Relational Collusion in the Colombian Electricity Market [*]

Mario Bernasconi[†], Miguel Espinosa[‡], Rocco Macchiavello[§] and Carlos Suarez[¶]

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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. Under collusion – however – firms' behaviour should instantaneously respond to the anticipation of future changes in their ability to sustain it. 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. A forensic analysis uncovers a type of coordinated behaviour among cartel members that likely required explicit communication and provides suggestive evidence that such communication might have occurred. A quantification exercise supports the reduced-form evidence on collusion and quantifies its costs. Dynamic enforcement constraints should be taken seriously by policy-makers fighting collusion.

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[†]Tilburg University, m.bernasconi@tilburguniversity.edu.

[‡]Bocconi University, CEPR & CESifo, miguel.espinosa@unibocconi.it.

[§]LSE & CEPR, r.macchiavello@lse.ac.uk.

[¶]UPF and BSE, carlosandres.suarez@upf.edu.

1 Introduction

Informal arrangements sustained by the value of future interactions enable parties to cooperate when contracts are unenforceable (Macchiavello, 2022). These arrangements benefit participants but may harm the market as a whole. 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 (World Bank, 2016; Besley et al., 2020) and markets thinner and more concentrated (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.[1] Furthermore, collusive behaviour is notoriously difficult to identify (Chassang and Ortner, 2023). 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 collusive conduct hard to infer from pricing behaviour alone. A key insight of these models, however, is that firms' behaviour instantaneously responds to the anticipation of future changes in their ability to sustain collusion.

This paper uncovers collusion in the Colombian wholesale electricity market by showing that a subset of firms lowered prices immediately after news that collusion might become harder to sustain in the future. Access to reliable and cheap energy is critical for development (Greenstone et al., 2014). Besides its intrinsic relevance, the context enables us to develop a novel test

[1] Only 5% of proven cartels are in Africa (72% of those in South Africa), 7% in Latin America and 11% in Asia (see the Private International Cartel database – Connor, 2020).

of collusive behaviour supported by future rewards. Besides detailed daily auction data, we take advantage of a regulatory change: on January $6^{th}, 2009$ (the *announcement* date), the regulator, invited Professor Peter Cramton to advise on the market transparency rules. At that time, the operator disclosed all information to all market participants with a two-day 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, on January $24^{th}, 2009$ Cramton recommended increasing to 90 days the delay to disclose information to market participants. The recommended change were adopted on January $30^{th}, 2009$, with effect from February $6^{th}, 2009$ onward (the *implementation* date).

A subset of the firms lowered bids by $30 - 40\%$ 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. This strategy allows us to rule out several confounders, including the fact that changes in market transparency itself could alter firms' bidding behaviour. We investigate whether firms reacted to an increased threat of enforcement, exploiting unannounced inspections both before and after the announcement date, but find little support for such a mechanism. A forensic analysis uncovers a type of coordinated behaviour among cartel members that likely required explicit communication and provides suggestive evidence that such communication might have occurred. A quantification exercise supports the reduced-form evidence on collusion and quantifies its costs. The evidence shows that dynamic enforcement constraints can be taken seriously by policy-makers fighting collusion.

It is important to clarify what our test is – and is *not* – meant to accomplish. The test aims at uncovering the existence of a collusive arrangement. Its logic doesn't rely on – and the evidence doesn't demonstrate – that cartel members perfectly foresaw the actual change in transparency enacted by regulators and how it would make the cartel unsustainable. The test simply requires (at least some of) the cartel members to become sufficiently pessimistic about their *future* ability to sustain collusion. The test thus also does not assume, nor does it aim at identifying, the particular equilibrium concept underpinning collusive conduct. For example, while subgame perfect equilibrium (SPE) relies on players' anticipatory behaviour (which is consistent with our evidence), it also assumes correct beliefs about future play both on- and off- equilibrium paths (about which, instead, our evidence is essentially mute).

Section 2 provides background information on the Colombian wholesale electricity market and the transparency reform.[2] Like many other wholesale electricity spot markets, the Colombian one is organised as a uniform-price multi-unit procurement auction. In the market, generating units submit availability and bids to supply electricity to the grid the following day, the market regulator aggregates bids to obtain the market supply and then, given demand, it determines the price that clears the market and the resulting allocation. We describe the ideal dispatch (i.e., the production allocation resulting from this process) and the real dispatch, in which the market regulator allocates production taking into account constraints to generation and transmission that arise after generating units have submitted their bids. 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 fixes ideas about the form of collusion we later uncover and il-

[2] Appendix A.1 describes the data.

lustrates the logic of our test. Chassang and Ortner (2023) elucidate the challenges involved in identifying collusive conducts in the data: e.g., in dynamic environments, pricing behavior can deviate from static profit maximization, firms might make mistakes, and so on. Collusive strategies are difficult to characterize in general and, perhaps, particularly so in our context in which capacity constraints, participation in forward markets, and shocks to the transmission network and the resulting reconciliations significantly affect behaviour in the uniform price auction even in the absence of collusion. Our test relies on an insight common to all repeated-games models of collusion – an *instantaneous* response in anticipation of *future* changes in the ability to sustain collusion – which arguably circumvents the challenges involved in modeling such a complex environment.

Section 4 implements the test, presents the main evidence for collusion, and rules out confounders. Unlike studies that rely on proven cartels, we do not know the identity of the firms participating in the collusive arrangement – if one existed. There are two ways to "assign" firms to the cartel. A first approach puts forward a priori hypothesis on which firms might be in the cartel and tests for differential changes in bidding behavior immediately after the announcement date. An alternative approach tests for structural breaks in firms' bidding behavior around the announcement date and assigns to the cartel firms for which a break is detected. The first approach doesn't rely on changes in behavior and can be used to test for the presence of a cartel. We thus begin with the first approach and then confirm its consistency with the second one. In our baseline definition, we conjecture that the cartel was formed by thermal units in the Atlantic region. 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. 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. We validate our baseline cartel definition by checking whether cartel units are more likely to experience a structural break in bids. The test detects a structural break between the announcement and the implementation date for 11 out of the 14 units in the cartel and for only 2 out of the 33 units not in the cartel according to our baseline definition.[3,4]

Section 5 presents a forensic analysis suggesting that cartel members might have coordinated actions through explicit communication. While the evidence of a sudden drop in bids following the announcement date is consistent with a game-theoretic definition of collusion, it does not meet a legal definition that typically requires evidence of the express agreement and overt communication (Chassang and Ortner, 2023). Our forensic analysis reveals a particular type of coordination among cartel members that would have likely been extremely difficult to achieve without communication. In particular, we show that cartel members increase bids in the previous day's auction precisely when other cartel units declare themselves unavailable, generating positive reconciliations. In other words, occasionally, cartel members bid on a particular day "as if" they have information about the behaviour of other cartel members that is

[3] Interestingly, 2 of the 3 cartel units that fail the test are public, while the 2 that pass the test but are not in the baseline cartel belong to the only firm that owns both units in and out of the cartel according to our definition. Results are robust to alternative definitions of the cartel that exclude publicly owned units and/or include all units belonging to this firm.

[4] Profits – particularly from positive reconciliations – fall relatively more after the reform for *all* units in the cartel: transfers may *not* have been needed to sustain collusion.

– in theory – only revealed in the future. This correlation in behaviour only appears among cartel units and vanishes after the cartel's demise. This particular form of coordination requires units to declare unavailability upon winning in the ideal dispatch – a behaviour that, if done too frequently, attracts the regulator's attention. This anomalous coordination was thus rare and *did not* play a quantitatively important role in generating extra profits for cartel members but reveals that cartel members likely communicated explicitly on certain actions. While we do not directly observe explicit communication, patterns of attendance at the meetings of the National Council of Operations (CNO, in Spanish) reveal strategic behaviour consistent with explicit communication. CNO holds regular in-person meetings in Bogota to discuss engineering problems on the network and its bylaws prohibit attendance of personnel involved in bidding. 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. Explicit communication about bidding strategies might thus have happened around the meetings.

Section 6 presents a quantification exercise. Unlike the previous analysis, this requires committing to a specific model and, inevitably, simplifying assumptions. We focus on the reconciliations market and thermal units. We estimate best responses and associated profits exploiting the fact that both units' production costs and residual demands are observed in wholesale electricity markets. First, we compare actual bids with those that would unilaterally maximise profits. Before – but not after – the cartel's demise, the distribution of the ratio of observed bids relative to profit-maximising bids is bimodal, with peaks around one and four. In contrast, for units not in the cartel and for units in the cartel after its demise, the distribution is unimodal with a single peak around one. In other words, cartel units could have often increased their static profits by bidding significantly lower. Second, we embed the corresponding optimal deviations into the dynamic enforcement constraint. For a wide range of discount factors consistent with interest rates in Colombia during the sample period, 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 prove that the change in transparency led to the cartel's demise. The estimates, nevertheless, provide a sanity check that such an interpretation would be quantitatively consistent with observed behavior.

Section 7 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). Using detailed notes on weekly meetings of the sugar-refining cartel, Genesove and Mullin (2001) contrast the strategies used by the cartel with those underpinning repeated-game models of collusion. While they find elements of alignment, they also show that communication played a key role and that retaliation for cheating is much more restrained than implied by the theory. Our test – which relies on detecting anticipatory behavior without making further assumptions on the nature of the collusive equilibrium – is in line with their conclusions.[5]

A second branch designs empirical tests to detect anti-competitive behavior when a cartel has not been proven. Porter (2005) and Harrington (2008)

[5] Igami and Sugaya (2021) calibrate the dynamic enforcement constraint of the collusive arrangement in the international vitamin C cartel and inspire our quantification exercise. Clark and Houde (2014) exploit search warrants following an investigation of collusion in Québec's retail gasoline market and inspire the test for structural breaks.

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 (2023) 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 can be applied in other contexts to help meeting the informational hurdle, as we discuss in the conclusions.[6]

There is a general perception, but limited evidence, that cartels are particularly common in developing countries (World Bank, 2016). Asker and Nocke (2021), however, cites only two studies on collusion in developing countries (Bergquist and Dinerstein, 2020, on Kenya maize and Barkley, 2023, on Mexican insulin). Electricity is key for development (Rud, 2012; Lipscomb et al., 2013; Allcott et al., 2016) and, as it is a key input to many other sectors, distortions – e.g., due to collusive practices – are particularly detrimental to aggregate welfare (Liu, 2019). A recent review (Greenstone et al., 2019) notes that "rigorous evidence from developing countries on market design is lacking" (see also World Bank, 2019).[7] Intrinsic features of electricity markets

[6]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). Albæk et al. (1997) analysis of the Danish antitrust authority's decision to publish firm-specific transaction prices of ready-mixed concrete in three regions, supports this view. Sugaya and Wolitzky (2018), however, argue that transparency can hinder cartels by helping firms devise more profitable deviations and discuss examples in which that appears to have been the case. Byrne et al. (2023) use a retail gasoline antitrust case in which – after settlement – a firm lost access to high-frequency data about rivals' prices and find that the change in transparency softened price competition.

[7]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.

make them prone to abuse of market power and even collusion – evidence on which policies improve market efficiency is thus particularly valuable. For example, Ryan (2021) finds that a more integrated grid would increase surplus by 22% in the Indian market. Several papers study the Colombian electricity market: Fioretti et al. (2024), focus on the interaction between market power and substitution between fossil fuels and hydropower, Suárez (2022*a*,*b*) on the interaction of market power and public ownership, and Camelo et al. (2018), on the transition to centralized commitment.

Finally, relational contracts – informal agreements sustained by the value of future interactions – might be particularly relevant in developing countries (see, Macchiavello (2022), 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 coffee market. We contribute a test for relational contracting that identifies changes in *current* behavior in anticipation of changes in the *future* value of the relationship – a central implication of relational contracting models.[8]

2 The Colombia Electricity Market

This section describes the Colombian wholesale electricity market and the timeline of events. Appendix A.1 describes the data.

[8]Consistent with dynamic enforcement constraints' logic, Macchiavello and Morjaria (2021) finds that competition hinders relational contracts in the Rwanda coffee chain.

2.1 Electricity Demand and Generation

During our sample period in 2008/9, 47 generation units produced an average daily ≈ 150 GWh of electricity. Among these units, 32 units were privately owned by 11 firms and accounted for about 70% of output, the remaining 15 units were publicly owned by local regional and municipal governments. The market was a moderately concentrated oligopoly with a Herfindahl-Hirschman (top 4) index of installed capacity of 1306 (65%) (see CREG, 2009). [9]

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 fuels). The Guajira basin, on the northern coast of the country, provided most of the gas for electricity generation. Most coal-fired units are located close to large coal mines. Coal, of which Colombia was a large exporter, 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 and competition, many countries trade electricity through auction mechanisms. Like many other wholesale electricity spot markets, the Colombian one works as a uniform-price multi-unit procurement auction.[10]

Once a day, each generation 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

[9] For comparison, daily electricity production 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. Market concentration was also similar to other developing countries. For instance, the HHI index was 3.500 in Kenya, 2,300 in Peru, and 677 in Pakistan World Bank (2016).

