Anticipatory Responses and Coordinated Bidding in the Colombian Wholesale Energy Market*

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

Weak institutions and concentrated markets in developing economies can foster informal agreements that limit competition. Profit maximization places few restrictions on firm behavior, making collusion difficult to detect, especially in settings where data are scarce. In the Colombian wholesale electricity market, certain firms reduced prices in a coordinated manner following the *announcement*—but before the *implementation*—of a reform designed to curb coordination. We complement this *announcement* design with a forensic analysis that uncovers a form of coordinated behavior that likely required explicit communication, and estimate overcharges of at least 12%. Policymakers should take dynamic enforcement considerations into account when fighting collusion.

Keywords: Cartels, Anticipatory Behavior, Relational Contracts, Development.
JEL Codes: L41, O12, O13, L14.

1 Introduction

Informal arrangements sustained by the value of future interactions enable parties to cooperate when contracts are unenforceable (Baker et al., 2002; 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).

*We thank Estelle Cantillon, Juan Camilo Castillo, Sylvain Chassang (discussant), Paola Conconi, Francesco Decarolis, Michele Fioretti, Guido Friebel, Bob Gibbons, Jonas Hjort (discussant), Ali Hortascu, Mitsuru Igami, Matti Liski, Juan Ortner, Andrea Pozzi (discussant), Mar Reguant, Tristan Reed, Alvaro Riascos, Ksenia Shakhgildyan, Otto Toivanen, John Van Reenen and conference and seminar audiences at Berkeley ARE, Bocconi, BU, CEPR IMO-ENT, CEPR IO, CEPR Paris Symposium, Chicago, CMA UK, Cornell, Ecares, EIEF, Frankfurt, Helsinki, KU Leuven, LSE, LUISS, NBER Org. Econ, Relational Contracts Workshop, SIOE, Thread, Tilburg, Tor Vergata, and UCL, for many comments and suggestions. Jairo Galvis provided exceptional research assistance. We thank comments and insights from experts in different Colombian Regulatory entities: XM, SSPD and CREG.

†University of Basel, mario.bernasconi@unibas.ch.
‡Bocconi University, CEPR & CESifo, miguel.espinosa@unibocconi.it.
§LSE & CEPR, r.macchiavello@lse.ac.uk.
¶Universitat Rovira I Virgili, carlosandres.suarez@urv.cat.

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 behavior is notoriously difficult to identify (Chassang and Ortner, 2023). In models of collusive behavior, firms deviate from current profit maximization in anticipation of future rewards. Profit maximization, however, places little restrictions on firms' behavior making collusive conduct hard to infer from pricing behavior alone. A key insight of these models, however, is that firms' behavior 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. This "announcement" design leverages a core insight of relational contracting models to identify coordinated behavior under minimal assumptions while enabling a novel, future-reward–based test of collusive conduct.[2]

Following sustained price increases throughout 2008, on January 6^{th}, 2009 (the *announcement* date), the regulators held a meeting discussing, for the first time, measures aimed at making communication between firms difficult to hinder potential price coordination practices. Following the meeting, the regulator hired Peter Cramton to advise on adequate regulatory changes. 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, however—as Cramton himself had previously argued—it can also facilitate the monitoring and punishment of deviations from coordinated arrange-

[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).

[2] Throughout the text, we will often refer to coordinated price setting as "collusion" and to firms that participated in such anti-competitive scheme as the "suspected cartel". These terms are used for expositional simplicity, and with the understanding that our analysis does *not* aim to establish evidence that would conclusively prove a conspiracy to fix prices in court. In fact, the Colombian antitrust law does not use the term "collusion" in a formal definition, and instead prohibits any tacit or explicit coordinated actions between competitors that limit market competition. These terms also reflect our interpretation based on the empirical evidence presented in this study. They do not imply any legal determination or admission of wrongdoing by any entity discussed herein.

ments, tacit or explicit, aimed at coordinating prices. Indeed, in a presentation delivered on January $24^{th}, 2009$ Cramton recommended to increase to 90 days the delay to disclose information to market participants. The recommended changes 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 $25 - 42\%$ immediately after the *announcement*—and well before the actual *implementation*—of the regulatory change. Consistently with the key implications of models of collusion and relational contracts, (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 behavior. Using detailed data on firms' characteristics and possible confounders, we also rule out that the drop in bids is driven by changes in marginal costs, input prices, hydrological conditions, or shocks that affect firms differently based on production technology. We further 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 behavior among cartel members that likely required explicit communication, and we provide suggestive evidence that such communication might have occurred. A quantification exercise provides evidence consistent with our interpretation of the reduced-form results and quantifies the cost of the coordinated conduct.

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 hypotheses 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. We begin with the first approach and then validate it with the second one.

In our baseline definition, we conjecture that the cartel was formed by thermal generation

units in the Atlantic region. Thermal units have higher costs than hydro units and thus are unlikely to profit in the daily auction based on submitted bids. We thus hypothesize, and later confirm, that thermal units profit from colluding on the positive reconciliations market.[3] 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, hereafter referred to as the *cartel*. Using both DID and event-study specifications, we show that the average bid for cartel units falls after the announcement, and before the implementation of the regulatory change.[4] We validate our baseline cartel definition by checking whether cartel units are more likely to experience a structural break in the time series of 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 other units.[5]

This leaves open the question of whether such coordination was tacit or involved explicit communication. Our forensic analysis uncovers 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, which facilitates positive reconciliations. In other words, occasionally, cartel members bid on a particular day "as if" they have information about the behavior of other cartel members that is (in theory) only revealed in the future. This correlation in behavior 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 daily auction based on submitted bids—a behavior that, if done too frequently, attracts the regulator's attention. This anomalous

[3] As it would be clear in Section 2, a positive reconciliation occurs when a generation unit is called upon to produce additional energy beyond what it initially expected to supply.

[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.

[5] 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 cartel belong to the only firm that owns both units in and out of the cartel.

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. Patterns of attendance at the meetings of the National Council of Operations (CNO, in Spanish) also reveal strategic behavior consistent with explicit communication. CNO holds regular in-person meetings in Bogotá 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, cartel units reduced the attendance at meetings of employees from the commercial area (the department in charge of setting bids). Explicit communication about bidding strategies might thus have happened around the meetings.

Finally, we present a quantification exercise. Unlike the previous analysis, this requires committing to a specific model and, inevitably, simplifying assumptions. Our goal is to provide a sanity check that our interpretation of the reduced-form evidence is consistent with the economic mechanisms at play, while developing a full structural model of bidding behavior that incorporates all institutional features is beyond the scope of the paper.[6] 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 maximize static profits in order to detect deviations from profit maximization. We find that before (but not after) the cartel's demise, cartel units could have increased their static profits by bidding significantly lower. Second, we embed these 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

[6]This would be particularly challenging because, 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 need to consider shocks to the transmission network and reconciliations, and the fact that colluding firms may try to devise strategies that reduce the risk of detection.

are not incentive-compatible under the old transparency rule but become so under the new rule. 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. Third, we use the model to quantify that the cartel led to overcharges of at least 12% in the reconciliation market.

