CBESS Discussion Paper 14-02

Institutional Authority and Collusion

By Axel Sonntag[‡] and Daniel John Zizzo[§]

[‡] Centre for Behavioural and Experimental Social Science (CBESS) and School of Economics, University of East Anglia.
[§] Centre for Behavioural and Experimental Social Science (CBESS), ESRC Centre for Competition Policy and School of Economics, University of East Anglia.

Abstract
A 'collusion puzzle' exists by which, even though increasing the number of firms reduces the ability to tacitly collude, and leads to a collapse in collusion in experimental markets with four or more firms, in natural markets there are such numbers of firms colluding successfully. We present an experiment showing that, if managers are deferential towards an authority, firms can induce more collusion by delegating production decisions to middle managers and providing suitable informal nudges. This holds not only with two but also with four firms. We are also able to distinguish compliance effects from coordination effects from the nudges.

JEL classification codes
L13, L22, C91.
Keywords
Collusion, Cournot, oligopoly, authority, delegation, coordination.

Centre for Behavioural and Experimental Social Science
University of East Anglia
Norwich Research Park
Norwich NR4 7TJ
United Kingdom
www.uea.ac.uk/ssf/cbess

Institutional Authority and Collusion

Axel Sonntag[1] and Daniel John Zizzo[2]

January 2014

Abstract

A 'collusion puzzle' exists by which, even though increasing the number of firms reduces the ability to tacitly collude, and leads to a collapse in collusion in experimental markets with four or more firms, in natural markets there are such numbers of firms colluding successfully. We present an experiment showing that, if managers are deferential towards an authority, firms can induce more collusion by delegating production decisions to middle managers and providing suitable informal nudges. This holds not only with two but also with four firms. We are also able to distinguish compliance effects from coordination effects from the nudges.

Keywords: Collusion, Cournot, oligopoly, authority, delegation, coordination.

JEL Classification: L13, L22, C91.

[1] Centre for Behavioural and Experimental Social Science (CBESS) and School of Economics, University of East Anglia, Norwich, UK, e-mail: a.sonntag@uea.ac.uk

[2] Centre for Behavioural and Experimental Social Science (CBESS), ESRC Centre for Competition Policy and School of Economics, University of East Anglia, Norwich, UK, e-mail: d.zizzo@uea.ac.uk.

We gratefully recognize the ESRC Centre for Competition Policy (CCP) for providing financial support for conducting this experiment. Furthermore we thank Subhasish Chowdhury, David Cooper, Erich Cromwell, Stephen Davies, Enrique Fatas, Steffen Huck, Alexandros Karakostas, Franco Mariuzzo, Matthew Olczak, Melanie Parravano, Jan Potters, Israel Waichman and participants to a presentation at Florida State University for their helpful comments and suggestions. The usual disclaimer applies.

1. Introduction

There are many plausible and well documented reasons why an increasing number of firms could make it more difficult to tacitly collude (Huck, Normann, and Oechssler 2004). However, it is still unclear why the experimental literature finds that collusion breaks down with four or more firms in the market, while in the real world larger numbers of firms seem to be able to tacitly collude (Davies, Olczak, and Coles 2011; Sen 2003). Although many dimensions affect the likelihood of a collusive market outcome (for a review see Potters and Suetens, 2013), a 'collusion puzzle' remains regarding what dimensions potentially facilitate the ability to collude in settings with larger numbers of firms.[3] In this paper we present an experiment showing that, if managers are deferential towards an authority, firms can induce more collusion by delegating production decisions to middle managers and providing suitable informal nudges. This holds not only with two but also with four firms.

Market size and the ability to collude. Experiments since Fouraker and Siegel (1963) and Dolbear et al. (1968) have shown a significant decrease in the equilibrium price in moving from 2 to 3 or 4 firms in the market. In a price setting environment, Dufwenberg and Gneezy (2000) showed that with 3 or 4 firms collusion broke down after some initial learning took place. Dolbear et al. (1968) provided two main reasons for the breakdown in collusion: first, the profit opportunities from collusion decrease in the number of firms in the market as any surplus needs to be split over more firms and second, it is harder to bargain and achieve a

[3] A huge experimental literature on collusive behavior found that fixed player matching (Huck et al., 2004b), pre-play communication and announcements (Cason and Davis 1995; C. A. Holt and Davis 1990; C. Holt 1985; Huck, Muller, and Normann 2001), within-play communication (Fonseca and Normann 2012; Sally 1995), leadership in the sense of letting one firm decide first (Güth, Levati, Sutter, and van der Heijden 2007), the opportunity to punish non-collusive behavior (Fehr and Gächter 2000), experience in market interaction (Benson and Faminow 1988; Dufwenberg and Gneezy 2000) and concrete knowledge about the form of cost and demand function (Huck et al. 1999) increased the likelihood of observing collusive behavior in oligopolistic settings. In contrast to collusion-increasing factors, complementary studies also suggested that e.g. full information on individual actions (Huck et al. 1999; Huck, Normann, and Oechssler 2000) or cost asymmetries (Mason, Phillips, and Nowell 1992; Mason, Phillips, and Redington 1991) decreased collusive behavior and increased competition instead.

tacit agreement. Selten (1973) used a model to show that collusion becomes harder as the number of firms increases, as the number of free riders increases with the number of firms. There are additional reasons why we may expect collusion to become more difficult. The ability to use price or quantity to signal the intention to collude decreases as the number of firms increases. Furthermore, in the lack of special punishment technologies, targeted punishment of deviators becomes impossible. Even if it were public knowledge who deviated from an implicit or explicit collusive agreement, it would be impossible to punish the deviator only, as extending production or lowering the price would hurt all market participants (and not only the deviator). As firms need not fear the potential punishment of the other firms, this works against collusion in markets with many firms (see e.g. Green et al., 2013; Ivaldi et al., 2003).

In the experimental design closest to our study, in a Cournot setting Huck et al. (2004b) found that, whereas firms in two player markets could collude considerably well, this ability decreased almost linearly with the number of firms, making firms even set quantities above the Nash prediction in markets with 4 or more players. In a Bertrand setting, Fonseca and Normann (2012) replicate the general finding that an increasing number of firms decreases the ability to collude but also showed that communication between firms could facilitate collusion (which is then not tacit anymore) with 4 or more firms.

Deference to authority and institutional delegation. This paper considers a different and possibly complementary mechanism that may also support collusion even in multiple firms markets. In the key experimental treatments, we use the experimenter as the authority providing nudges towards the subjects making decisions over quantities. This is meant to model middle managers being delegated to make market decisions but who can, nevertheless, get nudged by their line managers in suitable pro-collusive ways. We believe this to be a

considerably ecologically more valid experimental model of the role of the authority in the laboratory than would, say, providing the role of authority to an experimental subject (e.g. as a ring leader); the latter would make the experiment one about peer pressure rather than about authority.[4] An additional benefit of having the experimenter as the authority is that we can ensure there is an identical nudge across subjects and sessions.

