Collusion through price ceilings? In search of a focal-point effect*

Dirk Engelmann† — *Royal Holloway, University of London*

Wieland Müller‡ — *CentER, TILEC & Tilburg University*

June 22, 2009

Abstract

We resume the search for a collusive focal-point effect of price ceilings in laboratory markets. We argue that market conditions in previous studies were unfavorable for collusion which may have been responsible for not finding such a focal-point effect. Our design aims at maximizing the likelihood of a focal-point effect. Nevertheless, our results again fail to support the focal-point hypothesis. Collusion is as unlikely in markets with a price ceiling as in markets with unconstrained pricing. Overall, static Nash equilibrium predicts the data fairly accurately. We argue this might warrant re-interpretation of field studies on anti-competitive effects of price ceilings.

JEL Classification numbers: F13, L13, C92.

Keywords: Collusion, competition policy, experimental economics, focal point.

*We are grateful to Maria Bigoni, Hans Normann, Charles Plott and Sigrid Suetens as well as seminar participants at DIW Berlin, the University of Erlangen-Nuremberg and the University of Padua for helpful comments. Dirk Engelmann acknowledges financial support from the institutional research grant AV0Z70850503 of the Economics Institute of the Academy of Sciences of the Czech Republic, v.v.i. Wieland Müller acknowledges financial help from the Netherlands Organisation for Scientific Research (NWO) through a VIDI grant.

†Department of Economics, Royal Holloway, University of London, Egham, Surrey TW20 0EX, UK, E-mail: *dirk.engelmann@rhul.ac.uk*, Web: *http://personal.rhul.ac.uk/unte/247/.* Dirk Engelmann is also an Affiliate of the Centre for Experimental Economics at the University of Copenhagen and a Senior Researcher at the Economics Institute of the Academy of Sciences of the Czech Republic, v.v.i.

‡Department of Economics, Tilburg University, P.O.Box 90153, 5000LE Tilburg, The Netherlands, E-mail: *w.mueller@uvt.nl,* Web: *http://center.uvt.nl/staff/muller/*

1 Introduction

The theory of competitive markets predicts that the effect of a price ceiling on the level of prices is either negative (in case the ceiling is binding) or zero (in case the ceiling is at or above the competitive price). Industrial economists such as Scherer and Ross (1990), however, have argued that price ceilings can weaken competition as they may serve as collusive focal points for pricing decisions (Schelling, 1960). To see this, note that the Folk Theorem (see for example Tirole, 1988) predicts that infinitely many prices can occur as outcomes of collusive equilibria in infinitely repeated games if the discount factor is sufficiently high. This suggests a coordination problem when firms attempt to collude. Here, a price ceiling may function as a focal point on which firms coordinate. Thus, price ceilings could facilitate tacit collusion and lead to higher prices.

Indeed, there is mounting evidence for a collusive focal-point effect of price ceilings in various markets. Sheahan (1961), studying the effect of price controls in postwar France, reports that "the establishment of official ceilings provides a focus for individual quotations which might otherwise have differed" (p.532). Knittel and Stango (2003) investigate the interest rates of U.S. credit cards in the 1980s where various price ceilings were effective and report that "tacit collusion at nonbinding state-level ceilings was prevalent during the early 1980's." Eriksson (2004, p.1), analyzing the 1999 deregulation of the market for dental services in Sweden, states that the "Swedish government was worried that the ceiling was serving as a focal point for implicit price collusion" and that "a removal of the price ceiling could lead to increased competition and lower prices." DeYoung and Phillips (2006, p.1) document that "over time, payday loan prices in Colorado have gravitated toward the legislated price ceiling." Finally, Ma (2007) studies price ceilings in Taiwan's flour market, and finds evidence that firms in this market set prices "above competitive equilibrium levels during most of the regulation period" and that "[o]bservations on prices also show that all flour firms set their prices equal to ceilings, without exception."

In contrast to this field evidence, laboratory experiments have so far failed to find a collusive focal-point effect of price ceilings. In this paper, we report on a new experiment. We believe that such further research is called for, because earlier studies have made such collusive effects rather difficult. The first studies, Isaac and Plott (1981) and Smith and Williams (1981) find that price ceilings actually lower prices in double auctions. With the benefit of hindsight, this is perhaps not surprising as by now double auction markets are known to reliably converge to the competitive equilibrium, making it very unlikely for any collusive effects of price ceilings to occur. Coursey and

Smith (1983) arguably constitute a stronger case against the focal-point hypothesis, as they find convergence to the competitive equilibrium and thus no collusive effects of price ceilings in posted-offer markets, which are generally more vulnerable to collusion than double auctions. However, in the design of Coursey and Smith (1983) the incentives to collude are very small, because even for the highest price ceiling they study, the total collusive profit would be less than 10% above the equilibrium profit (and some firms might even be worse off). Furthermore, Nash-equilibria (of the game between the four sellers with buyers assumed to maximize payoffs) are in mixed strategies with positive probability on prices above the competitive equilibrium, such that it is possible that the price ceiling is binding with respect to the Nash-equilibrium prices.[1] Finally, the evidence against the focal-point hypothesis reported in Engelmann and Normann (2009), who study posted offer markets with four symmetric sellers but larger incentives for collusion at the price ceiling than in Coursey and Smith (1983) appears to just confirm that collusion with four firms in an experimental market is hard to achieve (see, e.g., Huck, Normann, and Oechssler, 2004).

The design of the experiments reported in this paper has several features that we expected to be conducive to collusive pricing (at the ceiling). First, we have fixed pairs of players interacting repeatedly. This has been shown to lead to higher collusion rates in laboratory markets (see, e.g., Huck, Normann, and Oechssler, 2004). Second, to prevent markets from being highly collusive already in the absence of a price ceiling, we chose asymmetric cost schedules for the sellers such that they disagree about the optimal collusive price. Imposing a price ceiling could potentially resolve this problem by making collusion at the price ceiling the preferred outcome for both firms. Third, the unique Nash equilibrium is the upper end of the competitive price range. Fourth, collusion at the price ceiling, instead of choosing the Nash equilibrium price, would increase profits by up to 31%. Hence, our design provides substantial incentives to collude at the price ceiling and we therefore believe it is better suited to test the hypothesis that price ceilings facilitate tacit collusion. As in previous designs, there are many prices at which firms could collude. This coordination problem might be resolved with a price ceiling since it presents a focal point for coordination.

We note that our design was not chosen with the aim of providing a realistic model of any one specific market but with the aim of designing a market most conducive to an experimental

[1] Due to a lack of detail reported in Coursey and Smith (1983) it is impossible to calculate the Nash equilibrium. For standard distributions of cost schedules (such that no seller sells more than two units in the competitive equilibrium), however, it is evident that there is no pure strategy Nash-equilibrium. Mixed-strategy equilibria in turn have to put positive probability on prices above the competitive price range.

existence proof of a collusive focal-point effect of price ceilings. We aim at achieving this by making collusion not generally easy or difficult, but by trying to maximize the ease of collusion with a ceiling compared to without. We also note that the issue of generalizability of results from experiments where the student subjects take the role of firms, is arguably less of a concern in the present experiment than for typical market experiments. The hypothesis we are testing, namely whether a price ceiling can serve as a focal point to solve a coordination problem is a psychological explanation that is not specific to certain markets and there is a-priori no reason why it should work better or worse for experienced managers than for students. Testing this psychological hypothesis in the laboratory would thus allow us to infer its plausibility in non-experimental markets.[2]

Despite choosing a design conducive to inducing price ceilings to have a collusive effect, price ceilings did not lead to higher average prices in our initial experiments. To the contrary, price ceilings even induced lower prices, typically in the early periods after their imposition. As a next step, in further treatments we removed information about the demand curve. This should make it more likely to induce the hypothesized collusive focal-point effect. That is, by removing common knowledge on kinks in the demand curve that could serve as alternative focal points, we give the hypothesis that price ceilings could lead to more collusion an even better shot. Hence, we conducted an additional series of markets in which we kept features of the first treatment, but gave sellers no information about the precise shape of the demand curve. More precisely, sellers were only informed that there is an unspecified number of simulated buyers who each have a certain (maximum) willingness to pay for a unit of the good without specifying what the buyers' maximal willingness to pay was. The markets with this minimal information design again did not show an increase of average prices when a price ceiling was in place, but, as before, a decrease of average prices. Apart from an absence of an effect of average prices, we neither find evidence of a focal point in the sense of clustering of prices at the ceiling (prices at the ceiling are more likely to result from censoring) nor an increase in the number of collusive markets in the presence of a price ceiling, nor an increase in a profit-based measure of collusiveness.

[2] Typical reasons why the results from market experiments may not generalize to actual product markets are that other-regarding preferences may be more important in the laboratory than in markets and that market participants are frequently more experienced than experimental subjects (in particular if firms are the focus). Other-regarding preferences might matter in our experiment, but the issue here is one of collusion, which is mutually beneficial both in the laboratory and the market. If decision makers are more experienced, we would rather expect them to find more sophisticated ways to coordinate than relying on focal points. Thus the role of a price ceiling as a focal point would appear to be rather more important in the laboratory than outside.

