Christoph Engel Klaus Heine

The Dark Side of Price Cap Regulation
A Lab Experiment

Abstract

In a nutshell, price cap regulation is meant to establish a quid pro quo: regulators are obliged by law to intervene only at rare, previously defined points in time, and only by imposing an upper bound to prices; firms are meant to justify regulatory restraint by engaging in socially beneficial innovation. In the policy debate, a potential downside of the arrangement has gotten short shrift: the economic environment is unlikely to be stable while the cap is fixed. If regulators take this into account, they have to decide under uncertainty, also about the way firms will react. In a lab experiment, we manipulate the degree of regulatory uncertainty. We compare a baseline where regulators have the same information as firms about demand with a treatment where they only receive a noisy signal, and another where they only know from which distribution demand is taken. In the face of uncertainty, regulators impose overly generous caps, which firms exploit. In the experiment, the social damage is severe, and does not disappear with experience.

JEL: C91, D02, D03, D42, H11, K21, L43

Keywords: price cap regulation, regulatory uncertainty, monopoly rent, experiment

1. Introduction

Genius does not routinely strike regulatory policy. But when price cap regulation was invented, many observers felt this was an ingenious solution to a thorny normative problem (Vogelsang 2002; Stern 2003). For many reasons, government is a poor entrepreneur. But if firms hold a monopoly, or at least a dominant position that is unlikely to erode, for instance since the conditions for a natural monopoly are fulfilled, firms use their entrepreneurial skills to exploit consumers. As a consequence, some form of regulatory intervention is hard to avoid. Seemingly price cap regulation cuts the Gordian knot. A regulatory authority gets power to impose a maximum price. Yet this power is constrained. The regulator may only intervene at intervals defined by law. These intervals are meant to be rather long. While the cap is in place, firms benefit from regulatory certainty. Under the shield of the current cap, they can engage in innovation. They may be certain that the regulator will not deprive them of the benefit from their innovation by reducing the cap.

Normatively price cap regulation is attractive because of expected welfare gains, and of innovation in particular. The purpose of regulatory certainty is not redistribution to the advantage of the firm. For sure, the legislator and the regulator expect the firm to gain a rent. But ideally this rent should result from an efficiency gain. Making the firm’s life harder (by imposing the cap) is meant to serve as a healthy shock that spurs creativity (Beesley and Littlechild 1989).

Now a firm’s profit is not only a function of its own entrepreneurial activity. There are also exogenous influences, like shifts in demand, in the cost of raw materials, changes in the macro environment, or legislative intervention that is unrelated to this industry. If the price cap is not legally guaranteed, the regulator is free to react to such developments. With price cap regulation, though, the regulator must (at least partly) decide under the veil of ignorance. To set the optimal cap, the regulator must gain a sense of the likely volatility of the environment. We wonder whether, and if so in which ways, this additional challenge distorts price cap regulation.

In the field, this concern would be difficult, if not impossible, to study. Essentially reality is never completely predictable. But the degree of unpredictability is not a constant. Different regulators are likely to be differently well equipped to second-guess changes of the environment. Yet for the academic observer it would be very difficult to assess how well some regulatory agency is calibrated. Most importantly, in the field we could only compare industries before and after the introduction of price cap regulation. Such industries are of course not randomly selected. Frequently the set-up of an independent regulatory agency even coincides with the introduction of price cap regulation. In that case, even the comparison at the time before and after regulatory change is meaningless

(Shughart 2008). In the interest of making the effect visible and, more importantly even, of having a chance to randomize, we go to the lab.

For this choice of method, we have to pay the usual price. While randomization guarantees internal validity, lab experiments always have limited external validity. One has to extrapolate from an artificially clean situation to a much more complex real-world context. Moreover, as is standard in the experimental literature, in our experiment regulators and firms are represented by students. Of course, in the field both are heavily institutionalized and historically embedded corporate actors (Engel 2010). Yet all of these qualifications also hold for the hundreds of lab experiments on competition (for an overview see the meta-study by Engel 2007). In the field, one-person firms are rare. Nonetheless firm choices are routinely studied in student participants. All we do is transferring this methodology to markets where there is no competition in the first place so that government entrusts consumers' interests, and total welfare for that matter, to a regulatory authority.

We nonetheless readily acknowledge the many limitations inevitably inherent in our method. We induce preferences. If they maximize payoff, firms exclusively maximize profit. We thus bracket any competing goals of management. Likewise, our regulators are induced to maximize welfare. They have no monetary incentive for any competing goal, like supporting government in the next election. We treat firms and regulators as unitary actors. We thus bracket the entire economic, legal and political debate on corporate governance. We also do not distinguish between the legislator, government and regulatory agencies. We thus abstract from the microstructure of the political system.

Given these limitations inherent in our method, we do not mean to make a recommendation about the overall desirability of price-cap regulation. All we want to contribute to the debate is evidence on one seemingly overlooked concern: price-cap regulation forces the regulator to fix a cap for an extended period of time despite uncertainty about the development of decision-relevant factors during this time span. We model this situation, assuming that regulators hold standard preferences. Standard theory expects a distortion. The expected welfare loss results from the ex ante cap being more generous than the cap a completely informed regulator would set. From a behavioral perspective, additional distortions might be expected. Our experiment is meant to test the predictions from standard theory, and potential behavioral effects that might exacerbate or mediate the problem.

In our experiment, the firm holds a monopoly, which creates the need for regulatory oversight. To realign business interest with social welfare, the regulator has power to impose a price cap. The firm has superior knowledge about business opportunities, which we design as an information set. Specifically, the firm always knows precisely the size of demand (or more technically: the intercept of the demand function), while we vary the information that is available to the regulator. In the *Baseline*, the regulator has the same information as the firm. In the *Upfront* treatment the regulator only knows the distribution. In the intermediate *Signal* treatment, the regulator only receives a signal.

At the outset of the experiment, we randomly assign roles, which are held constant throughout the experiment. We first play a one-shot game. This allows us to isolate the effect of the regulator being differently well informed about business opportunities. After a (first) surprise restart, we repeat the game for announced 10 periods, using a partner protocol. Each period, with probability 0.1 a shock occurs that replaces the initial realization of the intercept of demand with another random draw from the distribution. Participants know that no more than one shock may occur. This phase of the experiment allows us to identify the effects of confining the regulator to a single decision that takes effect for an extended period of time, without knowing ex ante (perfectly) how business opportunities will develop.

After the end of the second part of the experiment, we have another surprise restart, keeping the original pairs of principal and agent in place. Again, within 10 periods at most one shock may occur, leading to another draw from the lottery of business opportunities. In the third phase, we expose principals to the opposite of the regime they had been in before. If they were in the *Upfront* or in the *Signal* regime, they now are in the*Baseline*. If they were in the *Baseline*, they now are either in the *Signal* or in the *Upfront* regime. By this within subjects manipulation, we learn whether and, if so, how strongly behavior is influenced by past experiences with the opposite regime. We learn the effects of regulatory reform, rather than exposing new firms and new regulators to another regime.

Qualitatively, standard theory is supported. The point predictions assuming standard preferences significantly explain the choices of regulators and firms. Yet prices are considerably higher and consumer rent is considerably lower than predicted by theory. This is chiefly due to the fact that regulators set overly generous caps. They also do so if they are completely informed. But there is an additional distortion if they are only imperfectly informed.

The remainder of the paper is organized as follows. In the next section we relate our paper to the literature. In Section 3 we present the design of the experiment and develop hypotheses. Section 4 is the results section. Section 5 concludes with discussion.

2. Related Literature

Our research is related to four different strands of literature. The first is the long-standing debate over the differences between cost-of-service or rate-of-return regulation on the one hand and price-cap or revenue-cap regulation on the other (see only Joskow and Schmalensee 1986; Laffont and Tirole 1993; Vogelsang 2003).