Two ingredients are required for the nudge to help collusion. First, we rely on the behavioral notion that workers may wish to be deferential towards their line managers, in parallel to experimental subjects wishing to be deferential towards the experimenter. In the words of organizational psychologists Cialdini and Goldstein (2004, p.596), "most organizations would cease to operate efficiently if deference to authority were not one of the prevailing norms."[5] Second, we rely on the decision over the market variable (quantity, in our experiment) to be one that is delegated to the middle manager. As noted by Vickers (1985, p. 144), delegating a decision could "in some cases [be] essential for the credibility of some threats, promises and commitments". The combination of deference of authority and delegation could work as a commitment device which could facilitate collusion. It could also work as a coordination device in the presence of common knowledge of the same message being delivered to middle managers of different firms. We control for such coordination effect by implementing both a treatment where such assurance of common knowledge is given and a treatment where the nudge is private information only. We are not the first to

[4] Ours is a deliberate use of experimenter demand as the direct object of investigation rather than a confound relative to other experimental objectives: see Zizzo (2010) for a methodological discussion of experimenter demand effects and Cadsby et al. (2006), Karakostas and Zizzo (2012) and Silverman et al. (2012) for other applications of using the experimenter as authority in the context of a tax compliance, a money burning and a public good contribution experiment, respectively. We discuss Silverman et al. (2012) more below.

[5] In a recent economic experiment, Robin et al. (2012) found that workers are willing to change their opinions in order to comply with those of their managers. There is empirical evidence that subordinates in organizations may not worry about the ethical implications of their actions if cued by the authority (Ashford and Vikas 2003; Darley 2001), e.g. becoming willing to engage in race discrimination (Brief, Buttram, Elliott, Reizenstein, and McCline 1995).

establish a positive link between collusion and delegation,[6] but our experiment is the first to analyze the role of institutional delegation for a varying number of interacting firms (2 and 4).

Silverman et al.'s (2012) experiment looks at the effect of authority on public good contribution. Their motivation is inducing tax compliance; they vary the source of expertise supporting the nudge they provide (a recommendation to make a given public good contribution) and whether there is a penalty in case of audit. They find an effect of their nudge variable, but no evidence for a role of the source of expertise and only weak evidence in favor of an effect of an explanation for the nudge.[7] Their work is complementary to ours in linking a decision setting in the broad class of social dilemmas to deference to authority. The game employed as well as their focus is different from ours and they do not vary the number of interacting players, which is essential to our experiment. They also had nudge reminders every round, whereas our manipulation is quite weaker as there was only one reminder in our sequence of 24 rounds.[8]

In our experiment we vary the number of firms (2 or 4), whether there is a nudge to collude, whether there is an explanation for the nudge and whether the nudge was common or private knowledge. Our key finding is that our nudge is equally effective in reducing the market quantity with 2 and 4 firms, and an explanation for the nudge is not needed to achieve this result. Even removing the element of common knowledge did not significantly reduce the level of collusion. If we label as a *compliance effect* the pure effect of deference towards the

[6] Schotter et al. (2000) found that delegation can potentially facilitate collusion in oligopolistic markets, if agents are given free rein in the bargaining. Fershtman et al. (1991) argued that, if decisions were delegated, and there existed a high degree of commitment (i.e. the contract between principals with their agents was public knowledge, something which however may be impractical under antitrust laws) no formal agreement between the principals is needed to facilitate collusion.

[7] Specifically, an effect is found at $P < 0.05$ only in one of the regressions once a number of covariates are added, and only as an interaction term.

[8] Because of their different motivation, they also employ a random matching of players across rounds, whereas for us (interacting firms in an oligopolistic market) a fixed matching is more appropriate.

nudge by the authority and a *coordination effect* the effect of coordinating to a common sunspot provided by common knowledge of a nudge, we are able to distinguish a compliance effect towards collusion from a coordination effect, and to find evidence for the former. In section 2 we introduce the experimental design and state our hypotheses before we present the results in section 3. Subsequently, section 4 discusses our findings and section 5 concludes.

2. Experimental Design and Hypotheses

2.1. Experimental Parameters

We used a Cournot type market with homogenous products, linear production costs and a linear demand function and parameterized it identically to Huck et al. (2007, 2004b) and Requate and Waichman (2010). That is, we used an inverse demand function of $P = \max(100 - Q, 0)$ with $Q = \sum_{i=1}^{2,4} q_i$ and constant marginal costs equal to 1 ($C(q_i) = q_i$). The Nash prediction for the production quantity of firm i writes as $q_i^N = 99/(n+1)$ which for the four player case is: $q_i^N = 99/5 = 19.8$, and for the two player case: $q_i^N = 99/3 = 33$. The collusive prediction is $q_i^{Col} = 99/2n$ or for the four player case: $q_i^{Col\,4} = 99/8 = 12.375$ and for the two player case: $q_i^{Col\,2} = 99/4 = 24.75$.

2.2. Procedures

We used a fixed matching protocol, i.e. at the start of the session, depending on the treatment two or four participants formed a group, the composition of which did not change throughout the entire session. Each session lasted for two stages of 12 rounds each and participants were able to use a profit calculator to get an understanding of market demand and of the relevance

of their co-participants' actions on their own income.[9] Participants were told they would receive additional instructions at the beginning of stage 2 (round 13); these instructions just contained reminders. We used a market oriented frame to present the instructions (see Fonseca and Normann 2012).[10] The experiment was programmed in z-Tree (Fischbacher, 2007) and was run at the CBESS laboratory at the University of East Anglia. A random round payment mechanism was implemented.[11] On average one session lasted 70 minutes and 212 participants earned 18.75 pounds each.[12]

2.3. Treatments

We employed a 2 (market size) x 4 (levels of authority) full factorial design. The following three treatments were both implemented in 2-firms and 4-firms market settings.

Baseline (B): In the Baseline treatment participants simply repeatedly interacted in the Cournot market laid out above for 24 periods.

Authority (A): In the Authority treatment the instructions were exactly the same as in B but one additional sentence was added, nudging participants to produce a particular quantity. After the text of the Baseline instructions the following was added: "You are entirely free to produce as few or as many units of output as you like (from 0 to 100). That said, we would ask you (and your co-participants) to produce 12.4 units of output. We are telling this not just

[9] Requate and Waichman (2010) found no significant difference between the use of profit calculators and payoff tables as a means of presentation.

[10] Huck et al. (2004b) showed that whether an experiment is framed in an economic sensible or entirely neutral way can, but does not necessarily have to have an effect on behavior. Whereas in a five players Cournot setting Huck et al. (2004b) did not find any difference between the frames, the neutral frame caused significantly more competitive behavior in a two player situation. Although significant, the difference in means was only one unit, a qualitatively rather small difference, given a choice set of 0-100 and a range of useful strategies from 66 (Nash) and 49.5 (collusion).