[10] Uniform-price multi-unit auctions electricity markets include Spain (Fabra and Toro, 2005), Texas (Hortacsu and Puller, 2008) and U.K. (Crawford et al., 2007).

clears every hour. There are no intra-day balancing markets and the same spot price is paid in all the regions.[11]

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 gives rise to the *ideal dispatch*. It establishes how much energy each unit should supply and at which hour it should supply to the system. Due to their lower costs and high capacity, throughout the sample period, hydro-power units were the marginal bidder around 3/4 of the time.

Once the ideal dispatch has been determined, constraints in either generation or transmission may render the initially planned allocation unfeasible. For example, shocks to the transmission network may make it impossible for a unit to deliver electricity to the grid. Availability of a particular unit can also be lower than declared at the auction, e.g., due to production constraints.[12] As a consequence, *XM* proposes a different set of production assignments, 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.

A scheme of so-called *positive* and *negative* reconciliations. compensates the generators for the differences between the *ideal* and *real dispatches*. A unit receives a *positive reconciliation* when the real dispatch allocation is greater than the ideal dispatch. In that case, the system compensates the 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

[11] Units – not firms – submit bids in the wholesale market. Most of our analysis, therefore, considers units as the relevant decision-makers. We use information on firms' ownership of units for robustness checks and to gain further insights into the collusive agreement.

[12] The Regulator investigates units that declare unavailabilities too frequently.

called for positive reconciliations, *XM* selects the one with the lowest bid. A *negative reconciliation* arises when the real dispatch generation is less than the ideal dispatch generation. In such cases, the unit is paid a price equal to the average between the spot price and the unit's bid.

The process of positive and negative reconciliation is crucial for our analysis. The top panel of Figure A1 in the Appendix illustrates the *ideal* dispatch resulting from the bids alongside positive and negative reconciliation, while the bottom panel illustrates the resulting *real* dispatch. In the next section, we discuss how the reconciliation system affects potential collusive behavior.

2.3 Change in Transparency Policy

The average bid markedly increased during 2007/2008 (see Figure A2). The regulator began to suspect that, among the potential reasons to explain the sharp increase, anti-competitive practices might have been at play.[13]

Figure 1 summarizes the timeline of events leading to the policy change. The regulator held a meeting on January 6th, 2009, a date that we label *announcement date*, to discuss measures to deal with the increases in bids. At the meeting, it was announced that Professor Peter Cramton had been hired as a consultant to advise on the transparency rules and other potential changes to market design.

Cramton had previously advised several regulators and, possibly known by Colombian market participants, had consistently mentioned the importance of considering both the costs and benefits of transparency for market design. On the one hand, transparency might improve efficiency and simplify market oversight. On the other, it might facilitate collusion. When a market is prone to collusive behaviour, Cramton had argued against a fully-transparent policy (Cramton and Wilson, 1998; Cramton and Schwartz, 1998*a*,*b*).

[13] See (Superintendencia de Servicios Públicos, 2008) and CREG (2009), page 74.

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 30th, 2009 with effect on February 6th, 2009 (CREG, 2009). The new regulations 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 publicly disclosed in $t+2$. Each generation unit was *privately* informed whether or not they won in the auction or they had any reconciliations. The regulation mandated that generating units kept their bidding programs secret from other units. Failure to comply with the disclosure policy would be sanctioned.

3 Conceptual Framework: A Test for Collusion

3.1 Conceptual Framework

We later argue that (at least some) thermal units located in the Atlantic region likely colluded to raise bids and increase profits from positive reconciliations. The reason to focus on thermal units is that, during the sample period, these units had much higher production costs than hydro units and could not earn significant profits by bidding competitively in the ideal dispatch (see Figure A3). Indeed, hydro units cleared the ideal dispatch most of the time.

Relatively frequent disruptions to the network, however, imply that thermal units might be called in for positive reconciliations in the real dispatch. For positive reconciliations, units are paid the bid they submit (up to a maximum cap) rather than the market clearing price. Units that expect to be called in for positive reconciliation have the incentive to raise their bid.[14] When multiple units are eligible, the regulator assigns the positive reconciliation to the unit with the lowest bid. This creates a strategic interaction between eligible units, and thus an incentive to collude. If all eligible units coordinate, the inelastic demand implies that the higher price does not come at the cost of a lower quantity, potentially increasing profits substantially.

Due to the nature of the reconciliation market, we focus on a geographically isolated cluster of units in the Atlantic region (see Figure A4). Positive reconciliations mostly occur when shocks to the transmission network prevent units that won the ideal dispatch from supplying electricity. This implies that, despite the nationwide network for the ideal dispatch, units located nearby compete with each other for positive reconciliations as they are likely eligible simultaneously. Indeed, in 2008, Thermal units in the Atlantic region accounted for only 2.7% of the ideal dispatch but for 42.4% of positive reconciliations. This justifies our focus on the isolated cluster of thermal units in the Atlantic region in our baseline definition.[15]

Characterizing collusive strategies within a formal repeated-game model is difficult in general and, perhaps, particularly so in our market, and therefore lies beyond the scope of our analysis. First, even abstracting from capacity constraints considerations and participation in forward markets – important features of the wholesale electricity market – uniform-price auctions display multiple pure-strategy equilibria (Fabra et al., 2006). The model would also

[14] Using *security contingencies* – positive reconciliations exogenously awarded to compensate electricity overcharges and recover stability of the transmission system – as an instrument, Appendix A.2 confirms empirically that units submit higher bids when anticipating a higher likelihood of being called in for positive reconciliations (see Table A1 for details).

[15] Figure A4 reveals a second relatively isolated cluster of 6 units in the South-West region. The 2 thermal units in the cluster accounted for only 0.9% of positive reconciliations in 2008. This cluster is thus less likely to have had a cartel – a conjecture supported by a placebo analysis discussed below.

need to consider shocks to the network and reconciliations which, as explained above, play a crucial role in our analysis.[16] Finally, colluding firms may try to devise strategies that reduce the risk of detection. Without making strong assumptions, it would be hard to say much about how collusion is played out.

A simple insight common to all repeated-games models, however, suffice to derive a test for collusion. We can thus postpone until later a (highly-stylized) model to guide a "back-of-the-envelope" quantification exercise.

3.2 The Logic of the Test

Under collusive behavior, firms deviate from current profit maximization in anticipation of future rewards. Formally, firm i sticks to the collusive arrangement at time t if

$$\pi(a_{it}^c(\sigma_{it});\sigma_{it}) + \delta\pi(a_{it+1}^c(\sigma_{it+1});\sigma_{it+1}) + \ldots + \delta^n \mathbf{V}_{it+n}^c \geq$$
$$\geq \pi(a_{it}^d(\sigma_{it});\sigma_{it}) + \delta\pi(a_{it+1}^*(\sigma_{it+1});\sigma_{it+1}) + \ldots + \delta^n \mathbf{V}_{it+n}^*, \quad (1)$$

where $\pi(\cdot)$ are (expected) period profits, $a(\cdot)$ denotes a vector of actions as a function of the vector of state variables σ_{it} – which could include both past, present and future expected realizations of rainfall, prices, expected demand, ... – and δ the discount factor. $\mathbf{V}$ denotes (expected) continuation payoffs, and the superscripts c, d and $*$ refer to the collusive path, the (optimal) deviation, and the continuation equilibrium after such a deviation. The key challenge in testing for collusion is that profit maximization places little restrictions on firms' behavior $(a_{it}(\sigma_{it}))$: collusive conduct is hard to infer from (pricing) behavior alone (Ortner et al., 2022).

Repeated-game models of collusive behavior are built on a common insight: the *future* value of the relationship – the discounted (expected) difference in the payoffs from cooperation and defection $\mathbf{V}_{it'}^c - \mathbf{V}_{it'}^*$ – deters *current* temptations

[16] For example, Fioretti et al. (2024) abstract from reconciliations.

to deviate – the difference in payoff between deviating from the agreement $\pi(a_{it}^d(\sigma_{it}))$ and sticking to it $\pi(a_{it}^c(\sigma_{it}))$. A common implication of all these models is thus that a sufficiently large unanticipated reduction in the *future* value of the relationship can lead to a violation of the dynamic enforcement constraint and thus to an *instantaneous* change in behavior, from $\pi(a_{it}^c(\sigma_{it}))$ to $\pi(a_{it}^d(\sigma_{it}))$, holding constant σ_{it}.[17]

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 by exploiting the difference between the *announcement* and *implementation* dates. Of course, we cannot prove that any of the units anticipated the exact reform, nor that they fully worked out its implication for the equilibrium of the collusive arrangement. The test does not rely on and is not meant to prove that, the anticipation of a less transparent regime induced the cartel's demise. The test simply relies on the announcement making at least some firms sufficiently pessimistic about their future ability to sustain the cartel. The test allows us to check the existence of a cartel and whether it was dissolved by the regulator's actions.[18]

Less market transparency can worsen parties' ability to detect and punish deviations and can thus potentially destabilize the cartel (see, Perloff and Carlton, 1999; Whinston, 2008). Of course, changes in market transparency could also influence bidding behavior through other channels. For example, more

[17] This is analogous to models of relational contracting (Baker et al., 2002). A 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, 2022). The former depends on discount rates and 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 are typically not observed in the data.

[18] *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.

information gives firms more precise estimates of their residual demand curve potentially altering bidding behavior. The difference between the *announcement* and the *implementation* dates allows us to rule out such confounders: the anticipation of less transparency in the future makes it harder to satisfy current dynamic enforcement constraints leading to an instantaneous change in bidding behavior, holding constant the transparency regime.

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.*

4 Detecting the Cartel (Screening)

This Section provides evidence that a cartel was likely operating in the Colombia wholesale electricity market. We start by documenting a sudden decrease in bids right after the announcement date. Based on a priori assumptions, we introduce a baseline definition of cartel membership and present DID and event-study specifications. We then validate the baseline definition using structural break tests in bidding behavior. We conclude by discussing several robustness checks and ruling out potential confounding explanations.

4.1 Defining Cartel Membership

Figure A2 shows a large drop in the average bid around the policy change. While this is consistent with the logic of the test, it is far from conclusive.

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 the empirical test. There are two ways to "assign" units to the cartel. A first approach is to put forward an a priori hypothesis on which units might be in the cartel and verify whether they changed bidding behavior differentially around the announcement date in a way consistent with the test. A second approach is to test for structural breaks in a unit's bidding behavior around the announcement date and assign to the cartel those units that display changes in behavior consistent with the test. The first approach doesn't rely on changes in behavior around the announcement to assign units to the cartel and can be used to test for the presence of a cartel. The second approach, instead, can't be used for such a test. We begin with the first approach and then validate it using the second one.

Our baseline definition assigns *thermal units located in the Atlantic region* to the cartel. The logic for this choice was described above. All but one of the 15 units in the Atlantic region are thermal. The baseline definition yields 14 units in the cartel (9 private and 5 public) belonging to 5 firms.[19] Of these 5 firms, 4 firms only own units in the Atlantic region (for a total of 11 units). EMGESA also owns units outside the region. As shown later, results are similar if we define cartel membership at the unit or firm level and/or if we exclude publicly owned units from the baseline definition. Still, the parsimoniously constructed proxy might be imprecise and/or *ad hoc* and we will thus explore robustness along several other dimensions. The proxy might suffer from both type-I and type-II errors. Provided it 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 pass the test (see Mirenda et al., 2022, for a similar argument).

Table 1 presents descriptive statistics for units classified inside and outside the cartel before and after the announcement date. Besides providing descriptive statistics, observed patterns are consistent with intuition. For example,

[19] Barranquilla 3 and 4, Guajira 1 and 2 and Tebsab of firm GECELCA; Cartagena 1, 2 and 3 of firm EMGESA; Flores 2 and 3 of firm TERMOFLORES; Proelectrica 1 and 2 of firm PROELECTRICA; Termocandelaria 1 and 2 of firm TERMOCANDELARIA.

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. Conditional on receiving a positive reconciliation, cartel units earn higher revenues than non-cartel units from those. Cartel units have a lower share of capacity committed to forward contracts and thus a higher incentive to increase prices and collude (Wolak, 2007) and are more likely to be privately owned (64% vs. 48%). Turning to the comparison between the top and bottom panels, we see that the bids of units classified in the cartel decreased significantly more after the announcement of the policy than the bids of other units. Of course, patterns in Table 1 are only suggestive – we now subject our hypothesis to more rigorous testing.

4.2 Bidding Behaviour around the Transparency Reform

Zooming in around the regulatory change, Figure 2 shows a sharp drop in the average bid right after the *announcement* date, but *only* for cartel units. Their average bid falls by about 43% – the bid for non-cartel units barely moves.