This paper contributes to the literature on firms in developing countries and to empirical studies of collusion. There is a general perception that cartels are particularly common in developing countries (World Bank, 2016). Despite a growing interest in drivers and consequences of market power in developing countries (see, e.g., De Loecker and Eeckhout, 2018; Rubens, 2023; Leone et al., 2025), there are few studies of cartels in those contexts. For example, Asker and Nocke (2021)'s review of the literature on collusion cites only two studies from developing countries. On the one hand, developing countries have less robust competition enforcement (Besley et al., 2020) and, therefore, enable fewer studies of *known* cartels. Barkley (2023) study of the Mexican insulin cartel and Chaves and Duarte (2025) study of a hub-and-spoke cartel in the Brazilian automotive fuel industry provide rare examples. On the other hand, a lack of enforcement makes developing countries a potentially fertile ground to study anticompetitive practices less influenced by a threat of punishment, allowing more straightforward tests of the theory, but data limitations have hindered such inquiries. A recent literature has exploited the insight that the pass-through of cost shocks to prices is informative about conduct, conditional on knowledge of the demand's curvature (see, e.g., Atkin and Donaldson, 2015). For example, Bergquist and Dinerstein (2020) combines the pass-through of experimentally induced cost shocks with estimates of demand obtained from randomized subsidies to consumers, and finds that agricultural traders in Kenya act consistently with joint profit maximization. Using pass-through tests, Brugués and De Simone (2024) documents potentially collusive conduct among Colombian lenders.[7]

[7] Breza et al. (2019) documents through a field experiment that workers in rural India implicitly cooperate to prevent downward pressure on wages (see also Banerjee et al. (2022) on anti-competitive behavior supported by social norms among street vendors). Conversely, Sharma (2024) finds that a business association facilitated collusion among garment employers in India using a pass-through test.

We contribute to the literature on firms in developing countries with evidence of coordinated pricing in the wholesale energy market.[8] A reliable and extensive electricity network is crucial for development (Rud, 2012; Allcott et al., 2016), with cheap electricity fostering firms' output and productivity growth (Abeberese, 2017). Furthermore, distortions in the electricity market are particularly detrimental to aggregate welfare (Liu, 2019; Fried and Lagakos, 2023).[9] On the policy front, we provide a rare example of market design "in-action", highlighting how seemingly small changes in market design can lead to substantial improvements in market performance. On the methodology front, our approach relies on a novel design based on "announcement" effects that circumvents the need to estimate demand's curvature, a step that often requires strong assumptions (Miravete et al., 2024). Related to our study, Mercadal (2022) shows that firms' behavior changes following the announcement of a regulatory reform in the US wholesale energy MISO market, but uses a structural approach to assess whether the observed response reflects anticipation of future regulation or adjustments to contemporaneous changes in market conditions. Our contribution is to develop a test for coordinated behavior that instead relies on minimal assumptions. Unlike her market-wide response, we document heterogeneous deviations from competitive behavior, rule out that the effect is driven by changes in current market conditions, identify patterns consistent with explicit communication, and highlight the dynamic enforcement mechanism that makes collusion profitable for some units but not others.

Our primary contribution to the empirical literature on collusion is a novel test, based on a policy announcement design.[10] Chassang and Ortner (2019) study of procurement in Japan is particularly related to our paper. They argue, and find supporting evidence, that dynamic

[8]Several papers also study the Colombian electricity market: Fioretti et al. (2024) focus on the interaction between market power and substitution between fossil fuels and hydropower, Suárez (2022a,b) on the interaction of market power and public ownership, and Camelo et al. (2018), on the transition to centralized commitment.

[9]While the literature on energy markets in advanced economies is vast (see Kellogg and Reguant, 2021 for a review), "rigorous evidence from developing countries on market design is lacking" (Greenstone et al. (2019)) See also World Bank (2019), and Ryan (2021) for a notable exception.

[10]A branch of the literature studies *known* cartels (see, e.g., Porter and Zona, 1993; Asker, 2010; Genesove and Mullin, 2001). Within this branch, Clark and Houde (2014) motivates our analysis of structural breaks while Igami and Sugaya (2021) inspired our calibration of the dynamic enforcement constraint.

enforcement considerations imply that higher minimum prices can lead to lower winning bids by making punishment less effective. We provide evidence of announcement effects that, due to the timing of announcements and richness of the data, must also work through dynamic enforcement constraints. Our design isolates dynamic incentives without having to specify a potentially intractable model of information sharing and conduct. Chassang and Ortner (2023) discusses the processes involved in regulating collusion, including the information required not only to mark collusive behavior as illegal, but even to hear a case and begin an investigation. Formulating robust tests of non-competitive behavior—like Chassang et al. (2022) do—is thus crucial to support regulators fighting collusion. When combined with "investigation shocks", our "announcement" design could be applied in other contexts to help meet the informational hurdle.[11]

The rest of the paper is organized as follows. Section 2 provides background information on the market and institutional context. Section 3 presents the logic of our test for collusion. Section 4 presents the main results. Section 5 provides forensic evidence of coordinated behavior and suggestive evidence of explicit communication. Section 6 presents the quantification exercise. Section 7 discusses policy implications and concluding remarks.

2 The Colombia Electricity Market

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

2.1 Electricity Demand and Generation

During the years 2008 and 2009, 47 generation units produced an average daily ≈ 150 GWh of electricity. Among these units, 32 units were privately managed by 11 firms and accounted

[11] Indirectly, we also contribute to ongoing debates on the role of information transparency rules in facilitating or hindering anti-competitive behavior. Conventional wisdom holds that transparency facilitates collusion (see, e.g., Whinston, 2008; Perloff and Carlton, 1999). Sugaya and Wolitzky (2018) and Kawai et al. (2025), however, argue that transparency can hinder cartels. Instead, our evidence is consistent with the conventional view. Still, it is worth emphasizing that an evaluation of the costs and benefits of transparency in wholesale electricity markets lies beyond the scope of this paper.

for about 70% of output, the remaining 15 units were publicly managed by either regional or 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).[12] 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).

2.2 Colombian Wholesale Electricity Market

Electricity markets are characterized by volatile demand, 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.[13]

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 clears every hour. There are no intra-day balancing markets and the same spot price is paid in all the regions.[14] 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*: how much energy each unit should supply to the system in each hour under optimal conditions of the transmission system. Due to lower costs and high capacity, hydro-power units were the marginal bidder around 3/4 of the time throughout the sample period.

Constraints in either generation or transmission may render the allocation in the ideal

[12]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).

[13]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).

[14]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.

dispatch 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.[15] 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*. This process plays a crucial role in our analysis and is illustrated in Appendix Figure A1. A unit receives a *positive reconciliation* when the real dispatch exceeds 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 unit's bidding price.[16] Eligibility of a unit for a positive reconciliation depends on the availability of production capacity, the location in the transmission network, and the characteristics of the disruption that makes an adjustment to the ideal dispatch necessary. In case two or more units are eligible to be 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 compensated by half the difference between the spot price and its bid. Figure A1's top panel displays the *ideal* dispatch resulting from the bids, alongside positive and negative reconciliations, while the bottom panel depicts the resulting *real* dispatch.