Government is a poor entrepreneur. The breakdown of the socialist countries is not the least attributed to this. The statement would even hold true if government were to hire skilled managers and were to give them high-powered incentives similar to their counterparts in industry. Market incentives cannot easily be reproduced by hierarchies (for an early statement see von Hayek 1945; Milgrom and Roberts 1992; Zenger et al. 2011).

A first explanation roots in public choice theory. Success in the booth need not be the same as success in the market. The ultimate goal of government is to stay in office and to win the next election, irrespective of the consequences for total social welfare (for a survey see Tollison 1982).

But even if one were to assume a benevolent government, it is impossible to derive a well-defined utility function of government from first principles, because it is not possible to define these principles, let alone put them into a transitive order. There is no meta-theory that defines whether, say, efficiency is more important than distributive fairness, or adherence to the commands of religion (cf. Sen 1976). Consequently government must find ways how to bridge the contradictory expectations prevalent in society.

A way for government to parry the challenge might be to assign entrepreneurial tasks and managerial supervision to an independent agency which shields public managers from direct control by the electorate, making public managers less vulnerable to self-interested interventions by politicians. But such agencies hold power without control, neither by the market nor by the electorate, which gives room for rent seeking and for pursuing personal preferences, rather than welfare (Jensen and Meckling 1976).

By contrast, firms have hands on knowledge of their markets and a (more) clearcut incentive structure. If they are to stay in the market, and if they are to attract fresh capital on the financial markets, they have to understand what customers want. In principle, the recommendation is therefore well founded that government should stay out of business (Kornai 1982; Datta-Chaudhuri 1990). This recommendation even holds if government has reason to impact on firms' choices, say because the firm uses a technology that potentially imposes harm on outsiders. Ideally government should confine itself to constraining firms' behavior such that market outcomes stay in line with overall welfare (Auriol and Picard 2009). For that purpose, government might, for instance, impose a Pigouvian tax, or auction off emission certificates. Consequently, after the privatization of the network industries, many countries strived for a rule-based approach, price-cap regulation being the most prominent solution (Cambini and Rondi 2010).

Both in the theoretical and in the policy debate, the quest for a mere price or revenue-cap has chiefly been motivated with inducing firms to engage in socially beneficial product or process innovation (Beesley and Littlechild 1983; Crew and Kleindorfer 1986; Laffont and Tirole 1993). We do not question this bright side of price-cap regulation, but wonder whether its dark side has been neglected. The drawback potentially results from the fact that price-cap regulation constrains agencies to occasional intervention in the light of imperfect information.

A second debate to which our paper is linked is that of regulatory choice. In essence this literature is concerned with the sequencing of price regulation. Two generic approaches are distinguished (Sibley and Bailey 1978) (see also Klevorick 1971; Bawa and Sibley 1980; Sappington and Sibley 1988). In the first approach, the regulator anticipates the complete future and sets prices such that a long-term objective function is maximized. In the second approach the regulator is myopic and sets prices in reaction to the difference between a target level of returns and the firm's actual rate of return at this point in time. Our experiment contributes to this debate by manipulating the information set at the moment when the regulator decides. Note, however, that our regulators are not myopic in a strict sense. They do not disregard future consequences of today's decisions. Rather (in some treatments) when they decide they lack the information necessary for taking the exact future effects into account. Information about a distribution and, in one treatment, a noisy signal, is all they can rely on.

A third stream of literature to which our paper is connected is economic experiments on regulatory issues (Eckel and Lutz 2003). In a recent article Normann and Ricciuti (2009) survey this literature, including experiments concerning public utilities. More specifically Staropoli and Jullien (2006) and Kiesling (2005) survey the use of laboratory experiments for the design of energy market regulation. These experiments inform regulatory politics about the comparative advantages and disadvantages of competing proposals for intervention, and aim at spotting unanticipated counterproductive effects before a new policy is implemented (fine examples include Rassenti et al. 2003; Kench 2004; Brandts et al. 2008; Vossler et al. 2009; Henze et al. 2012). Our approach differs in that we endogenize the regulator.

Our experiment is finally linked to the general experimental literature on principal-agent problems resulting from project contracts (Cox et al. 1996; Healy et al. 2007) and to the debate in law and economics and accounting over rules vs. standards (Feldman and Harel 2008; Agoglia et al. 2011; Wright et al. 2011). For the price cap gives the firm certainty for the next 10 periods, while authorities may intervene at their discretion in our *Continuous* treatment. While conventionally it is argued that rules create greater predictability than standards, experiments question this. Wright et al. (2011) show that standards can be as predictable as detailed rules. Feldman and Harel (2008) further show that detailed rules are socially preferable over standards only if social norms strongly guide behavior, while standards lead to better regulatory outcomes in environments in which social norms are relatively weak. Our experiment gives room for the development of a trust relation between the regulator and the regulatee. Whether trust emerges might thus be critical for the performance of the competing regulatory regimes.

3. Design and Hypotheses

In each period, the firm chooses price p. The firm holds a monopoly. Inverse demand is given by $p = a - q$. Supply is characterized by $p = q$. Marginal cost thus increases in quantity, which means that the firm makes a positive profit even if it sets the market clearing price $p^{rncc} = \frac{1}{2}a$. The main reason for this specification of the supply function is experimental. Had we chosen constant marginal cost, or marginal cost decreasing in quantity, if regulators maximize welfare firms make zero profit. We would have had to compensate experimental firms by a substantial show up fee. Profit would only have had a negligible impact on their payoff. In the field regulating firms in markets with increasing marginal cost is not infrequent either. The benefit from regulation results from the difference between monopoly price and the welfare maximizing price. A practical illustration is monopoly resulting from the fact that one firm has superior access to the scarce input that causes marginal cost to increase in quantity. This is characteristic for an "essential facility", a frequent object of regulatory oversight.[1]

If unregulated, the firm sets monopoly price $p^M = \frac{2}{3}a$. In the *Baseline*, the regulator is also completely informed. We then have welfare w as in (1):

$$w = pr + cr = p(a-p) - \frac{1}{2}(a-p)^2 + \frac{1}{2}(a-p)^2 = p(a-p) \qquad (1)$$

[1] "Such a facility is equivalent to a so-called 'natural monopoly,' and to control the use of monopoly power by the owner of the facility, society is faced with the same unappetizing alternatives available in any public utility context: public ownership, regulation in the classic "rate-base/rate-of-return" mold, incentive regulation, and various in-between solutions familiar to policy makers and students of this problem" (Lipsky and Sidak 1999: 1220).

where the first two terms define producer rent pr, and the third term defines consumer rent cr. By our design, we impose regulator rent $rc = \frac{1}{2}w$, and hence $p^{rc} = p^{w} = \frac{1}{2}a$. The regulator prevents the firm from exceeding the welfare maximizing price. At the expected value of a, i.e. at $a = 101$, the regulator sets $p^{rncc} = 50.5$.

Since the monopoly price is strictly above this price, the regulatory constraint binds. Since cost increases in quantity, at the market clearing price the firm still makes a positive profit; this was the reason to choose increasing marginal cost. Therefore the regulator has no reason to consider the possibility that her choice imposes a loss on the firm.

In the *Upfront* regime, the regulator only knows that $a \in [1{,}201]$, and that all realizations of the intercept of the demand function are equally likely. Ex post, there may now be a conflict between welfare and regulation. But from an ex ante perspective, incentives are still aligned. In that case, the regulator in principle would want to maximize

$ru = \frac{1}{200}\int_{a=p}^{a=201}\frac{1}{2}p(a-p)da = \frac{1}{800}p^3 - \frac{201}{400}p^2 + \frac{40401}{800}p$	(2)

which yields

$p^{ru} = 67$	(3)

Note that the lower bound of integration is given by $a = p$, not by $a = 1$. This follows from the fact that quantity may not be negative. Therefore the regulator's problem is constrained by $p \leq a$. The regulator does not want to impose a price such that the firm would have to sell a negative quantity.

