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## Relational Collusion in the Colombian Electricity Market

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# Relational Collusion in the Colombian Electricity Market

## Abstract

Under collusion, firms deviate from current profit maximization in anticipation of future rewards. As current profit maximization places little restrictions on firms' pricing behaviour, collusive conduct is hard to infer. We show that bids from certain firms in the Colombian wholesale electricity market collapsed immediately after the announcement, and before the implementation, of a reform that potentially made collusion harder to sustain. After ruling out confounders, we uncover how the cartel functioned and how firms may have communicated. Calibrating the dynamic enforcement constraint confirms that collusion was sustainable before, but not after, the reform. The conclusions discuss policy implications.

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# RELATIONAL COLLUSION IN THE COLOMBIAN ELECTRICITY MARKET \*

Mario Bernasconi<sup>†</sup> Miguel Espinosa<sup>‡</sup> Rocco Macchiavello<sup>§</sup> and Carlos Suarez<sup>¶</sup>

## 1 Introduction

Parties often rely on informal arrangements sustained by the value of future interactions to cooperate when contracts are unenforceable ([Macchiavello, 2021](#)). These arrangements benefit their participants but may harm the market as a whole. Horizontally competing firms colluding to raise prices – cartels – offer perhaps the most prominent example. Such cartels might be particularly relevant in developing countries, where entry barriers protect colluding incumbents ([Djankov et al., 2002](#)), competition authorities are weaker – if at all existent ([World Bank, 2016](#); [Besley et al., 2020](#)) and markets thinner and more concentrated ([Mitton, 2008](#); [Leone et al., 2022](#)).

Despite the policy relevance, evidence on cartels in low-income countries and how they function remains scarce. Most empirical studies focus on cartels investigated by competition authorities. As those are weaker in developing countries, fewer documented examples exist.<sup>1</sup> Furthermore, collusive behaviour is notoriously difficult to identify (Chassang and Ortner, 2022). In models of collusive behaviour, firms deviate from current profit maximization in anticipation of future rewards. Profit maximization, however, places little restrictions on firms' behaviour making these models hard to test and collusive conduct hard to infer from pricing behaviour alone.

This paper uncovers collusion in the Colombian energy sector. Access to adequate sources of reliable and cheap energy is a critical engine for development (Greenstone et al., 2014). Besides its intrinsic relevance, the context enables us to develop a novel test of collusive behaviour supported by future rewards. First, the Colombia wholesale electricity market is regulated and therefore detailed data from its uniform price multi-unit daily auctions are available. Second, we take advantage of a regulatory change. In 2008 the market witnessed a significant increase in prices. During a meeting on January 6<sup>th</sup>, 2009 (the *announcement* date), the regulator, concerned with the price increase, invited Professor Peter Cramton to advise on market rules. At that time, the operator disclosed all information to all market participants with two days delay. Such transparency increases market efficiency and simplifies monitoring and implementation. Cramton, however, had previously advised regulators on how transparency also facilitates collusion and was thus expected to recommend a tightening of the transparency policy. Indeed, Cramton recommended increasing to 90 days the delay to disclose information to market

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<sup>1</sup>For example, the Private International Cartel database (Connor, 2020) reveals that only 5% of proven cartels are in Africa (72% of those were proven in South Africa alone), 7% in Latin America and 11% in Asia.

participants during a presentation delivered on January 24<sup>th</sup>, 2009. The regulator adopted the recommended change on January 30<sup>th</sup>, 2009, with effect from February 6<sup>th</sup>, 2009 onward (the *implementation* date).

A subset of the firms in the market lowered bids by between 47% and 30% immediately after the *announcement* – and well before the actual *implementation* – of the regulatory change. Consistently with the key implications of models of collusion, (at least some) members of the cartel reacted to the announcement in an anticipatory way, leading to its instantaneous unravelling. The logic of the test doesn't rely on cartel members perfectly foreseeing the actual change in transparency enacted by regulators and how it would make the cartel unsustainable. It simply requires some of them to become sufficiently pessimistic about their *future* ability to sustain a cartel. Inter alia, our strategy allows us to rule out several confounders, including the fact that changes in market transparency itself could alter firms' bidding behaviour. We also investigate the effect of unannounced inspections both before and after the announcement to explore whether participants might have also updated beliefs about the likelihood of enforcement and find little support for such a mechanism. The evidence shows that dynamic enforcement considerations underpin collusive behaviour – an observation with policy implications that we discuss in the conclusions.<sup>2</sup>

It is important to clarify what our evidence is *not* meant to prove. The evidence isn't sufficient to pin down a particular equilibrium concept of collusive conduct. For example, while subgame perfect equilibrium (SPE) relies on players reacting to information instantaneously (which is consistent with our evidence), it also assumes correct beliefs about future payoffs and play

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<sup>2</sup>We complement our main findings with a forensic analysis that uncovers the mechanisms through which firms colluded as well as suggestive evidence of communication. We also calibrate the dynamic enforcement constraints required to sustain the cartel and confirm that they were satisfied before, but not after, the reform.

both on- and off- equilibrium paths (about which, instead, our evidence is essentially mute). Our test for collusion does not assume – and the evidence certainly does not prove – that firms in the cartel were playing SPE. Similarly, the evidence is consistent with – but doesn’t prove that – limited feedback about auction outcomes leading to the cartel’s collapse.

Section 2 provides background information on the Colombian wholesale electricity market, the regulatory change, and the data. We describe the ideal dispatch (i.e., the production allocation resulting from submitted bids) and the real dispatch, in which the market regulator allocates production taking into account shocks to the transmission network. This is done through a process of positive and negative reconciliations which, as we later clarify, plays a critical role in our analysis.

Section 3 presents the main evidence and rules out confounders. [Chassang and Ortner \(2022\)](#) elucidate the challenges involved in identifying collusive conducts in the data: e.g., non-competitive behaviour is not necessarily collusive (e.g., firms might make mistakes); in dynamic environments, pricing behaviour can deviate from static profit maximization without implying collusive conduct. Our test identifies an *instantaneous* response in anticipation to future changes in market conditions that is the central implication of reward-punishment schemes at the heart of collusive equilibria and arguably overcomes most of these challenges.

Unlike studies that rely on proven cartels (see, e.g., [Porter and Zona, 1993](#); [Asker, 2010](#); [Igami and Sugaya, 2021](#)), we do not know the identity of the firms participating in the collusive arrangement – if one existed. We construct several proxies for cartel membership to sharpen our test. In our baseline definition, we conjecture that thermal units in the Atlantic region had the



incentives and ability to form a cartel.<sup>3</sup> This classification isolates a group of 14 units – henceforth, the *cartel*. Using both DID and more flexible event-study specifications, we show that the average bid for cartel units falls after the announcement, and before the implementation of the regulatory change.<sup>4</sup>

Section 4 conducts forensic analysis to uncover the incentives, and strategies used, to collude. When awarded a positive reconciliation, a unit is paid a price proportional to its bid rather than the lower market clearing price emerging from the ideal dispatch. Using an instrumental variable strategy, we thus begin by confirming that firms submit higher bids when they expect to be awarded a positive reconciliation. We then show that units in the cartel coordinated their bids. Specifically, some units increased bids particularly so at times in which other units in the cartel bid low prices, win the ideal dispatch, and subsequently declare unavailable thereby generating positive reconciliations for the high bidders in the cartel. This coordinated behaviour shows up only for cartel units and ceases after the reform.

Courts require evidence of the express agreement and overt communication to declare collusive behaviour illegal (Chassang and Ortner, 2022). We do not observe whether members of the collusive arrangements explicitly communicated and/or whether they used transfers to share the spoils. However, we look into both issues. First, we use data from the minutes of the meetings of the Association of Generating Units (CNO in Spanish). The association holds regular meetings to discuss engineering problems related to technical difficul-

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<sup>3</sup>The rationale for this choice is that thermal units have higher costs and can't make profits in the ideal dispatch. We thus hypothesize, and later confirm, that thermal units profit from colluding on the positive reconciliations market. Because positive reconciliations occur when there are disruptions to transmission or generation, units are more likely to compete for positive reconciliation with nearby units. This justifies the regional focus of the cartel despite the unique national market.

<sup>4</sup>Results are robust to alternative definitions of the cartel, several confounders and placebo specifications.

ties and constraints on the network and prohibits discussions about bidding behaviour. We downloaded the minutes of all the meetings in 2008 (during the cartel) and 2009 (after the announcement date). Within a DID framework, we find that after the reform, units in the cartel stopped sending employees involved in setting bids to the meetings. The meetings of the association might have thus been used to discuss bids and collude. Second, if the forensic analysis is correct, we expect that profits – particularly from positive reconciliations – fall relatively more for cartel units after the end of the cartel. We confirm this to be the case in the data.<sup>5</sup>

Section 5 quantifies the incentive to collude and the cost of the cartel for consumers. First, we calibrate the optimal static bidding strategy and show that, before – but not after – the announcement date, cartel units could increase static profits by submitting lower bids. In contrast, bids from similar non-cartel units are in line with profit maximization. We then embed such deviations into a dynamic incentive compatibility constraint. For reasonable parametrizations of the discount factor, such deviations are not incentive compatible under the old transparency rule but become so under the new rule. As noted above, our evidence is not meant to establish that the change in transparency led to the demise of the cartel. The estimates, nevertheless, provide a sanity check that such an interpretation would be consistent with economic magnitudes in our context.

Counterfactuals allow us to provide a lower bound of the excessive costs paid by consumers for electricity during the cartel. The cartel increased by 12% the price paid for positive reconciliations. Positive reconciliations account for approximately 10% of the electricity procured by the regulator, but since they are paid above the spot price, the overall increase in costs was 2.5-3%.

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<sup>5</sup>Across units in the cartel, however, profits fell for *all* units – regardless of their costs and role in the cartel. Transfers may *not* have been needed to sustain this cartel.

Section 6 discusses the policy implications of our results.

This paper contributes to three branches of the literature on firms in developing countries: on collusion, on energy markets, and on relational contracts.

We contribute to the empirical literature on collusion (see [Asker and Nocke, 2021](#), for a recent review). A first branch of the literature studies known cartels to gain insights into their functioning and quantify associated efficiency losses (see, e.g., [Porter and Zona, 1993, 1999](#); [Asker, 2010](#)). [Igami and Sugaya \(2021\)](#) calibrate the dynamic incentive compatibility constraint of the collusive arrangement in the international vitamin C cartel to perform counterfactuals and is particularly related to our paper.

A second branch designs empirical tests to detect anti-competitive behavior when a cartel has not been proven. [Porter \(2005\)](#) and [Harrington \(2008\)](#) provide overviews of the literature. [Chassang and Ortner \(2019\)](#) study of procurement in Japan derives a test from the dynamic enforcement constraint and is particularly related to our paper. They note that higher minimum prices can make punishment less effective and lead to lower winning bids. Instead, we exploit the fact that the announcement of a *future* change in market transparency leads to the instantaneous demise of the cartel. [Chassang and Ortner \(2022\)](#) discuss the processes involved in regulating collusion, including the information required not just to mark collusive behaviour as illegal, but even to hear a case and begin an investigation. The logic of our empirical test and the combination of forensic approaches are applicable to other contexts and might help meet the informational hurdle, at least in some cases.<sup>6</sup>

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<sup>6</sup>Indirectly, we also contribute to ongoing debates on collusion and market transparency. Conventional wisdom holds that transparency facilitates collusion (see, e.g., [Whinston, 2008](#); [Perloff and Carlton, 1999](#)). A number of notable contributions, e.g., [Genesove and Mullin \(2001\)](#) study of the sugar cartel in the U.S. and [Albæk et al. \(1997\)](#) analysis of the Danish antitrust authority's decision to publish firm-specific transactions prices of ready-mixed concrete in three regions, support this view. The evidence and theoretical literature on the matter, however, is less conclusive. [Sugaya and Wolitzky \(2018\)](#) argue that transparency

There is a general perception, but limited evidence, that cartels are particularly common in developing countries (World Bank, 2016).<sup>7</sup> The critical role of electricity for the development process is increasingly appreciated (Rud, 2012; Lipscomb et al., 2013; Greenstone et al., 2014; Allcott et al., 2016). A recent review (Greenstone et al., 2019) notes that “rigorous evidence from developing countries on market design is lacking” (see also World Bank, 2019).<sup>8</sup> Intrinsic features of electricity markets make them prone to abuse of market power and even collusion – evidence on which policies improve market efficiency is thus particularly valuable. For example, through counterfactual simulations, Ryan (2021) finds that a more integrated grid would increase surplus by 22% in the Indian market.<sup>9</sup>

Finally, markets in developing countries are characterized by weaker formal contract enforcement and governance, making the study of relational contracts particularly important (see, e.g., Macchiavello, 2021, for a review). The key difficulty in testing models of relational contracting is that neither the *future* value of the relationship nor the *current* temptations to deviate are typically observed. Macchiavello and Morjaria (2015) tests the implications of a relational contracting model exploiting information on temptations to deviate and an exogenous supply shock in the Kenya flower sector. Blouin and Macchiavello (2019) uses unanticipated increases in temptations to deviate to test for, and quantify, the extent of opportunistic behaviour in the international

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can hinder cartels by helping firms devise more profitable deviations and discuss examples in which that appears to have been the case.

<sup>7</sup>Asker and Nocke (2021) review cites only two studies on collusion in developing countries (Bergquist and Dinerstein, 2020, on Kenya maize and Barkley, 2020, on Mexican insulin).

<sup>8</sup>The literature on energy markets in advanced economies is vast (see Kellogg and Reguant, 2021 for a review). Fabra and Toro (2005) test for collusion in the Spanish market.

<sup>9</sup>A few papers study the Colombian electricity market, albeit with a different focus (Camelo et al., 2018, on centralized unit commitment, Fioretti and Tamayo, 2021, on substitution between fossil fuels and hydropower, and Suárez, 2022a,b, on the interaction of market power and public ownership).

coffee market. We contribute a test for relational contracting that relies on changes in *current* behaviour in anticipation of changes in the *future* value of the relationship – a central implication of relational contracting models.<sup>10</sup>

## 2 Institutional Setting & Background

This section describes the Colombian wholesale electricity market and the timeline of events used in Section 3 to detect the collusive arrangement.

### 2.1 Electricity Demand and Generation

The average daily generated electricity in Colombia was 149.81 GWh in 2009.<sup>11</sup> In 2008/2009 electricity was produced by 47 generation units. Among these units, 32 units owned by 11 private firms produce about 70% of the market output, and the rest is produced by publicly owned units. The market was a moderately concentrated oligopoly in 2008/2009 with a Herfindahl-Hirschman index of installed capacity around 1306 (see [CREG, 2009a](#)). The 4 largest firms accounted for 65% of installed capacity.<sup>12</sup> Data are described in Appendix A.1.

Bids to supply electricity in the wholesale market are submitted by individual units. Most of our analysis, therefore, considers units, rather than firms, as the relevant decision-makers. However, we use information on firms' ownership of units for robustness checks and to gain further insights into the functioning of the collusive agreement.

Electricity was generated using different technologies: 66.7% hydro-power, 32.9% thermal generation (20.4% gas-fired, 7.3% coal-fired, and 5.2% other

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<sup>10</sup>[Ghani and Reed \(2022\)](#) study how relationships evolved in response to an increase in supply in the Sierra Leone market for ice, [Macchiavello and Morjaria \(2021\)](#) finds that higher competition inhibits relational contracting in the Rwanda coffee chain.

<sup>11</sup>For comparison, it was: 1277,15 in Brazil, 340,82 in Argentina, 260,93 in Pakistan, 54.18 in Nigeria, 24.54 in Ghana, 937,02 in the UK, and 10822,82 in the US.

<sup>12</sup>These values are typical of other developing countries, for instance, the HHI index was 3.500 in Kenya, 2,300 in Peru, and 677 in Pakistan [World Bank \(2016\)](#).

fuels). Thermal generation mainly relies on gas and coal. In 2009, 82% of gas consumption for electricity generation came from the basin Guajira, located on the northern coast of the country. Colombia was the fourth largest exporter of Coal in 2009. Most coal-fired units are located close to large coal mines. Coal is usually transacted through long-term contracts with negotiated prices.

## 2.2 Colombian Wholesale Electricity Market

Electricity markets are characterized by volatile demand, prohibitively high storage costs, and economies of scale. To improve efficiency, encourage participation, and minimize expected payments to generators, many countries trade electricity through auction mechanisms. The Colombian wholesale electricity spot market works as a uniform price multi-unit procurement auction.<sup>13</sup> Once a day, each unit submits its hourly availability and a unique bidding price for the next day. Although only one bidding price is allowed for each unit per day, the Colombian wholesale electricity market clears every hour. There are no intra-day balancing markets and the same spot price is paid in all the regions.

Once the units have submitted their bids,  $XM$ , the system operator, minimizes the cost of fulfilling the demand for each hour, by arranging in increasing order the submitted bids. For each hour, the price that clears the market, the *spot* price, is the bidding price of the marginal unit necessary to fulfill the demand. This process, which does not consider transmission network restrictions, gives rise to the *ideal dispatch*. It establishes for each unit how much and at which hour it should supply energy to the system. Throughout the sample period, hydro-power units tended to have significantly lower costs than thermal units and were the marginal bidder around three-quarters of the time.

Once the ideal dispatch has been determined, contingencies such as trans-

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<sup>13</sup>Uniform price multi-unit auctions electricity markets include Spain (Fabra and Toro, 2005), Texas (Hortaçsu and Puller, 2008) and U.K. (Crawford et al., 2007).

mission constraints may arise and make unfeasible the initially planned allocation.<sup>14</sup> As a consequence, *XM* proposes a different set of production assignments, called the *real dispatch*. Units that were initially called upon to produce but cannot supply electricity to the network do not do it, while units that were not called upon may be called in. To compensate the generators for the differences between the *ideal* and *real dispatches*, the market operator has a scheme called positive and negative reconciliations.

A unit receives a *positive reconciliation* when the real dispatch allocation is greater than the ideal dispatch. In that case, the system compensates each energy unit at a price equal to the minimum between a cost-based regulated price and the generation unit's bidding price. In case two or more units are eligible to be called for positive reconciliations, the system regulator selects the one with the lowest bidding price. A *negative reconciliation* arises when the real dispatch generation is less than the ideal dispatch generation. The system compensated these units at a price equal to the average between the spot price and the unit's bid.

### 2.3 Change in Transparency Policy

The average bid in the wholesale electricity market markedly increased during 2007/2008 (see Figure A1). The electricity market regulator began to suspect that, among the potential reasons to explain the sharp increase, anti-competitive practices might have been at play.<sup>15</sup>

Figure 1 summarizes the timeline of events leading to the policy change. To deal with the price increase, the authorities held a meeting on January

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<sup>14</sup>The actual availability can be lower than the expected availability due to exogenous reasons (e.g., production shocks to the unit) or to strategic decisions (the unit decides to produce less than declared to the regulator ahead of the auction –i.e., declare unavailable some or all the initial production capacity). In the data, we are unable to distinguish between these two motives. Regulators investigate units that declare unavailabilities frequently.