[11] The profits of one randomly chosen round per stage were added up and converted into pounds at an exchange rate of 80 ECU = 1 pound. A participation fee of 3 pounds was added to the final earnings. We used ORSEE (Greiner, 2004) to invite for the sessions and did not restrict the CBESS subject pool.

[12] The number of independent observations (groups) was almost equally distributed across treatments (12 groups in treatments A4, EA4, PA4, B2, A2, EA2 and PA2 and 11 groups in treatment B4).

to you but also to your co-participants" (in the 2-firms setting 12.4 was replaced by 24.8). A reminder of this, and only one reminder, was given at the beginning of the second stage. The requested production quantity (either 24.8 or 12.4 units of output) was exactly equal to the collusive output level for the respective market size.

Explaining Authority (EA): In the Explaining Authority treatment, we used exactly the same instructions as in authority, but explained why it would be beneficial for the participants to obey the nudge by adding the following: "The reason you should do this is that, if you and your co-participants produce 12.4 units of output, the total profits of you and your co-participants will be the highest. You can use the profit calculator to check the profitability of producing 12.4 units of output per firm."[13] There was a corresponding reminder, and only one, at the beginning of the second stage.

Private Authority (PA): In contrast to the Authority treatment, in the Private Authority treatment, the number requested was not common information but participants were asked to produce a specific number in private, i.e. they did not received any information about the quantity we requested from any of their co-participants.[14] Subjects were reminded on that quantity once, and only once, at the beginning of the second stage.

2.4. Hypotheses

With the same parameters, Huck et al. (2004b) found that most markets in a 4 firm setting produced around 75 units (close to the Nash equilibrium of 79.2 units). We expect subjects to behave similarly to Huck et al.'s (2004b) in the Baseline.[15] Both theoretical (Fouraker and Siegel 1963; Selten 1973) and empirical (Dolbear et al. 1968; Gürerk and Selten 2012; Huck et al. 2007; Huck, Normann, et al. 2004) results suggest that increasing market size decreases

[13] The experimental instructions can be found in Appendix A.
[14] In the PA treatment no explanation similar to the EA treatment was provided.
[15] For a game-theoretical analysis of a comparable setting see Selten (1973).

the ability to collude. Therefore, we expect that 2-firms groups collude more than 4-firms groups.

Hypothesis 1: *In the B 4-firms treatment, on average firms set their production around the Nash equilibrium level. In the B 2-firms treatment, groups collude more than 4-firms groups.*

As discussed in the introduction, we expect the provision of the nudge to reduce production levels and increase collusion as a result of deference to authority and common knowledge.

Hypothesis 2: *Nudging subjects to produce the collusive quantity reduces the total output below the Nash output level, both for 2-firms and 4-firms markets.*

As in Karakostas and Zizzo (2012) and Silverman et al. (2012), we expect that explaining the usefulness of obeying the nudge should, if anything, further increase compliance with the requested production level.

Hypothesis 3: *In the EA treatment quantities produced are lower than in the A treatment, both in 2-firms and 4-firms markets.*

We expect any effect caused by the A treatment to have two causes: a *coordination effect* due to the fact of commonly knowing the quantity we asked all participants to produce and a *compliance effect* of being deferential towards an authority. Hence, in the PA treatment we expect the nudge to be still effective but weaker in size than in the A treatment.

Hypothesis 4: *In the PA treatment quantities produced are higher than in the A treatment but lower than the Nash prediction, both in 2-firms and 4-firms markets.*

As the opportunity costs of obeying the production request (i.e. unilaterally reducing the own production quantity below the Nash output level) are higher in 4-player than in a 2-player

settings (e.g. see Selten, 1973), we expect any requests to have a stronger effect in 2-player than in 4-player settings.

Hypothesis 5: *2-player groups reduce their average production by a larger extent as compared to 4-player groups, if requested to do so (treatments A, EA and PA).*

3. Results

3.1. Testing the Hypotheses

Figure 1 and Table 1 summarize the results of our experiment.[16]

[Insert Figure 1 and Table about here]

Result 1: Hypotheses 1's prediction of more collusion in 2-firms markets than in 4-firms markets is not supported. Production levels in both market sizes are not significantly different from the Nash prediction.

Support: In order to make production levels comparable across market sizes, we follow the approach of Huck et al. (2004b) and compute the ratio of the average total quantity in the market to the total quantity predicted by the Nash equilibrium: that is, the Nash ratio is $r = Q/Q^N$. Comparing the Nash ratios of 2-firms and 4-firms markets in B reveals that, in smaller markets, firms produce slightly less. However, this difference is not significant (Wilcoxon, p=0.695) and does not differ from the Nash prediction of 1 (sign test, p=0.678).[17] We also find no significant correlation between production levels and the number of firms (Spearman r=0.096, p=0.425).

[16] We used the software packages R and STATA to analyze the experimental data.

[17] All bivariate tests in this paper are reported as two sided and computed on session averages per group of subjects to control for within-session non independence of observations.

Result 2: Hypothesis 2 is supported. Relative to the B treatment, subjects reduced their production output if nudged to do so (treatments PA, A and EA) in 4-firms and in 2-firms markets.

Support: Table 1 shows a fall in overall production as a result of the nudge: in the 2-firms markets, mean production goes down from about 66 in the B treatment 63 in the PA treatment and further to 58-60 in the A and EA treatments; in the 4-firms markets, mean production goes down from about 82-83 in the B treatment 78 in the PA treatment and further to 73-74 in the A and EA treatments.[18] These quantities are significantly higher than if the nudge had been precisely followed in terms of requested production (sign test, all $p<0.01$), but they are also lower than the Nash equilibrium predictions.[19]

[*Insert Table about here*]

Table 2 looks at this matter further by running regressions on Nash ratios (with either random effects or error clustering on groups to control for non-independence of observations). The independent variables include treatment dummies (A, EA and PA = 1 in the respective treatments; 2 Firms = 1 in 2-firms markets; interaction dummies); round and stage dummies (Round and Round^2, Stage 2 = 1 in Stage 2); plus additional controls.[20] All regression models are consistent in pointing to a reduction of the Nash ratio of between 11 and 12%, indicating

[18] In the 2-firms case PA, A and EA significantly reduced production (one-sided Wilcoxon tests) with p=0.039, p=0.009 and p=0.019, respectively. In the 4-firms case PA, A and EA also resulted in lower production levels, significant at p=0.130, p=0.009 and p=0.067, respectively.

[19] In 2-firms markets production was significantly lower than the Nash equilibrium prediction (PA, A and EA: $p<0.05$). In 4-firms markets production also decreased; however, in the case of PA and EA only qualitatively (A, EA and PA with $p<0.001$, p=0.388 and p=1, respectively).