Our observations, together with previous experimental results, suggests that a simple focal-point effect might not be sufficient for inducing collusion and hence that other aspects might also matter in the field. Specifically, we discuss below that a price ceiling might also be interpreted as a signal of impunity for prices up to that level, which reduces the (perceived as well as possibly actual) risk of collusion. In the laboratory, where collusion is not illegal and not punished, such a signal would be irrelevant. Hence this hypothesis can better explain than the focal point hypothesis why price ceilings appear to have anti-competitive effects in the field, but not in the laboratory.

We describe the details of our experimental markets, the theoretical predictions, and the implementation in Section 2. In Section 3 we present the results concentrating both on average prices and their distribution and on indices measuring the degree of collusion observed in our markets. Finally, we briefly summarize and discuss our results and their implications in Section 4.

2 Experimental Design

2.1 The Experimental Market

Our experimental markets were designed to facilitate collusion at a price ceiling, while making collusion in the absence of a price ceiling relatively difficult. First, since double-auctions very robustly converge to the competitive equilibrium, we chose a posted-offer market design. Second, we are not interested in buyer behavior and buyers might resist high collusive prices, so we chose to simulate buyers. This also allowed us to implement a simple rule of how demand is split if the sellers set equal prices.[3] Moreover, using simulated buyers facilitates collusion as sellers with other-regarding preferences might refrain from colluding if buyers become substantially worse off.[4] A posted-offer market with simulated buyers is effectively just a Bertrand-game. We framed the experiments, however, like a posted-offer market, i.e. giving an explicit rule how the (simulated) buyers would shop after sellers set their prices. This was done to facilitate understanding of the market mechanism and also to make our study closely comparable to previous studies.

---

[3] The absence of this rule notoriously complicates the calculation of Nash-equilibria in posted-offer markets. See e.g. Borck et al. (2002) and below for a discussion.

[4] Other obvious advantages are that simulating buyers economizes on the number of experimental sessions and their costs as well as reduces noise due to potentially erratic buyer behavior. The most obvious disadvantage is that we remove the design further from the field. As argued above, however, the purpose of our study is to attempt to generate an existence proof for a focal-point effect of price ceilings in the laboratory and not to mimic any field market as closely as possible.

Unit	1	2	3	4	5	6
MC for Seller 1	1	14	14	23	31	31
MC for Seller 2	7	7	17	22	32	32

Table 1: Marginal costs for the sellers in the experimental market.

Willingness to pay	48	29	21	13
Number of buyers	2	2	2	2

Table 2: Willingness to pay for the eight (simulated) buyers in the experimental market.

In our markets, the same two sellers interact repeatedly. This is obviously the most likely scenario in order to generate collusion.[5] To prevent markets from being highly collusive already in the absence of a price ceiling, we chose asymmetric cost schedules for the sellers such that they disagree about the optimal collusive price. Specifically, each seller could produce up to six units and marginal costs are as in Table 1. Costs are only incurred for units actually sold. In each market there were eight buyers who could buy one unit each. Their willingness to pay is given in Table 2. The demand and supply curves are shown in Figure 1. Sellers post their prices simultaneously and only once in every period. Price offers were restricted to integers. Our design differs from standard posted-offer markets in various aspects. Sellers are automatically committed to sell (given sufficient demand) the maximal quantity such that marginal costs do not exceed the price. Simulated buyers are rationed efficiently, that is they buy in order of decreasing willingness to pay and from sellers in order of increasing price. If several sellers charge equal prices, buyers split their demand equally.[6]

[5] We note that with finite repetition collusion is theoretically less likely than in an infinitely repeated game. An infinitely repeated game cannot be implemented in the laboratory. Nevertheless, experimental evidence suggests that subjects do only few steps of backward induction. For example, Normann and Wallace (2006), comparing finitely repeated games with indefinitely repeated games (i.e. games where the exact number of repetitions is unknown to the subjects) find little difference in overall cooperation rates in Prisoner's Dilemma games and end-game effects confined to the last few periods.

[6] Posted-offer markets are often plagued by a multiplicity of Nash equilibria. Moreover, these are often derived assuming that buyers split their demand equally between sellers with identical prices (see, e.g. Davis and Holt, 1993), which even rational human buyers have no reason to do (and the experimental software would usually not even allow them to do so). Abolishing this assumption in general destroys all pure strategy Nash equilibria. Implementing the above rule we elimate this problem which simplifies the calculation of expected profits, eliminates any randomness and, for our specific cost and demand schedules, allows us to determine a unique pure-strategy Nash equilibrium. Sellers were also informed that units sold were not restricted to integers. While selling half-units can actually not

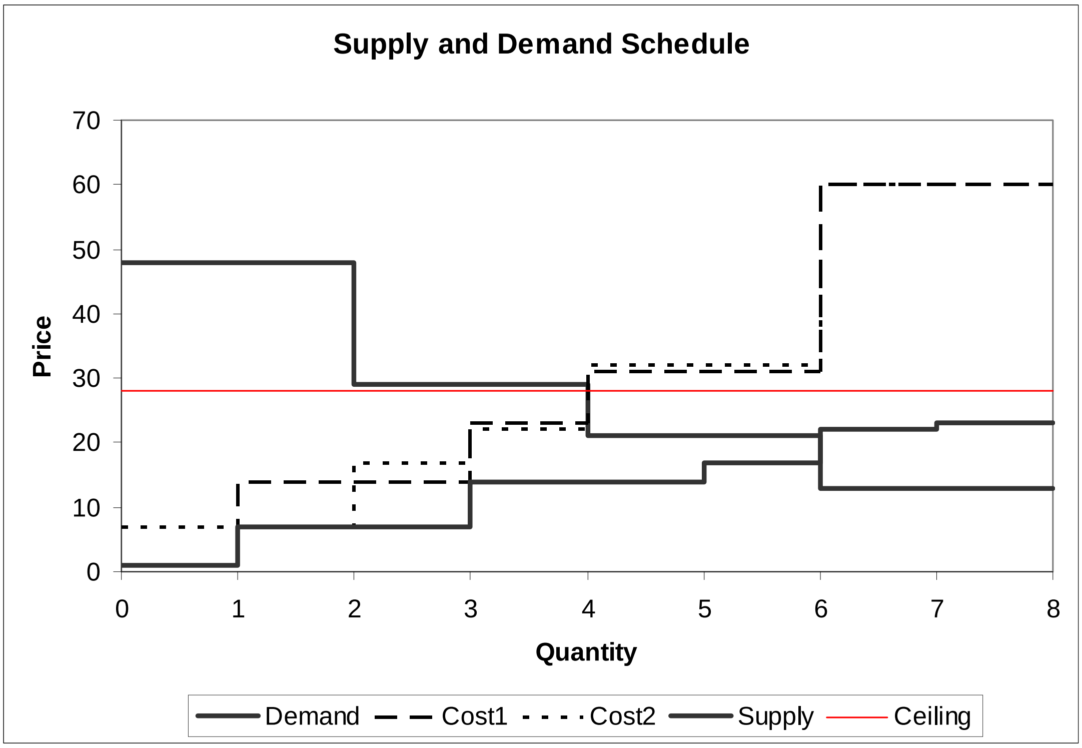

Figure 1: Supply and demand in the experimental market.

Note: The supply curve and demand curve overlap for prices between 17 and 21, which gives us thus a competitive price range of [17, 21], with the upper end equal to the unique Nash-equilibrium price.

Given the cost and demand schedules, an individual seller could actually never sell more than four units and sellers could in total not sell more than six units. Additional buyers and units were added in order to prevent sellers to focus immediately on the range of prices close to the Nash equilibrium or the collusive outcome.

2.2 Equilibrium Predictions

Inspecting Figure 1, it is easy to see that the competitive price range is $[17, 21]$ with market demand and supply both equal to six units. Each seller sells three units and at the upper end of the competitive price range (which is also the unique Nash equilibrium, as we will show below), profits are $\pi_1 = 3 \times 21 - (1 + 14 + 14) = 34$ and $\pi_2 = 3 \times 21 - (7 + 7 + 17) = 32$.

Obviously, the only candidates for joint-payoff maximizing prices are at the kinks of the

happen for the given cost and demand schedules, sellers in the treatment without information about the buyers' willingness to pay could not infer this.