<sup>15</sup>See (Superintendencia de Servicios Públicos, 2008) and CREG (2009a), page 74.

6th, 2009, a date that we label *announcement date*, to discuss measures to deal with the increases in bids. During this meeting, it was decided to hire Professor Peter Cramton as a consultant for the case to advise on potential changes to market design, including its transparency policy.

Cramton advised several governments on auction design before advising Colombian regulators. In particular, and possibly known by Colombian market participants, Cramton consistently mentioned the importance of considering the relative costs and benefits of market transparency. On the one hand, transparency might improve efficiency, but on the other hand, it might facilitate collusion (Cramton and Wilson, 1998). In those cases where the market is expected to suffer from collusion, Cramton argued against a fully-transparent policy (Cramton and Wilson, 1998; Cramton and Schwartz, 1998*a,b*).

On January 24th, 2009, Cramton recommended changing the bidding disclosure policy. Before the implementation of the policy, production schedules (ideal and real dispatches) and bidding prices at date  $t$  were released as public information *two* days after (in  $t+2$ ). Cramton recommended revealing all bids at  $t+90$ , only 90 days after the auction took place.

Following his advice, regulators approved Resolution 006 on January 30, 2009 with effect on February 6th, 2009 (CREG, 2009*a*). The law mandated that from then onward day  $t$  production schedules and bidding prices would become public information only ninety days after (in  $t+90$ ). The spot price for each hour of the day  $t$  was still available to everyone, two days after. Privately though, each generation unit was informed whether or not they won in the multi-unit auction or they had any type of reconciliations. The measure also mandated that the generating units kept their bidding programs' information secret from other units. The law established that failure to comply with the disclosure policy would be sanctioned.



### 3 Detecting Collusive Agreements

This Section provides evidence that a cartel was likely operating in the Colombia wholesale electricity market. We begin explaining the logic of our empirical test and present the sudden decrease in bids around the time of the reform. Then, we introduce a proxy for cartel membership and present DID and event-study specifications. Finally, we perform several robustness checks and rule out confounding explanations.

#### 3.1 The Logic of the Test for Collusive Behaviour

The argument has two parts. First, we argue that a central implication of repeated-games models of cartel behaviour is that shocks to parties' *future* ability to detect deviations should lead to *instantaneous* changes in behavior. Second, we review theoretical arguments regarding the role of information in auction markets and its role in facilitating collusion. Although our test does not rely on potential cartel members having correctly anticipated the ensuing regulatory change at the time of the announcement, we still describe how the reform likely reduced parties' *future* ability to punish deviations leading to an *instantaneous* unraveling of the cartel. The test allows us to check the existence of a cartel and whether it was dissolved by the regulator's actions. It is however important to stress that the test does not allow us to definitively conclude that it was the anticipation of a less transparent market regime that induced the cartel's demise.

In models of collusive behaviour firms deviate from current profit maximization in anticipation of future rewards. As current profit maximization places little restrictions on firms' pricing behaviour, these models are hard to test and collusive conduct is hard to infer from pricing behaviour alone (Ortner et al., 2022). Repeated-game models of collusive behaviour share a central

insight with models of relational contracting: the *future* value of the relationship – the discounted (expected) difference in the payoffs from cooperation and defection – deters *current* temptations to deviate – the difference in payoff between deviating from the agreement and sticking to it (Baker et al., 2002). The key difficulty in testing these models is that neither the *future* value of the relationship nor the *current* temptations to deviate are typically observed (Macchiavello, 2021). The former depends on discount rates, on beliefs about other players’ future behaviors on- and off-the-equilibrium-path. The latter on the off-the-equilibrium-path payoffs associated with defection. Discount rates are difficult to estimate, and beliefs and off-the-equilibrium-path actions are not observed in the data.

A key implication of these models, however, is that *anticipated* changes to *future* relationship value should lead to *instantaneous* changes in behaviour. To the extent that the announcement date induced at least some of the members of the potential cartel to become sufficiently pessimistic about their ability to sustain a collusive arrangement in the future, the ideal test can be implemented in our context exploiting the difference between the *announcement* and *implementation* dates. Of course, we cannot prove that any of the firms anticipated the exact reform, nor that they were able to work out its implication for the equilibrium of the collusive arrangement, if one was indeed being played at all. *A fortiori*, the test does not allow us to infer the exact equilibrium (e.g., a subgame perfect one) played by firms. We return to a discussion of this issue further below.

Transparency can potentially increase efficiency and simplify implementation (see, e.g., Cramton and Wilson, 1998).<sup>16</sup> Transparency, however, can also

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<sup>16</sup>For example, better knowledge of the residual demand allows hydro units to improve the inter-temporal allocation of scarce water resources. Also under transparency, the regulator does not need to worry about her employees or generation units sharing information with other market participants.

facilitate collusion (see, [Perloff and Carlton, 1999](#); [Whinston, 2008](#), and [Cramton and Schwartz, 1998a,b](#)). For instance, if the regulator reveals the bids of all bidders, a cartel faces a much easier problem in policing its agreement. A reduction in market transparency worsens parties' ability to detect, and increases payoffs from, defection. Of course, changes in market transparency could influence behaviour through different channels.<sup>17</sup> In our case, the difference between the *announcement* and the *implementation* dates allows us to rule out such confounders: holding constant current temptations to deviate, the anticipation of less transparency in the future makes it harder to satisfy current dynamic incentive constraints and immediately increases the likelihood of defection.

**Test for Collusion:** *At least some of the units that belong to a cartel sustained by a relational arrangement lower their bids after the announcement, and before the implementation, of the regulatory change.*

### 3.2 The Main Fact & Proxying for Cartel Membership

We do not know the identity of the firms participating in the collusive arrangement. Yet, such information, or a proxy for cartel participation, would allow us to sharpen our empirical test and investigate mechanisms. We thus construct a proxy for cartel membership. To define a baseline proxy, we put forward two characteristics of the units that we believe, on a priori grounds, to be correlated with units' incentives to enter, and ability to sustain, a collusive arrangement. Specifically, in our baseline definition, we hypothesize that cartel units are those *thermal units located in the Atlantic region*. All but one of the 15 units in the Atlantic region are thermal. The baseline definition yields

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<sup>17</sup>For example, more information provides firms with more precise estimates of their residual demand curve potentially altering bidding behaviour.

14 units in the cartel (9 private and 5 public) belonging to 5 firms.<sup>18</sup>

It is worth describing the rationale for our baseline choice. First, thermal units have larger marginal costs than hydro units (Knittel and Stango, 2003). Due to higher costs, thermal units do not win on the ideal dispatch even when they bid at marginal costs. We hypothesize, and later confirm, that thermal units might profit from colluding on high bids through the process of positive reconciliations. As explained above, conditional on receiving a positive reconciliation, a unit receives a price that is tightly linked to its bid – potentially strengthening the incentives to collude.

At the same time, because positive reconciliations occur when units that won the ideal dispatch face disruptions (e.g., due to network transmission reasons), units nearby are more likely to compete with each other for positive reconciliations. Units in the Atlantic region are relatively isolated and are thus more likely to interact in the positive reconciliation market (see Figure A2).<sup>19</sup>

Figure A1 shows a large drop in the average bidding price around the policy change described in Figure 1. Zooming in around the regulatory change and splitting units into two groups – cartel units and the rest –, Figure 2 shows a sharp decrease in the average bidding price right after the *announcement* date *only* for cartel units. The average price for these firms falls by about 43% – the price for other units barely moves.<sup>20</sup>

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<sup>18</sup>Barranquilla 3 and 4, Guajira 1 and 2 and Tebsab from firm GECELCA; Cartagena 1, 2 and 3 from firm EMGESA; Flores 2 and 3 from firm COLINVERSIONES; Proelectrica 1 and 2 from firm PROELECTRICA and finally, Termocandelaria 1 and 2 from firm TERMOCANDELARIA.

<sup>19</sup>Besides the local market created by network constraints, collusion might be more likely to occur under weak institutions: The Atlantic region has the worst governance and highest corruption in Colombia (Duque, 2014).

<sup>20</sup>To counter the negative effects of El Niño, the 90-days disclosure rule was eliminated on December 3, 2009 and the market reverted back to the older two-days disclosure policy (CREG, 2009b). Figure A3 does not show any different behavior around this policy change for cartel vs non-cartel units. Given the difficulty of building a collusive arrangement and its fragility (Byrne and De Roos, 2019), we do not expect the change in policy to necessarily result in a new cartel or the old cartel reverting back to its old behavior.

This parsimoniously constructed proxy might be imprecise and/or *ad hoc*. We thus explore robustness and alternative definitions along several dimensions. First, we note that the baseline definition *does not* rely on the unit’s bidding behaviour around the time of the policy change – there is thus no mechanical correlation between variation used to proxy for cartel membership and bidding behaviour around the time of the reform. We exploit, however, information on units’ bidding behaviour in a battery of robustness checks. Second, our proxy might suffer from both type-I and type-II errors. Provided our proxy is moderately positively correlated with actual membership in the cartel, miss-classification of units into (and out of) the cartel leads to attenuation bias, making it harder for us to detect collusive behaviour (see [Mirenda et al., 2022](#), for a similar argument). Nevertheless, we explore alternative definitions of the cartel proxy, relying both on variation in the characteristics considered for classification, on additional information, and on placebos.

Table 1 presents descriptive statistics for units classified inside and outside the cartel before and after the announcement date. Besides providing descriptive statistics, the top panel reveals patterns consistent with intuition.<sup>21</sup> Bids from cartel units are about 4 times larger than bids from non-cartel units. This contrasts with cost differences that are only about 2 times larger for the cartel group. Cartel units have a lower probability of receiving positive reconciliations, and conditional on receiving them, revenue from these reconciliations is larger for cartel units than non-cartel ones. Note that units receive positive reconciliation infrequently. It is thus almost impossible to infer deviations of other cartel members solely relying on one’s own reconciliations information. Market transparency might be needed for a cartel to function.<sup>22</sup>

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<sup>21</sup>As expected, units classified in the cartel are more likely to be privately owned (64% vs. 48%). Private units maximize profits while public firms might maximize profits as well as other objective functions ([Barros and Modesto, 1999](#)).

<sup>22</sup>Cartel units have a lower fraction of forward contracts, relative to other units. As firms

Turning to the comparison between the top and bottom panels, we see that the bids of units classified in the cartel decreased after the announcement of the policy significantly more than the bids of other units. Furthermore, the amount of positive reconciliations and associated revenues decrease, while availability increases more for cartel units than non-cartel ones. Of course, patterns in Table 1 are only suggestive – we now subject our hypothesis to more rigorous testing.

### 3.3 Cartel Membership & Bidding Behaviour around the Transparency Reform

We use a difference-in-differences specification to quantify the differential change in bidding behaviour across the two groups around the time of the reform. We distinguish the announcement and the implementation of the policy, controlling for time-invariant heterogeneity across units and heterogeneous time effects. The baseline specification is given by:

$$\ln(b_{it}) = \beta_1 \mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Announ\}_t + \beta_2 \mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Trnsp\}_t + \lambda_i + \mu_t + \epsilon_{it} \quad (1)$$

Where  $\ln(b_{it})$  is the logarithm of the bidding price of unit  $i$  at date  $t$ , the dummy variable  $\mathbb{1}\{Cartel\}_i$  takes the value of one if  $i$  is a unit is classified to be in the cartel and zero otherwise. The dummy variable  $\mathbb{1}\{Announ\}_t$  takes the value of one if  $t$  is a date after the announcement date (January 6th, 2009) and zero otherwise, the dummy variable  $\mathbb{1}\{Trnsp\}_t$  takes the value of one if  $t$  is a date after the implementation of the transparency policy (February 6th, 2009) and zero otherwise.  $\lambda_i$  are unit fixed effects and  $\mu_t$  are date fixed effects, which control for common market conditions (such as demand and input prices). We also explore specifications in which date fixed effects  $\mu_t$  vary

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serve forward contract obligations independently of the level of the spot price, firms that have committed a large portion of their capacity in forward sale contracts have a lower incentive to increase prices (Wolak, 2007) and collude.

either by technology type or by region, as different technologies (e.g., thermal vs. hydro), or different regions, might be exposed to different daily shocks. Standard errors are two-way clustered by date and generation unit.

Table 2 presents the results. Across a variety of specifications, we find a statistically significant decrease in bidding prices of cartel units after the announcement of the policy. Depending on the specification, the estimates range between a drop of 47% and 30%.<sup>23</sup> Column (1) reports results without including any fixed effect. Column (2) controls for unit and date fixed effects and finds identical results. Columns (3) and (4) control for forward contracts.<sup>24</sup> Column (3) allows for the interaction of date-fixed effects with technology-fixed effects. Column (4) instead controls for the interaction of date-fixed effects with regional dummies.<sup>25</sup> Overall, we find a significant and negative coefficient for cartel announcement.<sup>26</sup> Interestingly, the coefficient for  $\mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Trnsp\}_t$  turns out to be small and statistically insignificant in specifications that more adequately control for potential confounders in columns (3) and (4): Market transparency did not further change bidding

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<sup>23</sup>These estimates are quite sizeable. Connor and Bolotova (2006) provides a meta-analysis of cartel overcharges and finds, in a sample of 395 documented cartels, a median (average) overcharge of 19% (29%).

<sup>24</sup>Incentives to collude depend on the fixed-price forward contracts signed by the unit. Figure A4 plots the daily average ratio of forward contracts over total availability for cartel and non-cartel units. Two patterns emerge. First, given the differences in levels of contract commitments, cartel firms have more incentives to collude than the rest of the units. Second, the drop in bidding prices of the collusive units is unrelated to a sudden change in the profile of forward contracting around the dates of the transparency policy.

<sup>25</sup>We cannot include the interaction date fixed effects with *both* regional dummies and technology type since there is only one non-thermal unit in the Atlantic region. However, in further robustness checks in which we use additional criteria to define the cartel, we include both sets of interactions simultaneously and obtain similar results.

<sup>26</sup>The differential drop in bids is not explained by a change in production costs for cartel thermal units. As a matter of fact, Table 1 shows that, if anything, the decrease in marginal costs before and after the reform for cartel units was stronger than the decrease for non-cartel units. Figure A5 shows an abrupt fall in the margin ( $Bid - Mg.Cost$ ) for units in the collusive agreement but not for the rest of the units, on the dates after the announcement. Using margins instead of bidding price provides qualitatively similar results (see Table A1).

behaviour differently between units in the cartel (which had already collapsed) and units outside, once we account for the differential role of shocks (e.g., gas prices and rainfall patterns) across technologies in column (3).

Figure 3 reports estimates from a more flexible event-study specification. We extend the baseline specification – defined in equation 1 – including interactions between weekly dummies for leads and lags relative to the announcement date and the  $Cartel_i$  dummy. First, the specification rules out differences in pre-trends in bidding behaviour between units assigned and not assigned to the cartel. Second, the differential drop in bids right after the announcement remains persistent throughout the rest of the sample period.<sup>27</sup>

### 3.4 Announcement Date and Threats of Enforcement

The sudden relative decline in bids for units assigned to the cartel immediately after the *announcement* date is thus consistent with a shock to members’ perceptions about their ability to sustain collusive behaviour in the future. As noted above, it is not essential for the logic of the test that (all) members exactly anticipated the regulatory change eventually put in place. For example, the *announcement* date could have signaled to market participants a future tightening of enforcement or regulators’ willingness to act to uncover and prosecute collusive behaviour. Evidence from two sets of inspections –before and after the announcement– however suggests that the threat of enforcement is unlikely to explain the differential reaction to the announcement.

On January 20th i.e., after the announcement, the Supervisory Authority of Public Services (SSPD) conducted unannounced *in-situ* inspections to the four biggest electricity generation companies: EMGESA, ISAGEN, EPM and EPSA. The inspections aimed to find information related to potential collusive

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<sup>27</sup>Similar results are found when we use alternative specifications from Table 2 or when we use margins as a dependent variable (see Figure A6).



practices. Figure 4 extends the event-study specification in Figure 3 adding the interactions between dummies for leads and lags relative to the *inspection* date for inspected firms. Two patterns emerge. First, the results for the cartel units are virtually unchanged. Furthermore, the bulk of the differential drop in bids for cartel units happens *before* the inspection date. Second, after the inspection, inspected firms do not change much their bids. The point estimates are negative but small and not statistically different from zero, suggesting that a tightening of enforcement is unlikely to explain the differential drop in bids.

A potential concern in interpreting results from inspections that occurred *after* the announcement date is that the announcement might have already signaled an increase in the likelihood of tightening enforcement and that, once the cartel had collapsed, no further reaction should be expected. We can however use a separate episode of inspections that occurred *before* the announcement date to gain further insights into whether the threat of enforcement is likely to be driving the reaction that followed the announcement. On 5th December 2008, SSPD conducted a separate surveillance episode.<sup>28</sup> This surveillance action included three firms with units classified in the cartel. We thus replicate the event study including an event interaction for this surveillance action, split between cartel and non-cartel units. Figure 5 shows the results. After including the surveillance actions of the SSPD as control variables the effect of the announcement of the transparency policy remains economic and statistically significant. Furthermore, neither cartel nor non-cartel firms seem to have modified their bidding behavior following the December surveillance action. This suggests that firms might have not perceived enforcement to be a significant threat.

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<sup>28</sup>The SSPD called in to its headquarters a number of firms (MERILECTRICA, TERMOEMCALI, TERMOTASAJERO, TERMOFLORES, TERMOCANDELARIA, GENSA) to discuss high bidding prices and other firms (EMGESA, EPSA, EPM, GECELCA, and ISAGEN) for bidding behavior and frequent stops in the operation of their units.

### 3.5 Discussion

We have shown that cartel units decreased bids after the announcement of the policy and before the actual implementation of it. This not only provides suggestive evidence that there was a Cartel in the Colombian electricity market but that regulators' actions can reduce collusive behavior. Furthermore, Figures 4 and 5 suggest that this reaction was unlikely to stem entirely from anticipated threats of oversight and enforcement. It thus appears plausible that bidding behavior changed at least in part in anticipation of a transparency regime that would have made collusion harder to enforce.<sup>29</sup>

It is important to emphasize that the evidence from our test does not identify the exact equilibrium played by colluding members. In particular, we are certainly not arguing that the cartel was sustained by a subgame perfect equilibrium (SPE) and that all cartel units perfectly anticipated that under the new transparency rules incentives constraints would be violated. SPE relies on multiple assumptions (players optimize and react to information instantaneously and have correct beliefs about future payoffs and play both on- and off-equilibrium paths). While our evidence does not prove that cartel members were playing a SPE, it shows that dynamic incentive compatibility constraints can be taken quite seriously in empirical work.

