0.025*** (0.004)	0.054*** (0.011)	0.004 (0.007)
$N = 3$		-0.039* (0.015)		-0.039* (0.015)	0.018 (0.020)
$N = 4$		-0.031* (0.014)		-0.031* (0.014)	0.028** (0.010)
$N = 6$		-0.026 (0.014)		-0.026 (0.014)	0.047*** (0.010)
$N = 10$		-0.038** (0.012)		-0.038** (0.012)	0.029* (0.011)
$r = 2$			0.017*** (0.003)	0.017*** (0.003)	
$r = 3$			-0.014*** (0.004)	-0.014*** (0.004)	-0.084*** (0.009)
$r = 4$			0.023*** (0.004)	0.023*** (0.004)	0.028*** (0.006)
$r = 5$			0.005 (0.004)	0.005 (0.004)	-0.020** (0.007)
Observations	18355	18355	18355	18355	980
Adjusted $R^2$	-	0.052	0.054	0.106	0.225
F-statistic	-	2.607	31.289	19.288	27.003

Results of OLS regressions on specification 8 are reported. In model (5), the dependent variable is the change in excess market power,  $\Delta L_{i,t}^x$ . Below estimates, robust standard errors that are clustered at the market level are reported in parentheses.

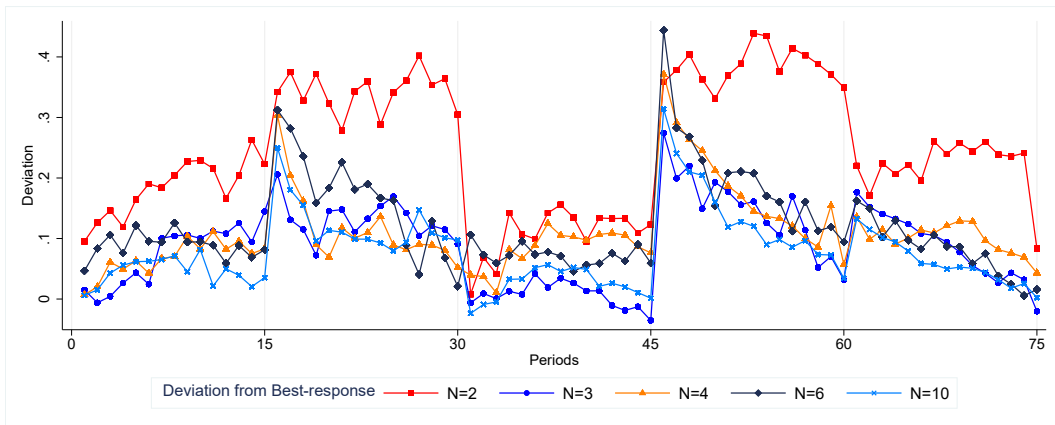
\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

at a confidence level of 0.05 (i.e.,  $\alpha + \delta_{\{N=3,4,10\}} + \gamma_5 = 0$ ). We can thus say that immediately after a negative (the large positive) shock, the excess market power increases (decreases). Third, coefficients of group size specific indicator variables are negative and significant, although at marginal level for  $N = 6$  ( $p$ -value= 0.071) in model (2). Here, we also reject the hypothesis  $H_0 : \alpha + \delta_s = 0$  for all  $s$  ( $p$ -value < 0.01). This suggests that tacit collusion is present in all markets but its magnitude is smaller when  $N > 2$ . The sign of these coefficients in model (5) suggests that markets larger than size 3 increase their market power in response to the first negative shock. Lastly, we generally





(a) Average of different deviations across all markets



(b) The deviations from best-response action by group size

Figure 4: Deviations from best-response action and errors in expectations.

reject the hypothesis  $H_0 : \alpha + \delta_s + \gamma_e = 0$  in the most unrestricted model (4) (11 times out of 15 tests at  $p$ -value  $< 0.05$ ). The overall interpretation of these tests provide the basis of our second result:

**Result 2:** *Excess market power (i.e., tacit collusion) is not invariant to shock direction and group size. It is persistently higher in duopolies, and in larger-sized markets it rises following negative cost shocks.*