We use a difference-in-differences specification to explore 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:

$$ln(b_{it}) = \beta_1 \mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Announ\}_t + \beta_2 \mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Trnsp\}_t + \lambda_i + \mu_t + \epsilon_{it}, \tag{2}$$

where $ln(b_{it})$ is the (log of the) bidding price of unit i at date t, the dummy variable $\mathbb{1}\{Cartel\}_i$ takes value one if i is a unit is classified to be in the cartel and zero otherwise. The dummy variable $\mathbb{1}\{Announ\}_t$ takes value one if t is a date after the announcement date (January 6th, 2009) and zero otherwise, the dummy variable $\mathbb{1}\{Trnsp\}_t$ takes value one if t is a date after the implementation of the transparency policy (February 6th, 2009) and zero otherwise. λ_i are unit fixed effects and μ_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 μ_t vary by technology type or by region. 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 policy announcement. Depending on the specification, the estimates range between a drop of 47% and 30%. Column (1) reports results without including any fixed effect. Column (2) controls for unit and date fixed effects and finds identical results. 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.[20]

The differential drop in bids is not explained by a differential change in forward positions or production costs for cartel units. Figure A5 shows that there was no change in the ratio of forward contracts over total availability for cartel and non-cartel units around the event dates. Turning to production costs, Figure A6 shows an abrupt fall in the margin ($Bid - Mg.Cost$) for cartel units but not for other units. Using margins instead of bids provides qualitatively similar results in the DID analysis (see Table A2).

Interestingly, the coefficient for $\mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Trnsp\}_t$ turns out to be small and statistically insignificant in specifications that control for more potential confounders (in columns (3) and (4)): Market transparency did not further change bidding behaviour differently between units in the cartel (which,

[20]We cannot interact date fixed effects with *both* regional dummies and technology type since there is only one non-thermal unit in the Atlantic region. However, in robustness checks that use additional criteria to define the cartel, we include both interactions simultaneously and obtain similar results.

at that point, had already collapsed) and units outside, once we let time effects vary across technologies.

Figure 3 reports estimates from a more flexible event-study specification. We extend the baseline specification in equation (2) 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.[21]

If there was a cartel and the announcement led to its demise, the profits of its members – particularly those earned from positive reconciliations – should decrease. Table 3 shows that – as expected – the likelihood of receiving positive reconciliations was unaffected, the profits from positive reconciliations (and also total profits) sharply decreased for the cartel units after the announcement date. Furthermore, Figure A9 shows that profits fell for *all* units in the cartel. Transfers might thus *not* have been needed to sustain the cartel in this case, as all units were better off colluding.

[21] Figure A7 replicates the event study looking at margins, and finds identical results. Figure A8 includes a (placebo) event study for the isolated cluster in the South-West. The estimates suggest that this cluster didn't change bidding behaviour following the announcement (baseline DiD estimate is 0.02, s.e. = 0.14). Results are similar when focusing on the thermal units in the South-West cluster. Recall that EMGESA is the only firm that owns units both in and out of the baseline cartel definition. Results are robust if we *ex*clude EMGESA units, or if we *in*clude all EMGESA units (see Appendix A.3.1 for details and further placebos). Results are robust when we refine the baseline definition considering additional criteria: private (vs public) ownership, forward contract positions, and bidding behaviour in 2008 (see Appendix A.3.2). Finally, Table A3 shows that results are robust to the exclusion of publicly owned units from the cartel (columns 1 to 4) or the exclusion of hydro units from the control group (columns 5 to 8).

4.3 A Validation: Structural Breaks Test

Thermal units in the Atlantic region – which, on a priori grounds, we have argued might have been particularly likely to collude – significantly lowered their bids after the announcement, but before the implementation, of the regulatory change relative to the other units. Here, we pursue a second approach and validate the baseline cartel definition by checking whether cartel units are more likely to experience a structural break in the time series of bids around the time of the announcement, similarly to Clark and Houde (2014). Consider the following time series model for bids

$$Bid_{i,t} = \alpha_i + \gamma_i \mathbb{1}\{t \geq \tau\} + \delta_i Bid_{i,t-1} + \varepsilon_{i,t}.$$

We test for a break in the intercept ($H_0 : \gamma_i = 0$ vs. $H_1 : \gamma_i \neq 0$) at every possible date τ for each unit i. We use data from December 1st 2008 to February 28th 2009 and a Quandt Likelihood Ratio (QLR) test to investigate whether a unit has a break between the announcement and the implementation dates. In practice, we test the null for every date τ and select the largest F-statistic to determine when the structural break – if any – occurred.

Table 4 tabulates the result from the QLR test against our baseline cartel definition. The test detects a structural break between the announcement and the implementation date for 11 out of the 14 units in the cartel (i.e., 79%). Of the 3 cartel units for which the test doesn't detect a structural break, one is privately owned and two are public. Our baseline definition of cartel thus includes 9 private units of which 8 have a break, and 5 public units, of which 3 have a break. In contrast, the test detects a structural break between the announcement and the implementation date only for 2 out of the 33 units (i.e., 6%) not classified in the cartel according to our baseline definition. Interestingly, these 2 are thermal units owned by EMGESA, the only firm that owns units both in and out of the cartel in the baseline definition.

This analysis thus suggests that our baseline cartel definition is likely quite accurate. The percentage of units detected by the structural break test differs across cartel and non-cartel units ($p < 0.01$). Furthermore, the drop in average bids for cartel units is not driven by the response of a few units. Instead, the majority of cartel units significantly altered their bidding strategy following the announcement. Figure 4 reports the average F-statistic, for cartel and non-cartel units, corresponding to the tests for a break at each date. The Figure reveals three spikes for cartel units between January 6th and February 6th, while the line is flat for non-cartel units.

4.4 Threat(s) 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 members 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 practices. Figure 5 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.

A potential concern in interpreting results from inspections that occurred *after* the announcement date as indicating that an increased threat of enforcement did not induce the drop in bids is that the announcement itself might have already signaled a tightening in enforcement. Once the cartel had collapsed, no further reaction should be expected. We can 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.[22] 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 6 shows the results. The effect of the announcement of the transparency policy remains economic and statistically significant. Furthermore, neither cartel nor non-cartel firms modified their bidding behavior following the December surveillance action. This suggests that firms might have not perceived enforcement to be a significant threat.

4.5 Discussion

We have shown that cartel units decreased bids after the announcement of the policy and before the actual implementation of it. This suggests that a cartel

[22] The SSPD summoned to its headquarters MERILECTRICA, TERMOEMCALI, TERMOTASAJERO, TERMOFLORES, TERMOCANDELARIA and GENSA to discuss high bids and EMGESA, EPSA, EPM, GECELCA, and ISAGEN to discuss both bidding behavior and frequent stops in the operation of their units.

likely operated in the market and that regulators' actions reduced collusive behavior. Furthermore, Figures 5 and 6 suggest that this reaction is unlikely to have stemmed entirely from an anticipated threat of 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.

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 the dynamic enforcement constraints required to sustain collusion would be violated. SPE relies on multiple assumptions: anticipatory behaviour – which is in line with our evidence – but also optimization, correct beliefs on- and off- the equilibrium path, about which our evidence is essentially mute. While our evidence does not prove that cartel members were playing a SPE,it shows that dynamic enforcement 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; Genesove and Mullin, 2001). 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 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 4 shows that following the announcement, units reduced their bids in different waves: some units reacted before others. Interestingly, the first units with 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 (Hortacsu 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 units in the cartel were fully anticipatory. Instead, some might display anticipatory reactions, and the rest adaptive reactions. We are unaware of empirical analyses that distinguish between these two different types of behavior in cartels. The experimental literature, however, has found evidence for both. While observed sophistication in the lab is generally lower than required by SPE, some subjects do show the kind of sophistication consistent with anticipatory behaviour (Dal Bo and Frechette, 2018). Participants in lab experiments can also be sufficiently sophisticated to understand the impact of monitoring and the frequency of interactions on the sustainability of collusion (Bigoni et al., 2019).

5 Did Cartel Members Coordinate and Communicate? (Verification)

The evidence in the previous section is consistent with collusion, at least in a game-theoretic sense. However, successful prosecution of collusion typically requires evidence of explicit coordination and communication. This section conducts a forensic analysis to provide suggestive evidence that cartel members might have indeed coordinated actions in a way that would have been difficult to achieve without communication. It also provides some suggestive evidence of communication among cartel members.

5.1 Did Cartel Members Coordinate Behaviour?

Our forensic analysis reveals a particular type of coordination among cartel members that would have likely been extremely difficult to achieve without communication. In particular, we show that cartel members sometimes increase bids in the day-ahead auction precisely when other cartel units generated positive reconciliations by declaring unavailable after winning in the ideal dispatch. In other words, occasionally cartel members bid on day t "as if" they have information about the behaviour of other cartel members that is – in theory – only revealed on day $t+1$. This correlation in behaviour only appears among cartel units and vanishes after the cartel's demise. Note that we do not argue that this particular behaviour played a quantitatively important role in generating extra profits for cartel members. On the contrary, this coordination must be quite rare as it requires units to declare unavailability upon winning in the ideal dispatch – a behaviour that, if done too frequently, attracts the regulator's attention. The anomalous behaviour, however, reveals that cartel members explicitly coordinated at least on certain actions.

This type of coordination requires several ingredients. First, some cartel units must, at least sporadically, win in the ideal dispatch. The top-left panel in Figure A10 shows that this is indeed the case. The top-right panel of the Figure then reports the likelihood that a unit declares unavailable upon winning in the ideal dispatch. This is larger for cartel units than non-cartel units – the second ingredient. Finally, the bottom panel shows that the probability that a cartel unit receives a positive reconciliation when another cartel unit wins in the ideal dispatch is much higher than when a non-cartel unit wins.

To test for this coordinated behaviour more rigorously, we would ideally know transmission 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 subsequently wins in the ideal dispatch but then declares unavailable. We proxy for these relationships between units relying on observed behaviour. For each unit j we identify its "friends", i.e., units i that are more likely to get a positive reconciliation when unit j has a negative reconciliation. For each unit j, we rank "friends" by the probability of receiving positive reconciliations when unit j declares unavailable. We focus on observations 6 months before and 6 months after the announcement date.

We test whether the average bid of j's friends increases when unit j declares (at least partially) unavailable. A striking pattern emerges. Figure 7 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. Note that, while the isolated and clustered location of cartel units correlates with identified friends, spatial proximity *per se* does not explain the observed pattern. The similarly isolated cluster of units in the South-West – our placebo – does not feature coordinated behaviour throughout the entire sample period.[23]

5.2 Suggestive Evidence of Communication

Cartel members thus coordinated some of their actions in a way that likely entailed explicit communication. Of course, the cartel might have communicated to achieve other forms of coordination that we haven't detected. We now uncover evidence that is suggestive of explicit communication.

We hand-collect data from the minutes of all the meetings of the Association of Generating Units in the year around the policy change, i.e., the second

[23]We also test for whether coordination occurs between units belonging to the same firm but find no evidence for it. Figure A11 shows that results are robust to alternative ways to construct the set of "friends" and the explanatory variable.

semester of 2008 and the first semester of 2009. This association – (CNO in Spanish, see Appendix A.1 for details) – holds meetings to solve technical difficulties and constraints to the system. The association's explicit rule was that only technical engineers could attend the meetings. Personnel from the commercial area – i.e., involved in setting bids – cannot attend. However, as we shall see, the rule was not enforced. The minutes report the names of attendees and the *firm* they work for. Within a DID framework, we test if there was any differential change in attendance between the cartel and non-cartel firms before and after the policy change.[24]

Since firms typically 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) mentioned the words *commercial* or *marketing* in their job titles.[25] 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.

Table 5 reports the results from DID specifications that control for firm and meeting fixed effects, focusing on the interaction between the post-reform period and firms in the cartel. Column (1) shows that after the reform, attendance from cartel firms didn't change relative to other firms. However, column (2) shows that the composition of the attendees changed: after the reform,

[24] Attendees can only be assigned to firms, not units. We include attendees for EMGESA, the only firm owning units both in and out of the baseline cartel definition.

[25] Results are robust if we drop job titles mentioning marketing.

firms in the cartel are relatively *less* likely to send someone from the commercial area. Finally, column (3) confirms that conditional on sending someone to the meetings, the probability of sending someone from the commercial area decreased for cartel firms relative to the others. While this does not prove that cartel firms explicitly communicated to coordinate bidding behaviour around the timing of the CNO meetings, the evidence points to strategic behaviour in attendance. Similar evidence could presumably be used to evaluate the possibility of prosecution in other cases.