Collusive arrangements are complex: even when members can explicitly communicate, successful collusion requires a mutual understanding of many elements of the agreement (Harrington, 2008; Byrne and De Roos, 2019). It is thus highly implausible that, following the announcement, all units in the cartel immediately reacted in an *anticipatory* way to the uncertain prospect of

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<sup>29</sup>In theory, cartel members could devise other ways to share information to police the cartel (McMillan, 1991). Leaving aside the fact that the new regulation explicitly forbids such information sharing, in the next Section we argue that the cartel colluded on the market for positive reconciliations. As positive reconciliations are relatively infrequent, devising new information-sharing strategies was likely complicated.

a less transparent market regime in the future. More likely, many (if not most) units might have reacted in an *adaptive* way to the (unexpected) behaviour of other units. Consistent with this interpretation, Figure 2 shows that following the announcement, units reduced their bids in different waves: some units reacted before others. Interestingly, the first units decreasing prices were the Cartagena units belonging to EMGESA – the largest firm among the collusive firms. This is potentially consistent with evidence from other contexts in which larger firms are more sophisticated bidders (Hortaçsu et al., 2019) and/or tend to take on the role of leaders that coordinate pricing (see Byrne and De Roos, 2019, for an example), as in basing points pricing systems common in, e.g., the cement industry.

In sum, the test of collusion does not assume – and the evidence certainly does not imply – that all of the units in the cartel were fully anticipatory. Instead, some of them (the ones that reacted first) can have anticipatory reactions and the rest adaptive reactions. While we are not aware of empirical analyses that try to test for cartel behaviour that distinguish between these two different types of behaviour, the experimental literature testing repeated game models in the lab, see., e.g., Dal Bó (2005) and Dal Bó and Fréchette (2018), has found evidence for both. While observed sophistication in the lab is generally lower than assumed in models that rely on notions of subgame perfect equilibrium, some subjects do show the kind of sophistication consistent with anticipatory behaviour.<sup>30</sup>

### 3.6 Robustness to Alternative Definitions

Before providing more direct evidence of how firms in the cartel colluded, we investigate the robustness of our results.

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<sup>30</sup>Bigoni et al. (2019) finds that participants in a lab experiment are sufficiently sophisticated to understand the impact of imperfect monitoring and the frequency of interaction on the sustainability of collusion.

Our cartel definition classifies units and not firms. We consider two alternative definitions based on firms' ownership of units: (1) we exclude from the cartel units that belong to firms that own other units not classified in the baseline cartel (*refined definition*), (2) we include all other units that belong to firms that have at least one unit in the baseline definition of the cartel (*extended definition*). Details are presented in Appendix [A.2.1](#), together with a placebo exercise that randomly determines which units belong to the cartel. The results are robust to the refined and extended definitions. The placebo exercise reveals that our findings are unlikely to be the result of chance.

Our baseline cartel definition is based on geographic location (Atlantic region) and production technology (thermal units). We consider additional criteria to classify units: private (vs public) ownership, forward contract positions, and bidding behaviour in 2008. We refine our baseline definition including these additional criteria progressively, building on our baseline definition. Appendix [A.2.2](#) presents the details of this exercise and finds results closely in line with our baseline findings.

#### **4 Incentives to Collude & Inner Functioning of the Cartel**

This Section provides evidence on cartel units' incentives to enter the agreement and on the inner working of the cartel. Subsection [4.1](#) shows that cartel units had incentives to enter a cartel to increase payments in the positive reconciliation market. We show that cartel units have costs high enough that they would not be able to earn the right to supply electricity through the ideal dispatch if they were to bid competitively. Given this, they maximize profits through the positive reconciliation market. We show that revenues and profits in the positive reconciliations market are inverted-U shaped in bids. Units thus benefit from a coordinated increase in bids.

Subsection [4.2](#) provides forensic evidence on the functioning of the cartel.

We show that cartel units coordinated bids increase, particularly so when the probability of being called for positive reconciliation increases. Concretely, the cartel worked as follows: certain low-cost units win in the ideal dispatch auction and, from time to time, declare unavailability and generate positive reconciliations for other units. Given network restrictions, these positive reconciliations are disproportionately awarded to other cartel units that coordinated increases in bids to maximize revenue from these positive reconciliations. We complement this analysis using data from the minutes of the meetings of the Association of Generating Units (CNO in Spanish). We find that prior to the reform, but not after, cartel units were sending more staff involved in setting bids (instead of personnel dealing with engineering problems) to these meetings relative to non-cartel firms. This strategic behaviour hints at the possibility that these meetings might have been used to communicate about bidding strategies. Finally, we confirm that, after the announcement of the regulatory change, profits from the reconciliation market (but not from the ideal dispatch) decreased relatively more for cartel units than for other units.

#### 4.1 Incentives to Collude

Figure A7 compares the distribution of calculated marginal costs and average spot price for cartel units and other units separately. The average marginal cost of the units in the collusive agreement is larger than the average spot price. This contrasts with the units that are not in the collusive agreement. Given their higher marginal costs, cartel units try to make profits in the only remaining possible way: the positive reconciliations market.<sup>31</sup>

We check that the positive reconciliation market features the usual price-setting trade-off: higher prices increase margins, but reduce quantity (in this

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<sup>31</sup>As a sanity check, Table A3 shows that under different scenarios cartel units obtain higher profits in the positive reconciliation market than in a counterfactual case in which they bid their marginal costs to win in the ideal dispatch.

case, the likelihood of being called for a positive reconciliation). In such cases, firms benefit from coordinated price increases. Using the same specifications of Table 2, Table A2 confirms that the revenue and profits from positive reconciliations are indeed inverted-U shaped in the submitted bid: If the bid is very low, the unit is not called for positive reconciliations as it would be allocated the right to produce in the ideal dispatch. However, when the bid increases, the potential payment and likelihood of being called for positive reconciliation increases. This is however true only up to a certain point. When the bid is too high, the unit is unlikely to be called in for positive reconciliation. This descriptive evidence should be interpreted cautiously: Bidding behaviour is endogenous and – as we shall see momentarily discuss – responds to the anticipation of positive reconciliations.

#### 4.2 Inner Working of the Cartel

We first check that units strategically increase bids when they anticipate a higher likelihood of being called for a positive reconciliation. In the positive reconciliation market, the price paid to the unit is equal to the submitted bid (at least up to a certain maximum allowed price). Note that this incentive applies to both cartel and non-cartel units.

We investigate the relationship between bidding behaviour and the likelihood of being awarded a positive reconciliation. This likelihood is not directly observed and must be proxied with actual positive reconciliations. However, those are endogenous to bidding behaviour. We, therefore, need an instrument for the probability of a positive reconciliation for unit  $i$  at date  $t$ .

We use *security contingencies* as an instrumental variable. Security contingencies provide us with an observable, unit-day level varying measure of exogenous shocks to the transmission network that increases the likelihood of positive reconciliations. Specifically, when contingent restrictions to the net-

work occur, certain units might be asked to produce security contingencies – small amounts of electricity to help the transmission system recover stability and compensate for overcharges. Security contingencies are exclusively based on engineering criteria: units are called in depending on exogenous shocks to the transmission network and independently of their bids and outcomes in the ideal dispatch. The exclusion restriction is thus likely to be satisfied. Shocks to the transmission network take time to repair. We use lagged contingencies to proxy for units’ beliefs about the likelihood of being called for positive reconciliations.

Table A4 shows that units increase their bids when they have a positive reconciliation in the previous period. Column 1 presents the OLS estimate which is negative but not significant. The OLS estimate could be either upward or downward biased as a higher bid can either increase (the unit is less likely to win the ideal dispatch) or decrease (the unit, if eligible, is less likely to be called in) the likelihood of being awarded a positive reconciliation. Column 2 reports a strong first stage (F-stat 25.37): Conditional on unit and date fixed effects, shocks to the infrastructure significantly increase the probability that the unit is awarded a positive reconciliation. Column 3 reports the second stage and finds a large, and statistically significant, increase in bids for units that anticipate being more likely to be awarded positive reconciliations.

We now show that cartel units coordinated to increase bids precisely when other cartel units generated positive reconciliations by winning the ideal dispatch and then declaring unavailable. For this coordination to happen, two conditions are necessary. First, it must be the case that the cartel comprises (specialized) units that win in the ideal dispatch, at least sometimes. The top-left panel in Figure A8 shows that during the collusive period, some of the units in the cartel submit relatively low bids, and are sometimes awarded

production in the ideal dispatch. Importantly, the average bid dropped after the announcement of the policy both for high and low-price cartel units, but the decrease in percentage terms was higher for high-price units. The top-right panel reports the likelihood that a unit declares unavailable upon winning in the ideal dispatch. This is larger for cartel units than non-cartel units. Finally, the bottom of the figure shows that the probability that high-price cartel units receive positive reconciliations when low-price cartel units win is much larger than when low-price no cartel units win. In sum, this provides suggestive evidence of coordination.

We now show that units are indeed able to coordinate. To conduct our test, we would ideally know network restrictions that make it more likely that a given unit  $i$  receives a positive reconciliation when unit  $j$  declares unavailable. This would allow us to test whether unit  $i$  increases bids precisely when unit  $j$  ends up declaring unavailable. Unfortunately, we do not observe the underlying electricity grid and we thus proxy these relationships between units relying on observed behaviour. For each unit  $i$  we identify “friends”, i.e., units that are more likely to get a positive reconciliation when unit  $i$  has a negative reconciliation. For each unit  $i$ , we rank “friends” by the probability of receiving positive reconciliations when unit  $i$  declares unavailable. We focus on observations 6 months before and 6 months after the announcement date.

We test whether the average bid of  $i$ ’s friends increases when unit  $i$  declares (at least partially) unavailable. While we are unable to separate negative reconciliations that arise from strategic considerations from those that arise due to exogenous shocks (either to production or to transmission), a striking pattern emerges. Figure 6 shows that cartel units before the reform coordinated higher bids with declared unavailabilities of their “friends”. Interestingly, this coordination only appears for cartel units and ceases once the cartel unravels.



This provides suggestive evidence that this coordination was part of how the cartel functioned.<sup>32,33</sup>

While the cartel might have also adopted additional collusive practices, our analysis suggests that one way through which the cartel functioned was to coordinate (strategically) declared unavailability and bids so as to increase profits for its members in the positive reconciliations market. Of course, units in the cartel did not engage in this behaviour too frequently, presumably to avoid detection from the regulator (to declare unavailability, units need to submit a report and, if they do it too often, they risk being investigated).

### 4.3 Suggestive Evidence of Communication

We complement our analysis using hand-collected data from the minutes of the meetings of the Association of Generating Units (CNO in Spanish) (see Appendix A.1 for details). This association holds meetings to solve technical difficulties and constraints to the system. The association’s explicit rule was that agents from the commercial area (i.e., likely involved in setting bids) cannot attend the meetings. However, as we shall see, the rule was not enforced.

We downloaded the minutes of all the meetings in the second semester of 2008 and the first semester of 2009. Meetings report attendees and the firm that they belong to. Within a DID framework, we test if there was any differential change in attendance between the cartel and non-cartel units before

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<sup>32</sup>Results are robust to changes in the number of friends considered, the baseline period used to define friends, and the definition of the explanatory variable. See Figure A9. While the geographic clustering of cartel units correlates with the set of identified friends, this, *per se*, does not explain the time pattern in observed coordination. We nevertheless consider whether units clustered in the South-West part of Colombia –which as the Atlantic units are relatively isolated (see Figure A2) – display a similar coordinated behaviour as a placebo. The estimates for the South-West area are zero throughout the years.

<sup>33</sup>We also explore an alternative exercise in which “friends” are units that belong to the same firm. Under this definition, we find no evidence of coordination between unavailability and bids for both cartel and non-cartel units. This suggests that bid coordination likely happened across and not within firms.

and after the policy change. Since firms send only one attendee per meeting (if any at all), we focus on two dependent variables: a dummy that takes the value of 1 if the firm sends someone to the meeting and a dummy that takes the value of 1 if the firm sends someone from the commercial area to the meeting. We also explore the composition of attendees conditional on sending someone to the meeting. We categorize participants as working in the commercial area if, at the time of the meeting, their CV (accessed through websites such as LinkedIn, newspapers and industry publications) reports that the attendee worked in the commercial area, proxied with job titles mentioning the words *commercial* or *marketing*.<sup>34</sup>

There are 97 attendees in 18 different meetings for a total of 435 attendee-meeting observations. We were able to assign a job title to 63% of these 435 observations. Descriptively, 47% of attendees from cartel firms were working in the commercial area before the reform. This percentage is only 12% for non-cartel firms. Nevertheless, we explore DID specifications that control for firm and meeting fixed effects focusing on the interaction between meetings in 2009 (i.e., after the reform) and firms in the cartel.<sup>35</sup>

Table 3 reports the results. First, column (1) shows that, after the reform, units in the cartel were relatively less likely to send someone to the meetings. Column (2) shows that the composition of the attendees also changed: after the reform, units in the cartel are *less* likely to send someone from the commercial area. Finally, column (3) confirms the pattern conditional on anyone from the firm attending the meeting.

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<sup>34</sup>Results are robust if we drop workers with job titles related to marketing.

<sup>35</sup>Note that attendees can only be assigned to firms, not units. For this exercise, our definition of cartel must be at the firm level. A firm belongs to the cartel if at least one unit belongs to the baseline definition of the cartel. Unfortunately, it is not possible to obtain results using the refined definition in which a firm is classified in the cartel if all its units are. Firms that are so classified in the cartel seldom send attendees to the meetings and we match the occupation of only one attendee in 2008.

In sum, while this does not prove that cartel units explicitly communicated to coordinate bidding behaviour around the timing of the meetings, the evidence points to strategic behaviour in attendance. Similar evidence could presumably be used to evaluate the possibility of prosecution in other cases.

#### **4.4 Lower Profits from Positive Reconciliations After the End of the Cartel**

Finally, the hypothesis of an implicit agreement in the bidding scheme implies that after the break of the agreement, the profits of the cartel units should decrease. As a sanity check, we, therefore, revisit our baseline specifications from Section 3.3 and consider as dependent variables a dummy for receiving positive reconciliations, profits from positive reconciliations, and total profits.

Table 4 shows that, while the likelihood of receiving positive reconciliations was unaffected, the profits from positive reconciliations as well as the total profits sharply decreased for the collusive group after the announcement date.

We further explore whether profits were differently affected by the announcement depending on the costs, or the role, of units in the cartel. The underlying idea is that some units might have been worse off colluding rather than competing, and therefore transfers within the cartel could have been necessary to sustain it. We classify units according to proxies for their ability to compete or for their role in the cartel. Table A5 shows that total profits fell for all units, and slightly more for high costs units that would unlikely be able to increase profits in the ideal generation market. Instead, profits from positive reconciliations fell more for low-cost units which again suggests that they are now focusing on the ideal generation market. Transfers might thus *not* have been needed to sustain the cartel in this case, as all units were better off colluding.<sup>36</sup>

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<sup>36</sup>In columns 1-2, ‘high’ units are those with average marginal cost in the second half of 2008 above the median, and ‘low’ otherwise. In columns 3-4, ‘high’ units are those with

## 5 Incentive to Deviate and Cost of the Cartel

In this section, we first show that cartel units could increase static profits by submitting lower bids and deviating from the collusive agreement. Second, we show that such deviations are unlikely to be profitable under the old transparency rule, but become profitable under the new 90 days transparency rule. Although, as noted in Section 4, our test does not require nor proves that the cartel unravelled because firms anticipated that collusion would become unsustainable following the change in transparency rule, these estimates provide a sanity check that such an interpretation is at least potentially consistent with the economic environment under consideration. Finally, we use our estimates to provide a back-of-the-envelope estimate of the cost of the cartel.

### 5.1 Bidding Strategy

Following the game-theoretic framework of [Chassang and Ortner \(2022\)](#), the existence of a cartel involves departures from a static Nash equilibrium for its members, which implies a short-run incentive to deviate from the cartel. The sustainability of a cartel depends on whether such deviations are incentive compatible or not, that is whether the gain from short-run deviations compensates for the loss of future gains from colluding. Our hypotheses are that non-cartel units set bids to maximize their individual static profits while cartel units do not. In particular, cartel bids should be larger than static profits-maximizing bids. We test these hypotheses with the following three-step procedure: First, since the cartel operates on the market for positive reconciliations, we select a suitable comparison group of non-cartel units that are also more likely to make profits with positive reconciliations. Second, we estimate how the amount of positive reconciliations that a unit gets would change had the unit submitted average bids above the median, and ‘low’ otherwise. In columns 5-6, ‘high’ units are those with an average amount of negative reconciliations *below* the median, and ‘low’ otherwise.

a different bid. Third, we use these estimates to compute the bid that maximizes static (daily) profits from positive reconciliations and compare it to the observed ones. Details on each of these steps can be found in Appendix [A.3.1](#).

At the unit-day level, we compute the ratio between the observed bids and the static profits maximizing bids and plot the density of this ratio separately for cartel and non-cartel units before and after the announcement. Figure [7](#) presents the results. Before the policy change, the distribution for cartel units is bimodal and displays a peak at around four (significantly larger than one): cartel bids are often much larger than static profit-maximizing bids. For non-cartel units, instead, the distribution is single-peaked with most of its mass closely around a ratio equal to one. After the policy change, instead, for both cartel and non-cartel units the density is centered around one, suggesting that both groups are now bidding competitively. A Kolmogorov-Smirnov test for the equality of the distributions of the ratio for cartel and non-cartel units rejects the null hypothesis of equality pre-reform (p-value = 0.00), but not post-reform (p-value = 0.62). In sum, cartel units appear to systematically deviate from static profit maximization before, but not after, the reform.

## **5.2 Dynamic Enforcement Constraints**

Cartel units could increase daily profits by deviating from the collusive bidding strategy. However, cartel sustainability relies on the fact that deviations are not profitable due to the future value of the collusive agreement. Similarly to [Igami and Sugaya \(2021\)](#), which however rely on a proven cartel, we check that the incentive to collude is positive for all cartel units under the old transparency rules, but negative for at least one unit under the new ones.

We assume that deviation of a unit from the collusive agreement triggers static Nash competition as soon as past bids are made public. Under the old transparency rule, this implies that a unit can unilaterally deviate for two

days and undercut other cartel's bids to increase static profits. But, from the third day onward, cartel units would bid competitively. However, under the new transparency rule, a unit can unilaterally deviate for 90 days. We thus define static profits for unit  $i$  at time  $t$  as  $\pi_{it}^j$  under three alternative scenarios:  $j = C$  (collusion),  $N$  (competition),  $D$  (optimal deviation from the collusive agreement). We assume that units hold static expectations and denote with  $\beta$  the (common) daily discount factor. We define the Dynamic Enforcement Constraint (DEC) under the old (2) and new (3) transparency rule as follows:

$$\frac{1}{1-\beta}\pi_{it}^C - \frac{1-\beta^2}{1-\beta}\pi_{it}^D - \frac{\beta^2}{1-\beta}\pi_{it}^N > 0 \quad \text{For all units} \quad (2)$$

$$\frac{1}{1-\beta}\pi_{it}^C - \frac{1-\beta^{90}}{1-\beta}\pi_{it}^D - \frac{\beta^{90}}{1-\beta}\pi_{it}^N < 0 \quad \text{For at least one unit} \quad (3)$$

Our hypothesis is that (2) is satisfied for *all* cartel units for all periods  $t$  before the policy change, while (3) is not satisfied for *at least* one cartel unit after the policy change. We empirically test this hypothesis by focusing on cartel units and constructing counterfactual bids and quantities for a one-year period around the policy change, as detailed in Appendix A.3.2.