From (1), and solving for p, we learn that producer rent turns negative whenever $p < \frac{1}{3}a$. Producers would have to serve a demand that is too large to leave them a profit. Negative profits of regulated firms are unlikely, if not illegal, in regulatory practice, and impractical in the lab. We therefore cut profit at 0 by imposing $p = \max\{p_{regulated}, \frac{1}{3}a\}$. We do, however, not have to adjust (3) to this fact.[2] As we have seen, if she maximizes her payoff, the regulator sets $p = 67$, which is precisely $\frac{201}{3}$, i.e. the price at which the firm makes zero profit if the actual a has its maximum size. Hence in equilibrium the regulated price is never so low that it is replaced by $\frac{1}{3}a$. (3) is a correct definition of the regulator's

[2] In the instructions, we explain that the regulator's payoff is based on welfare in case the firm had set the price as in the price cap if the cap is such that the firm would make a negative profit.

problem, even taking into account that firms' profit is not allowed to turn negative.

In the *Signal* regime, the regulator receives a signal a_s that is correct with probability 50%. With counterprobability 50% the true value of a is a random draw from the uniform distribution. Again, from an ex ante perspective, the payoffs of the regulator and society are aligned. We therefore have

$$rs = \frac{1}{2}rc + \frac{1}{2}ru \tag{4}$$

which leads to

$$p^{rs} = \frac{802}{3} - \frac{1}{3}\sqrt{522001 - 1200a_s} \tag{5}$$

The chosen price cap depends on the signal a_s. If the signal is at the expected value of a, i.e. at 101, the regulator sets $p^{rs} = 56.30$.[3]

Provided regulators are risk neutral and maximize rent, we thus predict price caps as in Table 1.

continuous	signal	upfront
50.5	56.30	67

Table 1
Predicted Price Caps per Treatment
predictions for signal treatment assumes $a_s = 101$

Provided both the firm and the regulator hold standard preferences and this is common knowledge, best responses determine choices. In the *Baseline*, the regulatory constraint always binds and determines price. In the *Upfront* and in the *Signal* treatments, the firm (potentially) has better information than the regulator. Using this information, it chooses

$$p^{fu} = p^{fs} = \min\{p^r, p^M\} \tag{6}$$

[3] Note that, depending on the signal, payoff maximizing regulators may set price caps that, depending on the later realization of a, would lead to a negative profit for firms. We do not adjust the regulator's payoff function to this fact for two reasons. The first reason is experimental. We would otherwise have two changes between the *Upfront* and the *Signal* treatment, one concerning the information environment, the other concerning the payoff function for the regulator. Moreover, plausible adjustments of the regulator's payoff function have undesirable properties. If we were to remunerate the regulator based on total welfare, i.e. on consumer rent only if producers make 0 profit, she would be indifferent between all possible caps. If we were to set her payoff to 0 in case the firm makes 0 profit, we would have a corner solution. Regulators would want to avoid this event at all cost by choosing $p = 201$, i.e. by setting a non-binding price cap.

Provided the monopoly price is below the price cap, the firm sets the monopoly price. Otherwise it goes to the regulated limit.

At the beginning of the experiment, we play this game one-shot. This lets us isolate the effect of different access to information. After a surprise restart, we repeat the experiment 10 announced times. The repeated game gives us a chance to test for the effects of experience, and we have more scope for the analysis of driving forces since we generate panel data. In each period, ex post both the firm and the regulator are informed about the current realization of a. In the *Upfront* and in the *Signal* treatments, the regulator may only once set a price cap, before the firm takes her first decision. At this point, in the *Upfront* treatment the regulator only knows the distribution of a. In the *Signal* treatment, the regulator is informed that, initially, $a = a_s$. The regulator further knows that, within each period, with probability $.1$ there is a random shock, and a is replaced by a new draw from the distribution (which may, of course, also be a_s). It is common knowledge that, during these 10 periods, no more than one shock occurs. Consequently, from an ex ante perspective, the regulator expects a to be replaced by a new draw from the distribution with probability $p = .5$, and it expects $a = a_s$ with counterprobability $1 - p = .5$. In the one-shot version of this game, we inform the regulator that it receives a signal that is correct with 50% probability. Provided both the firm and the regulator hold standard preferences and this is common knowledge, best responses determine choices. On these assumptions, repetition does not change best responses and hence equilibria. Yet comparing the one-shot experiment with the repeated experiment, we see whether having to decide for an extended duration has a separate effect.

After 10 periods we have another surprise restart. We keep pairs fixed. Pairs that were in the *Baseline* now either are in the *Upfront* or in the *Signal* regime. Pairs that were in the *Upfront* or in the *Signal* regime now are in the *Baseline*. After the regime change, money maximizing regulators adjust to the new opportunity structure. We thus have

H_1: Regulators and firms play best responses.

We use three combined approaches to make sure that all participants understand the game. In the instructions, we explain the opportunity structure in words, equations and graphs (see appendix). In control questions, we have participants calculate simple examples. They are only allowed to participate in the experiment after they have answered all questions correctly. Each period each participant has access to a simulator. Participants may try out as many combinations of parameters as they like. The simulator calculates quantity, firm and authority payoff and consumer rent for them. Despite all these safeguards, a first potential qualification is cognitive. Regulators and firms might nonetheless need experience to find their preferred choices. Moreover the standard framework assumes that the firm maximizes profit under the constraint of the cap. In reality this need not be true either. Consequently the regulator may not only face stochastic, but also strategic uncertainty. In that eventuality, the regulator should gain a sense of the firm's behavioral traits, and adjust its intervention. For all of these reasons the *Baseline* may have the advantage of easing regulatory learning. This yields

> **H_2:** In the *Baseline*, caps are closer to the prediction from standard theory

The standard framework further assumes that all agents are risk neutral. In reality this need not be the case. Since in our design firms can directly adjust to any new development, risk preferences do not lead to the prediction of treatment differences for firms. By contrast, risk averse regulators are likely to overshoot if they know they cannot adjust to observed shocks and to observing the firm's past behaviour. This provides an alternative motivation for **H_2**.

In the standard framework, conditional on the realization of a, the regulator can predict the firm's choices with certainty. If the regulator sets a generous cap, or no cap at all, the regulator knows the firm will exploit the opportunity. Experimental work on the "hidden costs of control" suggests that this might not be true (Falk and Kosfeld 2006). If the regulator gives the firm more leeway than it must, the firm might feel obliged to reciprocate by not abusing this freedom. In the *Baseline*, the regulator has enough information to perfectly impose the solution that is best for it. In the remaining treatments, the regulator is unable to observe the current state of the world. If the regulator sets a generous cap, this may of course also be an attempt at creating a trust relationship. Yet for the firm this signal is more difficult to interpret. A generous cap may also result from a lack of understanding, or from risk aversion. In anticipation, investment into a trust relationship is a less attractive strategy in the *Signal* and *Upfront* regimes. Moreover if firms abuse the trust regulators put into them, in the *Baseline* regulators can strike back in the next period by reducing the cap, potentially even by imposing an overly severe cap as a punishment. None of this is possible in the remaining regimes. This leads to a competing hypothesis:

> **H_3:** In the *Continuous* regime, the regulator is most likely to set an overly generous cap. In the *Continuous* regime, firms are most likely to react to a generous cap by setting a price below the monopoly price.