2.3 Change in Transparency Policy

Figure 1 summarizes the timeline of events leading to the policy change.[17] During 2007 and 2008, the average bid in the market increased significantly (Figure A2). The authorities

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

[16] During our sample period, there was no change in the rules used to calculate the cost-based regulated price (see the Appendix A.5 for details).

[17] The ensuing account of events is informed by internal documents and correspondence between the regulators and has been verified through conversations with regulators directly involved in the events.

started being concerned that non-competitive practices might be contributing to the price increase.[18] On December 5^{th}, 2008, the Superintendency of Residential Public Services (SSPD, the agency overseeing regulatory compliance by public utility service providers) summoned several units to its headquarters to discuss high bids and frequent stops in their operations. SSPD warned about a high degree of unilateral exercise of market power.

On January 6th, 2009, a date that we label *announcement date*, the regulators held a meeting at the Ministry of Mines and Energy's headquarters discussing, for the first time, measures aimed at making communication between firms difficult, to hinder potential price coordination practices.[19] Following the meeting, the energy regulator (the Commission for the Regulation of Energy and Gas (CREG)) initiated arrangements to hire Peter Cramton to advise on adequate regulatory changes. Cramton had previously advised the Colombian regulators on the design of the reliability charges in the capacity market, the wholesale natural gas market, and the forward contracts market.[20] Although Cramton had not advised CREG on transparency before, he had argued that full transparency regimes, as the one in force in Colombia's energy market at that time, facilitate price coordination.[21]

On January 24, 2009, Cramton presented his recommendations to the CREG.[22] The presentation argued that the full transparency policy in force at that time should be amended, since it allowed for daily monitoring and punishment of deviations from a tacit collusion agreement. At that time, 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 reforming the transparency policy so that bids would be revealed at $t+90$, 90 days after the

[18] See (SSPD, 2008) and CREG (2009), page 74.

[19] Discussed actions included legally challenging generator meetings and restricting the agendas of technical meetings, with mandatory SSPD presence.

[20] See CREG resolutions 043-2006 and 071-2006, 95-2008, and 044-2007, respectively, and Cramton (2007) and Cramton and Stoft (2007) for analyses.

[21] Specifically, in a Review of ISO New England's Proposed Market Rules, Cramton advised against full transparency, due to the high risk of implicit collusion in the electricity markets (see Cramton and Wilson (1998), pages 21-23).

[22] Before that, on January 20, SSPD conducted unannounced in-situ inspections of the four biggest firms (see Section 4.4 for further details).

auction took place.[23]

On January 30, 2009, CREG followed Cramton's recommendation and approved Resolution 006-2009, with effect from February 6th, 2009 (CREG, 2009) – the *implementation* date. 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 had any reconciliations. The regulation mandated that generating units keep their bidding programs secret from other units. Failure to comply with the disclosure policy would be sanctioned.[24]

From the perspective of our research design, January 6th, 2009, is thus the key *announcement date* not so much because of the appointment of Cramton as a consultant, but because it is when market participants arguably learned that the Ministry and the industry regulator intended to explore options to make communication between firms difficult.[25]

3 A Test for Collusion

Formally characterizing collusive strategies in our repeated-game setting is beyond scope, as uniform-price auctions admit multiple equilibria, market features such as capacity constraints, forward positions, network shocks, and reconciliations further complicate the environment, and colluders may adopt detection-avoidance strategies that cannot be described without strong assumptions. A simple insight common to all repeated-games models, however, is sufficient to derive an intuitive test.

[23]Much of Cramton's presentation pertains to issues not related to transparency (see Appendix Figure A3 for relevant extracts).

[24]Sanctions vary based on the severity of the conduct and may include fines (up to approximately $662,401 USD for the firm and up to approximately $99,360 USD for administrators/officials), cessation of activities, dismissal of executives, prohibition from providing public services, and SSPD takeover.

[25]Indeed, internal communication records we could access reveal that CREG Resolution 006 of 2009 was aligned with the intents discussed in the January 6 meeting.

3.1 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

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

where $\pi(\cdot)$ are expected period profits, $a(\cdot)$ a vector of actions that depend on a vector of state variables σ_{it} – which could include both past, present and future expected realizations of rainfall, prices, demand, ... – and the discount factor δ. $\mathbf{V}$ denotes expected continuation payoffs, and the superscripts c, d, and p 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 few 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 cooperative or collusive behavior are built on a common insight (Macchiavello and Morjaria, 2015): the *future* value of the relationship – the discounted (expected) difference in the payoffs from cooperation and defection $\mathbf{\Delta}_{it'} = \mathbf{V}_{it'}^{c} - \mathbf{V}_{it'}^{\star}$ – deters *current* temptations 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, $\mathbf{\Delta}_{it'}$, 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}.

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 coordinate actions 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 implications

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 breakdown of coordination. The test simply relies on the announcement making at least some firms sufficiently pessimistic about their ability to coordinate conduct in the future. *A fortiori*, the test does not allow us to infer the exact equilibrium (e.g., a subgame perfect one) played by firms, an issue we return to below.

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 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 Coordinated behavior Supported by Future Rewards: *At least some of the units involved in non-competitive, coordinated conduct sustained by a relational arrangement lower their bids after the announcement, and before the implementation, of the regulatory change.*

4 Main Test: Coordinated and Anticipatory Responses

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 alleged arrangement. Yet, a proxy for such participation would allow us to sharpen the empirical test. There are two ways to "assign" units to the suspected cartel. A first approach is to put forward a priori hypotheses 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 units' 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 is better suited for our test since it doesn't rely on changes in behavior around the announcement date to assign units to the suspected cartel.

4.1 First Approach: Suspected Cartel Firms

In this sub-section, we argue that up to the *announcement* date, (at least) some thermal units located in the Atlantic region were coordinating bids to increase profits from positive reconciliations. We focus on thermal units because, during the sample period, they had much higher production costs than hydro units and could not earn significant profits by bidding competitively in the ideal dispatch (Figure A4). Indeed, as noted above, hydro units cleared the ideal dispatch most of the time. Thermal units with high generation costs, however, might be called in for positive reconciliations in the real dispatch. Due to relatively frequent disruptions to the network and declared capacity unavailability, positive reconciliations account for approximately 10% of all the electricity procured by the regulator during our sample period. 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.[26] 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 2). Positive reconciliations mostly occur

[26] Using *security contingencies*—positive reconciliations exogenously awarded to compensate electricity overcharges and recover stability of the transmission system—as an instrument, we show in Appendix A.2 and Table A1 that units submit higher bids when anticipating a higher likelihood of being called in for positive reconciliations.

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 volume 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.[27,28]

This Section provides evidence that (at least some) thermal units in the Atlantic region were coordinating their bids. We start by documenting a sudden decrease in bids right after the announcement date, exploring DID and event-study specifications alike. We then validate our approach using structural break tests in bidding behavior. We conclude by discussing several robustness checks and ruling out potential confounding explanations.