	Nash Price	Nash Profit	Cartel Price	Cartel Profit
Seller 1	21	34	48	47
Seller 2	21	32	48	41

Table 3: Profits for both sellers in Nash equilibrium (which is also the upper end of the competitive price range) and in the joint-profit maximizing cartel.

demand curve, i.e. $P = 29$ and $P = 48$, because any intermediate price will lead to the same demand as at the next kink. Joint profits are maximized at $P_1 = P_2 = 48$. Each seller then sells one unit and $\pi_1 = 48 - 1 = 47$ (38% more than in the Nash equilibrium) $\pi_2 = 48 - 7 = 41$ (28% more than in Nash equilibrium). Seller 2, however, would prefer to collude at $P_1 = P_2 = 29$. Then each seller sells two units and $\pi_1 = 58 - 15 = 43$ (27% more than in the Nash equilibrium) $\pi_2 = 58 - 14 = 44$ (38% more than in the Nash equilibrium). Note that we intentionally designed the markets such that the sellers do not agree on their favorite collusive outcome, in order to hinder collusion without a price ceiling and thus maximize the possible impact of the price ceiling on collusion. We summarize the Nash and cartel prices and profits in Table 3. The unique Nash equilibrium is derived in the following proposition.[7]

Proposition 1 *The unique Nash equilibrium is both sellers setting $P_1 = P_2 = 21$. In this equilibrium, each seller sells three units and profits are $\pi_1 = 34$, $\pi_2 = 32$.*

Proof: That each seller sells three units at the given prices follows immediately from the cost and demand schedules. The profits were already derived above. We first show that setting $P_1 = P_2 = 21$ is an equilibrium. (*i*) Deviating to a lower price yields a lower profit: Given that the cost for the fourth unit are larger than 21, a firm would sell at most three units at any price lower than 21, which yields a lower profit than selling three units for 21. (*ii*) Deviating to a higher price does not pay: note that for prices larger than 21, demand is at most 4. The other seller offers 3 units at $P = 21$, so the residual demand is at most 1. Hence the best possible alternative is the maximal price where demand is equal to 4, i.e. $P = 29$. The profit is then $29 - 1 = 28 < 34$ for Seller 1 and $29 - 7 = 22 < 32$ for Seller 2, so deviating

[7] We note that we are calculating the Nash equilibrium as if sellers knew the cost function of the other firm. While this is not legitimate early on in the game, they can reasonably quickly learn their demand curves. Thus for experienced players the Nash equilibrium is a plausible benchmark. The same is true, albeit with more to be learned, for the markets where sellers do not know the demand function.

does not pay.

We next show that there is no other pure-strategy equilibrium. Note first that by setting $P_i = 21$, seller i always guarantees itself the Nash profit, because if seller j charged a lower price, j would offer no more than three units and hence the residual demand is at least 3. Since seller i would not offer more than three units, the profit when charging $P_i = 21$ will never be higher than in Nash, no matter what the other seller does. Thus, there cannot be a symmetric equilibrium at a price $P_1 = P_2 < 21$, because seller i would sell at most three units at this price, but can still sell 3 units at $P_i = 21$ and hence deviating to $P_i = 21$ would pay. Furthermore, there cannot be a symmetric equilibrium at a price $P_1 = P_2 > 21$. In any such equilibrium, both sellers would sell at most two units each. By deviating to the nearest lower integer price, seller i will always sell at least one more unit at a price of at least 21 and hence make additional profit of at least 7 (Seller 1) or 4 (Seller 2) while only lowering the price by 1 on at most two units. So the total profit definitely increases by deviating to a lower price. There cannot be an asymmetric equilibrium either. If $P_i < 21$, we see as for the case of a symmetric equilibrium with a price smaller than 21, that i can still sell 3 units at a higher price by deviating to $P_i = 21$. So let $P_1 \neq P_2$ and $P_1 \geq 21$, $P_2 \geq 21$. Then demand at the higher price is at most 4, while supply of the low-price seller is at least 3, so the residual demand for the high price seller i is at most 1 and for sure 0 if $P_i > 29$. So his maximal profit is 28 ($i = 1$) or 22 ($i = 2$), which is smaller than the profit from deviating to $P_i = 21$.

As the last step, we show that there is no mixed equilibrium. Since we have finitely many prices, each price in the support of the equilibrium would be chosen with positive probability (i.e. we have no density, the distribution has positive mass on each point of the support). No price $P < 21$ can be played with positive probability, because $P = 21$ strictly dominates each smaller price, as shown above. Let $P^m > 21$ be the highest price that is chosen with positive probability by either of the sellers. Let P^k be any price played by the other seller with positive probability. Thus $21 \leq P^k \leq P^m$ (with at least one of the inequalities strict). The same argument that we used above to show why there is no symmetric equilibrium with $P_1 = P_2 > 21$ implies that $P = 21$ is a better response than P^m to $P^k = P^m$. Furthermore, it is straightforward that if $P = 21$ is better against P^m than P^m, it is definitely better than P^m against any $P^k < P^m$ (because if the other seller sets a price $P^k < P^m$, the high price seller sells less than if $P^k = P^m$, while the profit at $P = 21$ is unaffected by the price of the other seller). Thus $P = 21$ yields a higher profit than P^m against any price that the other seller chooses with positive probability. Hence P^m cannot be played with positive probability, contradicting the assumption that there is a mixed equilibrium were a price > 21 is played with positive probability. This completes the proof.

2.3 Implementation

Two sellers interacted in the same market for 60 periods. A price ceiling was in place either in the first 30 periods (Treatment "YesNo") or in the last 30 periods (Treatment "NoYes"). Sellers were informed at the beginning of the experiment that a change in market conditions would occur after 30 periods without pointing out that this would relate to the introduction or the abolishment of a price ceiling. Written instructions were identical in both treatments and did not make a reference to price ceilings (the instructions can be found in the appendix). Treatment YesNo then started with a computer screen that informed participants that as an additional rule to what was stated in the written instructions a price ceiling would be in place. The price ceiling was always $C = 28$. We choose this price ceiling, because at this price joint profits are maximized subject to the constraint of the ceiling being below the kink in the demand curve at 29. Obviously, the ceiling needed to be different than either of the kinks to distinguish collusion at the ceiling from collusion at a kink. Given that, any ceiling larger than 29 would yield lower joint profits than collusion at 29, which would have lead to two competing candidates for collusive prices and hence made an effect of the price ceiling less likely. Note that by pricing at the ceiling, instead of the Nash-equilibrium price of 21, Seller 1 and Seller 2 would increase profits by about 21% and 31%, respectively.

Treatments furthermore differed by the information sellers had about the buyers. In the first two treatments (YesNo-Info and NoYes-Info), sellers were informed about the buyers' willingness to pay. In the second two treatments (YesNo-NoInfo and NoYes-NoInfo) they were only informed that each buyer had a given willingness to pay (they were also not informed about the number of buyers). This yields a 2×2 design as given in Table 4. Without demand information colluding in the absence of a price ceiling is particularly (and intentionally) difficult. This increases the scope for the price ceiling to make collusion more likely, as it provides a unique focal point.

In each of the Info treatments we had 17 pairs of sellers and in each of the NoInfo treatments we had 18 pairs. Hence our total number of subjects was 140, yielding a total of 8400 price choices. The Info treatments were run at Royal Holloway, and the NoInfo treatments at Tilburg University. In the Info treatments experimental points were converted into Pounds Sterling at a rate of 150 points = £1 and in the NoInfo treatments into Euros at a rate of 100 points = EUR 1,

	Ceiling in Periods 1-30	Ceiling in Periods 31-60
Information	YesNo-Info	NoYes-Info
No Information	YesNo-NoInfo	NoYes-NoInfo

Table 4: Overview of the experimental treatments.

reflecting roughly the official exchange rate at the time of the experiments. Participants received no show-up fee in addition. Before the first round was started subjects were asked to answer control questions. We checked whether answers were correct in order to make sure that everybody had an understanding of how the market works.

All sessions were computerized, using z-Tree (Fischbacher, 2007). Sessions took about 90 minutes and average earnings were about EUR 18.

3 Experimental Results

In the following discussion of the experimental results, we will study the effects of a price ceiling on various measures, namely the average prices, the number of high prices chosen, the number of colluding firm pairs and the degree of collusiveness. The focal point hypothesis predicts that in the presence of a price ceiling the firms in a market will more frequently manage to coordinate on a price than without a ceiling and specifically that the price they coordinate on will equal the ceiling. In addition to providing a focal point, a price ceiling might also enable collusion simply because it reduces the strategy space, which makes it easier to coordinate on any one supra-competitive price. Hence, before addressing the more specific prediction of collusion at the potential focal point, we take a broader look at the overall effects of the presence of the price ceiling and start with a look at the development of average prices by treatment.