The main experiment was followed by a standard trust game (Berg et al. 1995), with money sent by the trustor to the trustee tripled. Participants first played this game with authorities in the role of trustor, but participants newly matched to groups of two. At this point they did not know that another trust game was to follow with roles swapped, and again new partners. The next part of the experiment was the standard test for risk aversion by Holt and Laury (2002). The experiment ended by a questionnaire. Besides demographic questions, it contained the 10item version of the Big5 personality test (Rammstedt and John 2007) and a survey instrument to measure individual differences in justice sensitivity (Schmitt et al. 2005).

The experiment was conducted in the Bonn EconLab. It was fully computerized, using software zTree (Fischbacher 2007). Participants were invited using software ORSEE (Greiner 2004). 96 students of various majors participated. 55 of them (57.29 %) were female. Mean age was 24.40 years. The experiment lasted approximately two hours. Participants on average earned 22.27 € (28.66 $ on the days of the experiment), range [4.64, 45.68 €].

4. Results

We have designed the experiment such that we can first observe choices of unexperienced participants with no shadow of the future (a). In the second phase of the experiment we study choices of experienced participants who know that they are paired with each other for 10 periods (b). In the third phase we investigate the effects of regime change (c).

a) One-Shot Interaction

A number of findings become immediately visible from descriptive statistics, Figure 1. Given for each pair of participants another intercept of the demand function is randomly selected, the raw data would not be informative. In the left panel of Figure 1, we therefore normalize choices by this intercept, i.e. by variable a. Welfare is maximized at $a = .5$ (the lower line). The monopoly price is $a = .67$ (the upper line). As one sees, irrespective of treatment on average caps are not binding; they are even above the monopoly price. In the *Baseline*, actual prices are on average slightly above the welfare maximizing level, but clearly below the monopoly price. In the *Signal* treatment, actual prices are on average fairly close to the monopoly price. In the *Upfront* treatment, prices are on average even above the monopoly price.

In the right panel of Figure 1, we report how these choices translate into earnings and rents. In this panel, we normalize by the rent in the case welfare is maximized. Since we have chosen increasing marginal cost and the slope of demand to be -1, while the intercept of supply is 0 and the slope of supply is 1, we have a completely symmetric problem. If the market clears, producers and consumers both earn $\frac{1}{8}a^2$. This is also the authority's payoff since she is remunerated by $\frac{1}{2}(consumer\ rent + producer\ rent)$.

In several respects results are remarkable. On average, producer rent differs very little across treatments, despite the fact that prices are on average fairly distinct. This of course follows from the fact that, in treatment *Upfront*, prices are quite frequently even above the monopoly price. In this treatment consumers suffer heavily.

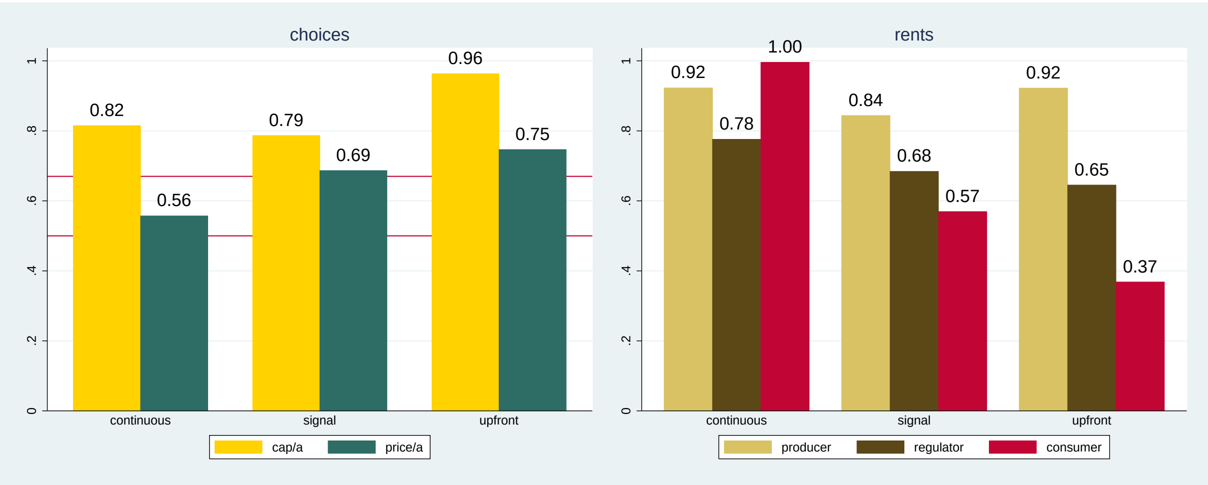

Figure 1
Choices and Rents in One-Shot Game
left panel: normalization through division by a
lower line at $.5 * a$, i.e. the welfare maximizing price
upper line at $.67 * a$, i.e. the monopoly price
right panel: normalization through division by $\frac{1}{8}a^2$, i.e. welfare maximizing rent.

Using a non-parametric Mann Whitney test, we find that (normalized) consumer rent is significantly lower if authorities do only know from which distribution demand is drawn (*Baseline*vs. *Upfront*, N = 36, p = .0136).[4] By the same test, we also find that (normalized) caps (p = .0357) and actual (normalized) prices are significantly higher (p = .0136).

This gives us

Result 1: Authorities set higher caps and firms set higher prices if authorities only know the distribution of demand functions.

b) Experience

As explained in the design section, we have exerted a lot of effort to make sure participants have understood the game. It could still be that the

4 All statistical tests are two-sided.

virtual experience from using the simulator is not as effective as experience from playing the game for real. In the first round, participants only have information about the opportunity structure, not about their counterpart. It could be that this additional information changes behavior. In terms of expected values and of the information environment, there is no difference between the first round and the next 10 rounds. Yet behaviorally it might matter that authorities have to set a cap that remains effective for 10, rather than a single period. Finally if authorities, in the *Continuous* treatment, are entitled to revise the cap, this might change both parties' choices. From these considerations it follows that we change more than one element between the first and the second phases of the experiment. We therefore do not present tests for differences across phases of the experiment. We confine ourselves to testing whether the dark side of price cap regulation is also present in this alternative setting, and use the richer data to test for the driving forces that stand behind our behavioural hypotheses.

As Figure 2 demonstrates, by and large the results from the one-short game replicate. Caps are on average still too high. Even in the *Baseline*, the average cap is close to the monopoly price. Yet caps are now straightforwardly ordered. Caps are lowest in the *Baseline*, higher in the *Signal* treatment, and highest in the *Upfront* treatment. The less the authority knows, the more generous the cap. In the *Signal* and in the *Upfront* treatments, caps are on average even substantially above the monopoly price. On average, prices set by experienced firms do not exceed the monopoly price. Prices are the higher the higher the cap, despite the fact that the average cap is not binding in any treatment. On average, in all treatments firms make an excessive profit, yet the effect is most pronounced in the *Upfront* treatment. In this treatment, consumers suffer most. On average, they only receive little more than 50% of the rent they would have had if the market clears. In this treatment, authorities visibly do a poor job.