All but one of the 15 units in the Atlantic region are thermal.[29] The baseline definition yields 14 units in the cartel belonging to 5 firms.[30] Of these 5 firms, 4 firms only manage 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 extend our definition to the firm, rather than the unit, level and/or if we exclude publicly managed units. 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 alleged scheme, miss-

[27] Figure 2 reveals a second 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. A placebo analysis discussed below reveals that this cluster didn't feature the same coordinated conduct identified in the Atlantic region nor the same reaction to the *announcement*.

[28] Borenstein et al. (2002) describe a similar situation in the California wholesale electricity market, where physical limitations preventing some generators from supplying electricity created conditions in which the production capacity of some market participants was essential for meeting the system's "imbalance energy" and "ancillary service" needs, regardless of the behavior of other participants.

[29] Furthermore, while the thermal units are clustered together near the coast, the only hydro unit is isolated and closer to the rest of the national network (see Figure 2).

[30] There are 9 privately owned *and* managed units (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), and 5 units that we refer to as public, that are managed by publicly owned GECELCA. Of those, units Guajira 1 and 2 are publicly owned *and* managed, while Barranquilla 3 and 4 and Tebsab are privately owned *but* publicly managed under a long-term power purchase agreement. Table A4 replicates our main result separately for private and public units.

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 before and after the announcement date, split by whether the unit is assigned to the suspected cartel or not. Besides providing descriptive statistics, observed patterns are consistent with intuition. For example, 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 managed (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, and we now subject our hypothesis to more rigorous testing.

4.2 Bidding behavior around the Announcement Date

Figure 3 shows a sharp and large drop in the average bid right after the *announcement* date, but only for thermal units in the Atlantic region. The bid for other units barely moves. We use a difference-in-differences specification to explore the differential change in bidding behavior across the two groups around the time of the reform. Our baseline specification is given by:

$$ln(b_{it}) = \beta_1(Cartel_i \times Announcement_t) + \beta_2(Cartel_i \times Implementation_t) + \lambda_i + \mu_t + \epsilon_{it}, \quad (2)$$

where $ln(b_{it})$ is the (log of the) bidding price of unit i at date t, the dummy variable $Cartel_i$ takes value one if i is a thermal unit in the Atlantic region and zero otherwise. The dummy variable $Announcement_t$ takes value one if t is a date after the announcement date (January 6th, 2009) and zero otherwise, the dummy variable $Implementation_t$ 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. Conditional on these controls, our identifying assumption is that, absent the announcement, the average bids of the suspected cartel units and of the other units would have followed parallel trends over time. Although this assumption cannot be tested directly, Figure 3 suggests that it is plausible, at least in the pre-announcement period. The event-study specification discussed below further supports this interpretation.

In our baseline specification, date fixed effects ensure that the results are not driven by changes in demand, input prices, or hydrological conditions that affect all units uniformly. However, changes in market conditions may affect units inside and outside the cartel differently. In that case, our baseline specification would not allow us to rule out that the observed bid adjustments reflects cost changes rather than a shift in strategic behavior induced by the announcement. To address this concern, we examine a range of alternative specifications that explicitly control for these potential confounding factors. First, we directly control for marginal costs and for the ratio of forward contracts to production capacity, both of which vary across units and time. We also include a vector of input controls interacted with fuel-type indicators: coal prices interacted with fuel type, gas prices interacted with fuel type, and water inflows and reservoir levels, each interacted with fuel type. Fuel indicators distinguish hydro, gas, and coal units. Second, we assess robustness to the inclusion of Date × Fuel and Date × Secondary Fuel fixed effects, where Fuel takes three values (hydro, gas, coal) and Secondary Fuel takes five values for thermal units (oil, coal, ACPM, Jet 1, or none). Third, we include Date × Cycle fixed effects, where Cycle (for thermal units) can be simple, combined, or STIG. Fourth, we examine whether location-specific shocks drive our results by including Date × Region fixed effects, where regions are defined as Atlantic, North-West, Central, and South-West. In all specifications, we cluster standard errors by date and unit and we estimate the regression equation using data from August 2008 to July 2009.

Table 2 reports the results. Columns 1 and 2 show a decrease in bidding prices for cartel

units after the policy announcement of around 42% ($1 - e^{-0.54}$), both controlling and not for unit and date fixed effects. Column 3 additionally controls for marginal cost, forward contracts, and a vector of Input levels x Fuel dummies. The coefficient of interest declines slightly in magnitude but remains economically significant, corresponding to a reduction in bids of about 38%. In column 4, we further include Date × Fuel and Date × Secondary Fuel fixed effects. The estimated effect remains sizable, with bids decreasing by roughly 25%. Column 5 and 6 show that results do not change if we alternatively control for Date × Cycle fixed effects or Date × Region fixed effects. Columns 7 to 12 also report estimates when using bids in levels rather than logs as the dependent variable, yielding qualitatively similar results. Overall, across a wide range of specifications, we consistently find a large decline in bids that cannot be explained by changes in marginal costs, input prices, water reserves or intake, or differential shocks affecting units by fuel type, thermodynamic cycle, or region.[31]

Figure 4 presents estimates and dynamic patterns 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. The figure reports the estimates for 18 leads and 18 lags, covering the period from the beginning of August 2008 to the beginning of May 2009. First, the result rules out differences in pre-trends in bidding behavior between units assigned and not assigned to the alleged cartel, in line with our main identification assumption. Second, the differential drop in bids right after the announcement remains persistent throughout the rest of the sample period: market transparency did not further change bidding behavior differently between units in the cartel and units outside. This is consistent with the idea that, by that point, coordinated bidding between thermal units in the Atlantic region had ceased, and their bidding

[31] The results in Table 2 show that the differential drop in bids is not explained by a differential change in forward positions or production costs for cartel units. Figure A6 further 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 A7 shows an abrupt fall in the margin ($Bid - Mg.Cost$) for cartel units but not for other thermal units. Using margins instead of bids provides qualitatively similar results in the DID analysis (see Table A2). A placebo exercise that uses observations one year after the announcement also rules out that our finding is driven by seasonal patterns.

was undistinguishable from that of other thermal units.[32]

If thermal units in the Atlantic region ceased to coordinate bids immediately after the announcement, their profits—particularly those earned from positive reconciliations—should decrease. Table 3 shows that, as expected, the likelihood of receiving positive reconciliations was unaffected, while profits from positive reconciliations (and also total profits) sharply decreased for thermal units in the Atlantic region after the announcement date. Furthermore, Figure A10 shows that profits fell for *all* those units: transfers might thus *not* have been needed to sustain coordination in this case, as all units benefited from it. This includes publicly managed units in the suspected cartel group, who (on average) also lowered bids after the announcement (see Table A4), and thus were possibly part of the collusive agreement.[33]

4.3 Second Approach – Validation: Structural Break Test

We now turn to a second approach and validate the baseline definition by checking whether thermal units in the Atlantic region 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:

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

[32]Figure A8 replicates the event study looking at margins for thermal units, and finds identical results. Figure A9 includes a (placebo) event study for the isolated cluster in the South-West. The estimates suggest that this cluster didn't change bidding behavior 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 thermal units both in and out of the Atlantic region. 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) management, forward contract positions, and bidding behavior in 2008 (see Appendix A.3.2). Finally, Table A3 shows that results are robust to the exclusion of hydro units from the sample.