6 Incentive to Deviate and Cost of the Cartel (Quantification)

This section presents a quantification exercise. Unlike our previous analysis, this requires committing to a specific model and, inevitably, simplifying assumptions. We focus on thermal units and the reconciliations market and estimate residual demand, best responses, and profits. Production costs are directly observed for thermal units. Since demand is inelastic, firms' residual demand is also directly observed in wholesale electricity markets (Wolak, 2007). This makes the analysis particularly transparent. First, we show that before the reform, but not after, units in the cartel could have increased profits by submitting lower bids. Second, we show that such deviations were likely not incentive-compatible under transparency, but became so after the reform. Finally, we quantify the cost of the cartel.

6.1 Expected Profits from Positive Reconciliations

Collusion involves a departure from unilateral profit maximization and thus implies a short-run incentive to deviate (Chassang and Ortner, 2023). Our first step is to get a handle on what unilateral profit maximization would look like for cartel units. Our evidence suggests that cartel units are unlikely to supply electricity via the ideal dispatch and exploited reconciliations to increase profits. We therefore focus on positive reconciliations throughout. Furthermore, given our baseline cartel definition, we restrict attention to thermal units. Unlike hydro units – for which the stock of water in the basin introduces dynamic considerations in bid setting – thermal units set bids to maximize static profits (Fioretti et al., 2024). This also simplifies the analysis.

Expected profits from positive reconciliations for thermal unit i in day t are given by

$$\pi_{i,t}(b_{i,t}; b_{-i,t}) = (b_{i,t} - c_{i,t}) \times \mathbb{E}[q^+_{i,t}(b_{i,t}; b_{-i,t})] \tag{3}$$

where $b_{i,t}$ and $b_{-i,t}$ are the unit's and its competitors' bids respectively, $c_{i,t}$ are constant marginal costs (which are observed), and $\mathbb{E}[q^+_{i,t}]$ the expected quantity of positive reconciliations awarded to the unit.[26]

We closely follow the empirical literature on electricity markets (Fabra et al., 2006; Wolak, 2007) to estimate expected profits under different scenarios. Positive reconciliations are assigned to fulfill a perfectly inelastic demand $Q^+_t = \sum_i q^+_{i,t}$. Given demand Q^+_t and competitors' bids $b_{-i,t}$, $\mathbb{E}[q^+_{i,t}]$ is a function of unit i's bid $b_{i,t}$ and of an idiosyncratic component, which reflects uncertainty about Q^+_t and about exogenous shocks that affect a unit's eligibility. Because demand is inelastic, $q^+_{i,t}(b_{i,t}; b_{-i,t})$ is observed.

Since positive reconciliations awarded to unit i at time t, $q^+_{i,t}$, are often zero, we parametrize expected reconciliations as $\mathbb{E}[q^+_{i,t}] = \Pr[q^+_{i,t} > 0] \times \mathbb{E}[q^+_{i,t} | q^+_{i,t} > 0]$. In the spirit of Porter and Zona (1993), we model the probability of having a positive reconciliation as a function of the rank of $b_{i,t}$ relative to $b_{-i,t}$ ($Rank_{it}$), its squared value ($Rank^2_{it}$), time (δ_t), and unit (γ_i) fixed effects which capture Q^+_t and a unit's location in the network. We estimate

$$\Pr[q^+_{i,t} > 0] = \frac{\exp(\beta_1 Rank_{i,t} + \beta_2 Rank^2_{i,t} + \gamma_i + \delta_t)}{1 + \exp(\beta_1 Rank_{i,t} + \beta_2 Rank^2_{i,t} + \gamma_i + \delta_t)}. \tag{4}$$

[26] Positive reconciliations are paid the minimum between the awarded unit's bid and a regulated price. In practice, the regulated price rarely binds.

We model the positive reconciliation awarded to unit i at time t, $q^{+}_{i,t} > 0$, as

$$\ln\left(q^{+}_{i,t}\right) = \tilde{\beta}_1 Rank_{i,t} + \tilde{\beta}_2 Rank^2_{i,t} + \tilde{\gamma}_i + \tilde{\delta}_t + \varepsilon_{it}, \text{ if } q^{+}_{i,t} > 0 \tag{5}$$

using logs to accommodate the tail in the distribution (see Figure A12).

Equations (4) and (5) can be directly estimated from the data, and then used to compute $\mathbb{E}[q^{+}_{i,t}]$ for any bid $b_{i,t}$. Because costs $c_{i,t}$ are observed (see Appendix A.6 for details), profits $\pi_{i,t}$ can be computed for any bid $b_{i,t}$ conditional on bids $b_{-i,t}$, taking into account capacity constraints. Note that we do not model how cartel units bid when colluding, as this requires assuming a certain cartel objective function. However, conditional on $b_{-i,t}$, the model allows computing (optimal) deviations.

6.2 Short-run Deviations

We hypothesize that cartel units could increase profits from reconciliations by deviating from observed conduct before the policy change but not after. Comparable thermal units not in the cartel, however, should not be able to do so. The comparison group excludes publicly owned units (as they might not maximize profits, Barros and Modesto, 1999) and the two units owned by EMGESA (as they might be part of the cartel). Results are robust to include all non-cartel thermal units.

We focus on a one-year period around the policy change and estimate Equations (4) and (5) for cartel units and the comparison group.[27] Given these estimates, we simulate alternative bids for unit i and select the one yielding the highest profits, conditional on observed $b_{-i,t}$. At the unit-day level, we compute the ratio between the observed bid and the simulated profit-maximizing bid and plot its distribution for the two groups before and after the reform.

[27] We estimate (4) and (5) separately for cartel and non-cartel units. Figure A12 shows that the model fits well the distribution of $q^{*}_{i,t}$ in both groups.

Figure 8 presents the results. Both before and after the reform, the distribution for the non-cartel units is unimodal with most mass around a ratio equal to one.[28] For cartel units, however, the pattern is starkly different. Before the reform, the distribution for the cartel units is bimodal, with one peak around one and one peak around four: cartel units could often increase profits by lowering bids. After the policy change, however, the distribution is unimodal with most of its mass around one, like for non-cartel units. A Kolmogorov-Smirnov test for the equality of the distributions for cartel and non-cartel units rejects the null hypothesis pre-reform (p-value = 0.00), but not post-reform (p-value = 0.62). In sum, cartel units systematically deviate from profit maximization before, but not after, the reform.

6.3 Dynamic Enforcement Constraints

Although our evidence doesn't imply that the cartel unraveled *because* firms correctly anticipated that collusion would no longer be sustainable under the new transparency rule, it is nevertheless instructive – as a sanity check – to ask whether the transparency regime was consistent with dynamic enforcement constraints being satisfied before, but not after, the reform.

We calibrate the dynamic enforcement constraints, assuming that deviation of a unit triggers competition as soon as past bids are made public (Igami and Sugaya, 2021). Before the reform, a unit could unilaterally deviate for two days but, from the third day onward, other cartel units would retaliate. After the reform, a unit can deviate unnoticed for 90 days. We define unit i's *incentive to collude*, Δ_i, as the slack in the unit's dynamic enforcement constraint. Assuming a stationary equilibrium, and plugging expected profits (3) into the dynamic enforcement constraint (1), the cartel was sustainable

[28] This provides a sanity check for the model described by (3), (4), (5). Conditional on observed competitors' bids, the model gives an optimal bid that is close to the observed one for non-cartel units implying a good fit of the model to the data.

under the old transparency regime, but not under the new one, if:

$$\forall i \quad \Delta_i \equiv \frac{1}{1-\delta}\pi_{i,t}(b^C_{i,t}; b^C_{-i,t}) - \frac{1-\delta^2}{1-\delta}\pi_{i,t}(b^D_{i,t}; b^C_{-i,t}) - \frac{\delta^2}{1-\delta}\pi_{i,t}(b^*_{i,t}; b^*_{-i,t}) > 0 \quad (6)$$

$$\exists i \quad \Delta_i \equiv \frac{1}{1-\delta}\pi_{i,t}(b^C_{i,t}; b^C_{-i,t}) - \frac{1-\delta^{90}}{1-\delta}\pi_{i,t}(b^D_{i,t}; b^C_{-i,t}) - \frac{\delta^{90}}{1-\delta}\pi_{i,t}(b^*_{i,t}; b^*_{-i,t}) < 0 \quad (7)$$

where δ is the discount factor, and the superscripts C, D, and $*$ indicate collusive bids, the optimal deviation, and the competitive continuation.

To calibrate the dynamic enforcement constraints in (6) and (7) we need to know the discount factor δ and the expected profits $\pi_{i,t}(\cdot)$ under the different scenarios. As usual, the discount factor is not identified. We thus set it to match the interest rate. The lending interest rate in Colombia in 2008 and 2009 was 17.2% and 13.0% respectively (IMF, link). In our baseline specification, we thus take the average of the two, 15.1%, implying a daily discount factor δ such that $\delta^{365} = 1/1.151 = 0.869$.

The expected profits $\pi_{i,t}(\cdot)$ under the different scenarios, however, are not all directly observable. *Before* the reform, strategies under collusion are observed and, therefore, expected profits under collusion, $\pi_{i,t}(b^C_{i,t}; b^C_{-i,t})$, and for the optimal deviation, $\pi_{i,t}(b^D_{i,t}; b^C_{-i,t})$ can be computed using the estimates for (3), (4), and (5). Strategies under competition, however, are not observed and, therefore, expected profits under competition $\pi_{i,t}(b^*_{i,t}; b^*_{-i,t})$ cannot be computed. *After* the reform, the opposite is true: we observe bids and profits under competition but do not observe collusive bids and thus cannot compute optimal deviations and the corresponding profits.

To circumvent this challenge, we extrapolate unobserved bidding strategies using a regression approach. The approach is discussed in detail in Appendix A.4 but, in a nutshell, it works as follows. We regress collusive bids, $b^C_{i,t}$, on production costs, demand, and unit fixed effects using observations from the *pre-reform* period. Given these estimates, we extrapolate collusive bids using the value of the covariates from the *post-reform* period. With these bids at hand, we use (3), (4), and (5) to compute $\pi_{i,t}(b^C_{i,t}; b^C_{-i,t})$ and $\pi_{i,t}(b^D_{i,t}; b^C_{-i,t})$ for the post-reform dates. Symmetrically, we extrapolate $b^*_{i,t}$ from the *post-reform* period to the *pre-reform* one. Conditional on those, we use (3), (4), and (5) to compute $\pi_{i,t}(b^*_{i,t}; b^*_{-i,t})$ for the pre-reform dates.[29]

The left panel of Figure 9 presents the results. The Figure reports the *smallest* incentive to collude across cartel units. The lines report estimates using the baseline discount factor, calibrated to match the 15.1% interest rate. For robustness, the shaded areas assume discount factors equivalent to 10% and 20% interest rates. All cartel units were better off colluding until before the transparency reform. This is no longer true after the reform. Our estimates reveal that the dynamic enforcement constraint (7), corresponding to the new transparency policy, was unlikely to hold for two units (Termocandelaria 1 and 2) after the reform, and that a minimum disclosure delay of around 60 days would have been necessary to trigger a unilateral deviation for at least one unit. If we further assume that Termocandelaria 1 and 2 optimally deviate and compute the incentive to collude for the remaining units, we find that four additional units (Cartagena 1 and 3, Flores 2 and 3) would prefer to deviate, potentially starting a chain effect.[30]

6.4 Cost of the Cartel

With the counterfactual estimates of bids and quantities used above, we conduct a back-of-the-envelope quantification of the additional costs generated by the cartel. We focus on the second half of 2008 and compare the cost of positive reconciliations under collusion and competition, that is $\sum_i q^+_{i,t}(b^C_{i,t}, b^C_{-i,t}) \times$

[29]For dates between the announcement and the implementation, we construct both collusive and competitive bids instead of relying on the observed one, as it is unclear whether observed bids reflect collusive, competitive, or deviation strategies in that period.

[30]Figure A13 shows that only for an interest rate below 8.6% – well below the interest rate in Colombia in 2008 and 2009 (17.2% and 13.0%) – collusion would have been sustainable both before and after the reform.

$b^C_{i,t} - \sum_i q^+_{i,t}(b^*_{i,t}, b^*_{-i,t}) \times b^*_{i,t}$ given that $\sum_i q^+_{i,t}(b^C_{i,t}, b^C_{-i,t}) = \sum_i q^+_{i,t}(b^*_{i,t}, b^*_{-i,t}) = Q^+_t$.[31]

The cartel generated an additional cost of at least 11 billion COP per month. This corresponds to an increase of around 12% relative to the counterfactual scenario without the cartel (right panel of Figure 9), an estimate in the ballpark of those reported in a meta-analysis of 395 proven cartel overcharges (Connor and Bolotova, 2006). Positive reconciliations account for approximately 10% of the electricity procured by the regulator, but since they are paid above the spot price this leads to an increase in overall costs of about 2.5-3%. 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 minimum wage of 461.500 COP).[32]

7 Policy Implications and Conclusions

This paper identified collusion among a subset of firms in the Colombia wholesale energy market. Our test uncovers a sudden change in bidding behavior after the announcement, but before the actual implementation, of a regulatory reform that could have made collusion harder to sustain. This anticipatory response, which our evidence suggests was unlikely to stem from anticipated threats of oversight and enforcement, suggests that dynamic incentive compatibility constraints can be taken seriously by empirical researchers and policy-

[31] Under the assumption that the total amount of positive reconciliations produced by the cartel is independent of its members colluding or competing, our estimate provides a lower bound of the benefit of competition (see Appendix A.5 for details)

[32] 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 prices.

makers fighting collusion.