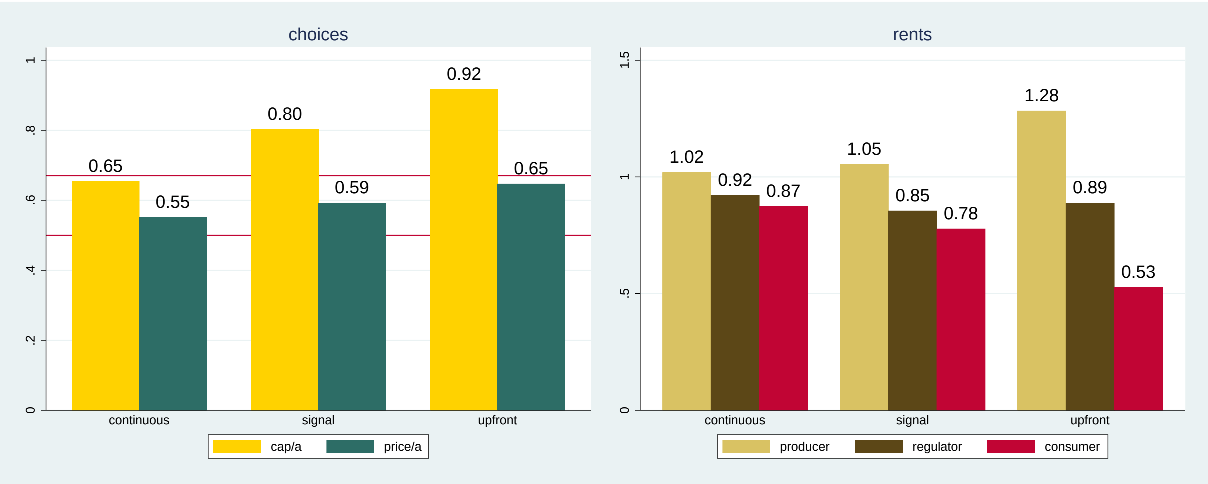

Figure 2
Choices and Rents in Repeated Game

left panel: normalization through division by a
lower line at $.5 * a$, i.e. the welfare maximizing price
upper line at $.67 * a$, i.e. the monopoly price
right panel: normalization through division by $\frac{1}{8}a^2$, i.e. welfare maximizing rent.

Non-parametric tests over means per all 10 periods support the visual impression. (Normalized) caps are significantly higher in the *Upfront* treatment than in the *Baseline* (Mann Whitney, N = 36, p = .0003). The difference between the *Signal* treatment and the *Baseline* is weakly significant (p = .0513). (Normalized) prices are significantly higher in the *Upfront* treatment than in the *Baseline* (p = .0014). (Normalized) consumer rent is significantly lower in the *Upfront* treatment than in the *Baseline* (p = .0003). In this phase of the experiment we also find a significant difference in (normalized) welfare between the *Baseline* and the *Signal* treatment (p = .0292), and between the *Baseline* and the *Upfront* treatment (p = .0810). The dark side of price cap regulation does thus not disappear with experience, or with a shadow of the future, which gives us

> *Result 2: The detrimental effect of forcing the authority to impose a price cap under imperfect knowledge about future development of the envinment on caps, prices, consumer rent and welfare does not disappear with experience.*

As Table 2 shows, we dofind a strong effect of limiting information to just the distribution on authorities' choices. In the *Upfront* treatment, authorities set caps that are considerably and significantly more generous than predicted by standard theory (which already predicts a substantially higher cap, Table 1). Also note the strong and significant constant. If caps were in line with standard theory, the constant should be zero. We thus observe two effects: in the *Baseline* and all treatments caps are higher than predicted by theory; in the *Upfront* treatment this excess is especially strong.[5]

prediction from standard theory	.686^{+} (.361)
Signal	25.893 (15.811)
Upfront	56.238** (17.058)
Cons	41.406* (19.778)
N	48
p model	.0003
adj. R^2	.2978

[5] We replicate the result from Table 2 for every single period of phase 2 if we do not study means over periods 2-11, but compare the choices in the *Signal* and *Upfront* treatment from period 2 with the choices authorities make in any individual period of phase 2.

Table 2
Explaining Cap in Second Phase of Experiment
OLS
dv: mean cap in periods 2-11, per authority
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, + p < .1

We thus have support for **H_2**: in the *Upfront* treatment, caps also exceed the prediction from standard theory in the repeated game. We next investigate whether the force we expected to drive this result is actually present, risk aversion. As Table 3 shows, this is indeed the case. Risk aversion has a weakly significant (p = .059) main effect in the expected direction: the more an authority is averse to risk, the higher a cap she sets. More interestingly even, from the significant interaction effect we learn that risk aversion matters most when this is normatively most troubling: when welfare would call for a particularly severe cap.

prediction from standard theory	1.767*** (.422)
relative risk aversion	89.127+ (45.916)
prediction from standard theory * relative risk aversion	-1.745* (.775)
cons	3.610 (23.741)
N	44
p model	.002
adj. R^2	.2548

Table 3
Effect of Risk Aversion
OLS
dv: mean cap in periods 2-11, per authority
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, + p < .1

In the *Baseline*, we now have scope for exploring the competing hypothesis **H_3**. In that hypothesis we expected that authorities would exploit the right to intervene in every period to establish a trust relationship. Table 4 shows that actually the opposite is true. Irrespective of the opportunity structure (i.e. when controlling for the intercept of demand, or parameter a), we do not find a negative, but a positive effect of the fact that the cap is above the monopoly price. If there is room for exploiting the opposite market side and, given her profit function, exploiting the regulator, firms seize that opportunity.

intercept of demand	.496*** (.020)
cap not binding	16.508*** (2.050)
cons	-4.352 (2.747)
N	240
p model	<.0001
overall R^2	.7913

Table 4
Reaction of Firms to the Fact that Cap Does Not Bind
data from *Continuous* treatment and periods 2-11
linear random effects
Hausman test insignificant
standard errors in parenthesis
*** p < .001

This leads to

> *Result 3: There is no trust relationship between the regulator and the firm.*

In the repeated game, we are in a position to explore a second reason in support of $\mathbf{H_2}$. In the *Baseline* the possibility for regulators to intervene every period might be advantageous since it eases regulatory learning. To test the hypothesis, we consider the decision to change the cap.

Model 1 of Table 5 is additional evidence in favor of $\mathbf{H_2}$: the higher the price (normalized by the opportunity structure, i.e. by parameter a) in the previous period, the more authorities react by reducing the cap.[6] The effect is highly significant (t = -5.18). Model 4 casts further light on the way how authorities use the power to continuously change the intervention. The higher the market clearing price, the more they increase the cap. Authorities do thus not only adjust to the behavior of the firm. They also improve the fit of their intervention with welfare. Interestingly the latter effect is only significant at conventional levels once we control for the presence of the exogenous shock on demand, and its interaction with the prediction from standard theory.[7] Once the shock has occurred, there is a need for adjustment. Essentially the independent effect from the prediction from standard theory is confined to the time before the shock; the main effect for the prediction from standard theory and interaction effect practically cancel out. The model predicts that authorities react to the shock by increasing the cap, irrespective of the direction of the shock.

[6] We estimate fixed effects models since the Hausman test turns out significant. Since all regressors in all models are time-dependent, we still identify all effects of interest.

[7] Variable second intercept of demand is a dummy that is 1 for the period when the shock occurs, and all later periods, until period 11.

This may seem as if the reaction of authorities to a change in the opportunity structure was undesirable. Yet note that the first regressor also captures the change in the opportunity structure. If demand shrinks, but a firm upholds the previous high price, price/a goes up. The model predicts that this triggers a strong reaction by the authority. The positive main effect of the exogenous shock therefore results from the fact that the model must make room for upward adjustments of the cap when they are most likely to be needed, namely after the exogenous shock. We thus indeed have additional evidence in support of $\mathbf{H_2}$. The additional opportunities for intervention provided in the *Baseline* give room for regulatory learning which leads to a better alignment with normative theory.