[33]Two considerations help in interpreting this result. First, Suárez (2022a) documents that public units also respond to market power incentives in the Colombian market, albeit less than private ones. Second, some of these publicly managed units were privately owned, and potentially responsive to different incentives. Units Barranquilla 3, Barranquilla 4, and TEBSAB were privately owned, but operated by the public firm GECELCA under a power purchase agreement. Interestingly, these are the only three publicly managed units for which we identify a structural break (see Section 4.3).

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, for each unit, the largest F-statistic to determine when the potential structural break occurred. We then only retain structural breaks with an associated p-value below 0.01, but results are similar if we consider a threshold of 0.001, 0.05 or 0.10.

Table 4 tabulates the result from the QLR test. The test detects a significant structural break between the announcement and the implementation dates for 11 out of the 14 (i.e., 79%) thermal units in the Atlantic region, with all the breaks corresponding to a decrease in bids. Of the 3 units for which the test doesn't detect a structural break, two are publicly managed. 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 remaining 33 units (i.e., 6%). The break for these two units also correspond to a decrease in bids. Interestingly, these are the thermal units owned by EMGESA, the only firm that owns units both in and out of the suspected cartel, according to our baseline definition. Regressing a dummy for having a significant break between the announcement and the implementation dates on a dummy for thermal units in the Atlantic region yields a statistically significant difference ($p < 0.01$).

Figure 5 reports the percentage of units for which the test detects a significant structural break corresponding to a decrease in bids at different points in time for the suspected cartel group and for other units. The figure reveals that the share of units with a structural break is comparable (and not significantly different) across groups before the announcement, but suddenly spikes afterward for the suspected cartel group. In particular, the breaks for Atlantic thermal units are concentrated on January 10th, January 22nd, and February 1st.

This exercise suggests again, consistent with the aforementioned results in Figure A10,

that the drop in average bids for cartel units is not driven by the response of a few units. Instead, and consistent with coordination, most of the Atlantic thermal units significantly altered their behavior following the announcement. This can also be seen in Figure A5, where we further present the time series of bids for each suspected cartel unit separately.

4.4 Threat(s) of Enforcement

The sudden and coordinated decline in bids immediately after the *announcement* date is thus consistent with a shock to members' perceptions about their future ability to coordinate. As noted above, it is not essential for the logic of the test that the units had anticipated the exact 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 behavior. 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 6 extends the event-study specification in Figure 4, adding the interactions between dummies for leads and lags relative to the *inspection* date for inspected firms. Two patterns emerge. First, the results for Atlantic thermal units are virtually unchanged. Furthermore, the bulk of the differential drop in bids for suspected cartel units happens *before* the inspection date. Second, after the inspection, inspected firms do not significantly change 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 en-

forcement. Once coordination had ceased, 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 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. This surveillance action included three thermal units located in the Atlantic region.

We replicate the event study, including an event interaction for this surveillance action, split between cartel and non-cartel units. Figure 7 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 not have perceived enforcement to be a significant threat.

4.5 Interpretation of the results

We have shown that thermal units in the Atlantic region decreased bids after the announcement but before the implementation of the transparency reform. Furthermore, we have shown that this reaction was unlikely to stem from an anticipated threat of enforcement, and also unlikely to be caused by changes in input prices or marginal costs. It thus appears plausible that bidding behavior changed in anticipation of a reform that would have made coordination harder to sustain.

The evidence from our test does not identify the exact equilibrium played. In particular, the evidence does not prove that *all* units correctly anticipated that, under the new transparency rules, the dynamic enforcement constraints would be violated. Still, our evidence reveals that at least some units became sufficiently pessimistic about the possibility to sustain the collusive behavior in the future, suggesting that dynamic enforcement considerations

can be taken seriously in the fight against anti-competitive conduct.

In fact, even when cartel members can explicitly communicate, successful collusion requires a mutual understanding of many elements of the agreement. It is thus implausible that 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, some units reacted in an *adaptive* way to the unexpected behavior of those units that lowered bids first (see Figure 5 and A5). Interestingly, the first units that decreased prices belong to EMGESA, the largest firm among the collusive ones. This is consistent with evidence that larger firms are more sophisticated bidders (Hortacsu et al., 2019) and tend to act as leaders to coordinate asymmetric cartels (Byrne and De Roos, 2019; Clark et al., 2024). We find no other characteristic that correlates with the ordering in which units decreased their bids.[34]

5 Further Evidence of Coordinated behavior

This section provides suggestive evidence that suspected cartel units may have coordinated actions in a way that would have been difficult to achieve without communication, and some suggestive evidence that such communication might have occurred. These exercises and are not meant to establish hard evidence of explicit communication, but they nevertheless provide a further lens to interpret the evidence uncovered in the previous section.

5.1 An Example of Coordinated Behavior

Our forensic analysis reveals a particular type of coordination among suspected cartel members that would seem hard to achieve without communication. In particular, we show that suspected cartel members tend to increase bids in the day-ahead auction precisely when other cartel units win in the day-ahead auction but then declare unavailable immediately

[34]We are unaware of empirical analyses that distinguish between anticipatory and adaptive reactions 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 behavior (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).

prior to the real operation. In other words, occasionally cartel members bid on day t "as if" they have information about the behavior of other cartel members that is – in theory – only revealed on day $t+1$. This pattern holds only among cartel units and vanishes after the reform. We do not argue that this particular behavior played a quantitatively important role in generating extra profits. On the contrary, this coordination must be quite rare as it requires units to declare unavailability after winning in the day-ahead auction but before the real operation of the market – a behavior that, if done too frequently, attracts the regulator's attention. This pattern, however, lends further support to the hypothesis that thermal units in the Atlantic region coordinated behavior before, but not after, the announcement date.

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 A11 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 behavior more rigorously, we would ideally know transmission network restrictions that make it more likely that a given unit j receives a positive reconciliation when unit i declares unavailable. This would allow us to test whether unit j increases bids precisely when unit i wins in the ideal dispatch but then declares unavailable. We proxy for these relationships between units relying on observed behavior. For each unit i we identify its "friends", i.e., units that are more likely to get a positive reconciliation when unit i has a negative reconciliation. For each unit i, we rank all the other units by the probability of receiving positive reconciliations when unit i has a negative reconciliation. In our baseline definition, the "friend" of each unit i is the first unit according to this ranking (but also present robustness checks in the Appendix).

We regress the average bid of i's friend(s) (submitted in t for day $t+1$) on a dummy

indicating whether unit i's production availability (in $t+1$) is below the ideal generation quantity awarded. We repeat the estimation separately for different years, and separately for i in the suspected cartel or not. A striking pattern emerges. Figure 8 shows that thermal units in the Atlantic region coordinated higher bids with declared unavailabilities of their "friends" before the reform. Interestingly, this coordination ceased after the reform, and is never detected for units that are not suspected cartel members.[35] 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 does not feature this type of coordinated behavior throughout the entire sample period (Figure A12).[36]

5.2 Suggestive Evidence of Communication

Suspected cartel members appear to have coordinated some actions in a way that seems difficult without explicit communication.[37] We now provide further evidence that is at least consistent with the hypothesis that explicit communication might have occurred.