[20] Soc.Des. is a social desirability index collected from a 16-items questionnaire at the end (Stöber 2001) that provides a psychological measure of sensitivity to social pressure. Male (=1 with male subjects), Economics (=1 for subjects with an Economics background), and Age are also included in some regressions. Apart from a $p<0.1$ significance in only two out of four the regression where Age is included, none of these variables is significant and we shall not refer to them further.

more collusion on average in the A and EA treatments.[21] The coefficient of PA lies almost exactly half way between the coefficients of B and the other nudge treatments A and EA, suggesting both a collusion and a coordination effect.[22] All interaction variables with the 2 Firms dummies are insignificant, pointing to a robustness of the finding to whether firms are 2 or 4.

Result 3: Hypothesis 3 is not supported. There is no evidence supporting lower production in the EA treatment than in the A treatment.

Support: Table 1 makes that clear by showing that aggregate production level in each markets was the same on average between A and EA +/- about 1 unit, and regardless of whether a 2-firms or a 4-firms market is considered.[23] The regression results of Table 2 show a similar picture, as the regression coefficient for the EA dummy is not significant at conventional levels.

Result 4: We find only mild support for hypothesis 4. The production levels in the PA treatment are only qualitatively higher than in the A treatment and they are significantly lower than the Nash prediction in 2-firms but not in 4-firms markets.

Support: The production levels observed in PA were 78.4 and 62.6 for 4-firms and 2-firms markets, respectively. This is slightly but not significantly higher than in the A treatment (Wilcoxon test: $p=0.128$ and $p=0.347$ for 4-firms and 2-firms markets respectively). Whereas the production in the PA treatment was significantly lower than the Nash prediction for 2-

[21] All coefficients on the A dummy are significant at the $p<0.05$ level; the random effects coefficients on the EA dummy are significant at the $p<0.05$ level, the error clustering coefficients at the $p<0.1$ level; in additional regressions pooling together the A and EA dummies as their coefficients are virtually identical, we found $p<0.05$ significance in all regressions.

[22] Although the regression coefficient on PA is not significant itself, it is not significantly different from the coefficients of A and EA (t-test: both $p>0.252$).

[23] In both 2-firms and 4-firms markets there is no statistically significant difference between A and EA (Wilcoxon test, both $p>0.799$). This result is replicated in the regression analysis of Table 2.

firms markets (sign test: p=0.006), in 4-firms markets, the decrease in production is much smaller and not significant (sign test: p=1). The regressions in Table show that production in the PA treatment was slightly higher than in the A treatment.[24]

Result 5: Against hypothesis 5, we find no difference between 2-firms and 4-firms groups in terms of reaction intensity for all nudge treatments (PA, A and EA).

Support: Although requesting a particular quantity significantly reduced the output, both in 4-firms and 2-firms settings (see Results 2 and 3), the magnitude of the reduction effect did not appear to be a function of market size. Although the regression analysis in Table finds significant main effects of the authority and the explaining authority treatments, the respective interaction terms with market size are far from being significant.

3.2. Supplementary Analysis

Time trend. As shown by Table 1, we find that there is a tendency for production to be reduced in the B treatment if one compares the first six with the last six rounds; this tends to reduce the gap with the A, EA and PA treatments. Nevertheless, the nudge appears to produce more collusion from the beginning, and this is what is being picked up by Result 2.

[*Insert Table 3 about here*]

Precise production matches. There is additional evidence that the nudges mattered. Table 3 presents the proportions of quantities chosen by firms that were precisely identical to the Nash equilibrium level, and to the requested amount in the A, EA and PA treatments, of 12.4 or 24.8 units. In the A, EA and PA treatments, subjects exactly followed the request to

[24] The coefficient of the treatment dummy PA was marginally significant in a specification with robust standard errors clustered by group ($p < 0.1$), but was insignificant in the specification with group level random effects.

produce the specific quantity of 12.4 or 24.8 units in 972 (out of 5184) cases, against 0 cases in which these quantities were produced in the B treatment.

Interestingly, while in the EA treatment the percentage of precise compliance to the nudge was of the order of 22-30%, this roughly halved to 9-14% in the A treatment (Wilcoxon test, p=0.079 for 2-firms markets and p=0.022 for 4-firms markets). The percentage of precise compliance to the nudge in the PA treatment was comparable to that in the A treatment (12-17%; in comparing PA and A treatments, Wilcoxon test p=0.583 and p=0.161 for 2-fims and 4-firms markets, respectively). This provides further support for a compliance effect independent of a coordination effect, as precise compliance takes place roughly to the same degree whether or not there is common knowledge of a common nudge.[25]

4. Discussion

There is a 'collusion puzzle' to explain the difference between the robust lab finding that having more than two firms competing in a market drastically reduces the ability to collude (Huck, Normann, et al. 2004) whereas in the real world collusion can be observed with a larger number of firms as well (Davies et al. 2011; Davies and Olczak 2008). The experiment presented in this paper is one piece of a jigsaw puzzle to explain these contrasting findings. While other factors may and clearly will be at work, such as communication (Fonseca and Normann, 2012), we have shown that a combination of delegation about production decisions and deference to the authority can also operate: we observed an increase in collusion regardless of roughly the same size (in terms of Nash ratios) regardless of whether we had 2 or 4 firms.

[25] Analyzing the response dynamics across treatments following the procedure used by Huck et al. (1999) revealed that the heuristic used most often was 'Best response'. For details see Appendix B.

The effect may have been further combined with a coordination effect, i.e. by having common knowledge (treatments A and EA) and asking decision makers to produce a specific output level we created a salient focal point that the players used to coordinate on. However, by implementing the Private Authority (PA) treatment we could distinguish between the compliance effect of deference to authority and a coordination effect and found that both dimensions are accountable for about a half of the combined effect, with no statistically significant difference however between A and PA.

We also found a significant increase in precise compliance rates – i.e. of subjects precisely following the nudge - in the EA as compared to the A treatment. Providing an explanation why a particular behavior would be beneficial to the decision maker roughly doubled the proportion of participants who followed the request to the point and significantly reduced overproduction in both market sizes. If our findings were purely driven by coordination effects, we should not have observed this difference between the EA and the A treatments, which in fact we did. Although we find strong EA vs. A treatment differences on the individual level, it appears that, given market response dynamics, these differences average out on a market level. This lack of an aggregate difference between the EA and A treatments is, of course, consistent with what Silverman et al. (2012) find in their different setting. Interestingly, and again in support of a compliance effect, the lack of common knowledge did not lead subjects to change the degree to which they followed the nudge precisely, as the percentages were the same in the A and PA treatments.

The fact that many corporate decisions are made in an institutional framework with superiors and subordinates naturally points to the question of whether and to what extent nudges from the higher level can facilitate collusion among delegated middle managers charged to set production levels. Although some studies formally showed that delegating a strategic

decision could serve as a commitment device, we are only aware of one paper that tested such implications experimentally.[26] Huck et al. (2004a) analyzed the actions of owners and managers in a two-stage Cournot setting. First, owners chose one of two possible contracts which they offered to their managers. All contracts offered at that stage were common knowledge among all players. Second, managers set the production quantities for their firms. The managers' production decisions were highly dependent on the owners' contract choices, i.e. managers used the signals they received from their own as well as their opponent's owner and reacted to them by setting their production levels accordingly.