	Model 1	Model 2	Model 3	Model 4
price/a in previous period	-120.427*** (23.227)	-119.404*** (23.181)	-119.343*** (23.123)	-119.115*** (22.932)
prediction from standard theory		.283 (.197)	.400^{+} (.213)	.763** (.271)
second intercept of demand			10.355 (7.197)	44.173* (17.367)
second intercept of demand* prediction from standard theory				-.723* (.338)
cons	62.961*** (13.119)	48.644** (16.455)	38.265* (17.929)	18.735 (19.994)
N	240	240	240	240
p model	<.0001	<.0001	<.0001	<.0001
within R^2	.1111	.1196	.1281	.1465
between R^2	.1071	.2314	.2282	.2637
overall R^2	.0918	.0939	.0924	.1009

Table 5
Explaining Dynamics of Regulator Action in *Continuous* Treatment
linear fixed effects
dv: first differences of caps
data from *Continuous* treatment and periods 2-11 only
standard errors in parenthesis
*** p < .001, ** p < .01, * p < .05, $^{+}$ p < .1

We conclude

Result 4: If regulators are not constrained from intervening whenever they deem fit, caps and prices are closer to socially optimal due to regulatory learning.

c) Regime Change

In the second phase of the experiment, authorities and firms had experience, but this was experience with the same regime. In the final 10 periods of the experiment we test how authorities and firms react to an unexpected regime change – think of the legislator imposing a change in rules, for instance in reaction to experiences made in other jurisdictions.

As Figure 3 shows, on average caps are still not binding, irrespective of treatment. Descriptively caps are still somewhat higher if the authority is incompletely informed. Yet after the regime change firms no longer exploit the opportunity structure to their benefit. Descriptively, prices are even lowest when caps are most generous. This translates into consumer rent being highest when the authority is least informed. After the regime change, non-parametric tests over means do not show any significant differences between treatments, neither with respect to caps or prices, nor with respect to consumer rent.

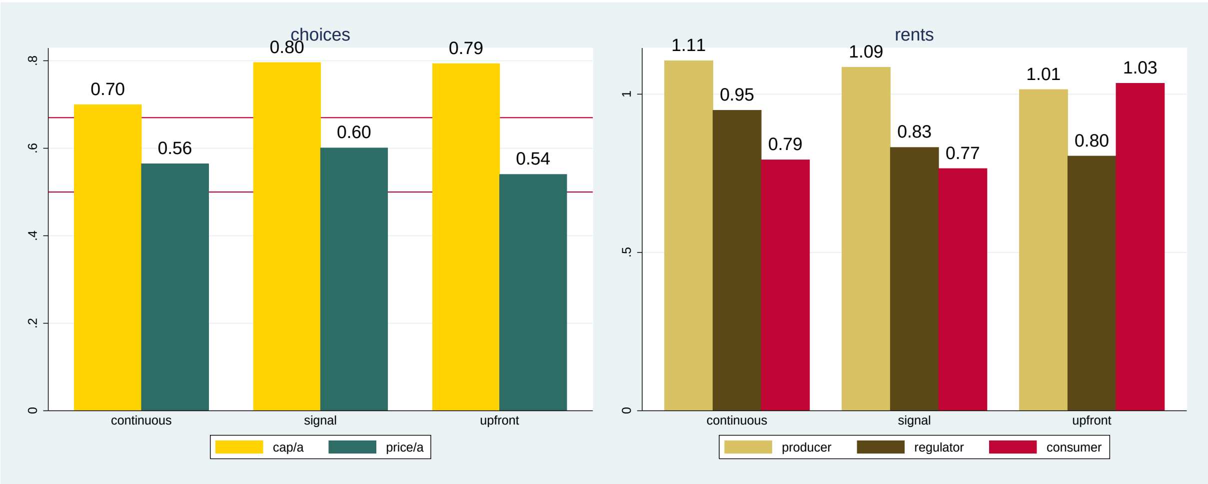

Figure 3
Choices and Rents after Regime Change
left panel: normalization through division by a
lower line at $.5 * a$, i.e. the welfare maximizing price
upper line at $.67 * a$, i.e. the monopoly price
right panel: normalization through division by $\frac{1}{8}a^2$, i.e. welfare maximizing rent.

How can we explain this surprising finding? Figure 4 gives a hint. Irrespective of the sequence, (normalized) prices are fairly close after the regime change to where they were before the change. The descriptive picture suggests that experiences made before the change carry over. Now if authorities previously had the opportunity to intervene continuously, prices have been closer to the social optimum, i.e. to $a/2$.

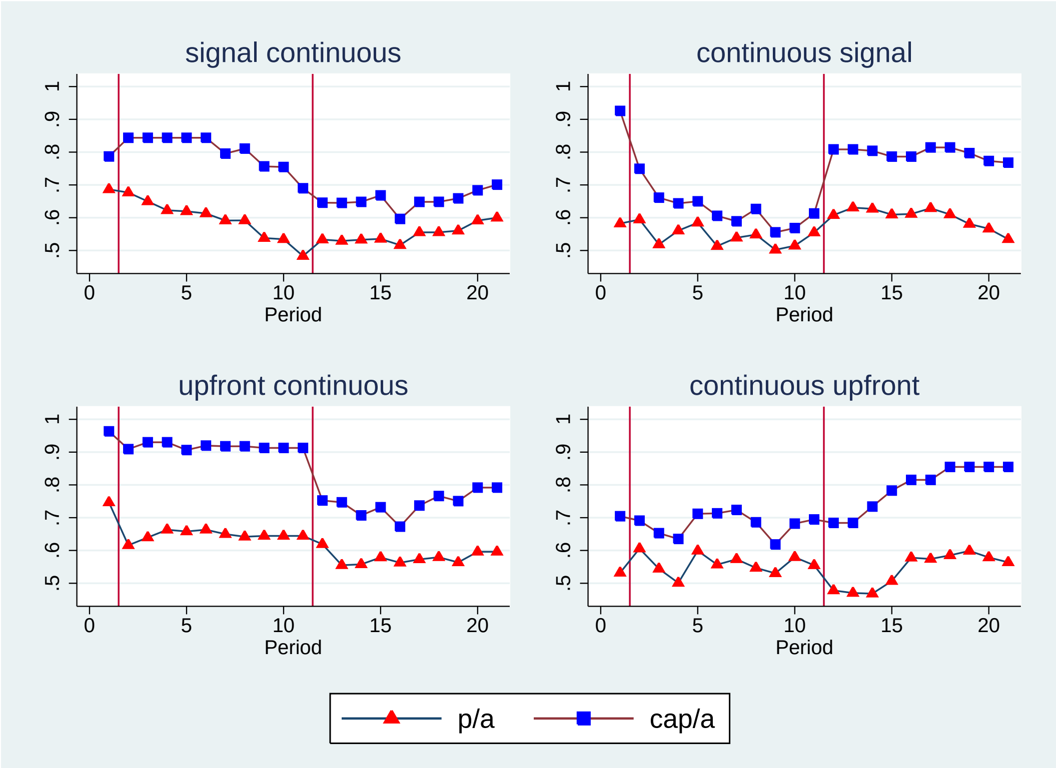

Figure 4
Development of Normalized Price and Cap over Time

The regression in Table 6 provides a statistical test.[8] Obviously this combination of parameters explains prices in the final phase of the experiment very well. All regressors are significant at the highest possible level. Note that the effect cannot be explained by an attempt of firms at building a mutually beneficial trust relationship with the regulator; if the regime shifts to either *Upfront* or *Signal*, the only intervention of the regulator is before the first move of the firm.

mean cap/a in periods 2-11	-.385*** (.099)
price predicted by standard theory	-.009*** (.001)
mean cap/a in periods 2-11* price predicted by standard theory	.009*** (.001)
cap	.001*** (.0001)
cons	.842*** (.078)
N	480
p model	<.0001
overall R^2	.2599

[8] We do not use p/a in the second phase of the experiment to guard against the statistical problems inherent in dynamic panels. As is well known, they are inconsistent, which would force us to use instrumentation, along the lines of Arellano/Bond.