We hand-collect data from the minutes of all the meetings of the National Council of Operations in the year around the policy change, i.e., the second semester of 2008 and the first semester of 2009. This council (CNO in Spanish, see Appendix A.1 for details) holds meetings to solve technical difficulties and constraints to the system. Only technical engineers, therefore, were supposed to attend the meetings, while personnel involved in setting bids should not attend. However, as we shall see, that was not the case in practice. The minutes report the names of attendees and the *firm* they work for. Within a DID

[35] Unfortunately, information on availability in 2009 (i.e., the post period) is missing for 63% of observations involving suspected cartel members, and only for 6% of observations for the other units. While this warrants caution in interpreting the "difference-in-differences", the distinct pattern between suspected cartel members and other units gradually emerges from 2005 through 2008, when the data is complete.

[36] We also test whether coordination occurs between units belonging to the same firm but find no evidence for it. Figure A12 shows that results are robust to alternative ways to construct the set of "friends" and the explanatory variable.

[37] Of course, we do not pretend to have uncovered all the types of coordination understaken by the cartel, and there may be other type of coordination that we haven't detected.

framework, we test whether there was any differential change in attendance between the cartel and non-cartel firms before and after the policy change.[38]

Since firms typically send only one attendee per meeting (if any at all), we focus on two dependent variables: a dummy that equals one if the firm sends someone to the meeting, and a dummy that equals one 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.[39] 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, 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 behavior around the timing of the CNO meetings, the evidence points to strategic behavior in attendance. Similar evidence could presumably be used to evaluate the possibility of prosecution in other cases.

[38] 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, in the cartel group.

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

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

This section presents a quantification exercise with the aim of providing a sanity check for our interpretation of the empirical evidence presented above. Unlike our previous analysis, this requires committing to a specific model and, inevitably, simplifying assumptions. Developing a full structural model of bidding behavior in the Colombian market is beyond the scope of this paper, and that would be particularly challenging given capacity constraints, participation in forward markets, the existence of multiple pure-strategy equilibria (Fabra et al., 2006), exogenous shocks to the transmission network, and flexible collusive conduct which possibly entails communication between (some) firms. Instead, we focus on thermal units and the reconciliations market and estimate best responses and profits. In particular, we exploit detailed information on both units' production costs and residual demands following standard practices in wholesale electricity markets empirical studies (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 (static) profits by submitting lower bids compared to the observed ones. Second, we confirm that such deviations were likely not incentive-compatible before the policy change, but could have been profitable after the reform. Finally, we quantify the cost of the observed anti-competitive conduct for consumers.

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 when competing (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.

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 $b_{-i,t}$, $q^{+}_{i,t}$ 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 units' eligibility. Because demand is inelastic, $q^{+}_{i,t}(b_{i,t}; b_{-i,t})$ is observed up to the idiosyncratic component.

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

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

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

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

using logs to accommodate the tail in the distribution (see Figure A13).

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.5 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 unilateral deviations.

6.2 Short-run Deviations

We hypothesize that suspected 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 suspected cartel, however, should not be able to do so. The comparison group excludes publicly managed 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). Still, results are robust to the inclusion of all non-cartel thermal units.

We focus on a one-year period around the policy change and estimate Equations (4) and (5) separately for suspected cartel units and the comparison group to reflect the geographic segmentation of the reconciliations market.[40] 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.

Figure 9 presents the results. Both before and after the reform, the distribution for the other units is unimodal with most mass around a ratio equal to one.[41] For suspected 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

[40] Figure A13 shows that the model fits well the distribution of $q^*_{i,t}$ in both groups.

[41] 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.

Kolmogorov-Smirnov test for the equality of the distributions for suspected cartel and other units rejects the null hypothesis pre-reform (p-value = 0.00), but not post-reform (p-value = 0.62). In sum, suspected cartel units systematically deviate from profit maximization before, but not after, the reform.

6.3 Dynamic Enforcement Constraints

Evaluating the costs and benefits of transparency in wholesale electricity markets is beyond the scope of this paper. In particular, the evidence presented above doesn't imply that coordination unraveled because firms correctly anticipated that it would no longer be sustainable due to the new transparency rule. However, it is instructive to ask whether observed behavior was consistent with dynamic enforcement constraints being satisfied before the reform, but potentially not afterward.

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.[42] 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 under the old transparency regime, but not under the new one, if:

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

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

where δ is the discount factor, and the superscripts C, D, and $*$ indicate collusive bids, the

[42] The transparency policy not only affects information regarding bids but also about the reconciliations. Therefore, a unit that is not awarded any positive reconciliation cannot necessarily infer deviations of other cartel members, because they do not know if there was some positive reconciliation to be awarded. The assumption that a unit can deviate unnoticed for 90 days is thus not implausible. Nevertheless, we also repeat the exercise assuming that units learn about deviations before the full 90-day period.

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). 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.[43]

The left panel of Figure 10 presents the results. The Figure reports the *smallest* incentive

[43]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.

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 present bounds to our estimate when assuming discount factors equivalent to 10% or 20% interest rates. Our estimates imply that all suspected cartel units were better off colluding under the pre-reform rules (purple line), i.e. the short-run deviations discussed in Section 6.2 were not incentive compatible. This is no longer true under the post-reform regime (green line). Our results 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.[44] While this exercise is not meant to prove that the change in transparency rules led to the cartel's demise, the results (coupled with those in Section 6.2) are consistent with the idea that suspected cartel units were often departing from competitive behavior, and that such departures were incentive compatible before, but not after, the transparency reform.

6.4 Cost of the Cartel

We use the (counterfactual) estimates of bids and quantities described above to conduct a back-of-the-envelope calculation of the additional costs generated by the coordinated behavior observed in the data up to January 6, 2009. Of course, these estimates should be interpreted cautiously given the simplifying assumptions underlying our model; nevertheless, they provide an indication of the welfare losses associated with anti-competitive behavior.

We focus on the second half of 2008 and compare the cost of positive reconciliations under collusion and competition, that is $\sum_i q^+_{i,t}(b^C_{i,t}, b^C_{-i,t}) \times b^C_{i,t} - \sum_i q^+_{i,t}(b^*_{i,t}, b^*_{-i,t}) \times b^*_{i,t}$ given

[44] Figure A14 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.

that $\sum_i q^+_{i,t}(b^C_{i,t}, b^C_{-i,t}) = \sum_i q^+_{i,t}(b^*_{i,t}, b^*_{-i,t}) = Q^+_t$. First, we assume that the total amount of positive reconciliations didn't change once coordinated conduct ceased. This appears to be in line with the available evidence. Second, we abstract from the possibility that the cartel's demise also increased competition in the ideal dispatch. While this might have been the case, hydro units clear the market most of the time, while thermal units are unlikely to earn profits in the ideal dispatch (Figure A4). Accordingly, we do not expect the cartel to have influenced bids in the ideal dispatch.[45] Finally, our calculation does not account for the downstream adverse effects of higher electricity prices, e.g, on firms' output and productivity growth (Abeberese, 2017), and our estimate should thus be interpreted as a lower bound.