The solid lines in Figure 8 report the *smallest* incentive to collude across cartel units over time, assuming a daily discount factor  $\beta = 0.9996$ .<sup>37</sup> The solid lines suggest that all cartel units were better off colluding until January 2009, but that afterwards the cartel became unsustainable as it was more profitable for at least one unit to deviate from the collusive agreement. As it happens, our estimates reveal that the DEC was unlikely to hold for two units after the reform (Termocandelaria 1 and 2). If we further assume that these two units

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<sup>37</sup>This corresponds to an annual rate  $\beta^{365} = 0.86$ . The lending interest rates in Colombia in 2008 and 2009 were respectively 17.2% and 13.0% according to the IMF, which correspond to discount rates of 0.85 (1/1.172) and 0.88 (1/1.130). As an additional robustness exercise, we repeat the calculations for slightly higher and lower values of the daily discount factor (0.9995 and 0.9997), corresponding to an interest rate of 20% and 11.6%. The shaded area in Figure 8 presents the lower and upper bounds of the smallest incentive to deviate using the different discount factors.

optimally deviate and compute the incentive to collude for the remaining cartel units, our model indicates that four additional units would prefer to deviate (Cartagena 1 and 3, Flores 2 and 3), potentially starting a chain effect that would lead to the collapse of the cartel.

Figure 8 further shows that the data are consistent with a drop in the incentive to collude in January 2009 following the policy change and not as a result of other differences between the pre- and post-reform periods.<sup>38</sup>

### 5.3 Cost of the Cartel

Our counterfactual estimates of bids and quantities allow us to provide a back-of-the-envelope quantification of the additional cost consumers paid due to the high cost imposed by the cartel in the reconciliations market. We focus on the second semester of 2008 and compare the total cost paid for positive reconciliations with the total cost that would have been paid if the cartel firms had behaved competitively. The former quantity is observed, while the latter is deduced from the counterfactual analysis.

The cartel generated at least an additional cost of around 11 billion COP per month, which corresponds to an increase of around 12% with respect to the competitive scenario (see the right panel of Figure 8). Positive reconciliations account for approximately 10% of the electricity procured by the regulator, but since they are paid above the spot price this lead to an increase in overall costs of about 2.5%. Around 10 million households lived in Colombia in 2008. If all the energy allocated via positive reconciliations is bought by households, and assuming a full pass-through of the cost increase to consumers, the average household paid 1,100 COP in excess per month in the second semester of 2008 due to the collusive agreement (with many household living with less than a

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<sup>38</sup>A minimum delay of  $\approx 60$  days would have been necessary to trigger a deviation for at least one unit.

minimum wage of 461.500 COP).<sup>39</sup>

## 6 Policy Implications and Conclusions

This paper identified collusion among a subset of firms in the Colombia wholesale energy market. Our test uncovers sudden changes in bidding behaviour after the announcement, but before the actual implementation, of a regulatory reform that reduced market transparency. Actions from the regulator, therefore, curbed collusion. Our evidence suggests that this reaction was unlikely to stem from anticipated threats of oversight and enforcement. It thus appears plausible that some firms changed their bidding behavior in anticipation of a transparency regime that would have made collusion harder to enforce. While this anticipatory response doesn't imply that subgame perfect equilibrium provides a tight description of this cartel, it does suggest that dynamic incentive compatibility constraints can be taken seriously by empirical researchers and policy-makers fighting collusion.

Our analysis has policy implications for market design – including energy markets – in developing countries. Distortions due to collusive practices in upstream sectors that provide inputs to many other sectors, such as energy, are particularly detrimental to aggregate welfare (Liu, 2019). The Colombia case provides a particularly interesting example. The country's energy sector was successfully reformed in the nineties and is generally considered one of the best-designed and regulated markets among developing countries (World Bank, 2019). We suspect collusive behaviour would be even more likely and create larger distortions in less well-designed energy markets.

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<sup>39</sup>Ideally, we would explore the reduced form quantification of the costs of the cartel to downstream sectors, for instance in manufacturing. However, contextual confounders and data limitations prevent such analysis. In particular, the Colombian manufacturing Census is yearly, and in the second semester of 2009, El Niño adversely affected the production capacity of hydro-power units, increasing equilibrium prices.



In our context, the Colombian regulator lacked sufficient evidence to open targeted investigations and attempt prosecution. This induced the regulator to instead alter the market design in the hope of hindering (potential) collusive practices. Changes in market design, however, can be costly. For example, in our context, market transparency facilitates the efficient inter-temporal allocation of scarce water resources. The fact that at least some cartel members reacted in an anticipatory way (i.e., dynamic incentive compatibility constraints underpin collusive behaviour) raises the possibility that regulators might be able to strategically use *announcements* to induce behavioural responses and acquire sufficient evidence to open investigations and attempt prosecution (Chassang and Ortner, 2022). A careful investigation of this possibility merits further theoretical and empirical scrutiny.

Finally, our analysis hints at how market transparency affects firms' conduct and how a (particular) policy that limited public information might have reduced anti-competitive behavior. In our context, the policy had an effect because cartel members likely did not have other ways to credibly share information and police the agreement. The impact of market transparency on collusion in other contexts – including public procurement, e-commerce, and agricultural markets – deserves further scrutiny. Digital technologies, for example, have the potential to increase sellers' visibility among buyers, reduce search costs and increase competition (Bai et al., 2020; Baldwin et al., 2021; Bergquist et al., 2021). Our evidence introduces a word of caution: increased transparency could backfire if it allows firms to detect and punish deviations from collusive agreements. More research is needed to evaluate the impact of market transparency in other contexts.

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## 7 Figures

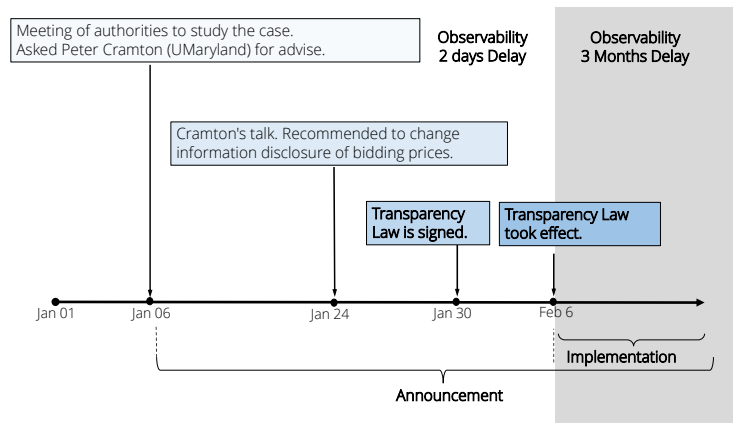


Figure 1: Timeline.

Note: Timeline of the announcement and implementation of the transparency policy.

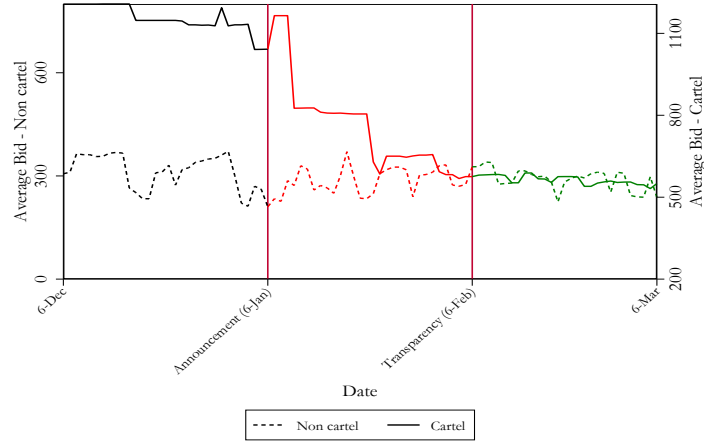


Figure 2: The Main Fact: Cartel and non cartel groups bids.

Note: Time series of the average bid of the cartel (solid line, right axis) and non-cartel groups (dashed line, left axis) around the dates of announcement and implementation of the transparency policy. The vertical lines show the announcement and implementation dates.

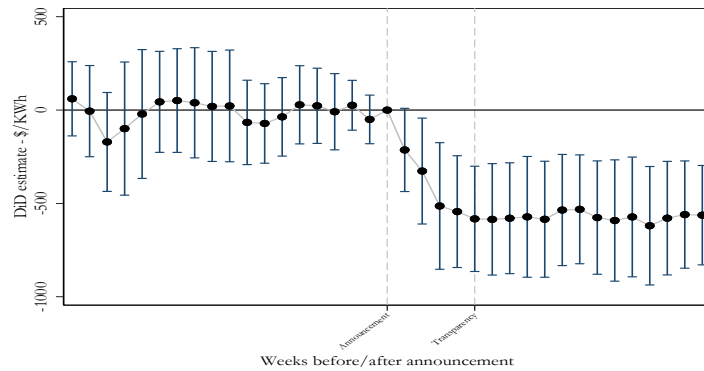


Figure 3: Event study representation of the differences-in-differences model.

Note: The figure presents event study estimates using bid as the dependent variable. We performed a two-way fixed effects model including a specific treatment effect for each week of the period studied. Robust s.e. are clustered by unit and date. The x-axis represents weeks around the policy announcement. The y-axis reports the estimates using the week of the announcement as baseline. Dots and bars represent point estimates and 95% confidence intervals. The dotted line labeled as “Announcement” represents the week of the announcement of the transparency policy. The dotted line labeled as “Transparency” represents the week of the implementation of the transparency policy.

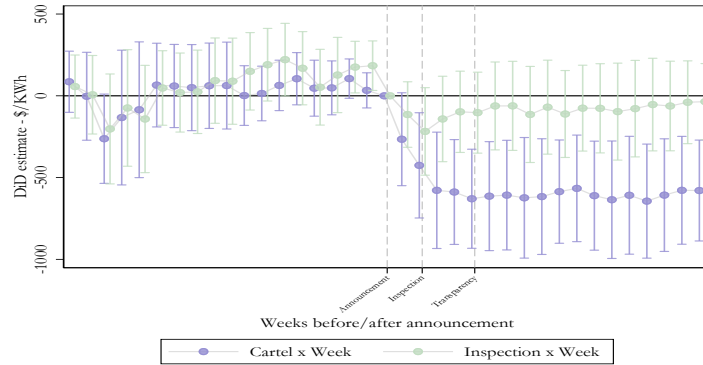


Figure 4: Event study representation of the differences-in-differences model.

Note: We investigate whether the threat of enforcement can explain the differential drop in bids using the inspection of January 20th (after the announcement). The figure presents the estimates using bid as the dependent variable and the event study of the inspection sites conducted on 20th January 2009 (inspected firms were EMGESA, ISAGEN, EPM and EPSA). We performed a two-way fixed effects model including a specific treatment effect for each week of the period studied. Robust s.e. are clustered by unit and date. The x-axis represents weeks around the policy announcement and inspection. The y-axis reports the estimates using the week of the announcement as baseline. Dots and bars represent point estimates and 95% confidence intervals. The dotted line labeled as “Announcement” represents the week of the announcement of the transparency policy. The dotted line labeled as “Transparency” represents the week of the implementation of the transparency policy. Finally, the dotted line labeled as “Inspection” represents the week of the inspection (20th January 2009).

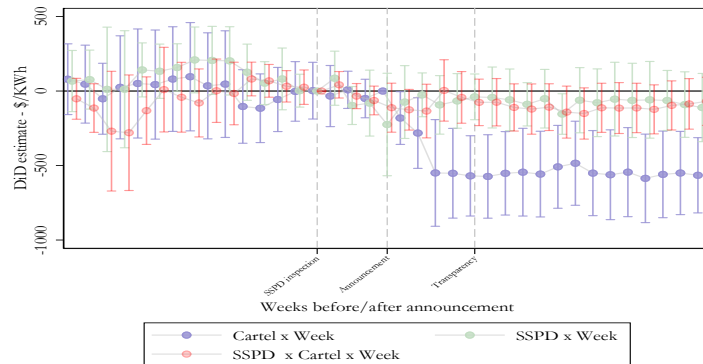


Figure 5: Event study representation of the differences-in-differences model SSPD Inspection.

Note: We investigate whether the threat of enforcement can explain the differential drop in bids using the inspection of December 5th (before the announcement). The figure presents the estimates using bid as the dependent variable and the event study of an inquiry action performed by the SSPD on 5th December 2008. We performed a two-way fixed effects model including a specific treatment effect for each week of the period studied. Robust s.e. are clustered by unit and date. The x-axis represents weeks around the policy announcement or of the SSPD inspection at inspections. The y-axis reports the estimates using the week of the announcement or of the SSPD inspection as baseline. Dots and bars represent point estimates and 95% confidence intervals. The dotted line labeled as “Announcement” represents the week of the announcement of the transparency policy. The dotted line labeled as “Transparency” represents the week of the implementation of the transparency policy. Finally, the dotted line labeled as “SSPD inspection” represents the week of the inquiry action (5th December 2008).



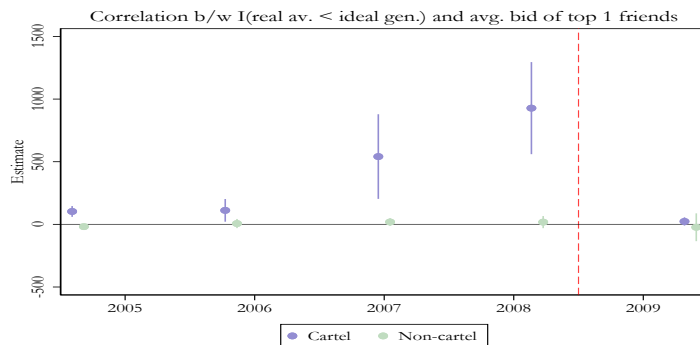


Figure 6: Baseline coordinated bids analysis.

Note: We investigate whether cartel units coordinated their bids before the policy change. The figure presents estimates from regressions where the outcome variable is the average bid of the friends of unit  $i$  and the explanatory variables is an indicator for unit  $i$  declaring a level of real availability below the ideal generation quantity it was awarded. We only include in the explanatory dummy the 75% cases where the difference between real availability and ideal generation is the largest. We run separate regressions for the two groups (cartel, non-cartel) and repeat for the years 2005 to 2009. The estimates for 2009 need to be interpreted cautiously. Data on real availability is missing for 63% of cartel observations and for 6% of non-cartel observations in 2009.

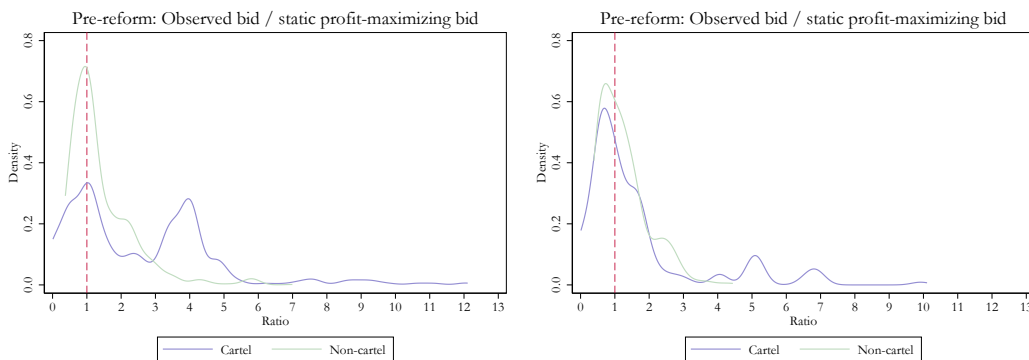


Figure 7: Density of the ratio between unit-daily observed and static profits maximizing bids for the cartel and non-cartel units over two different six-month periods.

Note: For cartel and non-cartel units, we simulate counterfactual bids and the corresponding amount of positive reconciliations and select the static profit-maximizing bids. We plot the density of the ratio between the observed bid and the profit-maximizing bid. The left (right) figure presents the density using data from the six months before (after) the policy change.

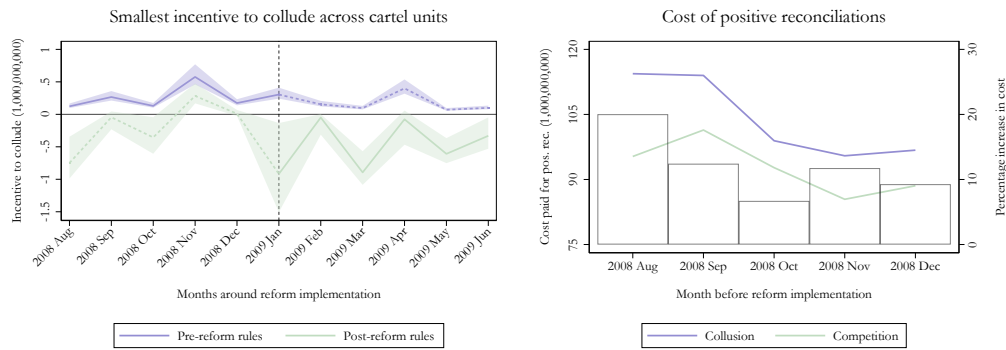


Figure 8: Smallest incentive to collude across cartel firms and cost of the cartel.

Note: The left figure presents the smallest incentive to collude across cartel firms. For each cartel unit, we compute the incentive to collude each day from August 2008 to June 2009 and then aggregate it into months. The purple line shows the smallest incentive to collude across cartel units assuming that a unit can unilaterally deviate for two days before triggering competition; for the the green line we assume that a unit can deviate for 90 days. Under the pre-reform rules we use a solid line in the pre-reform period and a dashed line in the post-reform; the opposite is true for post-reform rules. The incentive to collude is computed assuming a daily discount factor  $\beta = 0.9996$  ( $0.9996^{365} = 0.86$ ). The shaded area presents the boundaries of the result when the calculation is based on a daily discount factor of 0.9995 or 0.9997. The right figure presents the cost of the cartel for consumers. We multiply bids and amounts at the unit-day level and then sum over units. We then aggregate costs at the monthly level. The purple line (scale on the left axis) represents the total cost paid to cartel and non-cartel units for positive reconciliations in every month between August and December 2008. The green line (scale on the left axis) represents the counterfactual cost assuming cartel units were competing rather than colluding. The bars (scale on the right axis) present the percentage increase in the cost paid for positive reconciliations with respect to the competitive scenario.