Figure 2 shows average prices across periods for the treatments with demand information and Figure 3 for the treatments without demand information. These are averages of the posted prices, not weighted by quantity sold. Inspecting Figures 2 and 3, we make a number of observations. First, the treatments that start with a price ceiling (YesNo) show an almost identical pattern with and without demand information. Specifically, average prices are only slightly above the Nash-equilibrium price 21 and do not vary much within the first 30 periods with a price ceiling in place. Abolishing the price ceiling leads to an "explosion" of average prices above the former ceiling (28), but they then quickly decline and tend towards about 23. Such price explosions have also been

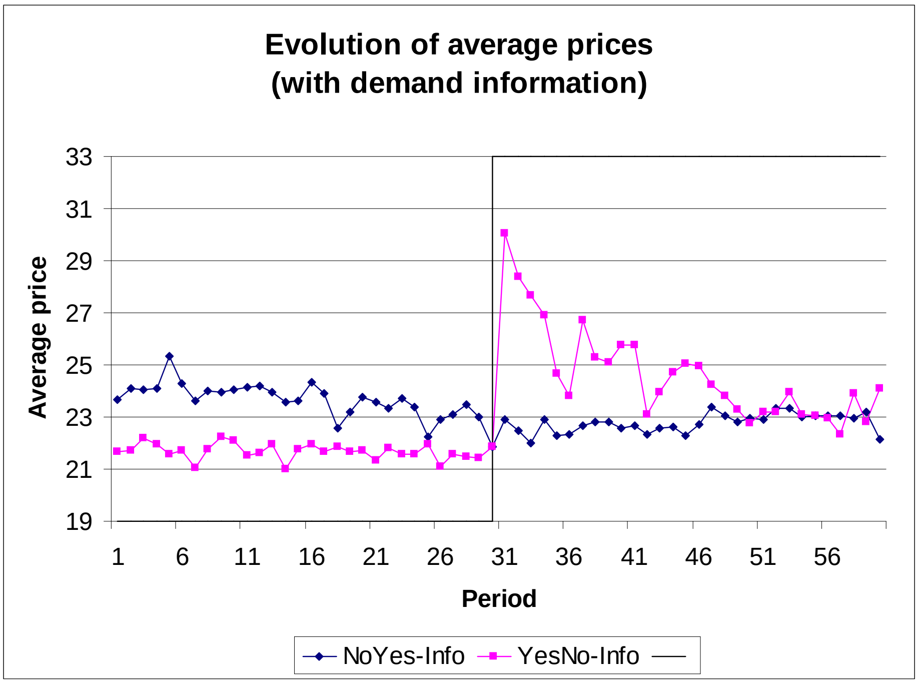

Figure 2: Development of average prices across time in the treatments with demand information.
Note: After Period 30 the price ceiling was removed in treatment YesNo and introduced in treatment NoYes.

observed in the previous experiments on price ceilings. In order to eliminate any biases based on these temporary effects, we will below also look separately at the latter half of each phase. Second, for both information conditions in the treatments that start without a price ceiling (NoYes) average prices decline slightly over the first 30 periods and the introduction of the price ceiling has no notable effect, with prices being nearly constant in the last 30 periods. However, for the NoYes treatments prices are consistently about 2 units higher with demand information than without. This appears to be driven by more widespread attempts (in both phases) to collude in the NoYes markets with information than without information (in the former, 19% of prices are ≥ 28, while in the latter only 5% of prices are ≥ 28).[8] Third, addressing our main question, across-treatment comparisons show that if a price ceiling has an effect, it *lowers* rather than increases average prices. Specifically, with demand information prices are higher without a ceiling except for about the last 15 periods,

[8]This could be simply the result of subjects in this session being more eager to cooperate. Alternatively, given demand information, in the absence of the ceiling, attempting to collude at $p = 29$ or $p = 48$ are attractive choices (and account for 6% and 2% of price choices), which drives up average prices. These attempts to collude could then translate to attempts to collude at the ceiling in the second phase, leading to higher prices throughout.

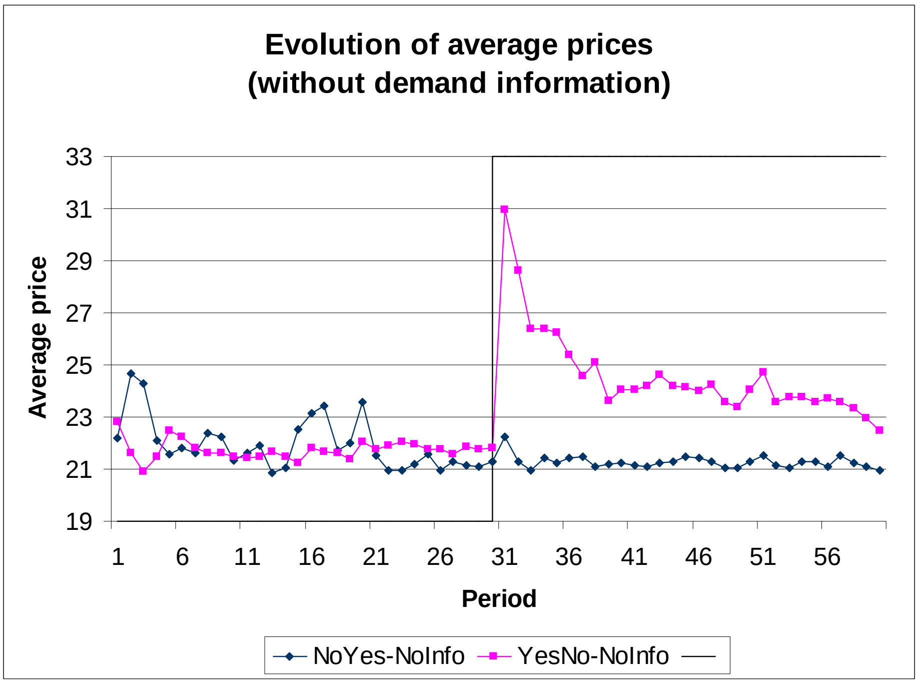

Figure 3: Development of average prices across time in the treatments without demand information. Note: After Period 30 the price ceiling was removed in treatment YesNo and introduced in treatment NoYes.

where they are essentially equal. Without demand information, there is essentially no difference in the first 30 periods, while in the last 30 periods, prices are consistently lower with a price ceiling.

Corresponding aggregate data and the results of statistical tests are presented in the upper half of Table 5. The collusion index data presented in the lower part of the table will be discussed below. We focus on the price data for the moment. The table contains average prices across pairs and blocks of periods for the various treatments. From left to right, the first column shows averages across the whole first phase (periods 1-30), the second column across the whole second phase (periods 31-60), the next two columns the averages across the first halves of the respective phases (periods 1-15 and 31-45, respectively) and the last two columns the averages across the second halves (periods 16-30 and 46-60, respectively). >From the top, we first present the average prices for the treatments with demand information and then for the treatments without demand information. In both cases, the treatment that starts without a price ceiling (NoYes) is presented above the treatment that starts with a price ceiling (YesNo).

As a first observation, we note that average prices in any treatment and any phase (or part thereof) are well below the price ceiling of 28 and in the second half of each phase, they are always closer to the Nash-equilibrium price of 21 than to the price ceiling. This already indicates that there is little collusion at the ceiling, if at all (we address the distribution of prices and individual groups below).

The “$<$”, “$>$”, and “$\approx$” signs summarize the results of statistical tests, with “$\approx$” indicating no significant difference at the 10%-level. The superscripts indicate the level of significance. Our focus is on the vertical comparisons, testing for differences across treatments in the same block of periods (i.e. testing for difference between the distributions underlying the number right above and the one right below the sign). We apply non-parametric Mann-Whitney tests here, with the average prices within pairs across the respective periods as independent observations.

Looking first at the treatments with demand information and the first two columns, we see that prices are significantly higher in NoYes than in YesNo in the first phase, but that this is reversed in the second phase. This implies that in both phases, the average prices are higher in the treatment without a price ceiling. This runs against the hypothesis that the price ceiling might enable collusion. Considering the different halves of each phases, we see that the differences are primarily driven by the early periods of each phase, where we get the same relation, whereas there are no significant differences in the late periods.

The results for the treatments without demand information differ somewhat. The difference in the first phase is not significant (note that prices here are actually very close to the Nash-equilibrium price 21 in both treatments). The difference in the second phase is even more pronounced than in the Info treatments and is even also (weakly) significant if we consider only the late periods in each phase. This is consistent with the observations made above in Figures 2 and 3. Prices in the NoYes treatments are consistently higher with demand information than without, while there is no difference between the two YesNo treatments. The crucial result, however, is the same with and without demand information: whenever there is a significant difference, it is in the direction of higher prices without a ceiling, contrary to the hypothesis of a ceiling serving as a focal point for collusion.

We also compare prices within treatments across the phases (the horizontal comparisons in Table 5). Since data are matched, we use Wilcoxon signed-rank tests for these comparisons, using the average per group as independent observation. Again we see that whenever there is a significant difference it is in the direction of higher prices without a price ceiling than with a price ceiling. We get the same results with demand information as without, except for one case (in NoYes the overall comparison is weakly significant in NoInfo, but not in Info). Imposing a price ceiling (in NoYes)

	1st Phase		2nd Phase	1st Phase		2nd Phase	1st Phase		2nd Phase
	All		All	Early		Early	Late		Late
Prices — Info	Per 1-30		Per 31-60	Per 1-15		Per 31-45	Per 16-30		Per 46-60
NoYes	23.63	$\approx$	22.77	24.04	$>^b$	22.55	23.23	$\approx$	22.99
	$>^c$		$<^b$	$>^b$		$<^c$	$\approx$		$\approx$
YesNo	21.68	$<^c$	24.62	21.73	$<^c$	25.80	21.63	$<^b$	23.45
Prices — NoInfo	Per 1-30		Per 31-60	Per 1-15		Per 31-45	Per 16-30		Per 46-60
NoYes	21.93	$>^a$	21.27	22.14	$>^b$	21.33	21.73	$\approx$	21.22
	$\approx$		$<^c$	$\approx$		$<^c$	$\approx$		$<^a$
YesNo	21.74	$<^c$	24.57	21.70	$<^c$	25.50	21.78	$<^b$	23.65
Collusion Index — Info	Per 1-30		Per 31-60	Per 1-15		Per 31-45	Per 16-30		Per 46-60
NoYes	-0.43	$<^b$	-0.19	-0.53	$<^a$	-0.26	-0.33	$<^b$	-0.12
	$\approx$		$\approx$	$\approx$		$\approx$	$\approx$		$\approx$
YesNo	-0.39	$<^c$	-0.16	-0.54	$<^c$	-0.24	-0.23	$<^a$	-0.08
Collusion Index — NoInfo	Per 1-30		Per 31-60	Per 1-15		Per 31-45	Per 16-30		Per 46-60
NoYes	-0.48	$<^c$	-0.24	-0.68	$<^c$	-0.31	-0.29	$<^c$	-0.18
	$\approx$		$\approx$	$\approx$		$\approx$	$\approx$		$\approx$
YesNo	-0.51	$<^c$	-0.17	-0.72	$<^c$	-0.30	-0.30	$<^b$	-0.04

Note: Comparisons within treatments (horizontal) are based on Wilcoxon signed-rank tests, comparions across treatments (vertical) on Mann-Whitney tests. Significance: a: $p < 0.1$, b: $p < 0.05$, c: $p < 0.01$.