Table 6
Prices after Regime Change
data from periods 12-21
linear random effects
Hausman test insignificant
standard errors in parenthesis
*** p < .001

Yet seemingly we have a puzzling finding. The main effect of the severity of the cap in the second phase of the experiment is negative, not positive. It might seem as if, instead of a carry-over from the second phase, we have firms distancing themselves from what they have experienced in the previous phase. Figure 5 shows that this impression is misleading. The figure collects average marginal effects of the severity of intervention in the second phase of the experiment. The figure shows that the reaction to experiences from the previous phase is conditional on the opportunity structure after the regime change. If circumstances are favorable, i.e. if a and therefore the monopoly price $\left(\frac{2}{3}a\right)$ are high, firms are positively guided by the experiences from the previous phase. If the cap in the previous phase was generous, they now set higher prices. If, by contrast, the cap was strict, they now set a lower price. Now both due to the predictions from standard theory and due to the additional effect of regulatory learning, if participants had previously been in the *Baseline*, caps have been considerably lower (Figure 4). This beneficial effect carries over to price setting behavior after the regime change, even if the authority now has little information and is only able to intervene once. Yet as Figure 5 shows, this beneficial effect is not present if conditions after the regime change are terse, i.e. if there is little demand and therefore monopoly price is low.

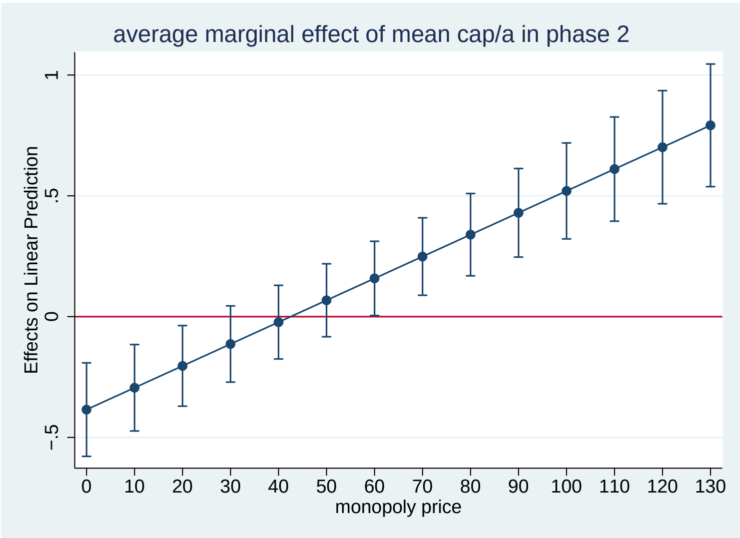

Figure 5
Carry Over from Second Phase on Prices
average marginal effects of mean cap/a in periods 2-11 on choice of prices in periods 12-21
model from Table 6

This gives us

> *Result 5: After a regime change, firms are positively guided by the severity of regulatory intervention before the regime change if the new environment is sufficiently profitable.*

5. Discussion

Price cap regulation has been invented with the best of all intentions. Public officials are less competent than managers to direct firms. They do not bear the pecuniary consequences of their interventions. And, most importantly, by limiting intervention to rare, legally defined moments, firms keep incentives for engaging in socially beneficial innovation. We do not deny any of these benefits. Yet in this paper we investigate the dark side of price cap regulation that has gotten short shrift in the policy debate.

If regulators lack experience, caps are overly generous, even if they have been completely informed about the opportunity structure. Yet if they only know from which distribution the determinants of the environment are drawn, caps are even more generous. In such a setting, prices are higher and consumer rent is lower.

Detrimental effects do not disappear with experience. Authorities still set overly generous caps. Firms still set prices such that the market does not clear. Consumers still get a lower rent than in the competitive case. Authorities do a poor job in aligning firms' behavior with societal interests. Yet experienced firms stop exceeding the monopoly price. By contrast if they only know from which distribution the environment is taken, experienced authorities still set caps in excess of the prediction from standard theory. We find no signs of authorities establishing a trust relationship with firms. We do, however, see that the right to intervene at any point in time eases regulatory learning.

Interestingly, after a change in regulatory regime, we find a carry-over effect. Prices remain close to where they had been before the regime change. If authorities had set generous caps, prices remain high under the new regime, and vice versa, provided the new environment leaves substantial room for making profit.

The overall provision of generous price caps implies a severe welfare problem. The consumer rent is not only smaller than it could be, but the firms' leeway to engage in monopolistic price setting strategies implies also a dead weight loss to society. In the field, over time innovation and a recalculation of the price cap formula might mitigate the welfare problem. However, our findings indicate that the regulator's behavior will constantly impose an excessive burden on society. One may ask whether there are opposing forces which counterweigh the welfare reducing behavior of regulators. Those forces might indeed exist. Beesley and Littlechild (1989) point to competitive market forces which may be induced by incentive regulation. They point to the fact that profitable price caps might trigger market entry. Even though sunk costs might be a hindrance for competitors to enter the market, entrepreneurial spirit and innovation may overcome barriers to entry and force incumbents to set lower prices (Beesley and Littlechild 1989). As a result, in a more dynamic setting our results will prevail, but the negative welfare implications might be mitigated bycompetition. Surely, a precondition for letting competitive forces come into play is that there are no legal barriers to entry for newcomers (Shughart 2008).

Oligopoly experiments have often not been presented as contributions to antitrust policy, but it is straightforward to read them in this way. Anti-trust authorities are increasingly interested in using experimental evidence. It chiefly proves useful for two purposes: many phenomena an antitrust authority would wish to understand are hard, if not impossible to observe in the field; even if observations can be collected, identifying causal relationships is fraught with challenges. Our paper is written in the same spirit. We are of course aware of, and mean to stress, the many ways in which laboratory experiments with student participants differ from the reality of regulatory practice. Extrapolating from the lab to the field always requires a leap of faith. All we are able to contribute to the policy debate is substantiating a concern. Regulators, and legislators for that matter, may have reason to be more optimistic. And they may of course weigh this social cost of price cap regulation against its benefit. When comparing alternative regimes, like rate of return regulation, a price cap may still appear preferable, all pros and cons taken into account.

Yet in the light of our evidence, the dark side of price cap regulation should not be taken lightly. As our model demonstrates, a detrimental effect is even to be expected if firms and regulators perfectly maximize profit. We find unequivocal support for this model prediction in our experimental data. If policymakers want to debate external validity, they should focus on the effects in excess of standard theory that we have found.

We deem it not unlikely that such effects are also relevant in regulatory practice, though. For example, in the UK in 1999 there was as a review of the efficiency of electricity distribution and transmission prices. Even though this review showcased that the incentive regulation scheme of electric utilities in the UK was quite successful, the 1999 review revealed that between the *Initial* and the *Final Proposals* to carry out the efficiency analysis (Ofgem 1999) some changes were made with regard to the benchmarks that electric utilities had to achieve (Pollit 2005). In general a relaxation of benchmark criteria took place, which allowed firms to retain more revenue. We can of course not rule out alternative driving force, but we note that the finding is also in line with our findings in the lab.

One driving force we have identified in the lab is likely to be at least as present in regulatory practice. In our experiment, regulators who set an overly stringent cap just put some experimental income of an anonymous second experimental subject at risk. By contrast if a regulatory authority overshoots, regulated firms can go bankrupt or otherwise be forced to leave the market. They will not be hesitant to blame the authority, which may lead to legislative intervention into the authority's powers. Therefore risk aversion is quite likely to lead to overly generous interventions that leave regulated firms monopoly rents. It also seems quite plausible that regulators do a poorer job if they are deprived of the benefits of regulatory learning. But we hasten to repeat that these extrapolations do, of course, not directly follow from our results. Eventually we can only offer a hypothesis. But this hypothesis is well grounded in experimental evidence.

References

AGOGLIA, CHRISTOPHER P., TIMOTHY S. DOUPNIK and GEORGE T. TSAKUMIS (2011). "Principles-based versus Rules-based Accounting Standards. The Influence of Standard Precision and Audit Committee Strength on Financial Reporting Decisions." Accounting Review 86(3): 747-767.