Differently from Huck et al. (2004a) who explicitly assigned participants to the roles of both owners and managers (2 each), we exogenously provide the owners' request and let all participants play the role of managers only. The way that we implement delegation in the laboratory is stylized and exploits the fact that the experimenter can be seen an authority towards the experimental subjects (Zizzo, 2010; Karakostas and Zizzo, 2012; Silverman et al., 2012). We do not manipulate the nature of the authority in our experiment but this has been looked upon by Silverman et al. (2012) in their public good contribution setting, and could clearly looked at in future research in ours.

Bearing in mind that our manipulation of delegation was very stylized indeed and deliberately spared out many dimensions delegation could have in a real corporate setting (e.g. incentivized contracts, pressure from the superior officer, no anonymity etc.), we think, makes our findings even stronger. If already simply being asked to do something could significantly affect behavior although complying individually was clearly payoff-dominated by not complying, one could imagine that making the request stronger or making non-

[26] In their theoretical analyses Fershtman and Judd (1987), Sklivas (1987) and Vickers (1985) predict that strategically delegating decisions about production quantities could result in a higher than beneficial output which in turn decreased the profits of the delegating firms. In that sense delegation would not be advisable to firms.

compliance a very unfavorable choice for the decision maker would potentially achieve higher collusion rates than found in this experiment. Clearly, future research is needed.[27]

5. Conclusion

A 'collusion puzzle' exists between, on the one side, the fact that increasing the number of firms should reduce the ability to tacitly collude, and leads to a collapse in collusion in experimental markets with four or more firms; and the fact that in natural oligopolistic markets there are cases of larger number of firms tacitly colluding (Davies et al. 2011; Sen 2003). We present an experiment showing that, if managers are deferential towards an authority, firms can induce more collusion by delegating production decisions to middle managers and providing suitable informal nudges. This holds not only with two but also with four firms. There is evidence of a compliance effect from deference to authority that works independently, though perhaps in combination with, a coordination effect depending on common knowledge by the firms that the same nudge is being given in other firms. Based on the experimental results we have presented, if firms want to engage in collusion, delegating decisions internally could be a useful device to exploit deference to authority as a commitment device.

References

Ashford, B. E., and Vikas, A. (2003). The normalization of norruption in nrganizations. In B. M. Staw and R. M. Kramer (Eds.), *Research in Organizational Behavior*. Oxford: Elsevier.

[27] In our experiment we deliberately nudged people towards the collusive output. Alternatively, one could imagine nudging people towards a less favorable production level. Another extension of our work could be along the lines of increasing the numbers of firms in the markets further; for example, Fonseca and Normann (2012) consider 6-firms and 8-firms markets.

Benson, B. L., and Faminow, M. (1988). The impact of experience on prices and profits in experimental duopoly markets. *Journal of Economic Behavior and Organization*, 345–365.

Brief, A. P., Buttram, R. T., Elliott, J. D., Reizenstein, R. M., and McCline, R. L. (1995). Releasing the beast: a study of compliance with orders to use race as a selection criterion. *Journal of Social Issues*, *51*(3), 177–193.

Cadsby, C. B., Maynes, E., and Trivedi, V. U. (2006). Tax compliance and obedience to authority at home and in the lab: A new experimental approach. *Experimental Economics*, *9*(4), 343–359.

Cason, T., and Davis, D. (1995). Price communications in a multimarket context: an experimental investigation. *Review of Industrial Organization*, *10*, 769–787.

Cialdini, R. B., and Goldstein, N. J. (2004). Social influence: compliance and conformity. *Annual review of psychology*, *55*(1974), 591–621.

Darley, J. M. (2001). The dynamics of authority in organization. In J. M. Darley, D. M. Messick, and T. R. Tyler (Eds.), *Social influences on ethical behavior in organizations* (pp. 37–52). London: Erlbaum.

Davies, S., and Olczak, M. (2008). Tacit versus overt collusion firm asymmetries and numbers: what's the evidence? *Competition Policy International*, *4*(2), 174–200.

Davies, S., Olczak, M., and Coles, H. (2011). Tacit collusion, firm asymmetries and numbers: evidence from EC merger cases. *International Journal of Industrial Organization*, *29*(2), 221–231.

Dolbear, F., Lave, L., Bowman, G., Lieberman, A., Prescott, E. C., Rueter, F., and Sherman, R. (1968). Collusion in oligopoly: an experiment on the effect of numbers and information. *The Quarterly Journal of Economics*, *88*(2), 240–259.

Dufwenberg, M., and Gneezy, U. (2000). Price competition and market concentration: an experimental study. *International Journal of Industrial Organization*, *18*(1), 7–22.

Fehr, E., and Gächter, S. (2000). Cooperation and punishment in public goods experiments. *The American Economic Review*, *90*(4), 980–994.

Fershtman, C., and Judd, K. L. (1987). Equilibrium incentives in oligopoly. *The American Economic Review*, *77*(5), 927–940.

Fershtman, C., Judd, K. L., and Kalai, E. (1991). Observable contracts: strategic delegation and cooperation. *International Economic Review*, *32*(3), 551–559.

Fischbacher, U. (2007). z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, *10*(2), 171–178.

Fonseca, M. A., and Normann, H.-T. (2012). Explicit vs. tacit collusion—the impact of communication in oligopoly experiments. *European Economic Review*, *56*(8), 1759–1772.

Fouraker, L., and Siegel, S. (1963). *Bargaining behavior*. New York: McGraw–Hill.

Green, E. J., Marshall, R. C., and Marx, L. M. (2013). Tacit collusion in oligopoly. Working Paper.

Greiner, B. (2004). The Online Recruitment System ORSEE 2.0 - A guide for the organization of experiments in economics. University of Cologne Working Paper Series, 10.

Gürerk, Ö., and Selten, R. (2012). The effect of payoff tables on experimental oligopoly behavior. *Experimental Economics*, *15*(3), 499–509.

Güth, W., Levati, M. V., Sutter, M., and van der Heijden, E. (2007). Leading by example with and without exclusion power in voluntary contribution experiments. *Journal of Public Economics*, *91*(5-6), 1023–1042.

Holt, C. (1985). An experimental test of the consistent-conjectures hypothesis. *The American Economic Review*, *75*(3), 314–325.

Holt, C. A., and Davis, D. (1990). The effects of non-binding price announcements on posted-offer markets. *Economics Letters*, *34*(4), 307–310.

Huck, S., Konrad, K. A., Mueller, W., and Normann, H.-T. (2007). The merger paradox and why aspiration levels let it fail in the laboratory. *The Economic Journal*, *117*, 1073–1095.

Huck, S., Muller, W., and Normann, H.-T. (2001). Stackelberg beats Cournot—on collusion and efficiency in experimental markets. *The Economic Journal*, *111*(474), 749–765.