Table 5: Summary statistics, Prices (upper half) and Collusion indices (lower half)

leads to lower prices in early periods, but the effect virtually disappears towards later periods. In contrast, lifting a price ceiling (in YesNo) leads to higher prices. It is particularly noteworthy that in YesNo, the higher prices in the second phase are not exclusively driven by the price explosion after the abolition of the ceiling. Even when we restrict the analysis to the late periods of each phase, the prices are significantly higher in the second phase at the 5% level, both with and without demand information.

One reason why we typically find lower prices with a ceiling in place than without is that the ceiling would occasionally be binding, at least in early periods. We can control for this by replacing all prices > 28 when no ceiling is in place with the ceiling price 28, hence implying the price we would obtain if a ceiling had been in place and had only affected high prices by becoming binding, but not affected lower prices. Even these censored prices are higher than prices when a ceiling is in place, both with and without demand information (22.91 vs. 22.23 and 22.41 vs. 21.51, respectively).[9]

We do not provide statistical tests for the comparison of prices with and without demand information. We are not interested in the question whether demand information leads to higher or lower prices.[10] As argued above, the rationale for conducting treatments with and without demand information is to provide a test of the effect of price ceilings in two different settings. More precisely, by removing common knowledge on kinks in the demand curve that could serve as alternative focal points, we give the hypothesis that price ceilings could lead to more collusion a better shot in NoInfo. The observation that prices in NoYes are significantly higher than in YesNo in the first phase with demand information but not without is consistent with the expectation that in the absence of a ceiling, it is easier to collude with demand information than without. We note, however, that prices in NoYes are higher in Info than in NoInfo even in the second phase, where a ceiling was in place and collusion at the kink not possible. There are two possible explanations. First, attempts at collusion at a kink may have been transferred to collusion at the ceiling. Second, the subject pool may have differed in their tendency towards collusion. In any case, we reiterate that the comparisons between information conditions is not our concern and that the crucial result is that under both conditions, we find that the presence of a price ceiling never leads to higher

[9] This is true also for each block of 15 periods, except for periods 16-30 without demand information and periods 46-60 with demand information.

[10] Furthermore, the treatments were run with different subject pools so that we would not be able to conclude with certainty that any differences are indeed driven by the information condition.

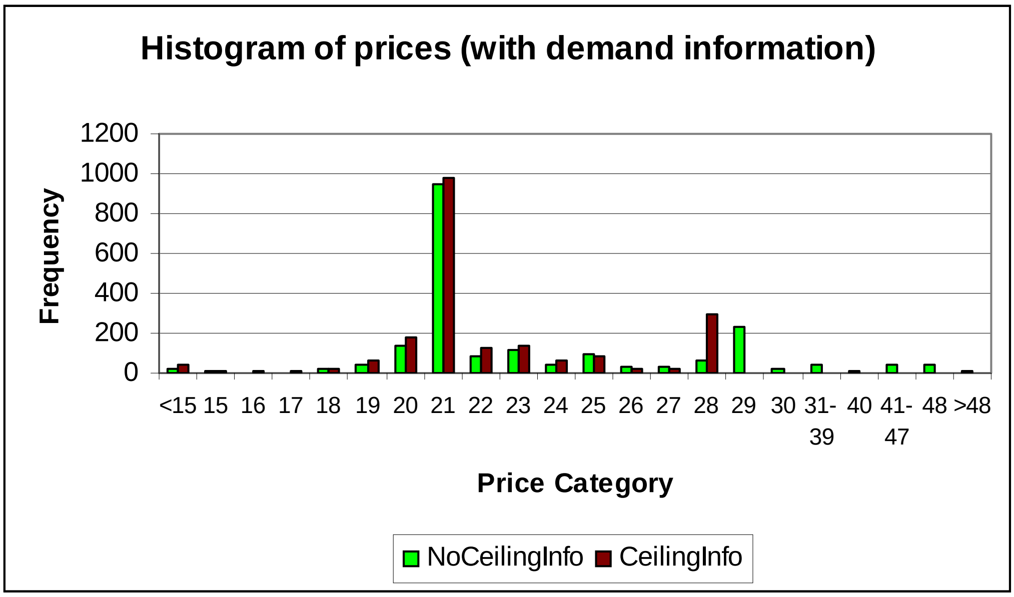

Figure 4: Histogram of price choices for the treatments with demand information.

Note: Extreme categories with few observations are aggregated.

prices but frequently to significantly lower ones. We summarize the results on average prices as:

Observation 1 *Comparing across treatments, price ceilings lead in no comparison to higher prices but often to significantly lower prices.*

Observation 2 *Comparing across phases within treatments, imposing a price ceiling leads to significantly lower prices in the early periods. Lifting a price ceiling leads to significantly higher prices both in early and late periods.*

To address the more specific prediction of the focal point hypothesis of increased clustering of chosen prices at the ceiling, we next consider the distribution of observed prices with a focus on the share of prices at the ceiling or above (if permitted), before studying the collusiveness in individual markets. Figure 4 shows for the treatments with demand information the distribution of individual price choices across all markets and periods, conditional on a ceiling being in place or not. Figure 5 shows the price distribution for the treatments without demand information. As Figures 4 and 5 show, while prices exactly equal to 28 are more frequent with a ceiling in place, prices larger or equal to 28 are more frequent without a price ceiling both with and without demand information. In particular, in the treatments with demand information, without a ceiling price

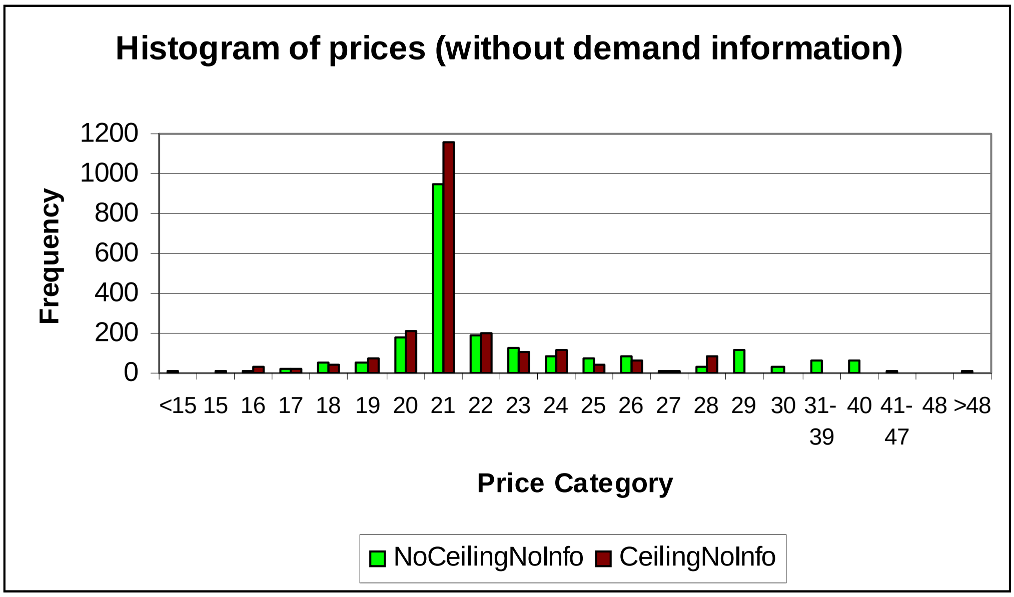

Figure 5: Histogram of price choices for the treatments without demand information.