AURIOL, EMMANUELLE and PIERRE M. PICARD (2009). "Government Outsourcing. Public Contracting with Private Monopoly." Economic Journal 119(540): 1464-1493.

BAWA, VIJAY S. and DAVID S. SIBLEY (1980). "Dynamic Behavior of a Firm Subject to Stochastic Regulatory Review." International Economic Review 21(3): 627-642.

BEESLEY, MICHAEL and STEPHEN LITTLECHILD (1983). "Privatization: Principles, Problems and Priorities." Lloyds Bank Review 149(7): 1-20.

BERG, JOYCE, JOHN DICKHAUT and KEVIN MCCABE (1995). "Trust, Reciprocity, and Social History." Games and Economic Behavior 10: 122-142.

BRANDTS, JORDI, PAUL PEZANIS-CHRISTOU and ARTHUR SCHRAM (2008). "Competition with Forward Contracts. A Laboratory Analysis Motivated by Electricity Market Design*." Economic Journal 118(525): 192-214.

CAMBINI, CARLO and LAURA RONDI (2010). "Incentive Regulation and Investment. Evidence from European Energy Utilities." Journal of Regulatory Economics 38(1): 1-26.

COX, JAMES C., R. MARK ISACC, PAULA-ANN CECH and DAVID CONN (1996). "Moral Hazard and Adverse Selection in Procurement Contracting." Games and Economic Behavior 17(2): 147-176.

CREW, MICHAEL A. and PAUL R. KLEINDORFER (1986). The Economics of Public Utility Regulation, MIT Press Cambridge, MA.

DATTA-CHAUDHURI, MRINAL (1990). "Market Failure and Government Failure." Journal of Economic Perspectives 4(3): 25-39.

ECKEL, CATHERINE and NANCY LUTZ (2003). "What Role can Experiments Play in Research on Regulation?" Journal of Regulatory Economics 23(2): 103-107.

ENGEL, CHRISTOPH (2007). "How Much Collusion? A Meta-Analysis on Oligopoly Experiments." Journal of Competition Law and Economics 3: 491-549.

ENGEL, CHRISTOPH (2010). "The Behaviour of Corporate Actors. A Survey of the Empirical Literature." Journal of Institutional Economics 6: 445-475.

FALK, ARMIN and MICHAEL KOSFELD (2006). "The Hidden Costs of Control." American Economic Review 96: 1611-1630.

FELDMAN, YUVAL and ALON HAREL (2008). "Social Norms, Self-interest and Ambiguity of Legal Norms. An Experimental Analysis of the Rule vs. Standard Dilemma." Review of Law and Economics 4(1): 81-126.

FISCHBACHER, URS (2007). "z-Tree. Zurich Toolbox for Ready-made Economic Experiments." Experimental Economics 10: 171-178.

GREINER, BEN (2004). An Online Recruiting System for Economic Experiments. Forschung und wissenschaftliches Rechnen 2003. Kurt Kremer und Volker Macho. Göttingen: 79-93.
HEALY, PAUL J., JOHN O. LEDYARD, CHARLES NOUSSAIR, HARLEY THRONSON, PETER ULRICH and GIULIO VARSI (2007). "Contracting Inside an Organization. An Experimental Study." Experimental Economics 10(2): 143-167.
HENZE, BASTIAN, CHARLES NOUSSAIR and BERT WILLEMS (2012). "Regulation of Network Infrastructure Investments. An Experimental Evaluation." Journal of Regulatory Economics 42(1): 1-38.
HOLT, CHARLES A. and SUSAN K. LAURY (2002). "Risk Aversion and Incentive Effects." American Economic Review 92: 1644-1655.
JENSEN, MICHAEL C. and WILLIAM H. MECKLING (1976). "Theory of the Firm. Managerial Behaviour, Agency Cost and Ownership Structure." Journal of Financial Economics 3: 305-360.
JOSKOW, PAUL L. and RICHARD SCHMALENSEE (1986). "Incentive Regulation for Electric Utilities." Yale Journal on Regulation 4: 1-50.
KENCH, BRIAN T. (2004). "Let's Get Physical! Or Financial? A Study of Electricity Transmission Rights." Journal of Regulatory Economics 25(2): 187-214.
KIESLING, LYNNE (2005). "Using Economic Experiments to Test Electricity Policy." Electricity Journal 18(9): 43-50.
KLEVORICK, ALVIN K. (1971). "The" Optimal" Fair Rate of Return." Bell Journal of Economics and Management Science 2: 122-153.
KORNAI, JANOS (1982). Growth, Shortage and Efficiency. A Macrodynamic Model of the Socialist Economy, University of California Pr.
LAFFONT, JEAN-JACQUES and JEAN TIROLE (1993). A Theory of Incentives in Procurement and Regulation. Cambridge, Mass., MIT Press.
MILGROM, PAUL ROBERT and JOHN ROBERTS (1992). Economics, Organization and Management, Prentice-Hall Englewood Cliffs, NJ.
NORMANN, HANS-THEO and ROBERTO RICCIUTI (2009). "Laboratory Experiments for Economic Policy Making." Journal of Economic Surveys 23(3): 407-432.
RAMMSTEDT, BEATRICE and OLIVER P. JOHN (2007). "Measuring Personality in One Minute or Less. A 10-item Short Version of the Big Five Inventory in English and German." Journal of Research in Personality 41: 203-212.
RASSENTI, STEPHEN J., VERNON L. SMITH and BART J. WILSON (2003). "Controlling Market Power and Price Spikes in Electricity Networks. Demand-side Bidding." Proceedings of the National Academy of Sciences 100(5): 2998-3003.
SAPPINGTON, DAVID E. M. and DAVID S. SIBLEY (1988). "Regulating Without Cost Information. The Incremental Surplus Subsidy Scheme." International Economic Review 29: 297-306.
SCHMITT, MANFRED, MARIO GOLLWITZER, JÜRGEN MAES and DIMA ARBACH (2005). "Justice Sensitivity: Assessment and Location in the Personality Space." European Journal of Psychological Assessment 21(3): 202-211.

SEN, AMARTYA (1976). "Poverty. An Ordinal Approach to Measurement." Econometrica 44: 219-231.

SIBLEY, DAVID S. and ELIZABETH E. BAILEY (1978). "Regulatory Commission Behavior. Myopic versus Forward Looking." Economic Inquiry 16(2): 249-256.

STAROPOLI, CARINE and CÉLINE JULLIEN (2006). "Using Laboratory Experiments to Design Efficient Market Institutions. The Case of Wholesale Electricity Markets." Annals of Public and Cooperative Economics 77(4): 555-577.

TOLLISON, ROBERT D. (1982). "Rent Seeking. A Survey." Kyklos 35(4): 575-602.

VOGELSANG, INGO (2003). "Price Regulation of Access to Telecommunications Networks." Journal of Economic Literature 41(3): 830-862.

VON HAYEK, FRIEDRICH-AUGUST (1945). "The Use of Knowledge in Society." American Economic Review 35: 519-530.

VOSSLER, CHRISTIAN A., TIMOTHY D. MOUNT, ROBERT J. THOMAS and RAY D. ZIMMERMAN (2009). "An Experimental Investigation of Soft Price Caps in Uniform Price Auction Markets for Wholesale Electricity." Journal of Regulatory Economics 36(1): 44-59.

WRIGHT, E.W., M.P. ELLINGHAUS, N.M. CLOSE and ANDREW HEATHCOTE (2011). The Effect of Rule Determinacy on Deciding Contract Disputes. Experimental Data and Network Simulation http://ssrn.com/abstract=1884195.

ZENGER, TODD R., TEPPO FELIN and LYDA BIGELOW (2011). "Theories of the Firm–market Boundary." Academy of Management Annals 5(1): 89-133.