Our analysis has policy implications for market design – including energy markets – particularly in developing countries. The Colombia case provides an interesting example because 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). Collusive behavior might be more widespread and induce larger distortions in less well-designed energy markets.

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 behavior) raises the possibility that regulators might be able to strategically use *announcements* to induce behavioral responses and acquire sufficient evidence to open investigations and attempt prosecution (Chassang and Ortner, 2023). For example, the announcement of market studies – that are in the powers of some competition authorities such as the CMA in the UK – could, at least in principle, lead to changes in behavior – e.g., participation in leniency programs – that could contribute to the acquisition of evidence to attempt prosecution. 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 policy that limits public information might have reduced anti-competitive behavior. In our context, 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, particularly in developing countries.

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8 Figures

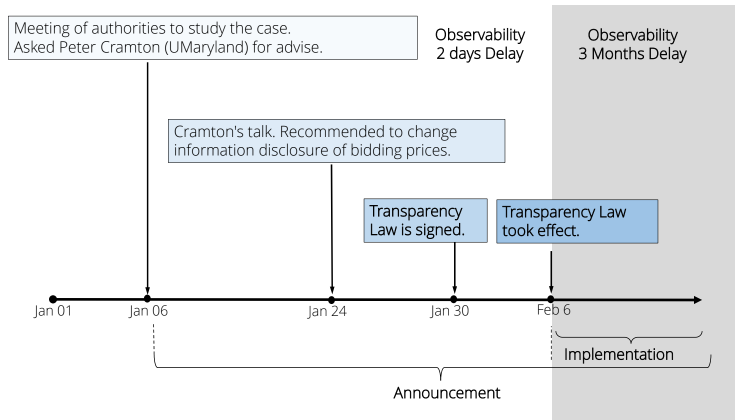

Figure 1: Timeline.

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

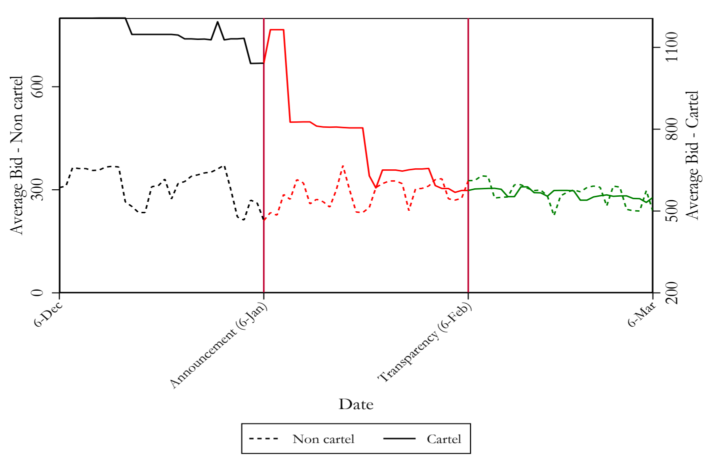

Figure 2: The main fact: Cartel and non-cartel bids.

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.

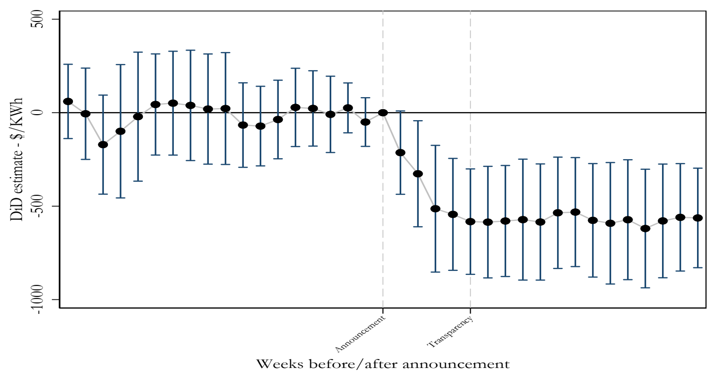

Figure 3: Event study estimates.

Note: The figure presents event study estimates using bid as the dependent variable. We estimate 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.

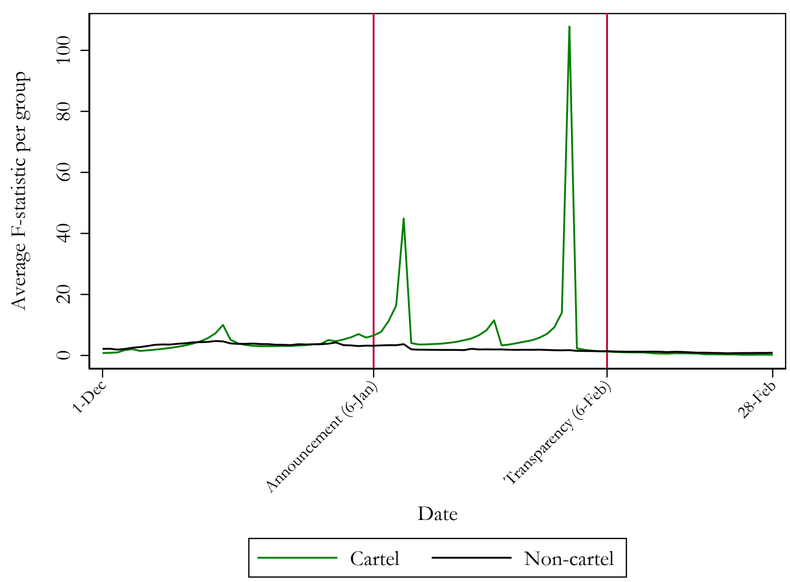

Figure 4: Test for structural breaks in the time series of bids.

Note: We use a QLR test to investigate whether a unit has a significant break in the intercept of the time series model of bids (Section 4.3) between the announcement and the implementation dates. In practice, we test the null for every date τ and select the largest F-statistic to determine where is the break. The figure reports the average F-statistic by group for each date.

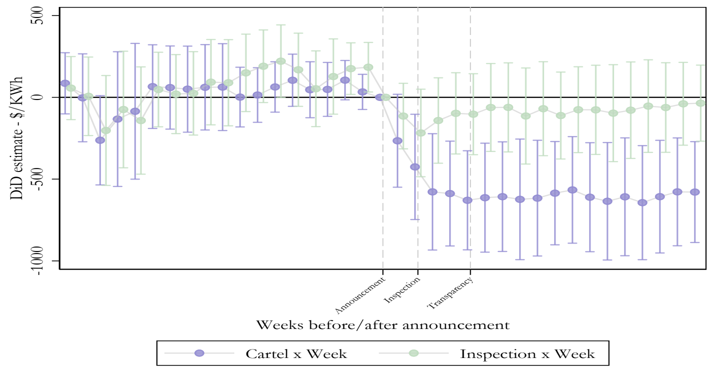

Figure 5: Event study estimates: Inspection.

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 estimate 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).

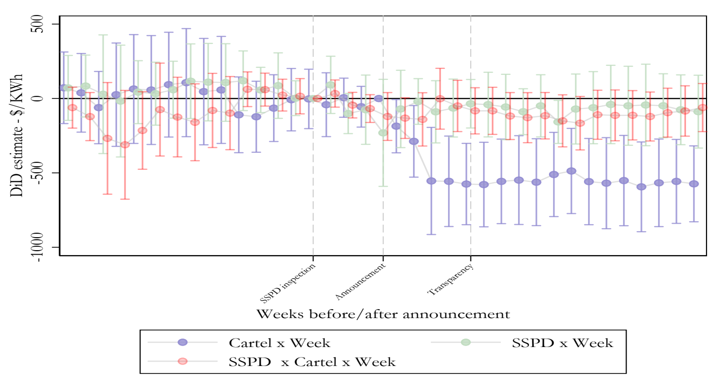

Figure 6: Event study estimates: SSPD inquiry.

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 estimate 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 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).

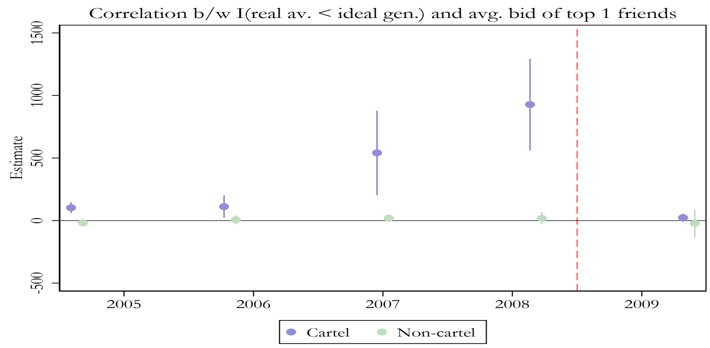

Figure 7: Bids coordination.

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 separate for each year from 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.

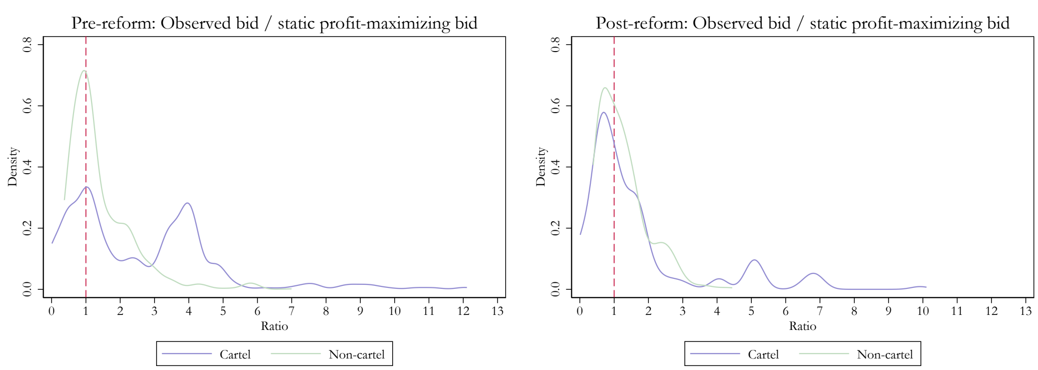

Figure 8: Distribution of the ratio of observed bids to profits maximizing bids.

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

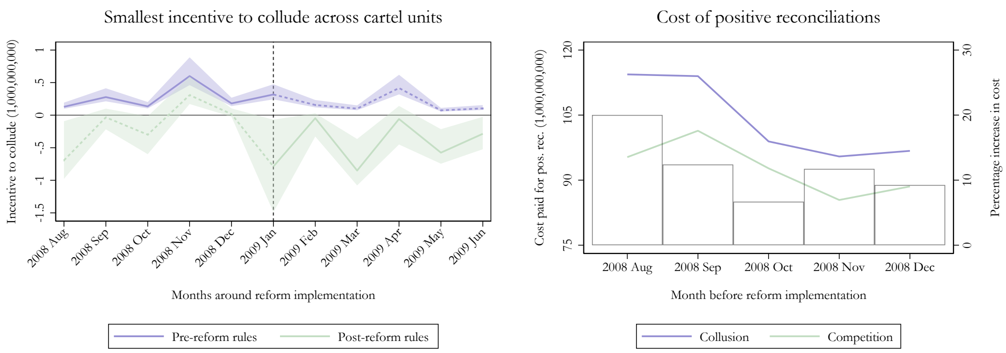

Figure 9: Smallest incentive to collude across cartel units and cost of the cartel.

Note: The left figure presents the smallest incentive to collude across cartel firms. For each cartel unit, we compute the incentive to collude in 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 lines report estimates using the baseline discount factor, calibrated to match the 15.1% interest rate. For robustness, the shaded areas assume discount factors equivalent to 10% and 20% interest rates.

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 from the system operator to cartel and non-cartel units. 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.

9 Tables

	Before	06/01/2009						
	Cartel			No Cartel				
Variable(Unit)	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
	After	06/01/2009					
	Cartel			No Cartel			
Variable(Unit)	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) LnBid	(2) LnBid	(3) LnBid	(4) LnBid
Cartel Announcement	-0.54***	-0.54***	-0.36**	-0.63***
	(0.13)	(0.14)	(0.13)	(0.12)
Cartel Implementation	-0.18**	-0.18*	-0.03	-0.08
	(0.08)	(0.10)	(0.12)	(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
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Difference-in-difference estimates.

Note: The table presents estimates from the difference in difference model described in equation (2) 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 × Technology FE or for Date × Region FE. Regions are Atlantic, North-West, Central, and South-West. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) Dummy for PR	(2) Profits from PR	(3) Total profits
Cartel Post	0.02	-135.88**	-74.29***
	(0.05)	(62.03)	(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
Date x Technology FE	NO	NO	NO
Forward Contracts	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Profits after the announcement.