Note: Extreme categories with few observations are aggregated.

choices of 29 (i.e. at the nearest kink in the demand curve) occur nearly as frequently as prices of 28 occur with a price ceiling. Specifically, with demand information, in YesNo, 8.8% of price choices are equal to 28 in the first phase with a ceiling, but 16.9% are equal to 29 in the second phase without a ceiling (with 2% at $p = 48$ and overall 28.1% of choices $p \geq 28$). In NoYes, 19.7% of price choices are equal to 28 in the second phase with a ceiling and 5.8% are equal to 29 in the first phase without a ceiling (with 2% at $p = 48$ and overall 17.3% of choices $p \geq 28$). This suggests that subjects attempt to collude nearly as often at a kink in the demand curve as at a price ceiling. Even more interesting, without demand information, with a price ceiling price choices of 28 are actually less frequent than choices of 29 without a price ceiling. In YesNo, 4.2% of price choices are equal to 28 in the first phase with a ceiling, but 10% are equal to 29 in the second phase without a ceiling (with overall 23.2% of choices $p \geq 28$). In NoYes, 3.7% of price choices are equal to 28 in the second phase with a ceiling. While only 0.4% are equal to 29 in the first phase without a ceiling overall 6.2% of choices satisfy $p \geq 28$. Overall, these results suggest that the concentration at 28 with a ceiling results rather from the ceiling being binding than from serving as focal point. Put differently, the ceiling supresses prices that would otherwise have been higher rather than pulling up prices that would otherwise have been lower.

More generally, prices above the Nash-equilibrium occur substantially more frequently without a price ceiling than with a price ceiling both with and without demand information. Furthermore, Nash-equilibrium prices are overwhelmingly the most frequent prices and occur even slightly more frequently with a price ceiling in place than without, both with and without demand information (with demand information: YesNo, with ceiling 51.3%, without ceiling 53.9%, NoYes, with ceiling 44.6%, without ceiling 38.9%; without demand information: YesNo, with ceiling 32.4%, without ceiling 45.5%, NoYes, with ceiling 74.9%, without ceiling 42.7%).

Non-parametric tests support these observations. Whenever there are significant differences in the frequency of high prices (that is, prices larger or equal than 28), both within and across treatments they indicate more high prices without a ceiling. The frequency of Nash-equilibrium prices typically increases within treatments over time independent of whether a ceiling is in place, whereas across treatments the only significant difference supports more Nash-equilibrium prices with a ceiling. We summarize the main result on the price distribution as:

Observation 3 *Whenever price ceilings lead to a significant difference in the number of high prices, they are less frequent with a ceiling in place.*

We have seen so far that the presence of a price ceiling does not increase average prices or the frequency of high prices. The focal point hypothesis more specifically predicts that firms will find it easier to coordinate on choosing prices at the price ceiling than to coordinate on any collusive price in the absence of a ceiling. Moreover, the restriction of the strategy space due to a price ceiling could make coordination easier. We thus consider next the cases where both firms charge the same price and then take a look at individual markets where both firms repeatedly set the same price.

On an aggregate level, simply considering how often both firms in a market charge the same price, there is at best weak evidence in support of the focal point story. With demand information, in NoYes in 15.5% of cases both firms charge $p = 28$ in the second phase, when the ceiling is in place, whereas only 1% of price pairs are at $p = 29$ in the first phase (and 0.4% at $p = 48$). While this is supporting the focal point hypothesis, in YesNo the pattern is reversed. In the first phase, 5.9% of price pairs are at the ceiling (all coming from one pair that colludes throughout), whereas 12% are at $p = 29$ in the second phase. This suggests that overall the price ceiling is slightly more successful as a focal point than the kink in the demand curve. We would then expect there to be a stronger effect of the price ceiling in the treatments without demand information where in the part without the ceiling in place no focal point is available. This, however, is not the case. In NoYes, in the second phase no pair ever charges jointly more than $p = 23$, and none charges above $p = 25$ in the first phase. In YesNo, a negligable share (0.6%) of price pairs is at the ceiling in the first phase, while 5.6% are at $p = 26$ and 5.4% at $p = 24$. In the second phase, 2.2%, 4.8%, 8.3% and 4.6% are at $p = 24, 26, 29$, and 40, respectively. Thus, without demand information, in the first phase coordination seems easier with a price ceiling than without, but this does not occur at the ceiling and hence would rather support an effect of reducing the strategy space rather than a focal point.

We now turn to individual markets. Table 6 shows the pairs that manage to coordinate on a supracompetitive price for several subsequent periods in at least one phase of the experiment. It also shows the price they coordinate on and the number of periods they coordinate on this price.[11] The main observation is that if pairs of sellers manage to collude, they typically do so in the second half of the experiment. Namely, 12 pairs (out of 70) achieve some degree of collusion in the second phase, evidenced by a repeated joint setting of prices above the Nash equilibrium.[12] Only five pairs do so in the first phase, all of which also collude in the second phase. More importantly, the presence of a price ceiling does not have any effect on the likelihood of achieving collusion. Of the 12 pairs who collude in the second phase, only 5 face a price ceiling, while 7 do not (where one each do not coordinate on a Pareto-improving price). Among the five pairs who collude in the first phase, four face a price ceiling, but only one of them actually colludes at the ceiling, and this pair colludes at the kink of the demand curve at 29 in the second half. This suggests that the price ceiling in the first phase enables coordination by restricting the strategy space rather than by providing a focal point.

Remarkably, without demand information where the focal point effect should be particularly strong, not a single pair colludes at the price ceiling, neither in the first nor the second phase. Collusion at the price ceiling does occur more frequently with demand information in the second phase.[13] While these four pairs support the focal point hypothesis because they did not manage to

[11] One pair (in NoYes-Info) reaches the same price (24) in 8 periods, but this appears to happen rather as a coincidence in a fluctuation pattern, as these periods are not subsequent. Similarly, another pair (in YesNo-Info) jointly charges 29 in 6 periods, but only 4 of them are subsequent and rather early in the phase, so that one could hardly argue they have settled on a collusive regime.

[12] We note that our definition of collusiveness is rather generous. Only prices ≥ 24 lead to an increase of total profits and even only those ≥ 25 lead to a Pareto-improvement. One might hence want to discard pairs 3 and 63, which does not affect our conclusions.

[13] It appears that without demand information, subjects find it difficult to figure out that the ceiling actually

Colluding Pairs — Info

With Ceiling						Without Ceiling					
YesNo			NoYes			NoYes			YesNo		
Pair No.	p	#	Pair No.	p	#	Pair No.	p	#	Pair No.	p	#
24	28	30	2	28	29				24	29	28
			(3)	(23)	(25)	(3)	(23)	(12)	34	29	26
			7	28	28						
			12	28	14						
			16	28	8						

Colluding Pairs — NoInfo

With Ceiling						Without Ceiling					
YesNo			NoYes			NoYes			YesNo		
Pair No.	p	#	Pair No.	p	#	Pair No.	p	#	Pair No.	p	#
									53	29	19
54	26	28							54	26	26
									55	29	25
61	24	26							61	40	25
(63)	(22)	(7)							(63)	(24)	(10)

Note: For each pair that repeatedly set the same price above the Nash-equilibrium (more than 5 times in a row) the price and how frequently they coordinate on this price is shown. The data is arranged so that in each With Ceiling or Without Ceiling block, data from the first phase appears on the left and data from the second phase on the right. Pairs that collude in both phases appear on the same line in both parts of the table. Data in parentheses is for pairs coordinating on a price that is above the Nash equilibrium but not Pareto-dominating it.

Table 6: Colluding Pairs

collude in the first phase, we also find four pairs that coordinate at $p = 29$ in the second phase, three of which did not collude in the first phase. Given that two are in the treatment without demand information, this is not just because the kink in the demand curve is equally focal.[14] Therefore, studying individual markets, we again see only little evidence that a price ceiling facilitates collusion and hardly at the ceiling.

So far we addressed only prices. These do not, however, completely reflect the degree of collusiveness in the market. Specifically, because of increasing marginal costs, firms are jointly better off if they set equal prices.[15] To measure the impact on firms' profits, we define collusion indices for the respective phases as follows:

$$I = \frac{\pi^{\text{actual}} - \pi^{\text{Nash}}}{\pi^{\text{Cartel}} - \pi^{\text{Nash}}} \qquad \text{(when no ceiling is in place)} \tag{1}$$

or

$$I = \frac{\pi^{\text{actual}} - \pi^{\text{Nash}}}{\pi^{\text{Ceiling}} - \pi^{\text{Nash}}} \qquad \text{(when a ceiling is in place),}$$

where π^{actual} is the actual achieved joint profit, π^{Nash} is the joint Nash-equilibrium profit, π^{Ceiling} is the joint profit if both firms set a price equal to the ceiling and π^{Cartel} is the maximum possible joint profit if there is no ceiling in place. This means we define the collusion index such that it equals 1 at the maximal joint profit. It is 0 if both sellers choose Nash-equilibrium prices. With a price ceiling the maximum profit is achieved if both firms choose the price at the ceiling, without a price ceiling it is obtained if both sellers choose $P = 48$ (see Section 2.2). The average collusion indices for the different treatments and phases are in the lower half of Table 5, which is structured in the same way as the upper half for the average prices.

The first crucial observation is that *all* aggregate collusion indices are negative, that is average profits are below Nash-equilibrium profits in all parts in all treatments.[16] Next, we see

---

constitutes an attractive price. If they have demand information, this is easier to do and then the ceiling can help to solve a coordination problem. For example, pair 7 shows attempts (occasionally successfully) to coordinate on $p = 29$ and $p = 48$ in the first phase and then immediately settles on the ceiling once it is introduced.

[14] Pair 55 shows an interesting path. Late in the first phase, they start setting equal prices, moving up by one per period, starting from 19 and eventually reaching 28 (only for the last three periods). After the abolishment of the ceiling they continue the trend and reverse it when profits decline, thus converging on 29.