The cartel generated an additional cost for positive reconciliations 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 10), 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%.

7 Implications and Conclusions

This paper identified coordinated, anti-competitive, conduct among a subset of firms in the Colombian wholesale energy market. Our test uncovers an anticipatory and coordinated response, which suggests that dynamic incentive compatibility constraints can be taken seriously by empirical researchers, firms and policy-makers fighting collusion.

Our analysis has implications for market design in settings where informal cooperation may arise, including energy markets. The Colombian case is especially informative: despite being widely regarded as one of the best-designed and best-regulated electricity markets in

[45] Indeed, we find no evidence of a structural break in the spot price time series following the announcement date, nor any change in the probability that cartel units are selected for production in the ideal dispatch relative to other units.

the developing world (World Bank, 2019), we uncover clear evidence of coordinated behavior supported by the value of future interactions. This suggests that collusive arrangements may be even more prevalent—and more distortionary—in less well-designed markets. For policy makers, the results highlight that even sophisticated market architectures remain vulnerable to informal cooperation when firms interact repeatedly and face strong incentives to sustain above-competitive outcomes.

The Colombian regulator, lacking direct evidence of wrongdoing, opted to modify the market design to hinder potential collusion. Such changes, however, can be costly: in this market, transparency facilitates the efficient intertemporal allocation of scarce water resources. The fact that suspected cartel members reacted immediately after a regulatory *announcement*, before any formal rule change, suggests that regulators may be able to use strategically timed announcements to induce behavioral responses and obtain sufficient evidence to justify targeted investigations. While predicting which announcements shift future values is challenging, the applicability of our test expands significantly when combined with "investigation shocks," such as the launch of market studies by authorities with such powers (e.g., the UK's CMA). These investigative signals could push insiders to alter their behavior—including by entering leniency programs—thereby generating evidence for prosecution. Exploring such strategies, is a promising direction for future research.

Our results also speak to the broader question of how transparency shapes firms' conduct. In Colombia, public disclosure of bidding information may have inadvertently facilitated coordination by allowing members to monitor each other's actions and punish deviations. Our results are consistent with the interpretation that limiting transparency ultimately reduced firms' ability to sustain collusion. This has broader relevance for markets such as public procurement, e-commerce, and agricultural trading, where digital technologies dramatically increase visibility and reduce search costs (Bai et al., 2020; Baldwin et al., 2021; Bergquist et al., 2024). While transparency can intensify competition, our findings provide a note of caution: it may also enable collusive monitoring. Firms operating in digital or plat-

form markets should be mindful that tools designed to improve efficiency or traceability can unintentionally strengthen informal agreements among competitors.

References

Abeberese, A. B. (2017). Electricity cost and firm performance: Evidence from india. *The Review of Economics and Statistics*, 99(5):839–852.

Allcott, H., Collard-Wexler, A., and O'Connell, S. D. (2016). How do electricity shortages affect industry? Evidence from India. *American Economic Review*, 106(3):587–624.

Asker, J. (2010). A Study of the Internal Organization of a Bidding Cartel. *American Economic Review*, 100(3):724–62.

Asker, J. and Nocke, V. (2021). Collusion, Mergers, and Related Antitrust Issues. In Ho, K., Hortacsu, A., and Lizzeri, A., editors, *Handbook of Industrial Organization*, volume 5 of *Handbook of Industrial Organization*, pages 177–279. Elsevier.

Atkin, D. and Donaldson, D. (2015). *Who's Getting Globalized?: The Size and Implications of Intra-national Trade Costs*. Number w21439. National Bureau of Economic Research Cambridge, MA.

Bai, J., Chen, M., Liu, J., Mu, X., and Xu, D. Y. (2020). Search and information frictions on global e-commerce platforms: Evidence from aliexpress. Technical report, National Bureau of Economic Research.

Baker, G., Gibbons, R., and Murphy, K. J. (2002). Relational Contracts and the Theory of the Firm. *The Quarterly Journal of Economics*, 117(1):39–84.

Baldwin, R. E., Chiarotti, E., and Taglioni, D. (2021). *Trading Through Platforms: Evidence from AliExpress*. Graduate Institute of International and Development Studies.

Banerjee, A., Fischer, G., Karlan, D., Lowe, M., and Roth, B. N. (2022). Do collusive norms maximize profits? evidence from a vegetable market experiment in india. Technical report, National Bureau of Economic Research.

Barkley, A. (2023). The Human Cost of Collusion: Health Effects of a Mexican Insulin

Cartel. *Journal of the European Economic Association*, 21(5):1865–1904.

Barros, F. and Modesto, L. (1999). Portuguese Banking Sector: a Mixed Oligopoly? *International Journal of Industrial Organization*, 17(6):869–886.

Bergquist, L. F. and Dinerstein, M. (2020). Competition and entry in agricultural markets: Experimental evidence from Kenya. *American Economic Review*, 110(12):3705–47.

Bergquist, L. F., McIntosh, C., and Startz, M. (2024). Search costs, intermediation, and trade: Experimental evidence from ugandan agricultural markets. Working Paper 33221, National Bureau of Economic Research.

Besley, T., Fontana, N., and Limodio, N. (2020). Antitrust Policies and Profitability in Non-Tradable Sectors. *American Economic Review: Insights*, 3(2):251–65.

Bigoni, M., Potters, J., and Spagnolo, G. (2019). Frequency of interaction, communication and collusion: an experiment. *Economic Theory*, 68:827–844.

Borenstein, S., Bushnell, J. B., and Wolak, F. A. (2002). Measuring market inefficiencies in california's restructured wholesale electricity market. *American Economic Review*, 92(5):1376–1405.

Breza, E., Kaur, S., and Krishnaswamy, N. (2019). *Coordination without organization: Collective labor supply in decentralized spot markets*. National Bureau of Economic Research.

Brugués, F. and De Simone, R. (2024). Taxation when markets are not competitive: Evidence from a loan tax. Technical report, IDB Working Paper Series.

Byrne, D. P. and De Roos, N. (2019). Learning to coordinate: A study in retail gasoline. *American Economic Review*, 109(2):591–619.

Camelo, S., Papavasiliou, A., de Castro, L., Riascos, Á., and Oren, S. (2018). A Structural Model to Evaluate the Transition from Self-commitment to Centralized Unit Commitment. *Energy Economics*, 75:560–572.

Chassang, S., Kawai, K., Nakabayashi, J., and Ortner, J. (2022). Robust Screens for Noncompetitive Bidding in Procurement Auctions. *Econometrica*, 90(1):315–346.

Chassang, S. and Ortner, J. (2019). Collusion in Auctions with Constrained Bids: Theory

and Evidence from Public Procurement. *Journal of Political Economy*, 127(5):2269–2300.

Chassang, S. and Ortner, J. (2023). Regulating Collusion. *Annual Review of Economics*, 15:177–204.

Chaves, D. and Duarte, M. (2025). The inner workings of a hub-and-spoke cartel in the automotive fuel industry. *American Economic Journal: Microeconomics*, 17(1):41–65.