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). Profits are measured in 1,000,000 of Colombian Peso. Robust s.e. clustered by unit and date in parenthesis.

	No break	Break	Total
Non-cartel	31	2	33
%	94	6	100
Cartel	3	11	14
%	21	79	100

Table 4: Cartel definition and structural breaks.

Note: The table presents the number (and percentage) of cartel and non-cartel units with or without a significant break in the intercept of the time series of bids between the announcement and the implementation dates.

VARIABLES	(1) Someone	(2) Someone Commercial	(3) Cond. Probability Commercial
Cartel x 2009	-0.001	-0.293**	-0.817***
	(0.220)	(0.128)	(0.068)
Observations	480	480	170
R-squared	0.519	0.425	0.818
Firm FE	YES	YES	YES
Meeting FE	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Meetings minutes evidence.

Note: The table presents differences in differences estimates for various outcomes controlling for firm and time-fixed effects. Because attendance is defined at the firm level, the 'Cartel' variable is also defined at the firm level here (i.e. including EMGESA). We interact it with a dummy for meetings that took place in 2009, which were all after the announcement of the reform. We use data from meetings that took place in the second semester of 2008 and the first semester of 2009. Column 1 presents the estimates for the probability of a firm sending someone to the meetings. Column 2 presents the estimates for the probability of sending someone from the commercial area. Column 3 for the probability of sending someone from the commercial area conditional to sending someone to the meetings. Robust s.e. clustered by firm 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.6 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).[1] 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

[1]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 Expected Positive Reconciliations and Bidding Behaviour

We show 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 (up to a certain maximum allowed price). Note that this incentive applies to both cartel and non-cartel units.

The expectations of being awarded a positive reconciliation is not observed and must be proxied with actual reconciliations. However, actual reconciliations 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 instrument. Security contingencies provide us 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 network 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 A1 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. Column 4 presents the estimates from the 'reduced-form' regression of bids on the instrument, which confirms that units use security contingencies to form expectations about reconciliations, and consequently increase bids.

A.3 Robustness in the Cartel Definition

A.3.1 Robustness in the Cartel Definition: Firms' Ownership

Our cartel definition has classified units and not firms. Recall that EMGESA is the only firm that owns units both in and out of the baseline cartel definition. Figure A14 shows that results are robust if we *ex*clude EMGESA units, and if we *in*clude all EMGESA units. It further shows that our results are unlikely to be driven by chance (Placebo). 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 A14 presents the results and shows two main patterns. First, excluding or including EMGESA units, 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.3.2 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 A4 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 A5 shows the DiD 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.[2]

Figure A15 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.

While our baseline definition of the cartel focuses a priori on Thermal units, the alternative ones 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.4 Details on Extrapolating Bids

In order to calibrate the dynamic enforcement constraints (6) and (7), we need profits $\pi_{i,t}(b^*_{i,t}; b^*_{-i,t})$, $\pi_{i,t}(b^C_{i,t}; b^C_{-i,t})$ and $\pi_{i,t}(b^D_{i,t}; b^C_{-i,t})$ for all cartel units and dates t. As explained in Section 6.3, we can readily compute $\pi_{i,t}(b^C_{i,t}; b^C_{-i,t})$ and $\pi_{i,t}(b^D_{i,t}; b^C_{-i,t})$ for the pre-reform dates given our estimate of Section 6.2. However, since competitive bids are not observed for these dates, we use a regression approach to extrapolate them from the post-reform period. Symmetrically, we readily compute $\pi_{i,t}(b^*_{i,t}; b^*_{-i,t})$ for post-reform dates. However, since collusive bids are not observed for these dates, we use a regression approach to extrapolate them from the pre-reform period.

We use the model in equation (A1) and regress bids on unit fixed effects $\bar{\gamma}_i$, costs $c_{i,t}$, the logarithm of the demand of positive reconciliation in $t-1$ $\ln(Q^+_{t-1})$, and the logarithm of the demand of ideal generation $\ln(Q_t)$ – the two exogenous quantities known at the time of submitting bids.

$$b_{i,t} = \bar{\beta}_1 c_{i,t} + \bar{\beta}_2 \ln(Q^+_{t-1}) + \bar{\beta}_3 \ln(Q_t) + \bar{\gamma}_i + \nu_{i,t} \tag{A1}$$

[2] Unreported result are robust to the contemporaneous inclusion of the interaction between 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.

We estimate (A1) separately for the pre and post-reform periods, that is we estimate the regression once with data from the six months before the announcement, and once with data from the six months after the implementation of the reform. Table A6 presents the estimates. As argued by Porter and Zona (1993) and Ishii (2009), our estimates suggest that bids do not necessarily respond to changes in the underlying market fundamentals when units are colluding (column 1), but they do when units are competing (column 2).

We then use the estimates from the pre-reform period (column 1), together with the value of the covariates for each date, to predict collusive bids. For observations before the reform, these correspond to in-sample predictions. For observations after the reform, these are out-of-sample predictions or extrapolated bids. The opposite is true when we use estimates from the post-reform period (column 2). Figure A16 plots the distributions of in-sample predictions versus observed bids.

A.5 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 calibrate the enforcement constraints and then sum over units. We aggregate costs at the monthly level and present the results in the right panel of Figure 9.

A.6 Calculation Marginal Costs

As common 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:

$$\underbrace{Exchange\ R._t}_{\frac{COP\$}{US\$}} \times \Big[\underbrace{Heat\ R._i}_{\frac{MBTU}{KWh}} \times \underbrace{(Transp.\ fuel\ cost_i + Fuel\ cost_t)}_{\frac{US\$}{MBTU}} \Big] = \underbrace{Marginal\ Cost_{it}}_{\frac{COP\$}{kWh}}$$

where COP are Colombian pesos, $MBTU$ are one thousand of British thermal units, US are US 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).[3]

Regarding fuel prices, for non-internationally tradable inputs, we used a reference price of the contracts as in Wolfram (1999) and for tradable inputs,

[3] See: http://paratec.xm.com.co/paratec/SitePages/generacion.aspx?q=capacidad (website might be slow or not accessible from outside Colombia).

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 sale price of gas. This maximum price at period t, p_t, is given by the formula $p_{t-1}[index_{t-1}/index_{t-2}]$ where $index_{t-1}$ 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.[4] 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)[5].

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).

[4] The formula was established in Resolution 119/2005 of CREG (CREG, 2005)

[5] See: https://www.banrep.gov.co/es/estadisticas/trm.

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 Colombian Ministry of Energy and Mines (Ministerio de Minas y Energía , MME).[6] 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.[7]

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[6] 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://drummondco.com/wp-content/uploads/coalconversionfacts200704_06_2009.pdf).

[7] See: https://www.mintransporte.gov.co (website might be slow or not accessible from outside Colombia).

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A.7 Appendix Figures

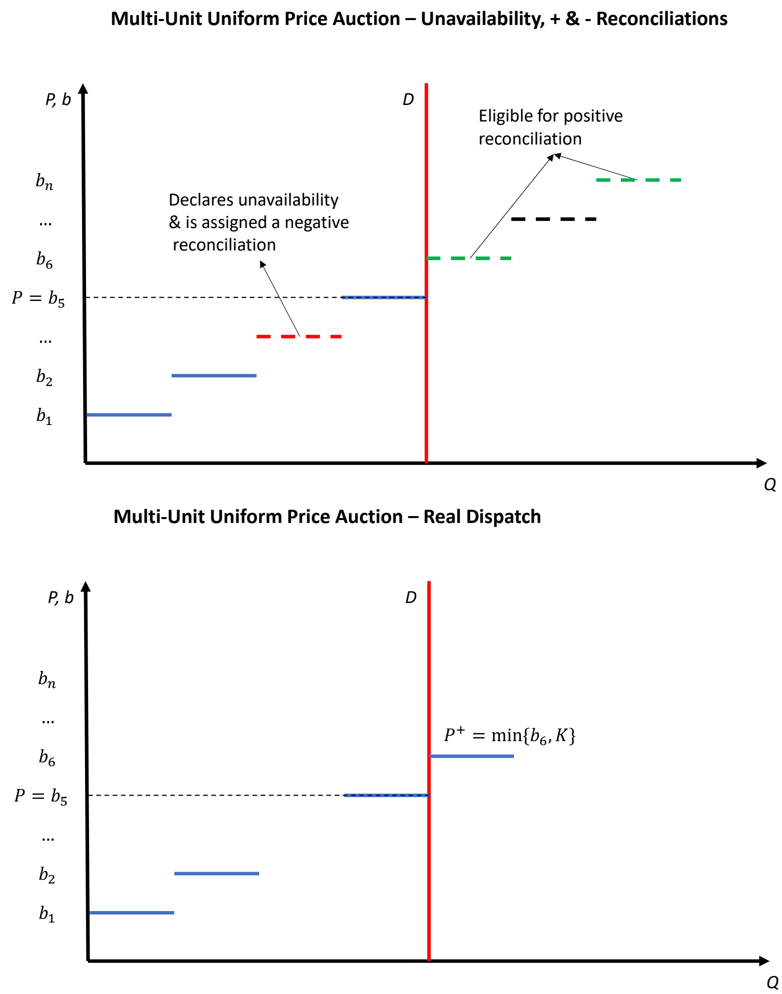

Figure A1: Uniform price auction, ideal dispatch, and reconciliations.

Note: The system operator arranges bids in increasing order $(b_1, b_2, \ldots, b_n)$ to fullfill an inelastic demand (vertical red line D) at the smallest possible cost. The spot price (P) is the bid of the marginal unit necessary to fulfill the demand. Once the ideal dispatch has been determined, contingencies may arise and make unfeasible the planned allocation. The operator proposes then a different allocation: the real dispatch. A unit receives a positive reconciliation when its real dispatch allocation is greater than the ideal one. In that case, the operator compensates a price (P^+) equal to the minimum between a cost-based regulated price (k) and the unit's bids (b_i).

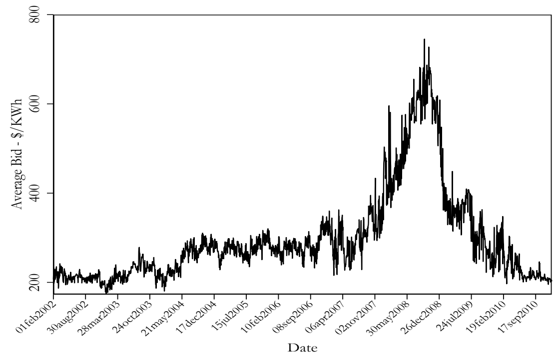

Figure A2: Average bid time series.

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

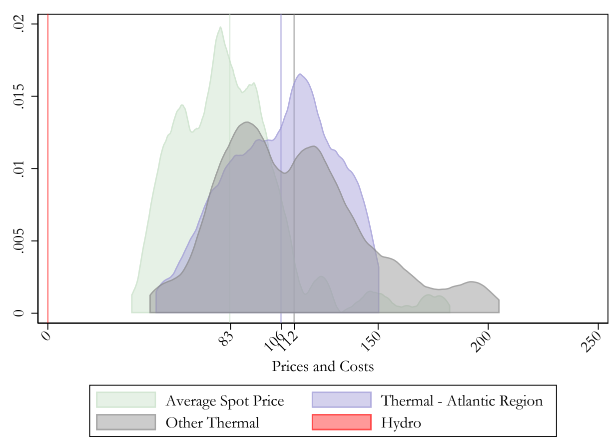

Figure A3: Spot price and production costs.

Note: The figure presents the distribution of the average daily spot price (green shaded density), of the marginal cost of thermal units in the Atlantic region (purple shaded density), other thermal units (grey shaded density), and hydro units (red) – for which marginal costs are zero – for the second semester of 2008. The vertical lines report the average values of the different distributions, with similar average costs for thermal units in different regions that are higher than the average spot price.