[15] Furthermore, once one firm sells nothing, raising its price even further will not affect profits and welfare in any way.

[16] Remember that the Nash equilibrium coincides with the upper end of the competitive price range. Joint profits below the Nash-equilibrium profits do not in general imply that firms set prices below the equilibrium, but rather that

that in each comparison between phases the collusion index is significantly higher in the second phase. This holds for the complete phases, as well as when we restrict the analysis to the early or late periods within each phase. This means profits move towards Nash profits over time. This happens primarily because firms converge towards setting equal prices (which is then frequently the Nash price). Finally, for no phase or part of a phase does the collusion index differ significantly between treatments.[17] We again do not compare between the treatments with and without demand information but note that the pattern is very similar with and without demand information. Hence, the presence of a price ceiling never affects the degree of collusiveness significantly.[18] We summarize the results on the collusion index as:

Observation 4 *In all treatments the degree of collusiveness increases over time, but stays below Nash-equilibrium levels, i.e. joint profits approach the Nash-equilibrium level from below.*

Observation 5 *Comparing across treatments, the presence of a ceiling never has a significant impact on the degree of collusiveness.*

Finally, from the perspective of the competition authority, the ultimate goal is not competitiveness per se but welfare. We thus also compared welfare (as the sum of the, hypothetical, consumer surplus and producer surplus) across phases and treatments. In line with the results above, we cannot detect a detrimental effect of price ceilings. Whenever there are significant differences across or within treatments, welfare is higher with a price ceiling than without.

4 Conclusions

In this paper we report on a new attempt to induce a collusive focal-point effect of price ceilings in laboratory markets. Our design features elements that we considered favorable to induce such an

they choose different prices. This yields lower profits because of increasing marginal costs, which lead to inefficient splits of demand for unequal prices.

[17] If we consider simply market profits instead of the collusion index, we find that these are about 2% higher with a ceiling than without. This holds both with and without demand information. Interestingly, in both conditions, in the last 15 periods, profits are slightly higher without a ceiling. The marginally higher profits with a ceiling are not driven by firms setting higher prices, but by more frequently setting equal prices, which is easier if the strategy space is smaller.

[18] We also ran regressions taking into account the dependence of data within a pair. Considering the impact of a dummy for the presence of a price ceiling on average prices and the collusion index, these results are fully consistent with the non-parametric tests, with minor differences in significance levels.

effect: (a) posted-offer markets; (b) relatively larger incentives to collude at the ceiling as compared to the competitive prices and the price in the Nash equilibrium (and as compared to earlier studies); (c) fixed pairs of two sellers; (d) simulated demand; (e) cost asymmetries among sellers; and (f) very limited information about demand.

Yet, contrary to our expectations, in none of our treatments do price ceilings lead to more collusion. This is true with respect to several measures: average prices; the degree of collusiveness (as measured by a standard collusion index); the number of high prices chosen; the number of colluding pairs and welfare. More precisely, whenever we do observe significant differences with respect to any of these measures, these differences point in the direction of *less* collusion when a price ceiling is in place. Hence, this new search for a collusive focal-point effect in laboratory markets was not successful either. Since our design was more favorable for such an effect to occur than the designs in previous studies, our results strengthen the finding that establishing a collusive effect of price ceilings is difficult in the laboratory. As a result, they further question the empirical validity of the focal point hypothesis.

This is reminiscent of Isaac and Smith's (1985) elusive search for predatory pricing in the laboratory.[19] In light of the evidence for collusive effects of price ceilings in various field markets cited in the introduction this is puzzling. More research is needed to pin down the exact circumstances that enable a "behavioral existence proof" (Goeree, Gomez and Holt, 2008) for a collusive focal-point effect of price ceilings.[20]

There are two distinct possible explanations for the apparent difficulty to induce a focal point effect of price ceilings in the laboratory in spite of empirical evidence in line with the focal point hypothesis in the field. The first is that there is something fundamentally different about market structure or market participants between the laboratory and the field and that all experiments failed to reflect crucial aspects that enable the focal-point effect in the field. As we argued in the introduction, we believe this to be less relevant for a test of the focal point hypothesis than it might be for market experiments in general. However, one such issue could be that firms in the field may have a long history of unsuccessful attempts at collusion and hence can appreciate the presence of

[19] Harrison (1988), Jung, Kagel, and Levin (1994), and Capra et al. (2000) later found evidence of predatory pricing in the laboratory.

[20] We note that achieving collusion in the laboratory is not in general impossible. Among many examples, Li and Plott (in press) as well as Brown, Plott, and Sullivan (in press) show that a so-called "collusion incubator" environment quickly and reliably generates tacit collusion in a simultaneous ascending price auction.

a coordinating device more. Our results that show successful collusion at the price ceiling more often in the second phase of the experiment than in the first (and following unsuccessful attempts at coordinating on high prices) are in line with such a view. On the other hand, in the experiment this might just be a pure re-start effect, as we see collusion without a ceiling also more frequently in the second phase. Furthermore, one would expect that firms that have long been attempting to collude have found other ways of achieving this than relying on a simple focal point.

The second possible explanation for the lack of evidence in favour of the focal point hypothesis in the laboratory, in spite of its apparent support in the field is that the reason for the field phenomena is not the focal point effect either. According to this interpretation, also in the field we appear not to simply have a selection problem for tacit collusion that is solved by the price ceiling because the same problem is not solved by the price ceiling in controlled laboratory conditions. Instead, these field results would thus be driven by other factors. These might include explicit collusion (which might either be triggered by the introduction of the price ceiling or might be easier to keep up in its presence) or other reasons that the empirical studies failed to control for. We consider the main contribution of laboratory experiments failing to replicate certain phenomena from the field precisely to inspire us to think more about alternative explanations for the field phenomena themselves.

As one such potential additional explanation for collusive effects of price ceilings in the field, a price ceiling might well be interpreted (and indeed quite possibly correctly) as a signal that up to that price firms would not be investigated for collusion. After regulating a price there seems little reason to investigate if prices are actually clustered at the ceiling, because a ceiling would typically be set at a level were the authorities would expect it to be potentially binding. Therefore collusion, be it tacit or explicit, is far less risky than it would be at a comparable price level if no ceiling was in place. Thus collusion at a price ceiling would not be facilitated because it reduces the coordination problem but because it reduces the (perceived as well as possibly actual) risk for firms of being accused of collusion. This interpretation is also consistent with observed patterns in the field. For example, if the price ceiling is very high, firms can expect that the competition authority would not expect it to be binding so that it would be likely to investigate relatively high prices. This in turn discourages collusion and could hence explain why sometimes lower prices are observed with higher ceilings, since the perceived save collusive price has been removed. More generally, any observation that is consistent with collusion at the price ceiling is consistent with this signalling of impunity explanation as well as with the focal point hypothesis because both explanations suggest that pricing at the ceiling might be collusive, but the data can typically not shed light on why collusion might happen.

In contrast to the focal point hypothesis, we would not expect any such signalling of impunity effect of price ceilings in the laboratory, simply because collusion is not discouraged or in any way punished in the experimental markets. Therefore, the signalling of impunity hypothesis is better able than the focal point hypothesis to explain simultaneously why price ceilings appear to enable collusion in the field but not in the laboratory.[21] Identifying which of these two hypothesis (or which conceivable alternative) underlies apparent collusion appears to be very difficult with field data.

---

[21] At face value, the signalling of impunity hypothesis would rather predict that we see collusion in the laboratory both with and without a ceiling because it is never punished there. This suggests that in the field, collusion must generally be easier than in the laboratory, for example due to abilities to communicate. This is then discouraged by possible punishment, which is in turn countered by the price ceiling. We also note, however, that experimental designs for tests of the focal-point hypothesis, including ours, have typically been chosen to make collusion not too easy without a price ceiling, so that we can potentially observe a collusive effect of price ceilings. To test for the signalling of impunity hypothesis in the laboratory requires an altogether different design, namely were collusion is easy, but discouraged through possible punishment, with a price ceiling serving as a signal for the prices which may or may not imply punishment.

References

Borck, R., D. Engelmann, W. Müller and H.-T. Normann (2002): Tax Liability Side Equivalence in an Experimental Posted Offer Market, *Southern Economic Journal* 68, 672-682.

Brown, A.L, C.R. Plott, and H.J. Sullivan (in press): Collusion facilitating and collusion breaking power of simultaneous ascending price and descending price auctions, *Economic Inquiry*, forthcoming.

Capra, M., J. Goeree, R. Gomez, and C. Holt (2000): Predation, Asymmetric Information, and Strategic Behavior in the Classroom, *International Journal of Industrial Organization* 18, 2000, 205-225.

Coursey, D. and V.L. Smith (1983): Price Controls in a Posted Offer Market, *American Economic Review* 73, 218-21.

Davis, D. and C. Holt (1993): *Experimental Economics*, Princeton, NJ: Princeton University Press.

DeYoung, R. and R.J. Phillips (2006): Strategic Pricing of Payday Loans: Evidence from Colorado, 2000-200, Indiana State University, Networks Financial Institute, *Working Paper* No. 2006-WP-05.