Huck, S., Müller, W., and Normann, H.-T. (2004). Strategic delegation in experimental markets. *International Journal of Industrial Organization*, *22*(4), 561–574.

Huck, S., Normann, H.-T., and Oechssler, J. (1999). Learning in Cournot oligopoly - an experiment. *The Economic Journal*, *109*(454), 80–95.

Huck, S., Normann, H.-T., and Oechssler, J. (2000). Does information about competitors' actions increase or decrease competition in experimental oligopoly markets? *International Journal of Industrial Organization*, *18*, 39–57.

Huck, S., Normann, H.-T., and Oechssler, J. (2004). Two are few and four are many: number effects in experimental oligopolies. *Journal of Economic Behavior and Organization*, *53*(4), 435–446.

Ivaldi, M., Jullien, B., Rey, P., Seabright, P., and Tirole, J. (2003). The economics of tacit collusion. *Final Report for DG Competition - European Commission* (March).

Karakostas, A., and Zizzo, D. J. (2012). Obedience. Working Paper, 1–33.

Mason, C. F., Phillips, O. R., and Nowell, C. (1992). Duopoly behavior in asymmetric markets: An experimental evaluation. *The Review of Economics and Statistics*, *74*(4), 662–670.

Mason, C. F., Phillips, O. R., and Redington, D. B. (1991). The role of gender in a non-cooperative game. *Journal of Economic Behavior and Organization*, *15*, 215–235.

Potters, J., and Suetens, S. (2013). Oligopoly experiments in the current millennium. *Journal of Economic Surveys*, *27*(3), 439–460.

Requate, T., and Waichman, I. (2010). "A profit table or a profit calculator?" a note on the design of Cournot oligopoly experiments. *Experimental Economics*, *14*(1), 36–46.

Robin, S., Rusinowska, A., and Villeval, M.-C. (2012). Ingratiation and favoritism: experimental evidence. CES Working Papers, 2012.32.

Sally, D. (1995). Conversation and cooperation in social dilemmas: a meta-analysis of experiments from 1958 to 1992. *Rationality and Society*, *7*, 58–92.

Selten, R. (1973). A simple model of imperfect competition, where 4 are few and 6 are many. *International Journal of Game Theory*, *2*(1), 141–201.

Sen, A. (2003). Higher prices at Canadian gas pumps: international crude oil prices or local market concentration? An empirical investigation. *Energy Economics*, *25*(3), 269–288. doi:10.1016/S0140-9883(02)00097-X

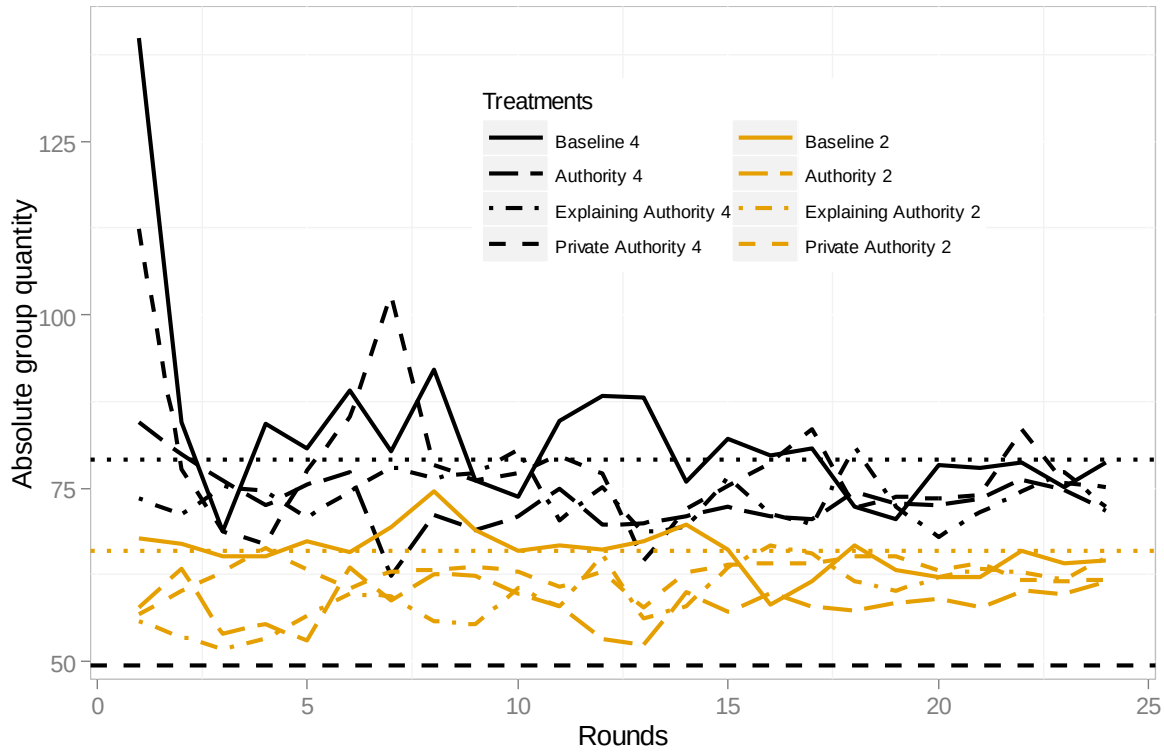
Silverman, D., Slemrod, J., and Uler, N. (2012). Distinguishing the role of authority "in" and authority "to." Working Paper.

Sklivas, S. D. (1987). The strategic choice of managerial incentives. *The Rand Journal of Economics*, *18*(3), 452–458.

Stöber, J. (2001). The social desirability scale-17 (SDS-17): convergent validity, discriminant validity, and relationship with age. *European Journal of Psychological Assessment*, *17*, 222–232.

Vickers, J. (1985). Delegation and the theory of the firm. *The Economic Journal*, *95*, 138–147.

Figure 1: Average absolute group quantities per treatment and round



Note: The dotted horizontal lines at 79.2 and 66.0 indicate the Nash prediction for 4 firm and 2 firm settings, respectively. The dashed horizontal line at 49.5 indicates the collusive production quantity.

Table 1: Summary statistics for the average total quantities

		Players	Nash	Coll.	B	A	EA	PA	Overall
Overall	Mean (median)	4	79.2	49.5	82.5 (77)	73.1 (72.8)	73.7 (69.8)	78.4 (74.7)	76.8 (74)
		2	66	49.5	65.9 (64.2)	58.5 (59.8)	59.7 (60)	62.6 (63.2)	61.7 (62)
	Nash ratio	4	1	0.63	1.04	0.92	0.93	0.99	0.97
		2	1	0.75	1.00	0.89	0.90	0.95	0.93
First 6 Rounds only	Mean (median)	4	79.2	49.5	91.2 (82)	77.7 (75.8)	73.3 (69.9)	81.4 (67.8)	80.7 (74.6)
		2	66	49.5	66.4 (65)	57.9 (56.5)	55.1 (55)	61.7 (62.5)	60.3 (60)
	Nash ratio	4	1	0.63	1.15	0.98	0.93	1.03	1.02
		2	1	0.75	1.01	0.88	0.84	0.93	0.91
Last 6 Rounds only	Mean (median)	4	79.2	49.5	76.6 (76.5)	73.6 (74.5)	72.6 (69.8)	75.9 (75)	74.6 (74.5)
		2	66	49.5	63.7 (64)	59.4 (61)	62.6 (61.9)	61.7 (62.5)	62.2 (64)
	Nash ratio	4	1	0.63	0.97	0.93	0.92	0.96	0.94
		2	1	0.75	0.97	0.90	0.95	0.95	0.94

Notes: B, A, EA and PA denote the treatments Baseline, Authority, Explaining Authority and Private Authority, respectively. Nash denotes the Nash-Cournot prediction and Coll. describes the collusive output level. The Nash ratio is defined as the actual market production divided by Nash prediction for the overall market production.