### 4.3 Deviations from Best-Response

Finally, we assess deviations from subjects’ best-response actions. Deviations from the best-response action can be attributed either to error, or alternatively to strategic motives. To argue that the deviations we observe in our experiments are not entirely emerging from erroneous behavior, we compare the magnitudes of such deviations to the average magnitude of expectation errors (i.e.,  $\mathbb{E}_{i,t-1}[p_{-i,t}] - p_{-i,t}$ ) and the average of absolute expectation errors.<sup>20</sup>

Figure 4(a) depicts the average value of these deviations over time. The average expectation errors are remarkably close to zero, with no obvious trend across periods. Although this suggests that beliefs are on average correct, it does not imply the complete absence of errors: The average measure of absolute expectation errors lies well above zero throughout the experiment. The latter peaks following cost shocks but subsequently trends downward. These results on expectation errors are consistent with those of prior experiments in which prices are strategic complements (e.g., Hommes et al. 2005). However, deviations from the best-response action reveal a different and rather interesting pattern: They peak sharply following negative shocks and remain high during these rounds, but do not peak similarly following positive shocks. The second graph in Figure 4 depicts the average of deviations from best-response action by group size. The same pattern can be traced across our treatment groups.

We perform OLS regressions to study deviations from best-response. Consider the following specification:

$$p_{i,t} - p_{i,t}^{BR|\mathbb{E}} = \alpha + \sum_{s \neq 2} \delta_s \cdot \mathbb{1}[N = s] + \sum_{e \neq 1} \gamma_e \cdot \mathbb{1}[r = e] + \epsilon_{i,t} \quad (9)$$

where the deviation of subject  $i$ ’s price from the best-response action conditional on the submitted guess (i.e.,  $p_{i,t}^{BR|\mathbb{E}}$ ) is modeled as a function of group size- and round-specific indicator variables. The theory postulates the joint nullity of all coefficients.

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<sup>20</sup>We label these latter two as “errors” rather than as “deviations” as there is no strategic benefit to knowingly submitting inaccurate guesses/expectations in our experiment.

Table 4 reports the estimates in a step-wise manner. In model (5), we truncate the data to the periods where shocks shift marginal cost, the same way in previous section and replace the dependent variable with the change in deviation from best-response action.

Table 4: Deviations from best-response

	(1)	(2)	(3)	(4)	(5)
Constant	0.120*** (0.013)	0.083*** (0.012)	0.248*** (0.041)	0.212*** (0.040)	0.044 (0.028)
$N = 3$			-0.159** (0.049)	-0.159** (0.049)	0.122* (0.055)
$N = 4$			-0.138** (0.050)	-0.138** (0.050)	0.163** (0.051)
$N = 6$			-0.128* (0.051)	-0.128* (0.051)	0.210*** (0.041)
$N = 10$			-0.170*** (0.044)	-0.170*** (0.044)	0.144** (0.046)
$r = 2$		0.080*** (0.010)		0.080*** (0.010)	
$r = 3$		-0.028** (0.010)		-0.028** (0.010)	-0.260*** (0.031)
$r = 4$		0.112*** (0.015)		0.112*** (0.015)	0.131*** (0.025)
$r = 5$		0.018 (0.013)		0.018 (0.013)	-0.118*** (0.024)
Observations	18355	18355	18355	18355	980
Adjusted $R^2$	-	0.044	0.051	0.095	0.170
F-statistic	-	37.840	4.013	19.546	40.442

Results of OLS regressions on specification 9 are reported. In model (5), the dependent variable is the change in deviation from the best-response action,  $\Delta(p_{i,t} - p_{i,t}^{BR|\mathbb{E}})$ . Robust standard errors are clustered at the market level and are reported in parentheses. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

The joint nullity of all coefficients can be rejected in all specifications. The hypotheses  $H_0 : \alpha + \gamma_s = 0$  in model (2) and  $H_0 : \alpha + \delta_s = 0$  in model (3) can be rejected at significance of 0.01. This points out to the following two results: (i) Sellers deviate more (less) from the associated best-response action following negative (positive) shocks and (ii) deviations are lower when  $N > 2$ .