## 8 Tables

Variable(Unit)	Before 06/01/2009						
	Cartel			No Cartel			
	Obs	Mean	SD	Obs	Mean	SD	T-Test
Bid(COP/KWh)	2212	1213.57	714.17	5214	362.06	557.36	49.99
Ratio forward contracts/availability(Percentage)	2212	0.27	0.25	5046	0.67	1.17	-23.11
Probability positive reconciliation(probability)	2212	0.13	0.31	5214	0.24	0.34	-13.50
Average Positive reconciliation(KWh)	2212	22702.29	76145.57	5214	10127.97	29856.41	7.53
Revenue from Positive reconciliation(Millions COP)	2212	107.76	347.30	5214	17.87	53.33	12.11
Average Availability(KW)	2212	126946.42	164209.28	5214	282285.07	299716.71	-28.64
Estimated Marginal Cost(COP/KWh)	2212	113.22	19.07	5214	60.55	63.09	54.69
Variable(Unit)	After 06/01/2009						
	Cartel			No Cartel			
	Obs	Difference	T-Test	Obs	Difference	T-Test	T-Test
Bid(COP/KWh)	2898	-631.84	35.70	6831	-73.00	7.65	27.38
Ratio forward contracts/availability(Percentage)	2898	0.01	-1.35	6799	0.26	-9.43	-29.03
Probability positive reconciliation(probability)	2898	0.01	-1.39	6831	0.04	-5.47	-18.23
Average Positive reconciliation(KWh)	2898	-3157.89	1.51	6831	2263.29	-3.64	5.12
Revenue from Positive reconciliation(Millions COP)	2898	-36.15	4.10	6831	5.94	-5.17	9.75
Average Availability(KW)	2898	8199.63	-1.71	6831	731.54	-0.13	-30.20
Estimated Marginal Cost(COP/KWh)	2898	-27.14	50.93	6831	-7.95	7.22	44.44

Table 1: Descriptive Statistics

Note: The table presents the descriptive statistics of the cartel and non-cartel groups for two different periods before and after the announcement of the policy. Columns 2 to 4 present information on the cartel group while columns 5 to 7 present information on the non-cartel group. The top panel presents information for the period 1st August of 2008 until 6th January of 2009. The bottom panel starts on the 6th January of 2009 and ends 31st July 2009.

VARIABLES	(1)	(2)	(3)	(4)
	LnBid	LnBid	LnBid	LnBid
Cartel Announcement	-0.54 (0.13)	-0.54 (0.14)	-0.36 (0.13)	-0.63 (0.12)
Cartel Implementation	-0.18 (0.08)	-0.18 (0.10)	-0.03 (0.12)	-0.08 (0.05)
Announcement	-0.01 (0.06)			
Implementation	-0.12 (0.08)			
Observations	17,155	17,155	16,955	16,955
R-squared	0.29	0.82	0.83	0.84
Unit FE	NO	YES	YES	YES
Date FE	NO	YES	N/A	N/A
Date x Technology FE	NO	NO	YES	NO
Date x Region FE	NO	NO	NO	YES
Forward Contracts	NO	NO	YES	YES

Robust standard errors in parentheses

Table 2: Difference-in-difference estimates

Note: The table presents the estimation results of the difference in difference model proposed in equation 1 in subsection 3.3 using the logarithm of the bid as the dependent variable. In columns 3-4 we further control for forward contracts over total capacity and alternatively for Date x Technology FE or for Date x Region FE. Regions are Atlantic, North-West, Central, and South-West. Robust s.e. clustered by unit and date in parenthesis.

	(1)	(2)	(3)
VARIABLES	Someone	Someone Commercial	Cond. Probability Commercial
Cartel x 2009	-0.289 (0.0489)	-0.569 (0.0679)	-0.817 (0.0683)
Observations	276	276	170
R-squared	0.624	0.569	0.818
Unit FE	YES	YES	YES
Meeting FE	YES	YES	YES

Robust standard errors in parentheses

Table 3: Meetings Minutes Evidence

Note: The table presents the relationship between having any worker or someone from the commercial area on an interaction term Cartel×2009. Cartel takes the value of 1 for the units classified in the baseline collusive agreement and 0 otherwise. A firm belongs to the cartel if at least one unit belongs to the baseline definition of the cartel. There are three dependent variables: 1. Sending someone to the meetings, 2. Sending someone from the commercial area and 3. Sending someone from the commercial area conditional to sending someone to the meetings. Robust s.e. clustered by unit and date in parenthesis.

	(1)	(2)	(3)
VARIABLES	Dummy for PR	Profits from PR	Total profits
Cartel Post	0.02 (0.05)	-135.88 (62.03)	-74.29 (21.80)
Observations	17,155	6,725	17,155
R-squared	0.43	0.68	0.79
Unit FE	YES	YES	YES
Date FE	YES	YES	YES

Robust standard errors in parentheses

Table 4: Effects of announcement on profits

Note: The table presents differences in differences estimates for various outcomes controlling for unit and time-fixed effects, where the Post period refers to the period after the policy announcement. Column 1 presents the estimates for the probability of receiving positive reconciliations. Column 2 presents the estimates for the profits from positive reconciliations, conditional on receiving some positive reconciliations. Column 3 presents the estimates for the total profits (unconditional). Robust s.e. clustered by unit and date in parenthesis.

## A For-Online-Publication Appendix

### A.1 Data

In this paper we use three main sources of data. The first one, available from the webpage of *XM*, contains detailed information on market variables of the Colombian wholesale electricity market from August 2008 to July 2009. The database has the universe of submitted bidding programs, the forward contracts hourly sales of each firm, the hourly demand and spot price, the daily water intakes of the reservoirs for each hydro unit, the quantities and revenues from positive and negative reconciliations as well as the contingencies of the transmission infrastructure.

The second dataset provides time-varying marginal costs for each generation unit. To construct them, we follow a standard engineering methodology (Green and Newbery, 1992; Wolfram, 1998, 1999; Wolak, 2000; Fabra and Reguant, 2014) that uses technical specifications of each generation unit (i.e. heat rate), fuel prices and transportation costs (see Appendix A.4 for details about calculations and data sources).

Finally, we hand-collected data from the minutes of the meetings of the Association of Generating Units (CNO in Spanish).<sup>1</sup> We first download the minutes and type the name of each attendee in an excel file. Then, we give the excel file to two different RAs to complete the occupation. They searched for the CV of the attendees of these meetings through LinkedIn and other web sources. We were particularly interested to know if attendees had a job position in the commercial area, and therefore were likely to be directly involved in setting bids at the time of the meeting. The great majority of information collected was uniform across RAs. In case of discrepancies, the authors took

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<sup>1</sup>For more information, see <https://www.cno.org.co/content/quienes-somos> and the report from the regulators (Superintendencia Delegada para Energía y Gas, 2008).

a decision. The rule we follow is that unless there is clear evidence of the occupation, we will leave it as a missing value.

## A.2 Robustness in the Cartel Definition

### A.2.1 Robustness in the Cartel Definition: Firms' Ownership

Our cartel definition has classified units and not firms. To know the extent of which this can bias our results, Figure A10 reports estimated coefficients of the interaction between the dummies for announcement,  $\mathbb{1}\{Announc\}_t$ , and implementation,  $\mathbb{1}\{Trnsp\}_t$ , with four alternative definitions of cartel membership: Baseline, Refined, Extended and Placebo units.

The first group comprises the 14 units from the baseline definition. The second group includes only 9 units that belong to firms for which *all* their units were initially classified in the baseline Cartel. The *extended* units group, with 22 units, includes the baseline units plus other units that also belong to the firms that have at least one unit in the baseline definition of the cartel. Finally, to conduct the placebo exercise, we randomly allocate some of the units to the placebo cartel and the rest to the control group. In doing so, we keep the same proportion of cartel and non-cartel units as is in our baseline definition (14/47). We repeat this procedure 1,000 times and report the mean of the effect across repetitions along with confidence intervals constructed with the standard deviation across repetitions.

Figure A10 presents the results and shows two main patterns. First, for the refined and extended units groups, both the announcement and implementation coefficients are significantly lower than zero. The coefficient of the interaction term of the announcement is lower than the coefficient of the interaction term of the implementation for both groups. Second, the previous pattern is different for the placebo exercise. Units randomly allocated to the cartel group sometimes have an increase and sometimes a decrease in bidding

prices after the announcement or the implementation period, which results in a zero average effect. Importantly, the standard deviation of the estimates from the bootstrap exercise suggests that our baseline estimates are unlikely to be the result of chance.

### **A.2.2 Robustness in the Cartel Definition: Alternative Criteria**

So far, we have assumed that the cartel was formed by Thermal Atlantic units and have explored robustness using firms' ownership of units. In this subsection, we pursue a different approach in which we consider additional criteria to define our proxy for cartel membership. Specifically, we consider the role of (1) private (vs public) ownership, (2) forward contract positions, and (3) bidding behaviour in 2008, i.e., *before* the announcement date. We refine our baseline definition including these additional criteria progressively building on our baseline definition. In particular, we use factor analysis to define cartel membership based on different sets of variables. Given a set of explanatory variables, we define the cartel as being composed by those units to which the factor analysis assigns positive factors. Changing the variables used in the factor analysis leads to four alternative definitions of cartel:

1. **Cartel 2:** *Three dummies: Atlantic, Thermal, and Private.* The logic of this definition is to question the extent that private ownership matters for our results (in our baseline cartel, 36% of units are public). For instance, [Barros and Modesto \(1999\)](#) argue that private units maximize profits while public firms maximize welfare or other objective functions.
2. **Cartel 3:** *Two dummies: Atlantic and Thermal, and one continuous variable: Forward Contracts.* We include forward contracts to capture the incentive to modify short-term market aggregates. Since forward contracts are defined at the firm level, we include in the factor analysis

the share of a firm’s capacity that is not covered by forward contracts.

3. **Cartel 4:** *Three dummies: Atlantic, Thermal and Private, and one continuous variable: Trend in Bidding Behaviour in the Pre-Period.* We construct a proxy for the bidding behavior of each unit in all of the period of 2008 by regressing the logarithm of bids on unit fixed effects interacted with a linear time trend during 2008. We then include in the factor analysis the average estimated fixed effect for each unit. This exercise yields a parsimonious estimate of how a given unit changed its bidding behaviour during 2008.
4. **Cartel 5:** *Three dummies: Atlantic, Thermal, Private , and two continuous variable: Forward Contracts, and Bidding Behaviour in the Pre-Period.* Finally, we include in the factor analysis all the considered variables: A dummy for being located in the Atlantic coast, a dummy for Thermal production technology, a dummy for private ownership, our continuous measure for Forward Contract coverage, and our proxy for Bidding Behavior in 2008.

Table A6 shows the correlation matrix for the different definitions. Although the correlation is always positive and significant –at 1%–, it ranges from moderate (0.45) to high (0.95).

Table A7 shows the difference in difference estimates for these four alternative definitions. The coefficient of Cartel Announcement is always negative and significant and ranges from -0.27 to -0.73, suggesting that the effect of the policy change could be larger than that captured by our baseline definition. The coefficient of Cartel Implementation is not significant at conventional levels.<sup>2</sup>

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<sup>2</sup>Unreported result are robust to the contemporaneous inclusion of the interaction be-



Figure A11 shows the event study for these four definitions. For all of them, the level of the coefficients after the announcement is lower than before the announcement. In particular, for all definitions, there is a sharp and discontinuous drop in the coefficients right after the announcement date.

Figure A12 shows that when we refine or extend the Cartel definitions as well as when we conduct a similar placebo exercise as proposed above, the coefficient estimated for the announcement interaction is always negative and larger in magnitude than the coefficient estimated for the implementation interaction.

While our baseline definition of the cartel focuses a priori on Thermal units, the alternative definitions do not. In fact, Cartel 3 to 5 include one hydro unit each (not always the same) and suggest the main finding is robust to their inclusion.

### **A.3 Details on the Incentive to Deviate and the Cost of the Cartel**

#### **A.3.1 Details on the Bidding Strategy Exercise**

In the first step, we identify a suitable comparison group of non-cartel units using the following criteria. (i) Thermal units, because all cartel units are thermal. Furthermore, the assumption that units maximize static profits is realistic for thermal but not for hydro units. (ii) Private units, because publicly owned units might not be profit maximizers (Barros and Modesto, 1999). (iii) Units that are not owned by a firm that also owns cartel units, because we want to limit the possibility of considering units that are actually in the cartel or pursuing different goals, as in the robustness exercise on the cartel definition presented in Section 3.3. These criteria leave us with a comparison group

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tween date and technology fixed effects as well as date and region fixed effects. The additional criteria introduce variation within our baseline characterization that enables us to include this more exhaustive set of controls.

consisting of five non-cartel units.<sup>3</sup> We focus on a one year period: six months before the policy change and the six months after the policy change. For these two periods, we compare observed bids with static profits maximizing bids for both cartel and non-cartel units.

We compute, for each day, the amount of positive reconciliations that a unit would have got had it submitted a counterfactual bid. In the spirit of [Porter and Zona \(1993\)](#), our analysis is based on the rank of bids rather than bids themselves, because a change in bid does not affect the winning probability unless it also changes the rank of the bid with respect to the other units. We follow a two steps estimation procedure to accommodate the non-linearity between bids and quantities: We first model the probability of having a positive reconciliation, and then we model the expected amount of positive reconciliation conditional on having some.<sup>4</sup> Since, due to transmission network constraints, the geographical position of a unit is crucial in determining who gets a positive reconciliation, and since cartel and non-cartel units are located in different regions, we estimate the two models separately for cartel and non-cartel units. In particular, for cartel units the rank of bids is computed with respect to all the others cartel units, while for non-cartel units the rank of bids is computed with respect to all the others non-cartel units. [Figure A13](#) presents in-sample predictions from this estimation procedure versus

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<sup>3</sup>The main results are robust to relaxing the second restriction (by also including public units in the control group) and/or the third restriction (by also including units owned by firms that also owns cartel units in the control group).

<sup>4</sup>First, to model the probability of having a positive reconciliation we regress a dummy for having a positive reconciliation on the rank of the bid with respect to its competitors, its squared value, unit and time fixed effects. Since our goal is to make predictions, we estimate the model with maximum likelihood assuming the errors follow a logistic distribution. Second, to model quantities, we regress the natural logarithm of the awarded amount of positive reconciliation, conditional on being awarded some, on the same covariates as in the first step. We estimate the second model with OLS. Given these two sets of estimates, we compute for each unit, day and bid the expected amount of positive reconciliations as the product of the predicted probability of being awarded a positive reconciliations with the (conditional) expected amount.

observed quantities. We further compare the distribution of our predictions and of observed quantities in Figure A14. Both diagnostic figures suggest that our model is able to replicate fairly well the amount of positive reconciliations awarded.

Given these estimates, we simulate, for each unit and day, alternative bids to the observed one and the corresponding quantities of positive reconciliation conditional on other units' bids. We then compute counterfactual profits and select the bid yielding the highest static daily profits. At the unit-day level, we compute the ratio between the observed bids and the static profits maximizing bids and plot the density of this ratio separately for the two groups of units and the two periods of time. Figure 7 presents the results of this exercise.

### **A.3.2 Details on Testing the DEC**

Given the evidence presented in the body of the paper, we assume that cartel units were colluding before the announcement of the policy change and competing after the implementation. Thus, for each unit and day in these two periods we observe bids and quantities under one of the three possible scenarios. We then need to compute bids and quantities for the other two counterfactual scenarios. From August 2008 until the announcement of the new policy we observe bids and quantities under collusion and need to compute bids and quantities under competition and deviation from collusion, while from the policy implementation until June 2009 we observe bids and quantities under competition and need to compute bids and quantities under collusion and deviation from collusion. For the period between the announcement and the implementation we remain agnostic about the moment in which each unit moved from collusive to competitive bids, and we construct counterfactual variables for all the three scenarios.

We focus on cartel units and on a one year period around the policy change

and aim at estimating the profits they can make with positive reconciliations under the three scenarios.<sup>5</sup> First, we model how units set bids based on market fundamentals both when colluding and when competing. This allows us to construct counterfactual bids. Second, we model counterfactual quantities of positive reconciliations conditional on bids using the same approach presented in the previous section. Third, given these bids and quantities, we compute profits under collusion and competition using our estimates for the production costs. Fourth, we simulate possible alternative bids for each unit and the corresponding profits, assuming other units stick to the collusive bids, in order to define the optimal deviation bids and profits. In the last step we compute the DEC for each unit.

First, we need counterfactual competitive bids  $b_{it}^N$  for the pre-reform period and counterfactual collusive bids  $b_{it}^C$  for the post-reform period. We model bids a function of market fundamentals similarly to [Pesendorfer \(2000\)](#): We regress observed bids on unit fixed effects, production costs, the logarithm of the total amount of positive reconciliation in the previous day, and the logarithm of the total amount of ideal generation (that is the two exogenous quantities that are known at the time of submitting bids). We estimate this regression separately for the pre and post-reform periods and use these estimates to simulate counterfactual bids. [Table A8](#) presents the results. As argued by [Porter and Zona \(1993\)](#) and [Ishii \(2009\)](#), our estimates suggest that the levels of bids do not necessarily reflect the underlying market fundamentals when units are colluding, but they do when units are competing. In order to assess the goodness of our method, in [Figure A15](#) we plot in-sample predictions versus observed bids for the collusive and competitive period separately, and the respective distributions in [Figure A16](#). Our model seems to replicate well

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<sup>5</sup>We do not consider the unit Proelectrica 2 because it was never awarded positive reconciliations in the considered period of time. We thus focus on 13 of the 14 cartel units.

how cartel units set bids under both scenarios.

Second, we need counterfactual competitive quantities  $q_{it}^N$  for the pre-reform period and counterfactual collusive quantities  $q_{it}^C$  for the post-reform period. However, to correct for differences in units' availability and transmission problems, we also simulate  $q_{it}^C$  for the pre-reform period and  $q_{it}^N$  for the post-reform period instead of relying on observed ones. In practice, we thus simulate both counterfactual quantities - collusion and competition - for all days and units. We model how awarded quantities of positive reconciliation depends on the rank on the bids and rely on the same method used in the previous exercise (results in Table A9). We then predict  $q_{it}^C$  using observed bids for the pre-reform period and using simulated bids for the post-reform period. We do the opposite for  $q_{it}^N$ . Figures A17 and A18 show the goodness of in-sample predictions. Again, the model seems to replicate fairly well how cartel bids translates into positive reconciliations under both scenarios. The comparison between Figure A16 and A18 also suggests that the collapse of the cartel changed the distribution of bids but not so much the distribution of awarded positive reconciliations.