Table 2: Regressions on group level Nash ratios

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
B	0.119**	0.119**	0.119**	0.119**	0.119**	0.119**	0.119**	0.119**
	(0.050)	(0.047)	(0.050)	(0.047)	(0.050)	(0.047)	(0.050)	(0.047)
EA	0.008	0.008	0.008	0.008	0.008	0.009	0.008	0.009
	(0.049)	(0.046)	(0.049)	(0.046)	(0.049)	(0.046)	(0.049)	(0.046)
PA	0.067	0.067*	0.067	0.067*	0.067	0.067*	0.067	0.067*
	(0.049)	(0.039)	(0.049)	(0.039)	(0.049)	(0.040)	(0.049)	(0.040)
2 Firms	-0.037	-0.037	-0.037	-0.037	-0.038	-0.040	-0.038	-0.040
	(0.049)	(0.040)	(0.049)	(0.040)	(0.049)	(0.039)	(0.049)	(0.039)
B * 2 Firms	-0.006	-0.006	-0.006	-0.006	-0.006	-0.003	-0.006	-0.003
	(0.071)	(0.070)	(0.071)	(0.070)	(0.071)	(0.070)	(0.071)	(0.070)
EA * 2 Firms	0.010	0.010	0.010	0.010	0.011	0.016	0.011	0.016
	(0.070)	(0.065)	(0.070)	(0.065)	(0.070)	(0.067)	(0.070)	(0.067)
PA * 2 Firms	-0.004	-0.004	-0.004	-0.004	-0.004	-0.002	-0.004	-0.002
	(0.070)	(0.057)	(0.070)	(0.057)	(0.070)	(0.057)	(0.070)	(0.057)
Period			-0.011***	-0.011**			-0.011***	-0.011**
			(0.002)	(0.005)			(0.002)	(0.005)
Period_2			0.000***	0.000**			0.000***	0.000**
			(0.000)	(0.000)			(0.000)	(0.000)
Stage 2			-0.013	-0.013			-0.013	-0.013
			(0.012)	(0.020)			(0.012)	(0.020)
Soc.Des.					0.000	0.000	0.000	0.000
					(0.001)	(0.002)	(0.001)	(0.002)
Male					0.001	0.006	0.001	0.006
					(0.008)	(0.015)	(0.008)	(0.015)
Age					0.000	0.000	0.000	0.000
					(0.001)	(0.001)	(0.001)	(0.001)
Economics					0.002	0.023*	0.002	0.023*
					(0.008)	(0.012)	(0.008)	(0.012)
Intercept	0.923***	0.923***	0.992***	0.992***	0.921***	0.907***	0.989***	0.975***
	(0.035)	(0.020)	(0.036)	(0.038)	(0.042)	(0.039)	(0.043)	(0.052)
AIC	385.815		362.350		433.053		409.626	
BIC	454.073		451.080		528.607		525.648	
Log Likelihood	-182.907		-168.175		-202.526		-187.813	
R^2		0.033		0.041		0.034		0.042
Adj. R^2		0.032		0.039		0.033		0.040

Notes: Number of observations: 6816. The baseline for the above regressions is the 4 firm Authority treatment. B, EA and PA represent dummies for the Baseline, the Explaining Authority and the Private Authority treatment, respectively. 2 Firms and Stage 2 denote dummies for a market size of 2 and the Rounds 13-24 respectively. Soc.Des. denotes a numeric measure for social desirability. Age is the numeric age in years. Male and Economics are dummies that are one if the participant was male and an economics major, respectively. Models 1, 3, 5 and 7 are estimated with random effects on group level; Models 2, 4, 6 and 8 estimated with robust standard errors clustered at group level; standard errors in parentheses[28]; Significance levels of coefficients: ***p < 0.01, **p < 0.05, *p < 0.1

[28] We did not estimate a Tobit model as the lowest and highest observed values for the group Nash ratios were 0.15 and 3.36, respectively.

Table 3: Proportion of production quantities less than, equal to and greater than the requested quantity and the proportion of production quantities equal to the theoretical Nash prediction

	Treatment	Less than requested	Exact match request	More than requested	Exact match Nash
Overall	B4	0.330	0.000	0.670	0.000
	A4	0.180	0.140	0.680	0.003
	EA4	0.160	0.300	0.540	0.000
	PA4	0.120	0.170	0.720	0.002
	B2	0.110	0.000	0.890	0.049
	A2	0.140	0.090	0.770	0.016
	EA2	0.120	0.220	0.660	0.023
	PA2	0.070	0.160	0.760	0.056
First 6 Rounds only	B4	0.470	0.000	0.530	0.000
	A4	0.250	0.150	0.600	0.000
	EA4	0.210	0.370	0.420	0.000
	PA4	0.250	0.180	0.570	0.000
	B2	0.200	0.000	0.800	0.021
	A2	0.270	0.040	0.690	0.000
	EA2	0.250	0.230	0.520	0.007
	PA2	0.170	0.150	0.690	0.028
Last 6 Rounds only	B4	0.240	0.000	0.760	0.000
	A4	0.100	0.110	0.790	0.003
	EA4	0.140	0.230	0.630	0.000
	PA4	0.060	0.140	0.810	0.007
	B2	0.090	0.000	0.910	0.021
	A2	0.060	0.150	0.790	0.035
	EA2	0.010	0.190	0.790	0.049
	PA2	0.030	0.190	0.780	0.153

Notes: B, A, EA and PA denote the treatments Baseline, Authority, Explaining Authority and Private Authority, respectively. The numbers 4 and 2 indicate the number of firms in the market.

Appendix

A. Experimental Instructions

Printed below are instructions for the 4 firm Baseline setting. Parts in [ ] were added to the baseline instruction in the A treatment. In the EA treatment, the A treatment instructions were extended by parts in [[ ]]. Parts in { } were added to or cut from the baseline instructions in the PA treatment. Instructions for 2 firms treatments were identical with the necessary changes for the different number of firms.