We see that the sign of group size indicator coefficients in models (4) and (5) are flipped. In model (4), they reflect the fact that groups of size 3 and larger deviate less, on average, relative to duopolies. In model (5), they correspond to the immediate reaction of these groups to the first negative shock. These deviations rise further when a large negative shock shifts the marginal cost down ( $\hat{\gamma}_4 = 0.131$ ) while they drop significantly in response to the large positive shock ( $\hat{\gamma}_3 = -0.260$ ). In consequence, we reach to the following results:

**Result 3.1:** *Sellers deviate on average from their best-response action.*

**Result 3.2:** *Deviations from the best-response action grow (shrink) following negative (positive) shocks.*

## 5 Discussion

Our results point to the co-appearance of asymmetric price transmission and tacit collusion. The latter seems to be the result of strategic behavior, as our analysis of deviations from best-response action reveals. These findings are consistent with theories that cast tacit collusion as having a significant role in the emergence of APT, such as the trigger price model in Borenstein et al. (1997). Most of the other theoretical explanations of APT in the literature cannot account for the pricing behavior observed in our results. We can reasonably exclude, for example, the influence of explicit collusion (i.e., involving direct communication), capacity constraints, inventory limitations, (a)symmetric menu costs, consumer loss aversion, (a)symmetric search costs, contexts of alternating price moves and price lockup periods, and so forth, as being necessary conditions for APT, since these features are excluded by our design.

We cannot, however, claim a monotonic relation between the magnitude of APT and tacit collusion: the pricing behavior of duopolies in our experiment is revealed to be fairly symmetric. We explain the exceptionality of the result for duopoly markets as follows: In duopolies, collusion is so strong that sellers

are, by and large, able to maintain cooperative (tacitly collusive) pricing over a sustained period of time, with pricing showing no reversion to Nash. We therefore argue that APT requires significant, but imperfect, collusion.<sup>21</sup>

If tacit collusion is indeed a significant causal force behind APT, then our work has important implications for antitrust enforcement policy against collusion and price-fixing. In particular, regulators may consider APT in a market as a signal for the presence of collusion between firms in that market. Since many real-world interactions between competitive firms are repeated indefinitely, such collusion may even be sustainable as a NE. Further research is needed to determine whether collusion is an important cause of APT behavior in field settings, and if so, whether suitable forms of regulatory intervention might exist to reduce such collusion without increases in inefficiency.

We propose that follow-on research may yield further insights into the mechanisms through which tacit collusion leads to APT, as well as potential policy responses that might diminish its frequency and magnitude. In particular, future experiments should address the impact of different levels of information transparency. Most notably, testing the effects of providing feedback on individual prices and/or payoffs of rivals on APT may provide particularly helpful insights. The latter is shown to lead to more rivalistic outcomes in experimental oligopoly studies, as it initiates imitation dynamics (Fiala and Suetens, 2017), while the former can lead to more collusive levels. Nevertheless, both may reduce the degree of asymmetry in price transmission. Another area of needed research is to explore the roles of market power and market concentration in shaping APT pricing behavior. In our experimental design we explicitly kept the theoretical market power of sellers the same across mar-

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<sup>21</sup>The fact that our duopolies reached almost stable collusion, while larger markets did not, is consistent with the literature we review in Section 1.3. This can be attributable to a combination of two factors: first, coordination between market participants becomes increasingly difficult with each new seller, and three may well be the number from which the difficulties and costs involved in maintaining coordination start to exceed the marginal benefits; second, our duopolies are unique in that each participant can deduce the choices made by the other participant by observing aggregate market outcomes. In a triopoly or larger market, by contrast, it is not possible for sellers to detect whether an aggregate market outcome is due to the defection of a single competitor, or from a broader but shallower defection by multiple competitors.

kets of varying sizes to study the impact of group size on coordination channel, alone. Finally, future studies may benefit from testing the robustness of our findings to alternative demand specifications, or to the introduction of human subjects acting as buyers.

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