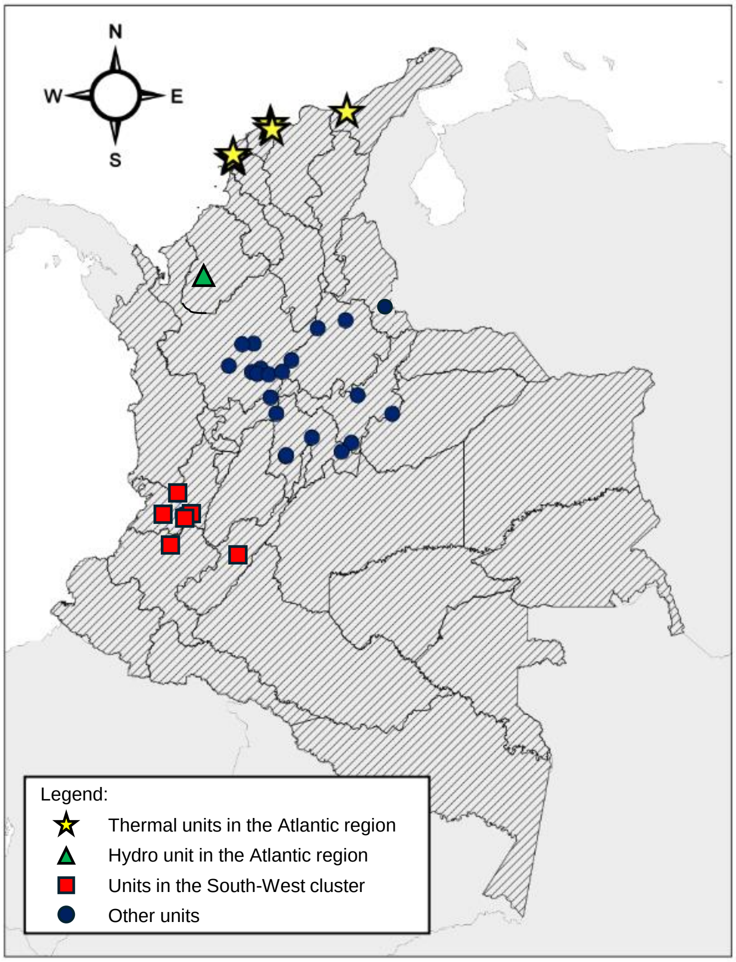

Figure A4: Geographical location of cartel units.

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 the Colombian territory. The black thick line represents the division of the country in political units called "departamentos".

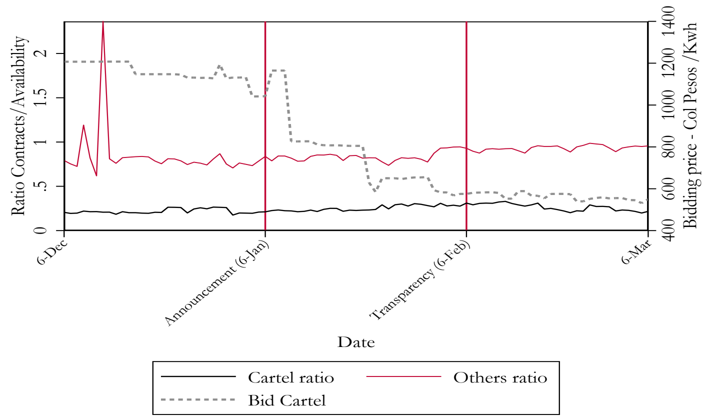

Figure A5: Forward contracts.

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.

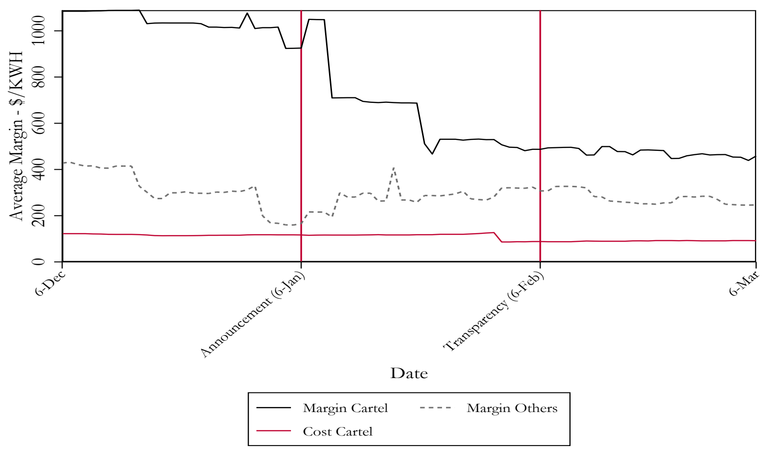

Figure A6: Average margin.

Note: The figure presents the average margin of the cartel units (solid black line), of the non-cartel units (dotted grey line), and the average marginal production cost of cartel units (solid red line), around the dates of announcement and implementation of the transparency policy. The margin is computed as the difference between the bid minus the marginal cost.

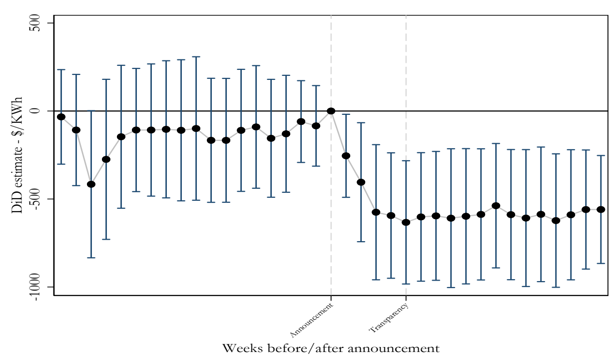

Figure A7: Event study estimates for margin.

Note: The figure presents event study estimates using margin (bid minus cost) as the dependent variable. We estimate 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.

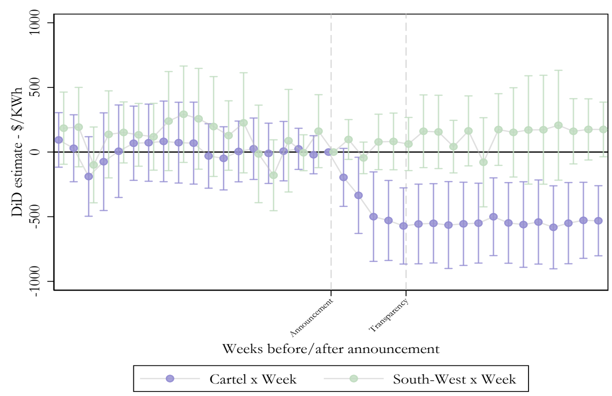

Figure A8: Event study estimates for units in the South-West cluster.

Note: The figure presents event study estimates using bid as the dependent variable. We estimate 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.

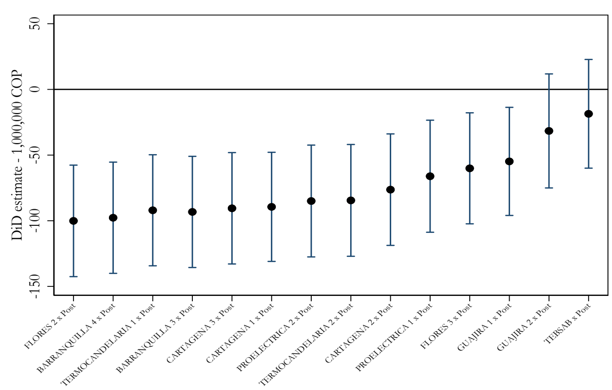

Figure A9: DiD estimates for total profits of each cartel unit.

Note: The figure presents the estimates from a difference in difference model with outcome variable total profits, where the Post period dummy (which refers to the period after the policy announcement) is interacted with each cartel units’ dummy. The control group includes all non-cartel units. Dots and bars represent point estimates and 95% confidence intervals.

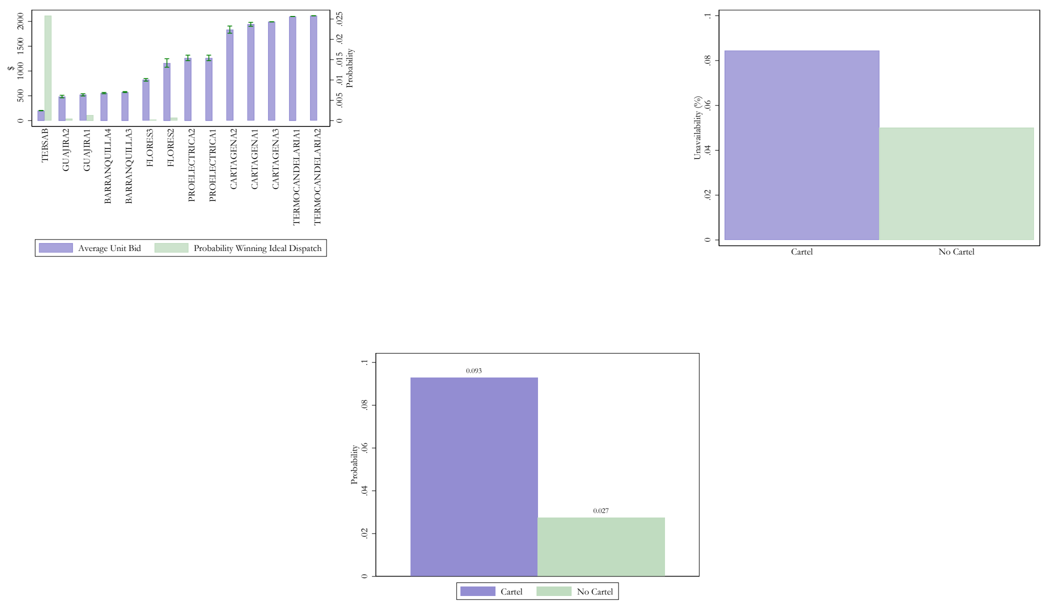

Figure A10: Probability of winning, of unavailability, and of reconciliations.

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 cartel and non-cartel units. 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. 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. All of the graphs only use data for the second semester of 2008.

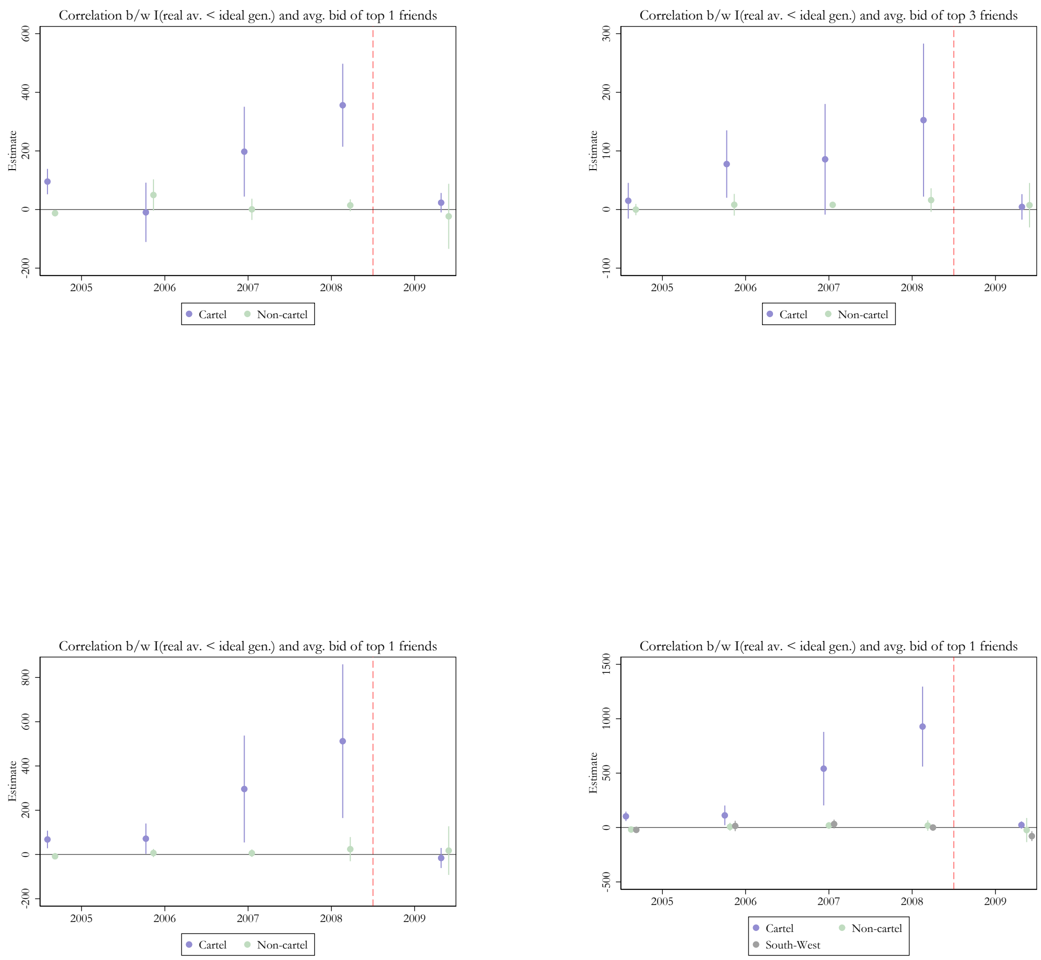

Figure A11: Robustness: Bids coordination.

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 or three groups (cartel, non-cartel, South-West) and repeat for years 2005 to 2009. Compared to the baseline analysis in Figure 7, 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.