Instructions

Introduction

Welcome to today's experiment on decision making. The session will begin shortly. Before we start, we have a few reminders. First, to help us keeping the lab neat and tidy, we ask you not to eat or drink in the lab. Also, we ask you to turn off your mobile phone and other devices completely. Please refrain from talking to other participants during the experiment. If you have a question at any point in the experiment, please raise your hand.

In this experiment, you will repeatedly make decisions. By doing this you can earn money. How much you earn depends on your decisions and on the decisions of other participants.

This experiment consists of 2 stages lasting 12 rounds each. You will receive additional instructions on screen before stage 2. {~~All participants receive the same instructions.~~} Your decisions will be absolutely anonymous, i.e. your identity will neither be revealed to your co-participants nor to the experimenters at any time during or after the experiment.

Groups of firms

In this experiment, you will be matched with three other participants to form a group. These groups of four will stay the same throughout the full duration of the experiment.

You represent a manger in a firm that, like the three other firms in your group, produces and sells one and the same product in a market. The costs of production are 1 experimental currency unit (ECU) per unit of output (this holds for all firms). All firms will always have to make one decision, namely, set the quantity they wish to produce. Every firm can produce from 0 to 100 units of output in every round.

Profits

The following important rule holds: the larger the total quantity of all firms in your group, the smaller the price that will emerge in the market. Moreover, the price will be zero from a certain amount of total output upwards. Note that the market will always be cleared, i.e. whatever price results from the total produced quantity every firm will sell all of its quantity. Your profit per unit of output will then be the difference between the market price and the unit cost of 1 ECU. Your profit per round is thus equal to the profit per unit multiplied by the number of units you sell. Note that you can make a loss, if the market price is below the unit costs.

In each round the outputs of all four firms of your group will be registered, the corresponding price will be determined and the respective profits will be computed. From the second period on, you will learn about the average output produced by the other firms, your own output, the resulting market price and your own profit in the previous period, in every period.

Furthermore, you will have access to a profit calculator. You can use it to simulate your profit for arbitrary quantity combinations. You can enter two values, an average quantity for the other companies in your group and a quantity for yourself. The profit calculator then tells you what your profit would be, given the typed in quantities. You can use the profit calculator to simulate as many combinations as you want before each actual decision.

Final Payment

This experiment has 2 stages that last 12 rounds each. At the end of the experiment one winning round per stage is chosen at random. Whatever your earnings in these two rounds were, they are summed up and converted into pounds at the rate of 80 ECU = 1 pound. If the sum of your stage 1 and stage 2 earnings is negative, this loss will be deducted from your participation fee of 3 pounds.

{[Request]}

{[You are entirely free to produce as few or as many units of output as you like (from 0 to 100).]}

[That said, we would ask you (and your co-participants) to produce 12.4 units of output. We are telling this not just to you but also to your co-participants.]

[[The reason you should do this is that, if you and your co-participants produce 12.4 units of output, the total profits of you and your co-participants will be the highest.]]

[[You can use the profit calculator to check the profitability of producing 12.4 units of output per firm.]]

{That said, we would ask you to produce a specific number of units. This number will be displayed on your computer screen soon. When you see the number on the screen, please write it down here: _____.}

Please remain seated until the experimenter tells you to collect your payment. Before starting to take decisions, we ask you to fill in a questionnaire. The only purpose of the questionnaire is to check whether you have understood these instructions.

B. Further Analysis

Response dynamics. We analyze the response dynamics across treatments following the procedure used by Huck et al. (1999); see Table 4. By defining two heuristics that subjects might have used to respond to the behavior of other group members, we can analyze which heuristic subjects on average were more likely to use under which treatment condition. A participant is described to use a ‘Best response’ heuristic if he or she sets quantity such that it maximizes his or her payoff, provided that the other participants in the group produce the

same quantity in total as in the previous round. A participant is deemed to 'Imitate the average' if he or she produces the average of the other group members' production in the previous round. If BR_i^{t-1} is the best response of subject i to period $t-1$, IA_i^{t-1} denotes the imitated average of subjects $-i$ in period $t-1$, estimating $q_i^t - q_i^{t-1} = \beta_1(BR_i^{t-1} - q_i^{t-1}) + \beta_2(IA_i^{t-1} - q_i^{t-1})$ makes visible which of the two heuristics can best explain the observed behavioral dynamics.[29] We find more explanatory power of the 'Best response' and 'Imitate the average' heuristics in the A, EA and PA treatments with 4 firms than with 2 firms.[30]

Table 4: Regressions on change in individual quantity ($q_t - q_{t-1}$)

Part A	**B4**	**A4**	**EA4**	**PA4**	**B2**	**A2**	**EA2**	**PA2**
Best response	0.321***	0.289***	0.262***	0.355***	0.273***	0.357***	0.226***	0.315***
	-0.04	-0.029	-0.047	-0.034	-0.063	-0.097	-0.05	-0.063
Imitate the average	0.140***	0.353***	0.242***	0.342***	0.161***	0.237***	0.166***	0.05
	-0.05	-0.099	-0.079	-0.062	-0.053	-0.043	-0.038	-0.056
R^2	0.273	0.374	0.268	0.405	0.257	0.307	0.222	0.246
Adj. R^2	0.272	0.372	0.267	0.404	0.254	0.304	0.219	0.243

Part B	**B4**	**A4**	**EA4**	**PA4**	**B2**	**A2**	**EA2**	**PA2**
Best response	0.365***	0.364***	0.381***	0.385***	0.345***	0.587***	0.348***	0.450***
	-0.027	-0.025	-0.028	-0.025	-0.032	-0.037	-0.032	-0.033
Imitate the average	0.120***	0.320***	0.193***	0.327***	0.143***	0.179***	0.136***	0.016
	-0.022	-0.024	-0.023	-0.025	-0.024	-0.03	-0.025	-0.018
AIC	7986.67	7789.92	8280.28	8314.92	3932.49	3987.99	3735.18	3420.88
BIC	8006.34	7809.94	8300.3	8334.94	3949.73	4005.23	3752.42	3438.12
Log Likelihood	-3989.3	-3891	-4136.1	-4153.5	-1962.2	-1990	-1863.6	-1706.4
Num. obs.	1012	1104	1104	1104	552	552	552	552

Note: Response dynamics analysis following the procedure by Huck et al. (1999). Individual quantity (t) and individual quantity (t-1) describe the absolute number of units produced per firm in Round t and t-1, respectively. EA, A, PA and B denote the treatments Explaining Authority, Authority, Private Authority and Baseline, respectively. 4 and 2 indicate the number of firms in the market. Each column is estimated with data from the relevant treatment only. The regressions of part A and part B were estimated with group level error clustering and group level random effects, respectively. Significance levels of coefficients: ***p < 0.01, **p < 0.05, *p < 0.1

[29] The larger a regression coefficient in Table 4 is, the more explanatory power the underlying heuristic has.

[30] Huck et al. (1999) also tested for an 'Imitate the best' heuristic, which subjects could not have used in our experiment as they only received information about the other players' average but not individual production levels.