Given simulated and observed bids, estimated costs, and simulated quantities, we predict profits  $\pi_{it}^C$  and  $\pi_{it}^N$  for all days and units. We finally need bids and quantities under the optimal deviation scenario to compute the corresponding profits. For each unit and day separately, we simulate quantities and profits for different possible values of a deviation bid  $b_{it}^D$  (above production cost and below the collusive bid) and select the one yielding the highest profits  $\pi_{it}^D$ . Figure A19 presents the resulting average profits under the three different scenarios. By construction, for each unit, deviation yield the largest average profits, competition the lowest with the collusion payoff in the middle. The Figure reveals significant variation in how much units stand to gain from

collusion relative to competition. We average profits at the monthly level and compute the incentive to collude in each month assuming static expectations. Results are presented in Figure 8.

### **A.3.3 Details on the Cost of the Cartel**

We assume that the total amount of positive reconciliations produced by the cartel is independent of its members colluding or competing. That is, (i) units cannot strategically create positive reconciliations; (ii) the collusive behavior only changes the particular allocation of production of energy within cartel units. Our measure thus provides a lower bound estimate of the benefit of competition. The rationale of why this is the case is that if (i) does not hold, competition would imply that a share of positive reconciliation is awarded via the ideal dispatch and paid at the lower spot market price. Similarly, if (ii) does not hold, lower cartel members' bid could increase the market share of these units in the positive reconciliation market if their bids are lower than non-cartel units. If that is the case, we ignore the lower cost consumers would pay on the additional market share.

In practice, we multiply the bids and amounts constructed to test the DEC at the unit-day level and then sum over units. As for the previous exercises, we aggregate costs at the monthly level and present the results in the right panel of Figure 8.

### **A.4 Calculation Marginal Costs**

As previous studies in the literature on market power in electricity markets (Green and Newbery, 1992; Wolfram, 1998, 1999; Wolak, 2000; Fabra and Reguant, 2014), we use information about the fuel burned, the thermal efficiency, and the price and transportation cost of the corresponding fuel to compute an estimate of the unit cost per kilowatt hour of each generation

plant.

We calculated marginal costs of thermal plants using the heat rate, fuel costs and fuel transportation costs with the following formula:

$$\underbrace{\text{Exchange } R_t}_{\frac{COP\$}{US\$}} \times \left[ \underbrace{\text{Heat } R_i}_{\frac{MBTU}{KWh}} \times \underbrace{(\text{Transp. fuel cost}_i + \text{Fuel cost}_t)}_{\frac{US\$}{MBTU}} \right] = \underbrace{\text{Marginal Cost}_{it}}_{\frac{COP\$}{kWh}}$$

Where *COP* are Colombian pesos, *MBTU* are one thousand of the British thermal unit, *US* are United States dollars and *KWh* is one kilowatt per hour. The heat rate is a measure of the thermal efficiency of the generation unit. It represents the quantity of fuel measured in *MBTU* necessary to generate one kilowatt per hour. As previous studies, we obtained heat rates from statistical reports issued by public entities (Green and Newbery, 1992; Wolfram, 1998, 1999). The parameters of the heat rate of thermal electricity generation Colombian units were extracted from the website of the market operator (XM).<sup>6</sup>

Regarding fuel prices, for non-internationally tradable inputs, we used a reference price of the contracts as in Wolfram (1999) and for tradable inputs, we used public information on prices in international energy markets as in Fabra and Reguant (2014).

In 2008 and 2009 natural gas was a non-tradable input in Colombia, given that it did not have import regasification facilities nor it was connected to an international gas hub. We use as a reference of the price of the natural gas contracts the price of the basin Guajira which is the most important gas supply source for Colombian thermal generation. From September 1995 Until August 2013, the Colombian Government regulated the prices of the sales contracts of this gas source. The regulation consist in imposing a maximum

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<sup>6</sup>See: <http://paratec.xm.com.co/paratec/SitePages/generacion.aspx?q=capacidad>.

sale price of gas. This maximum price at period  $t$ ,  $p_t$ , is given by the formula  $p_{t-1}[\text{index}_{t-1}/\text{index}_{t-2}]$  where  $\text{index}_{t-1}$  is the average of the last semester of the New York Harbor Residual Fuel Oil 1.0 % Sulfur LP Spot Price according to the series that was published by the Energy Information Administration of the United States. A period  $t$  is defined as semester and it changes 1st February and 1st August of each year.<sup>7</sup> This price is given in *US dollars/MBTU*.

We calculated the Guajira regulated price applying the formula presented above and converting the resulting price (*US dollars/MBTU*) to *Colombian pesos/KWh*. The exchange rate data was obtained from the Colombian central bank (Banco de la República)<sup>8</sup>.

As the previous studies of [Green and Newbery \(1992\)](#) and [Wolfram \(1999\)](#) we included the transportation cost in the marginal cost computation.

Consequently with the fuel cost reference, for gas fired units, we take as transportation costs the sum of the fees for the use of each segment of the gas transmission network necessary to take the gas from Guajira well to the respective generation units. These fees are regulated by the CREG and are published in regulatory acts ([CREG, 2003a,b](#)).

Regarding the coal fired units, we use as price reference the coal price in international energy markets as suggested by [Fabra and Reguant \(2014\)](#). Given that Colombia is a net exporter of coal we use the weighted average FOB export price as fuel cost. We computed it as the ratio between the total value of coal exportation (in *US dollars*) and the quantities exported (Tons) according to the data from the non-traditional exports report of the National Department of Statistics (DANE). The price in dollars per ton was transformed to dollars per *MBTU* units, multiplying for a calorific value of the Colombian thermal coal of 1,370 btu per pound GAR (Source: regulation 2009 180507

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<sup>7</sup>The formula was established in Resolution 119/2005 of CREG ([CREG, 2005](#))

<sup>8</sup>See:<https://www.banrep.gov.co/es/estadisticas/trm>



Colombian Ministry of Energy and Mines ([Ministerio de Minas y Energía](#) , [MME](#))).<sup>9</sup> For computing the coal transportation costs, an importation parity approach is adopted. According to this criteria, we estimate it as the road freight transportation fee from the closest importation port to the respective location of the generation unit. These fees were extracted from the system of information of efficient costs for road freight transportation provided by the Transportation Ministry of Colombia.<sup>10</sup>

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<sup>9</sup>Coal sales contracts have two methods to specify the amount of energy traded: GAR (Gross as received) and NAR (Net as received). The difference between GAR and NAR is the decrease in heat caused by volatile material in the coal, which also decreases the effective calorific value in the boiler of the unit. Since we want to convert amounts of coal to btu units, we must perform the conversion from GAR to NAR. The difference between NAR and GAR is 470 btu/lb (See:<https://www.drummondco.com/wp-content/uploads/coalconversionfacts200704062009.pdf>).

<sup>10</sup>See: [https://www.mintransporte.gov.co/publicaciones/359/sistema\\_de\\_informacion\\_de\\_costos\\_eficientes\\_para\\_el\\_transporte\\_automotor\\_de\\_carga\\_sicetac/](https://www.mintransporte.gov.co/publicaciones/359/sistema_de_informacion_de_costos_eficientes_para_el_transporte_automotor_de_carga_sicetac/)

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## A.5 Figures

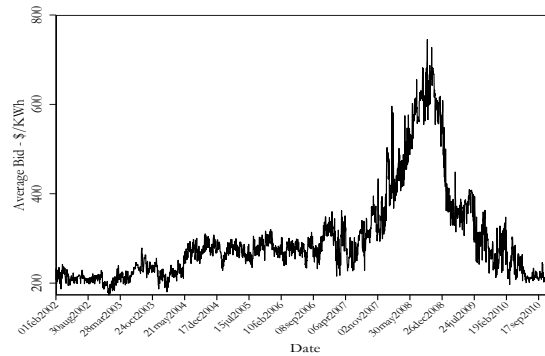


Figure A1: Average bid time series.

Note: Daily time series of the average bid from 2002 to 2010 in the Colombian wholesale electricity market.

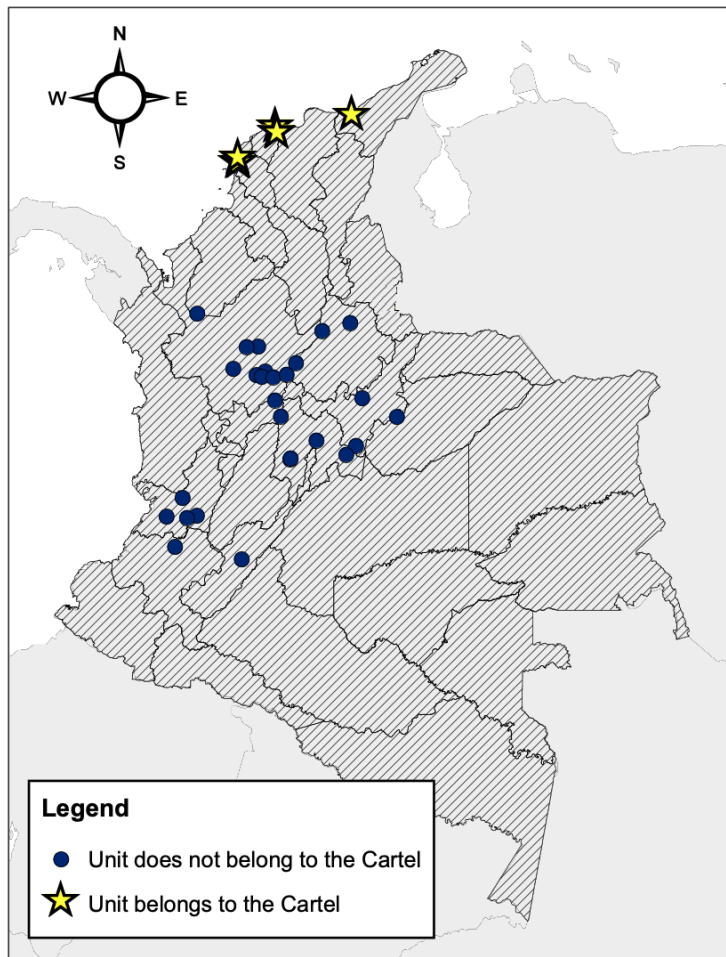


Figure A2: Geographical location of cartel units.

Note: The figure presents Colombia's map and the location of the electricity generation units participating in the wholesale electricity market in 2008/2009. The dark-shaded area represents Colombian territory. The black thick line represents the division of the country in political units called "departamentos". The star-shaped yellow shows cartel generation units and the circle-shaped blue shows non-cartel units.

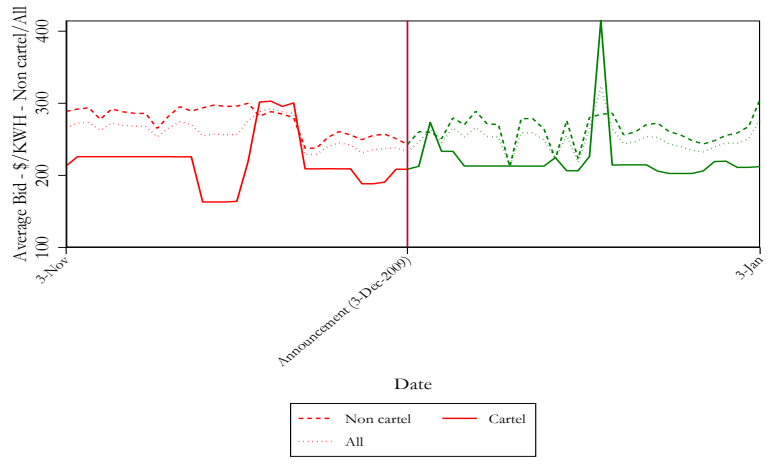


Figure A3: Cartel and non-cartel bids around the policy change of December 2009

Note: Time series of the average bid of the cartel (solid line) and non-cartel groups (dashed line) around the announcement date related to the (second) transparency policy change in December 2009. We also report the overall average (dotted line). The vertical line points to the announcement date which is the same implementation date.

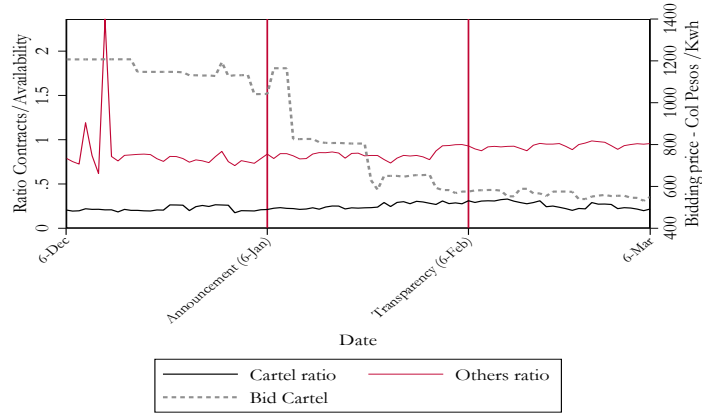


Figure A4: Time Series of Forward Contracts for Cartel and Non-Cartel Members

Note: The figure presents the time series of the portion of capacity sold through forward contracts of the cartel and non-cartel groups around the dates of announcement and implementation of the transparency policy. The black line represents the time series of the fraction of the forward contracts for the cartel group, while the red line represents the non-cartel group time series. The dotted line represents the average bidding price for the cartel group. As the availability and contract variables are set for each hour, we simply sum across hours to have a daily measure.

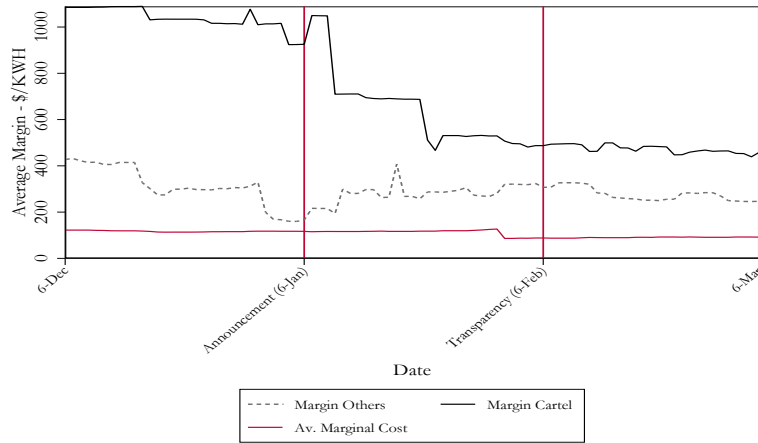


Figure A5: Average margin time series.

Note: The figure presents the time series of the average margin of the cartel (solid black line) and non-cartel (dotted grey line) groups around the dates of announcement and implementation of the transparency policy (From November 2008 to April 2009). The margin is computed as the difference between the bid minus the marginal cost. It also presents the time series of the average marginal cost of the cartel units (solid red line).

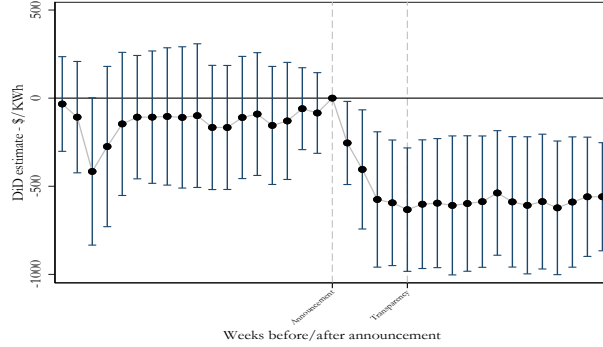


Figure A6: Event study representation using margin as the dependent variable

Note: The figure presents the event study representation of the difference-in-difference model using margin as the dependent variable, computed as bid minus marginal cost. We performed a two-way fixed effects model including a specific treatment effect for each week of the period studied. Robust s.e. are clustered by unit and date. The x-axis represents the weeks around the policy announcement. The y-axis reports the estimates using the week of the announcement as baseline. Dots and bars represent point estimates and 95% confidence intervals. The dotted line labeled as “Announcement” represents the week of the announcement of the transparency policy. The dotted line labeled as “Transparency” represents the week of the implementation of the transparency policy.

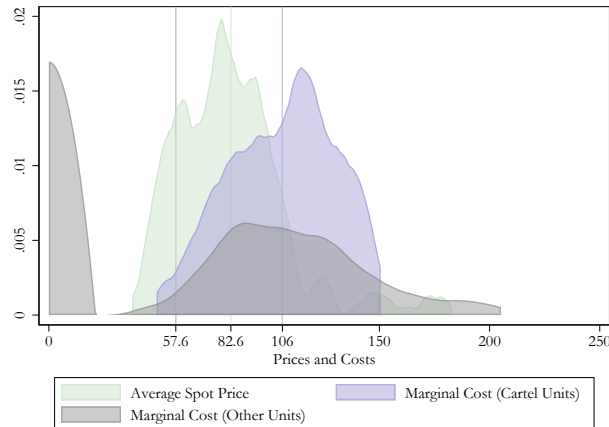


Figure A7: Competitiveness of the cartel and non-cartel units.

Note: The figure presents the kernel densities of the average daily spot price (green shaded density), marginal cost of non-cartel units (grey shaded density) and cartel units (purple shaded density) for the second semester in the year 2008. The grey vertical line indicates the mean marginal cost for non-cartel units, the green vertical line denotes the average daily spot price and finally, the purple vertical line denotes the average marginal cost for cartel units.

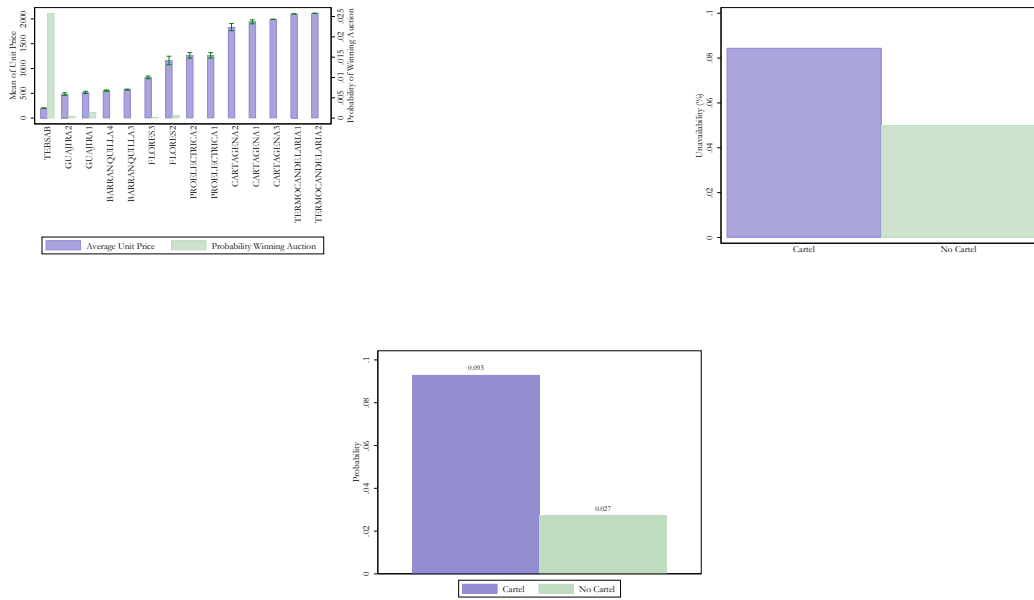


Figure A8: Inner Working of the Cartel

Note: The top left figure shows the average unit prices for the cartel units and their probability of winning the auction. The top right figure shows the fraction of unavailabilities over the total number of times that they have won in the auction for high and low-bid cartel and non-cartel firms. High-bid cartel units are those for which their average bid in the second semester of 2008 was above the median of all of the average bids. Low bids are those below the median. The bottom figure shows the probability that high-price cartel units receive positive reconciliations when low-price cartel units win, or low price no cartel units win. All of the graphs only use data for the second semester of 2008.