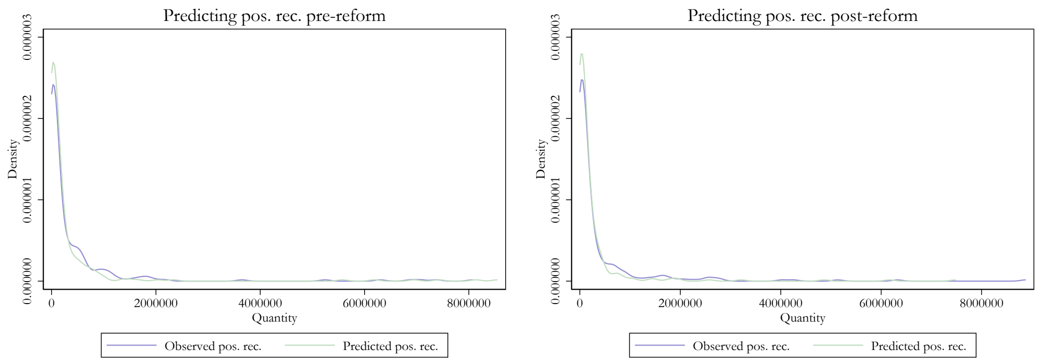

Figure A12: Distribution of observed quantities of positive reconciliations and in-sample predictions.

Note: We estimate how the quantity of positive reconciliation awarded to a unit depends on the rank of its bid, as in (4) and (5). We use cartel and non-cartel units but estimate the regression separately for the two groups. We use the estimates to make in-sample prediction for positive reconciliations at the day-unit level given observed bids. In the figure, we compare the distribution of the predicted quantity (green line) with the distribution of the observed quantity (purple line). The left (right) figure refers to observations from the six months before (after) the reform.

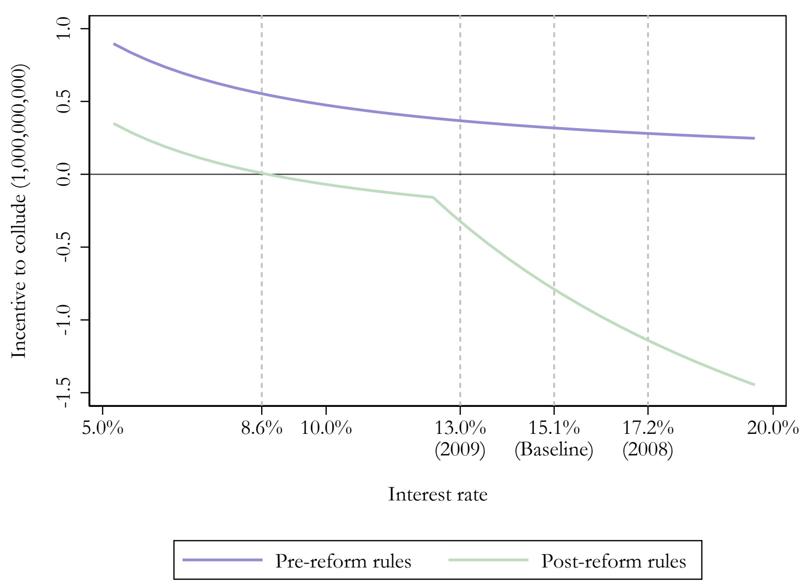

Figure A13: Cartel sustainability for different interest rates.

Note: The figure reports the smallest incentive to collude across cartel units, as computed in January 2009, for different values of the interest rate (and thus of the discount factor). The vertical lines refer to the interest rate used in our baseline exercise (15.1%, see Figure 9), the ones observed in 2008 and 2009 (17.2% and 13%), and the critical one for cartel sustainability under the new regime (8.6%).

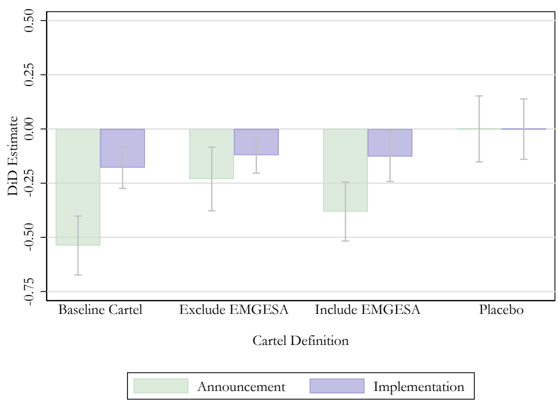

Figure A14: Robustness of cartel definition and placebo.

Note: The figure shows estimates of the 'announcement' and 'implementation' parameters from different DiD regressions. 'Baseline cartel' refers to the estimates for our baseline cartel definition, i.e. thermal units in the Atlantic region. Recall that EMGESA is the only firm that owns units both in and out of the baseline cartel definition. We thus show that results are robust if we *ex*clude EMGESA units from the cartel group or if we *in*clude all EMGESA units in the cartel group. The 'Placebo' estimates refer to a placebo exercise. 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.

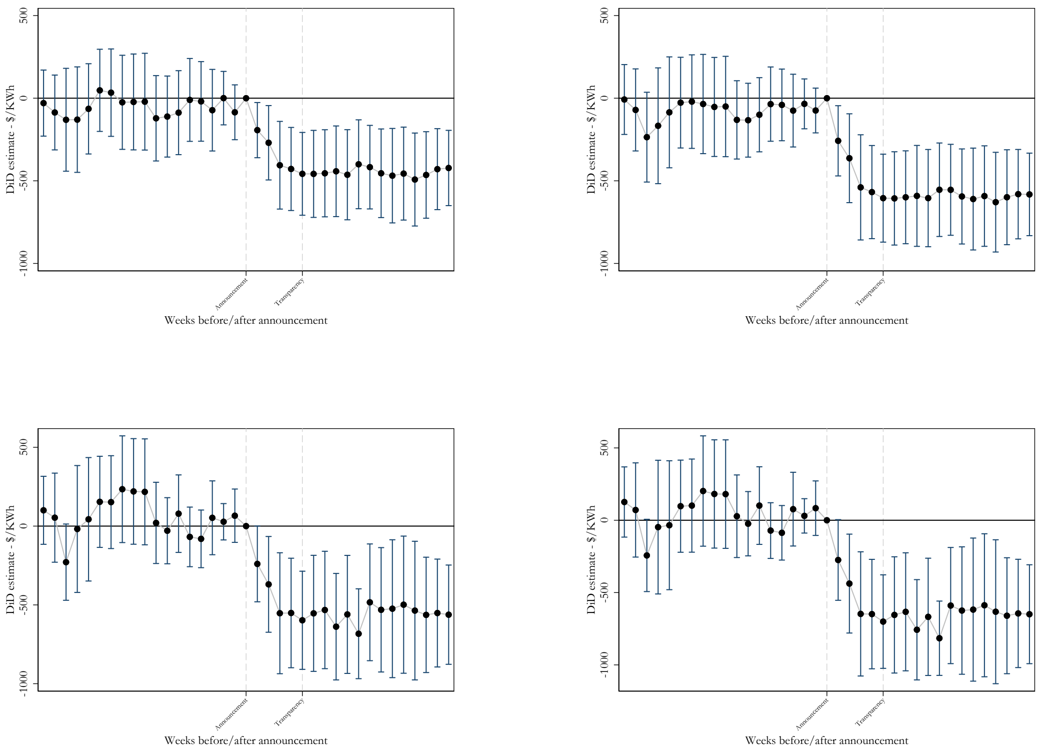

Figure A15: Event study estimates for alternative cartel definitions.

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.

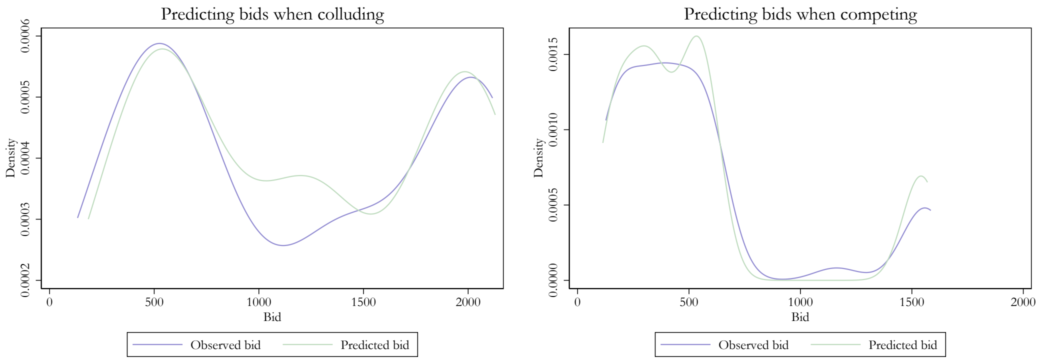

Figure A16: Distribution of observed bids and in-sample predictions.

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 as in (A1). 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.

A.8 Appendix Tables

	(1)	(2)	(3)	(4)
	Ln(Bid)	Probability Pos. Rec. (t-1)	Ln(Bid)	Ln(Bid)
Probability Pos. Rec. $(t-1)$	-0.199		0.620***	
	(0.130)		(0.168)	
Security Contingencies $(t-1)$		0.113***		0.0701***
		(0.0225)		(0.0182)
Observations	17,087	17,087	17,087	17,087
R-squared	0.838	0.539	-0.135	0.839
Unit F.E.	YES	YES	YES	YES
Date F.E.	YES	YES	YES	YES
Sample	2008	2008	2008	2008
Estimation	OLS	First Stage	Second Stage	Reduced-form
Kleibergen-Paap F	-	25.369	-	-

Table A1: Security contingencies, positive reconciliations, and bids.

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) Margin	(2) Margin	(3) Margin	(4) Margin
Cartel Announcement	-320.52**	-320.52**	-308.62**	-454.11***
	(125.83)	(130.48)	(130.39)	(116.92)
Cartel Implementation	-146.57***	-146.57**	-146.40**	-145.18***
	(47.66)	(57.51)	(56.76)	(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 A2: 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 × Technology FE or for Date × Region FE. Regions are Atlantic, North-West, Central and South-West. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) LnBid	(2) LnBid	(3) LnBid	(4) LnBid	(5) LnBid	(6) LnBid	(7) LnBid	(8) LnBid
Cartel Announcement	-0.63***	-0.63***	-0.47***	-0.44**	-0.54***	-0.54***	-0.36**	-0.63***
	(0.15)	(0.15)	(0.15)	(0.20)	(0.13)	(0.14)	(0.13)	(0.12)
Cartel Implementation	-0.14	-0.14	-0.00	0.02	-0.18**	-0.18*	-0.03	-0.08
	(0.09)	(0.10)	(0.12)	(0.08)	(0.08)	(0.10)	(0.12)	(0.05)
Announcement	-0.04				-0.01			
	(0.06)				(0.06)			
Implementation	-0.14**				-0.12			
	(0.07)				(0.08)			
Observations	17,155	17,155	16,955	16,955	17,155	17,155	16,955	16,955
R-squared	0.31	0.82	0.84	0.84	0.29	0.82	0.83	0.84
Unit FE	NO	YES	YES	YES	NO	YES	YES	YES
Date FE	NO	YES	N/A	N/A	NO	YES	N/A	N/A
Date x Technology FE	NO	NO	YES	NO	NO	NO	YES	NO
Date x Region FE	NO	NO	NO	YES	NO	NO	NO	YES
Forward Contracts	NO	NO	YES	YES	NO	NO	YES	YES
Sample	Private cartel	Private cartel	Private cartel	Private cartel	Thermal control	Thermal control	Thermal control	Thermal control

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3: DiD estimates with alternative treatment and control groups.

Note: The table presents the estimation results of the differences in differences model using only private units in the cartel group (columns 1 to 4) or only thermal units in the control group (columns 5 to 8).

	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 A4: Correlation 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) LnBid	(2) LnBid	(3) LnBid	(4) LnBid	(5) LnBid	(6) LnBid	(7) LnBid	(8) LnBid	(9) LnBid	(10) LnBid
Cartel Announcement	-0.54***	-0.36**	-0.48***	-0.27*	-0.49***	-0.33**	-0.63***	-0.50***	-0.67***	-0.54***
	(0.14)	(0.13)	(0.12)	(0.14)	(0.14)	(0.13)	(0.13)	(0.14)	(0.14)	(0.14)
Cartel Implementation	-0.18*	-0.03	-0.15	0.09	-0.18*	-0.06	0.03	0.16	0.02	0.12
	(0.10)	(0.12)	(0.11)	(0.21)	(0.10)	(0.12)	(0.13)	(0.13)	(0.15)	(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
*** p<0.01, ** p<0.05, * p<0.1

Table A5: DiD estimates with 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) Pre-reform	(2) Post-reform
Marginal cost	1.065	1.763
	(1.245)	(1.464)
(log) total amount of positive reconciliations (t-1),	51.43	-22.72*
	(77.33)	(10.72)
(log) total ideal generation	20.99	-95.14***
	(93.33)	(27.74)
Observations	2,506	2,534
R-squared	0.859	0.940
Unit FE	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A6: Regression of bids on market fundamentals.

Note: The table presents the estimates of the model in (A1), 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.