Engelmann, D. and H.-T. Normann (2009): Price Ceilings as Focal Points? An Experimental Test. In: *Experiments for Antitrust Policies,* edited by J. Hinloopen and H.-T. Normann. Cambridge, UK: Cambridge University Press.

Eriksson, R. (2004): Testing for Price Leadership and for Reputation Goods Effects: Swedish Dental Services, Swedish Institute for Social Research (SOFI), Stockholm University, *Working Paper* 5/2004.

Fischbacher, U. (2007): Z-Tree, Zurich Toolbox for Readymade Economic Experiments, *Experimental Economics* 10, 171-178.

Goeree, J., R. Gomez and C. Holt (2008): Predatory Pricing: Rare Like a Unicorn? In *Handbook of Experimental Economics Results*, C. Plott and V. Smith, eds., 178-184.

Harrison, G.W. (1988): Predatory Pricing in a Multiple Market Experiment. A Note, *Journal of Economic Behavior and Organization* 9, 405-417.

Huck, S., H.-T. Normann, and J. Oechssler (2004): Two Are Few and Four Are Many: Number Effects in Experimental Oligopoly, *Journal of Economic Behavior and Organization* 53, 435-446 .

Isaac, R.M. and C.R. Plott (1981): Price Controls and the Behavior of Auction Markets: An Experimental Examination, *American Economic Review* 71, 448-459.

Isaac, R.M. and V.L. Smith (1985): In Search of Predatory Pricing, *Journal of Political Economy* 93, 320-345.

Jung, Y.J., J.H. Kagel, and D. Levin (1994): On the Existence of Predatory Pricing: An Experimental Study of Reputation and Entry Deterrence in the Chain-Store Game, *RAND Journal of Economics* 25, 72-93.

Knittel, R.K. and V. Stango (2003): Price Ceilings as Focal Points for Tacit Collusion: Evidence from Credit Cards, *American Economic Review* 93, 1703-1729.

Li, J. and C.R. Plott (in press): Tacit collusion in auctions and conditions for its facilitation and prevention: Equilibrium selection in laboratory experimental markets, *Economic Inquiry*, forthcoming.

Ma, T.-C. (2007): Import Quotas, Price Ceilings, and Pricing Behavior in Taiwan's Flour Industry, *Agribusiness* 23, 1-15.

Normann, H.-T. and B. Wallace (2006): The Impact of the Termination Rule on Cooperation in a Prisoner's Dilemma Experiment, *Working Paper*, Goethe University Frankfurt.

Schelling, T. (1960): *The Strategy of Conflict*, Cambridge, MA: Harvard University Press.

Scherer, F.M., and D. Ross (1990): *Industrial Market Structure and Economic Performance*, 3rd ed., Boston: Houghton Mifflin.

Sheahan, J. (1961): Problems and Possibilities of Industrial Price Control: Postwar French Experience, *American Economic Review* 51, 345-359.

Smith, V., and A. Williams (1981): On Non-Binding Price Controls in a Competitive Market, *American Economic Review* 71, 467-471.

Tirole, J. (1988), *The Theory of Industrial Organization*, Cambridge, MA: MIT Press.

A Instructions for Seller 1 (Not intended for publication)

(Differences between the treatmemts with and without demand information are indicated in brackets. Instructions for Seller 2 only differed in the cost schedule.)

This is an experiment on market decision-making. Take the time to read carefully the instructions. A good understanding of the instructions and well thought out decisions during the experiment can earn you a considerable amount of money. In this experiment, you will be one of two sellers in a market. You will have to decide at which price you are going to sell a fictitious commodity. There will be 2 times 30 periods of trade. After the first 30 periods, we will change the market conditions. We will inform you in detail about theses changes after the first 30 periods are over. During the 60 periods the other seller in your market is always the same participant. That is, though there are 12 participants in the room, you and the same other seller will serve your market for the entire course of the experiment. The other 10 participants will serve other markets. The trade of the commodity determines your payoff in "Points". After the experiment, your payment in Pound Sterling [in NoInfo: Euro] will be computed and you will be paid immediately in cash. You will get £1 [in NoInfo: Euro 1] for every 150 [in NoInfo: 100] Points you earned. In what follows, we will explain to you how sales in the market will be made and how your earnings are computed. After you have read the instructions, you will have the opportunity to ask questions. Before we start with the actual experiment, we will ask you a few questions in order to review these instructions and ensure everybody has fully understood them. Then we will begin the first trading period.

How sales are done:
In every period, you and the other seller in the market choose a price at which you wish to sell units of the commodity. [Only in NoInfo: Note that this is the price per unit.] While you may sell more than one unit, you can post only one price. That is, you cannot sell different units at different prices. Prices must be integer numbers. Each seller can produce [Only in Info: and sell] up to six units in every period. You have to pay production costs for each unit you sell. The production

costs for each unit you may sell are as follows:

Unit	Costs in Points
1	1
2	14
3	14
4	23
5	31
6	31

What the table says is that, for the first unit you may sell, you have costs of 1 Point. If you sell a second unit, this second unit costs you 14 Points, and so on for units 3 to 6. For example, if you sell two units, your total costs are 1 + 14 = 15. You do not incur any costs for units, which you do not sell. Note that the other seller has different costs of production. The number of units of the commodity you offer will automatically be chosen by the computer in a way that ensures that you do not make losses. At most, you will sell the quantity that can be produced at costs below or equal to your chosen price. For example, if you post a price of 18, you will not sell more than 3 units. That is, the computer programme ensures that you will never sell any units at a price below production costs. Note that this is the maximum quantity you may sell. The actual quantity you will sell will depend on the demand and on the prices that you and the other seller choose.

How purchases are done:

No buyers participate in this experiment. Instead, buyers are simulated by the computer. Each simulated buyer can buy exactly one unit. Each buyer has a certain (maximum) willingness to pay for the unit. This willingness to pay is simply the highest price at which the simulated buyer will purchase the commodity. These maximum prices buyers are willing to pay and the according number of buyers who have this willingness to pay are as follows: [In NoInfo, the last sentence was replaced by: You will not be informed about the buyers' maximal willingness to pay.]

[The following table only in Info.]

willingness to pay	number of buyers with
	this willingness to pay
48	2
29	2
21	2
13	2

[The following para only in Info.] From the table you see that there are 8 buyers in total. Two buyers are willing to pay up to 48 Points for their unit, 2 more buyers are willing to pay up to 29 Points for a unit, and similarly 2 buyers are willing to pay 21 and 13 respectively. Note that, given these values, no unit will be purchased from sellers who set a price higher than 48. After you and the other seller have chosen the price, the simulated buyers make purchases according to the following rules:

1. Buyers purchase in decreasing order of their willingness to pay. That is, the first two buyers who purchase are those who pay up to 48 for their unit. Then come the buyers who pay up to 29, and so on. [In NoInfo: Buyers purchase in decreasing order of their willingness to pay. That is, the first buyer who purchases is the one who has the highest willingness to pay for their unit. Then comes the buyer with the second-highest willingness to pay, and so on.]

2. Buyers first purchase from the seller with the lower price, then buyers purchase from the seller with the higher price.

3. Buyers purchase their unit only if the price of that unit does not exceed their willingness to pay. As long as the price posted by you or the other seller does not exceed this maximum price, the buyer will buy.

4. If you and the other seller post the same price, the buyers who want to buy at this price will split their demand equally between you and the other seller. Consider the following hypothetical example. Assume that you and the other seller charge a price of 33. Then you and the other seller face a demand of 2 units jointly together, and our rule number four would imply that you and the other seller sell one unit each. You would incur costs for only one unit, that is 1 Point. [in NoInfo: If you and the other seller post the same price, the buyers who want to buy at this price will split their demand equally between you and the other seller.

Consider the following hypothetical example. Assume that you and the other seller charge a price of 24 and that there are 2 buyers who are willing to buy at this price. Then you and the other seller face a demand of 2 units jointly together, and our rule number 4 would imply that you and the other seller sell 1 unit each. You would incur costs for only 1 unit, that is 1 Point.]

Earnings and feedback:

Your earnings are as follows. For every unit you sell, you earn the difference between your price and production costs for that unit. Total earnings in every period are the sum of earnings for all units sold. The computer will calculate the earnings for all units sold and also total earnings. At the end of each period, you will see a screen informing you about the following:

- The price charged by the other seller in the market.
- [Only in Info: The total demand at your price.]
- The number of units you sold.
- Your total costs for the units you sold.
- Your resulting profit.

Summary of instructions:

- Sellers:
  - Sellers can produce [Only in Info: and sell] up to 6 units. You and the other seller have different costs of production.
  - Sellers earn the difference between the price and their unit costs.
  - By posting a price, sellers decide to sell any number of (up to six) units at this price as long as production costs do not exceed the price.
- Buyers:
  - Buyers are simulated by the computer.
  - Each buyer can buy one unit.

  - Buyers buy as long as the price does not exceed their willingness to pay.
  - Buyers shop in order of decreasing willingness to pay.
  - Buyers purchase first from the cheaper seller.

- The experiment is divided into a series of 2×30 trading periods. You and the same other seller will serve one market for the whole 60 periods.