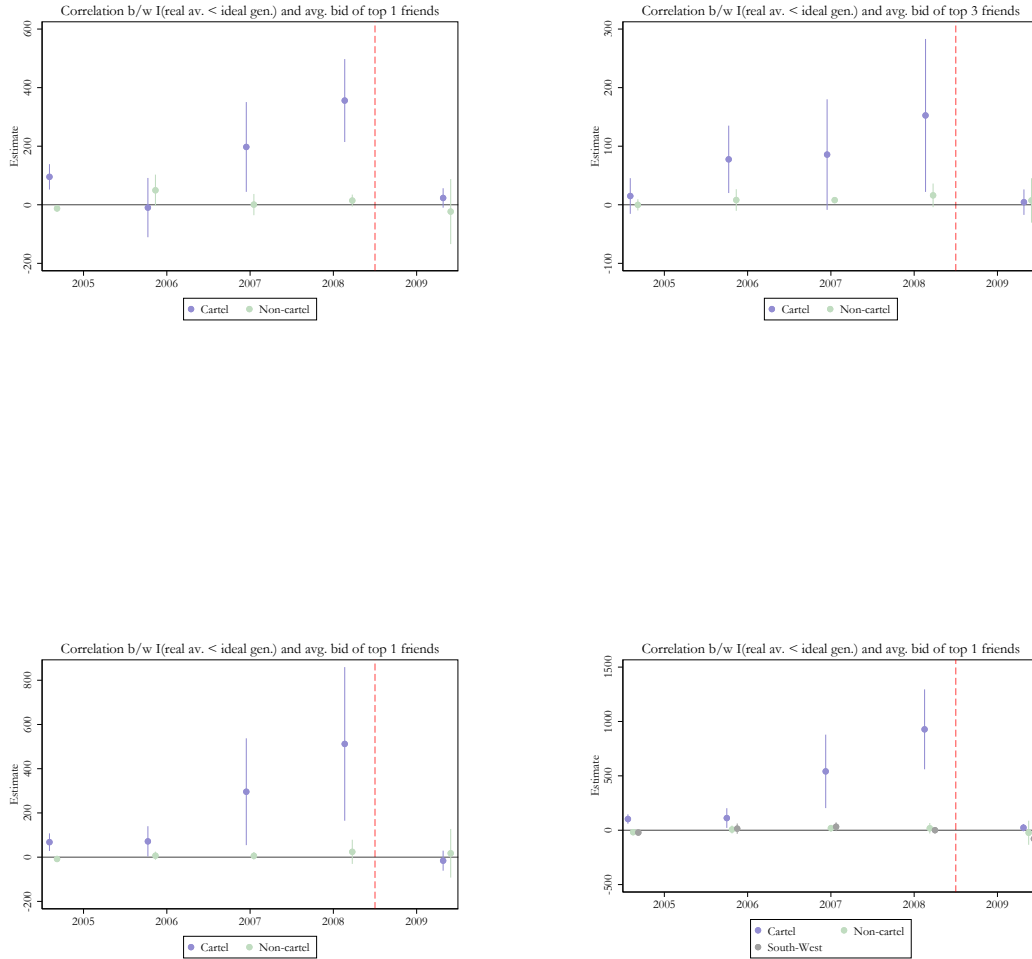


Figure A9: Robustness for coordinated bids analysis.

Note: Estimates from regressions where the outcome variable is the average bid of the friends of unit  $i$  and the explanatory variables is an indicator for unit  $i$  declaring a level of real availability below the ideal generation quantity it was awarded. We run separate regressions for the two groups (cartel, non-cartel) and repeat for years 2005 to 2009. Compared to the baseline analysis in Figure 6, we perform four robustness exercises. (i) In the top left panel, we still consider 'top 1' friends from the same period as in the baseline, but we include in the explanatory dummy **all** cases where the real availability is smaller than ideal generation (differently from the baseline, where we consider the 75% cases where the difference between real availability and ideal generation is the largest). (ii) In the top right panel, we consider the same period and same cases as in the baseline, but use the 'top 3' friends. (iii) In the bottom left panel, we consider 'top 1' friends and the same cases as in the baseline, but we construct 'friends' using observations from a longer period (2005-2008) compared to the baseline. (iv) In the bottom right panel, we repeat the same analysis as in the baseline but also report separately the estimates for the units clustered in the South-West part of Colombia. The estimates for 2009 needs to be interpreted cautiously. Data on real availability is missing for 63% of cartel observations and for 6% of non-cartel observations in 2009.



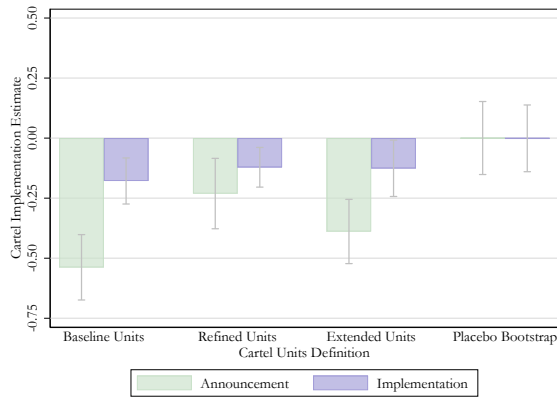


Figure A10: Robustness Exercises.

Note: The figure shows estimates of the ‘announcement’ and ‘implementation’ parameters from 4 different DiD estimations. ‘Baseline units’ reports estimates for our baseline cartel definition (14 units). ‘Refined units’ reports estimates when we include in the cartel group only units (9 units) that belong to firms that have all their units in the baseline cartel definition. ‘Extended units’ reports estimates when we include in the cartel group all the units (22 units) of firms for which at least one unit belong to the baseline cartel definition. For the placebo exercise, we randomly allocate some of the units to the placebo cartel and the rest to the control group. In doing so, we keep the same proportion of cartel and non-cartel units as is in our baseline definition. We repeat this procedure 1000 times. All estimates control for unit and date fixed effects and robust s.e. are clustered by unit and date.

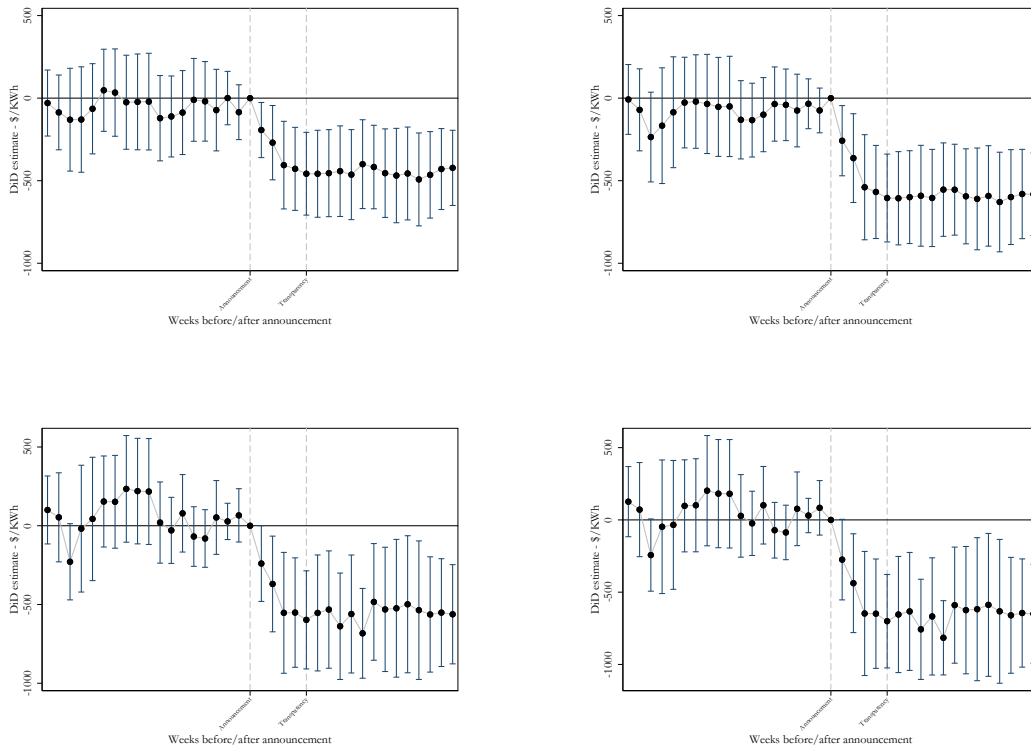


Figure A11: Event study representation for alternative cartel definitions

Note: The figure presents the event study representation for bids from a two-way fixed effects model including a specific treatment effect for each week of the period studied. Robust s.e. are clustered by unit and date. The x-axis represents the weeks around the policy announcement. The y-axis reports the estimates using the week of the announcement as the baseline. Dots and bars represent point estimates and 95% confidence intervals. The top left figure shows the event study for cartel 2 (PCA on Atlantic, Thermal, and Private) definition. The top right figure shows the event study for cartel 3 (PCA on Atlantic, Thermal, and Forward Contracts) definition. The bottom left figure shows the event study for cartel 4 (PCA on Atlantic, Thermal, Private, and Bid slope) definition. The bottom right figure shows the event study for cartel 5 (PCA on Atlantic, Thermal, Forward Contracts, Private and Bid slope) definition.

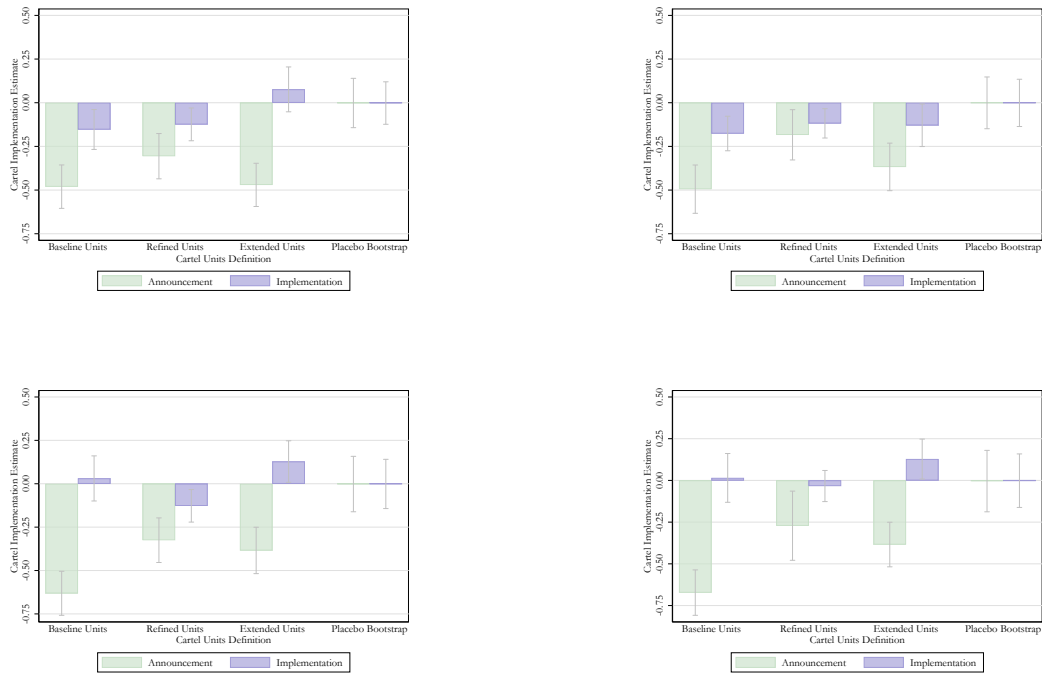


Figure A12: Refined and extended units from four cartel definitions

Note: Each sub-figure shows the estimates for 'announcement' and 'implementation' parameters from 4 different DiD regressions. The top left figure shows the result for cartel 2 (PCA on Atlantic, Thermal, and Private) definition. The top right figure shows the result for cartel 3 (PCA on Atlantic, Thermal, and Forward Contracts) definition. The bottom right figure shows the result for cartel 5 (PCA on Atlantic, Thermal, Forward Contracts, Private and Bid slope) definition. In each sub-figure, baseline units refers to each of the corresponding cartel definition (2, 3, 4 or 5). The refined units group only includes in the cartel all of the units of firms for which at least one unit belongs to the baseline cartel definition. For the placebo exercise, we randomly allocate some of the units to the placebo cartel and the rest to the control group. In doing so, we keep the same proportion of cartel and non-cartel units as is in the baseline definition. We repeat this procedure 1000 times. All estimates control for unit and date fixed effects and robust s.e. are clustered by unit and date.

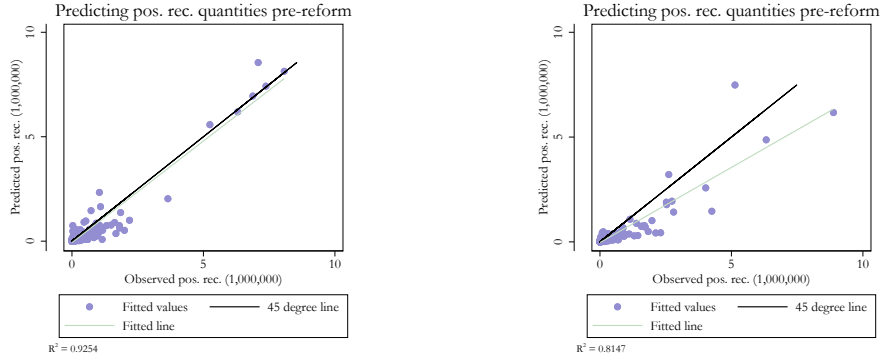


Figure A13: Comparing observed quantities of positive reconciliations with in-sample predictions for the cartel and non-cartel units in the pre and post-reform periods.

Note: We estimate how the quantity of positive reconciliation awarded to a unit depends on the rank of its bid. We use cartel and non-cartel units in this exercise (while Figure A17 refers to cartel units only). We use the estimates to make in-sample predictions for positive reconciliations at the day-unit level based on units' bids. In the figure, we compare the average predicted quantity (y-axis) with the average observed one (x-axis). The left (right) figure refers to observations from the six months before (after) the reform.

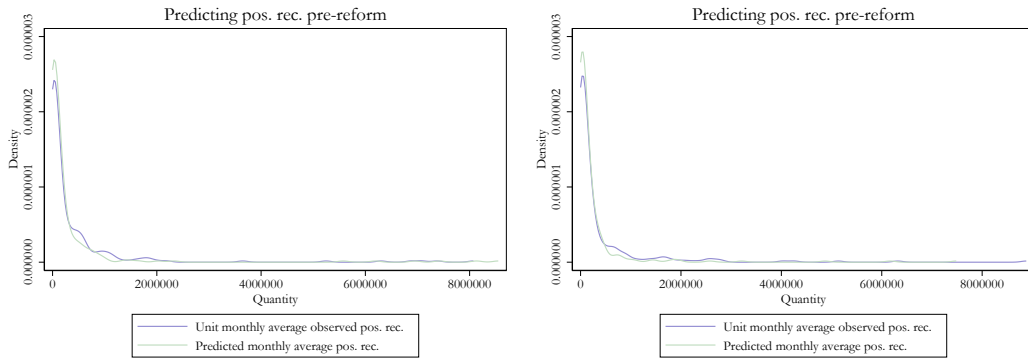


Figure A14: Comparing the distribution of observed quantities of positive reconciliations with in-sample predictions for the cartel and non-cartel units in the pre and post-reform periods.

Note: We estimate how the quantity of positive reconciliation awarded to a unit depends on the rank of its bid. We use cartel and non-cartel units in this exercise (while Figure A18 refers to cartel units only). We use the estimates to make in-sample prediction for positive reconciliations at the day-unit level based on units' bids. In the figure, we compare the density of the average predicted quantity (green line) with the density of the average observed one (purple line). The left (right) figure refers to observations from the six months before (after) the reform.

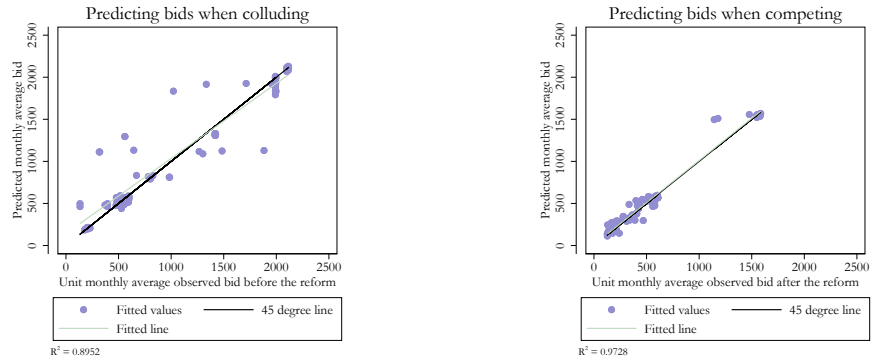


Figure A15: Comparing observed bids with in-sample predictions.

Note: We estimate how cartel units set bids by regressing bids on costs, the lagged logarithm of the total amount of positive reconciliations, and the logarithm of the ideal generation quantity. We use the resulting estimates to make in-sample predictions and average at the monthly level for each unit. In the figure, we compare the average predicted bid (y-axis) with the average observed one (x-axis). The left (right) figure refers to observations from the six months before (after) the reform.

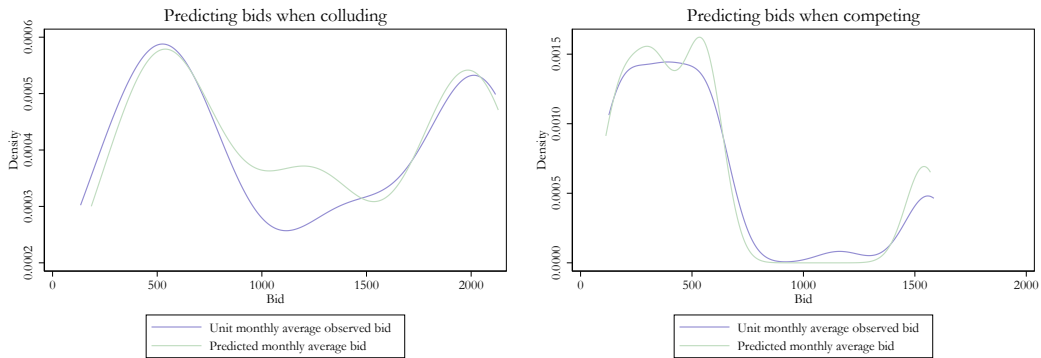


Figure A16: Comparing the distribution of observed bids with in-sample predictions.

Note: We estimate how cartel units set bids by regressing bids on costs, the lagged logarithm of the total amount of positive reconciliations, and the logarithm of the ideal generation quantity. We use the resulting estimates to make in-sample prediction and average at the monthly level for each unit. In the figure, we compare the density of the average predicted bid (green line) with the density of the average observed one (purple line). The left (right) figure refers to observations from the six months before (after) the reform.

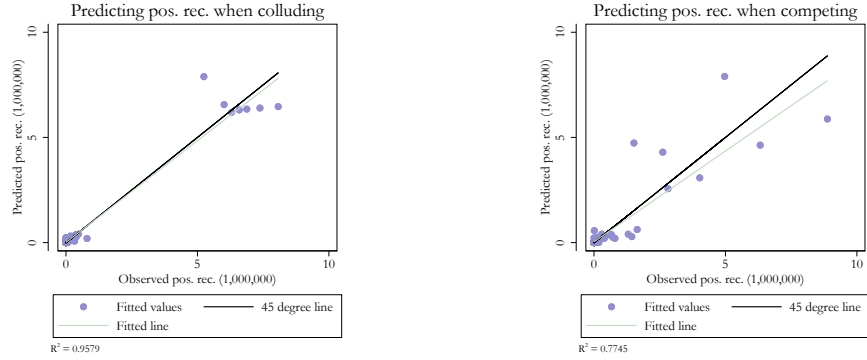


Figure A17: Comparing observed quantities of positive reconciliations with in-sample predictions for cartel units in the pre and post-reform periods.

Note: We estimate how the quantity of positive reconciliation awarded to a unit depends on the rank of its bid. We focus on cartel units in this exercise (while Figure A13 refers to all units only). We use the estimates to make in-sample predictions for positive reconciliations at the day-unit level based on units' bids. We then average at the monthly level for each unit. In the figure, we compare the average predicted quantity (y-axis) with the average observed one (x-axis). The left (rights) figure refers to observations from the six months before (after) the reform.

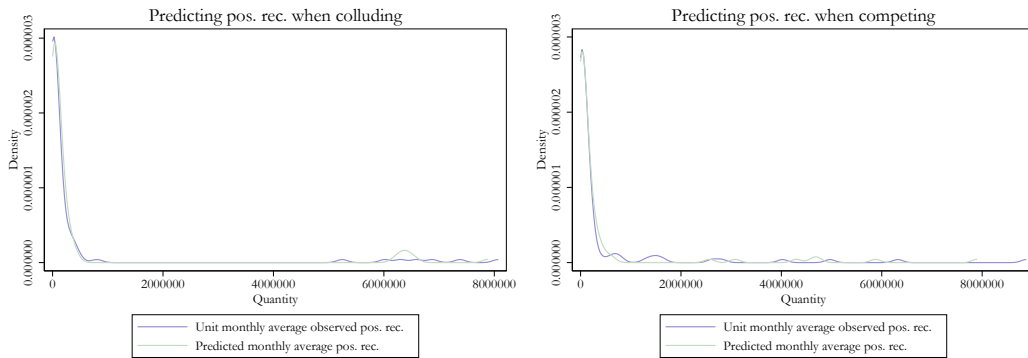


Figure A18: Comparing the distribution of observed quantities of positive reconciliations with in-sample predictions for cartel units in the pre and post-reform periods.

Note: We estimate how the quantity of positive reconciliation awarded to a unit depends on the rank of its bid. We focus on cartel units in this exercise (while Figure A14 refers to all units only). We use the resulting estimates to make in-sample predictions for positive reconciliations at the day-unit level based on units' bids. We then average at the monthly level for each unit. In the figure, we compare the density of the average predicted quantity (green line) with the density of the average observed one (purple line). The left (right) figure refers to observations from the six months before (after) the reform.

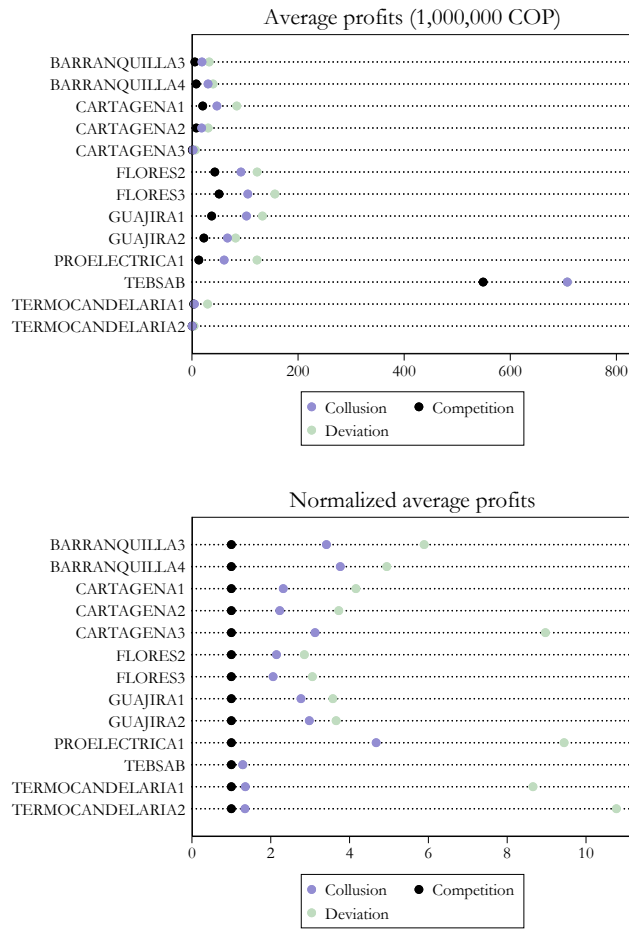


Figure A19: Average profits of cartel units under competition, collusion, and optimal deviation.

Note: We construct counterfactual bids and quantities under three alternative scenarios: Collusion, competition, and optimal deviation from collusion. Based on these variables we construct profits for each unit under the three scenarios and average over a one-year period around the reform. The top figure reports the level of the average profits (for some units, profits under different scenarios overlap in the figure), while the bottom one reports the ratio with respect to competitive profits.

## A.6 Tables

VARIABLES	(1) Margin	(2) Margin	(3) Margin	(4) Margin
Cartel Announcement	-320.52 (125.83)	-320.52 (130.48)	-308.62 (130.39)	-454.11 (116.92)
Cartel Implementation	-146.57 (47.66)	-146.57 (57.51)	-146.40 (56.76)	-145.18 (35.37)
Announcement	-130.83 (41.96)			
Implementation	-33.78 (24.93)			
Observations	11,315	11,315	16,955	16,955
R-squared	0.23	0.82	0.81	0.82
Unit FE	NO	YES	YES	YES
Date FE	NO	YES	N/A	N/A
Date x Technology FE	NO	NO	YES	NO
Date x Region FE	NO	NO	NO	YES
Forward Contracts	NO	NO	YES	YES

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table A1: Difference-in-difference estimates - Margin

Note: The table presents the estimation results of the differences in differences model using margin as the dependent variable, computed as bid minus marginal cost. Only thermal units are included in the sample. In columns 3 and 4 we further control for forward contracts over total capacity and alternatively for Date x Technology FE or for Date x Region FE. Regions are Atlantic, North-West, Central and South-West. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) LnRevenues	(2) LnRevenues	(3) LnRevenues	(4) LnProfits	(5) LnProfits	(6) LnProfits
Ln(Bid)	13.41 (2.96)	13.16 (2.95)	14.09 (2.57)	16.68 (3.73)	16.52 (3.67)	18.36 (2.76)
Ln(Bid) 2	-1.06 (0.25)	-1.05 (0.25)	-1.13 (0.22)	-1.29 (0.31)	-1.28 (0.30)	-1.43 (0.24)
Observations	991	991	788	907	907	700
R-squared	0.67	0.68	0.72	0.68	0.69	0.72
Unit FE	YES	YES	YES	YES	YES	YES
Date FE	YES	N/A	N/A	YES	N/A	N/A
Date x Technology FE	NO	YES	NO	NO	YES	NO
Date x Region FE	NO	NO	YES	NO	NO	YES
Forward Contracts	NO	YES	YES	NO	YES	YES

Robust standard errors in parentheses

Table A2: Revenues and Profits from Positive Reconciliations

Note: The table presents the estimation results of the DiD model using the logarithm of the daily revenues or profits from positive reconciliation as the dependent variables. The sample is restricted to the observations with positive values of positive reconciliations. All the columns control for unit fixed effects. Columns (1) and (3) include additional controls for Date Fixed Effects. Columns (2) and (4) control for Technology x Date and levels of forward contracts. Robust s.e. clustered by unit and date in parenthesis.



Scenario	Variable	Obs	Mean	Std. Dev.	Min	Max	Freq. Negative	T-Test
1	Actual profit	2142	61.81	201.43	-29.84	1275.53	0.0014	13.89
	Counterf. spot market profit	2142	2.79	15.57	0.00	259.88	0.0000	
2	Actual profit	1064	62.87	208.94	0.00	1139.43	0.0000	7.63
	Counterf. spot market profit	1064	20.68	45.41	0.00	341.46	0.0000	
3	Actual profit	1064	62.87	208.94	0.00	1139.43	0.0000	9.82
	Counterf. spot market profit	1064	0.01	0.16	0.00	4.70	0.0000	
4	Actual profit	1078	60.77	193.82	-29.84	1275.53	0.0028	5.58
	Counterf. spot market profit	1078	34.57	68.54	0.00	672.81	0.0000	
5	Actual profit	1078	60.77	193.82	-29.84	1275.53	0.0028	9.98
	Counterf. spot market profit	1078	2.42	16.84	0.00	239.49	0.0000	

Table A3: Comparison of profits form positive reconciliation and counterfactual competition

Note: We performed a comparison between the profits that the units of the cartel group obtained from the positive reconciliation collusive agreement and the profits that those units would obtain in the counterfactual case in which they bid their marginal costs and try to win in the ideal dispatch. First, we computed the profits from the positive reconciliation collusive agreement as the value of income from positive reconciliations minus the cost of generating the energy. Second, for computing the counterfactual of the profits if the units bid as a competitive firm, we assumed that if cartel firms would bid competitively it had the same probability of being in merit as the competitive units. Hence, we computed the probability of being on merit of the no cartel units. We computed the counterfactual profits for cartel units as the product of the probability of being on merit (if the unit is competitive) multiplied by the profit obtained by the unit if it would sell its energy at the spot price and would generate its declared availability. We allow the possibility of inaction of the unit. Hence if the profit above is negative we replace it with zero. For this computation, we only consider thermal units. We use the data for the second semester of 2008. We compute five counterfactual scenarios. **Scenario 1:** Average spot price and average hydro resources condition. The spot price used for computation is the average spot price. All the days in the sample are considered. **Scenario 2:** High spot price and high hydro resources condition. The spot price used for computation is the spot price in the higher demand hour (7 p.m.). Only the days with hydro resources higher than the median are considered. **Scenario 3:** Low spot price and high hydro resources condition. The spot price used for computation is the spot price in the lower demand hour (3 a.m.). Only the days with hydro resources higher than the median are considered. **Scenario 4:** High spot price and low hydro resources condition. The spot price used for computation is the spot price in the higher demand hour (7 p.m.). Only the days with hydro resources lower than the median are considered. **Scenario 5:** Low spot price and low hydro resources condition. The spot price used for computation is the spot price in the lower demand hour (3 a.m.). Only the days with hydro resources lower than the median are considered.

	(1)	(2)	(3)
	Ln(Bid)	Probability Positive R (t-1)	Ln(Bid)
Probability Rec.Pos ( $t - 1$ )	-0.199 (0.130)		0.620 (0.168)
Security Contingencies ( $t - 1$ )		0.113 (0.0225)	
Observations	17,087	17,087	17,087
R-squared	0.838	0.539	-0.135
Unit F.E.	YES	YES	YES
Date F.E.	YES	YES	YES
Sample	2008	2008	2008
Estimation	OLS	First Stage	Second Stage
Kleibergen-Paap F	-	25.369	-

Table A4: Positive Reconciliations and Electric Network Contingencies

Note: The table presents the instrumental variables regression of the logarithm of the bid price on the first lag of the probability of positive reconciliation using observations from the year 2008. The first column presents the results of the OLS estimates. The second column presents the first stage of the IV estimation. We use the security contingencies in the transmission system as instruments of the lag of the probability of positive reconciliation. The coefficient estimate of this column is multiplied by 10,000 to facilitate interpretation. The last column presents the second stage of the IV estimation. All the columns control by Unit and Date fixed effects. The probability of positive reconciliation in day  $t$  for unit  $i$  is computed as the mean across the 24 hourly dummies that equal one if unit  $i$  got a positive reconciliation in hour  $h$  in day  $t$ . We then use its lagged value as this is known at the time of submitting bids. Robust s.e. clustered by unit in parenthesis.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Profits from PR	Total profits	Profits from PR	Total profits	Profits from PR	Total profits
Cartel High Post	-66.25 (27.57)	-89.11 (21.05)	-67.85 (43.89)	-83.41 (21.21)	-91.02 (37.14)	-85.83 (21.20)
Cartel Low Post	-153.99 (70.58)	-59.47 (22.92)	-146.14 (67.54)	-65.17 (23.66)	-141.79 (68.03)	-62.75 (23.35)
Observations	6,725	17,155	6,725	17,155	6,725	17,155
R-squared	0.69	0.79	0.68	0.79	0.68	0.79
Unit FE	YES	YES	YES	YES	YES	YES
Date FE	YES	YES	YES	YES	YES	YES
Split high/low	Mg. Cost	Mg. Cost	Bid	Bid	Neg. Rec.	Neg. Rec.

Robust standard errors in parentheses

Table A5: Effects of announcement on profits

Note: The table presents difference in differences estimates controlling for unit and time fixed effects, where the Post period refers to the period after the policy announcement, for two outcome variables: The profits from positive reconciliations, conditional on receiving some positive reconciliations, and the total profits (unconditional). We split the cartel group in two using different measures. In columns 1-2, 'high' units are those with average marginal cost in the second half of 2008 above the median, and 'low' otherwise. In columns 3-4, 'high' units are those with average bids in the second half of 2008 above the median, and 'low' otherwise. In columns, 5-6, 'high' units are those with an average amount of negative reconciliations below the median in the second half of 2008, and 'low' otherwise. Robust s.e. clustered by unit and date in parenthesis.

	Cartel 1	Cartel 2	Cartel 3	Cartel 4	Cartel 5
Cartel 1	1.000	0.694	0.951	0.579	0.684
Cartel 2	0.694	1.000	0.638	0.526	0.450
Cartel 3	0.951	0.638	1.000	0.541	0.648
Cartel 4	0.579	0.526	0.541	1.000	0.888
Cartel 5	0.684	0.450	0.648	0.888	1.000

Table A6: Correlation Table of Alternative Cartel Definitions

Note: The table shows the correlation between the different cartel definitions. All the correlations are significant at 1% level.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LnBid	LnBid	LnBid	LnBid	LnBid	LnBid	LnBid	LnBid	LnBid	LnBid
Cartel Announcement	-0.54 (0.14)	-0.36 (0.13)	-0.48 (0.12)	-0.27 (0.14)	-0.49 (0.14)	-0.33 (0.13)	-0.63 (0.13)	-0.50 (0.14)	-0.67 (0.14)	-0.54 (0.14)
Cartel Implementation	-0.18 (0.10)	-0.03 (0.12)	-0.15 (0.11)	0.09 (0.21)	-0.18 (0.10)	-0.06 (0.12)	0.03 (0.13)	0.16 (0.13)	0.02 (0.15)	0.12 (0.14)
Observations	17,155	16,955	17,155	16,955	17,155	16,955	17,155	16,955	17,155	16,955
R-squared	0.82	0.83	0.82	0.83	0.82	0.83	0.81	0.83	0.81	0.84
Unit FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Date FE	YES	N/A	YES	N/A	YES	N/A	YES	N/A	YES	N/A
Date x Technology FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Forward Contracts	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Cartel Definition	1	1	2	2	3	3	4	4	5	5

Robust standard errors in parentheses

Table A7: Difference in Difference Estimations for Alternative Cartel Definitions

Note: The table presents the estimation results of the DiD model using the logarithm of the bid as the dependent variable. Column 1 controls for unit and date Fixed effects. Column 2 controls for Date x Technology and unit Fixed Effects as well as forward contracts. The next columns have similar patterns. We repeat the same estimation for different cartel definitions as reported in the bottom row. Cartel 1 is the baseline. Cartel 2 comes from using PCA to Atlantic, Thermal, and Private. Cartel 3 comes from using PCA to Atlantic, Thermal, and Forward Contracts. Cartel 4 comes from using PCA to Atlantic, Thermal, and Bid slope. And Cartel 5 comes from using PCA to Atlantic, Thermal, Forward Contracts, and Bid slope. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1)	(2)
	Pre-reform	Post-reform
Marginal cost	1.065 (1.245)	1.763 (1.464)
(log) total amount of positive reconciliations ( $t-1$ )	51.43 (77.33)	-22.72 (10.72)
(log) total ideal generation ( $t$ )	20.99 (93.33)	-95.14 (27.74)
Observations	2,506	2,534
R-squared	0.859	0.940
Unit FE	YES	YES

Robust standard errors in parentheses

Table A8: Estimation of bids on market fundamentals

Note: The table presents the estimates of the model used to predict the bids of cartel units. We regress bids on costs, the lagged value of the logarithm of the total amount of positive reconciliations, and the logarithm of the total amount of ideal generation. We use observations from cartel units from a one-year period around the reform (six months pre and six months post-reform in columns 1 and 2 respectively). Robust s.e. clustered by unit in parenthesis.

VARIABLES	(1)	(2)
	Logit	OLS
Rank	-1.089 (0.267)	0.189 (0.233)
Rank 2	0.0468 (0.0266)	-0.0156 (0.0150)
Observations	4,211	1,033
R-squared		0.648
Unit FE	YES	YES
Date FE	YES	YES

Robust standard errors in parentheses

Table A9: Estimation of the positive reconciliation quantities model

Note: The table presents the estimates of the models used to predict the expected quantity of positive reconciliations for cartel units. In the first column, we present the logit estimates of a binary model where we regress a dummy for receiving positive reconciliations in a day on the rank of the bid, its squared value, and unit and date fixed effects. The second column presents the OLS estimates of a linear model where we regress the logarithm of the amount of positive reconciliations in a day on the same covariates as above, using only observations with some amount of positive reconciliations. We use observations from a one-year period around the reform. Robust s.e. clustered by unit in parenthesis.