Clark, R., Horstmann, I., and Houde, J.-F. (2024). Hub-and-spoke cartels: Theory and evidence from the grocery industry. *American Economic Review*, 114(3):783–814.

Clark, R. and Houde, J.-F. (2014). The effect of explicit communication on pricing: Evidence from the collapse of a gasoline cartel. *The Journal of Industrial Economics*, 62(2):191–228.

Connor, J. M. (2020). Private International Cartels Full Data 2019 edition. Purdue University Research Repository.

Connor, J. M. and Bolotova, Y. (2006). Cartel overcharges: Survey and meta-analysis. *International Journal of Industrial Organization*, 24(6):1109–1137.

Cramton, P. (2007). Colombia's forward energy market. *Report for Colombia's Comisión de Regulación de Energía y Gas (CREG)*.

Cramton, P. and Stoft, S. (2007). Colombia firm energy market. *40th Annual Hawaii International Conference on System Sciences (HICSS'07)*, 20:124–124.

Cramton, P. and Wilson, R. (1998). A Review of ISO New England's Proposed Market Rules. Technical report, Market Design Inc.

Crawford, G. S., Crespo, J., and Tauchen, H. (2007). Bidding asymmetries in multi-unit auctions: implications of bid function equilibria in the British spot market for electricity. *International Journal of Industrial Organization*, 25(6):1233–1268.

CREG (2009). Manejo de la Información en el Mercado Mayorista. Technical report, Comisión de Regulación de Energía y Gas, Número 005.

Dal Bo, P. and Frechette, G. R. (2018). On the determinants of cooperation in infinitely repeated games: A survey. *Journal of Economic Literature*, 56(1):60–114.

De Loecker, J. and Eeckhout, J. (2018). Global market power. Technical report, National

Bureau of Economic Research.

Djankov, S., La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2002). The regulation of entry. *The Quarterly Journal of Economics*, 117(1):1–37.

Fabra, N. and Toro, J. (2005). Price wars and collusion in the Spanish electricity market. *International Journal of Industrial Organization*, 23(3-4):155–181.

Fabra, N., von der Fehr, N.-H., and Harbord, D. (2006). Designing electricity auctions. *The RAND Journal of Economics*, 37(1):23–46.

Fioretti, M., He, J., and Tamayo, J. (2024). Saving for a Dry Day: Coal, Dams and the Energy Transition. Working paper.

Fried, S. and Lagakos, D. (2023). Electricity and firm productivity: A general-equilibrium approach. *American Economic Journal: Macroeconomics*, 15(4):67–103.

Genesove, D. and Mullin, W. P. (2001). Rules, communication, and collusion: Narrative evidence from the sugar institute case. *American Economic Review*, 91(3):379–398.

Greenstone, M., Reguant, M., Ryan, N., and Dobermann, T. (2019). Evidence paper.

Hortacsu, A., Luco, F., Puller, S. L., and Zhu, D. (2019). Does strategic ability affect efficiency? evidence from electricity markets. *American Economic Review*, 109(12):4302–42.

Hortacsu, A. and Puller, S. L. (2008). Understanding strategic bidding in multi-unit auctions: a case study of the Texas electricity spot market. *The RAND Journal of Economics*, 39(1):86–114.

Igami, M. and Sugaya, T. (2021). Measuring the Incentive to Collude: The Vitamin Cartels, 1990–99. *The Review of Economic Studies*, 89(3):1460–1494.

Kawai, K., Nakabayashi, J., and Ortner, J. (2025). The value of privacy in cartels: an analysis of the inner workings of a bidding ring. *Review of Economic Studies*, page rdaf023.

Kellogg, R. and Reguant, M. (2021). Energy and environmental markets, industrial organization, and regulation. In Ho, K., Hortaçsu, A., and Lizzeri, A., editors, *Handbook of Industrial Organization, Volume 5*, number 1, pages 615–742. Elsevier.

Leone, F., Macchiavello, R., and Reed, T. (2022). Market size, markups and international price dispersion in the cement industry. Technical report, Centre for Economic Performance.

Leone, F., Macchiavello, R., and Reed, T. (2025). The high and falling price of cement in africa. *American Economic Journal: Applied Economics*, 17(2):1–40.

Liu, E. (2019). Industrial policies in production networks. *The Quarterly Journal of Economics*, 134(4):1883–1948.

Macchiavello, R. (2022). Relational Contracts and Development. *Annual Review of Economics*, 14:337–362.

Macchiavello, R. and Morjaria, A. (2015). The value of relationships: evidence from a supply shock to Kenyan rose exports. *American Economic Review*, 105(9):2911–45.

Mercadal, I. (2022). Dynamic competition and arbitrage in electricity markets: The role of financial players. *American Economic Journal: Microeconomics*, 14(3):665–99.

Miravete, E. J., Seim, K., and Thurk, J. (2024). Pass-through and substitution of discrete choice demand. Technical report.

Mirenda, L., Mocetti, S., and Rizzica, L. (2022). The economic effects of mafia: firm level evidence. *American Economic Review*, 112(8):2748–73.

Ortner, J. M., Chassang, S., Kawai, K., and Nakabayashi, J. (2022). Screening adaptive cartels. Technical report, National Bureau of Economic Research.

Perloff, J. M. and Carlton, D. W. (1999). *Modern industrial organization*. Addison-Wesley: Massachusetts.

Porter, R. and Zona, J. (1993). Detection of Bid Rigging in Procurement Auctions. *Journal of Political Economy*, 101(3):518–38.

Rubens, M. (2023). Market structure, oligopsony power, and productivity. *American Economic Review*, 113(9):2382–2410.

Rud, J. P. (2012). Electricity provision and industrial development: Evidence from India. *Journal of Development Economics*, 97(2):352–367.

Ryan, N. (2021). The competitive effects of transmission infrastructure in the indian electricity market. *American Economic Journal: Microeconomics*, 13(2):202–42.

Sharma, G. (2024). Collusion among employers in india. Technical report, Working Paper.

SSPD (2008). Actos relevantes del mercado de energía mayorista. Technical report, Superintendencia de Servicios Públicos, Número 34.

Suárez, C. (2022a). Mixed Oligopoly and Market Power Mitigation: Evidence from the Colombian Wholesale Electricity Market. *Journal of Industrial Economics*.

Suárez, C. (2022b). Private management and strategic bidding behavior in electricity markets: Evidence from Colombia. *Energy Economics*.

Sugaya, T. and Wolitzky, A. (2018). Maintaining privacy in cartels. *Journal of Political Economy*, 126(6):2569–2607.

Whinston, M. D. (2008). Lectures on antitrust economics. *MIT Press Books*, 1.

Wolak, F. (2007). Quantifying the Supply-side Benefits from Forward Contracting in Wholesale Electricity Markets. *Journal of Applied Econometrics*, 22:1179–1209.

World Bank (2016). Breaking Down Barriers: Unlocking Africa's Potential through Vigorous Competition Policy. Technical report, World Bank.

World Bank (2019). *Rethinking power sector reform in the developing world*. World Bank Publications.

8 Figures

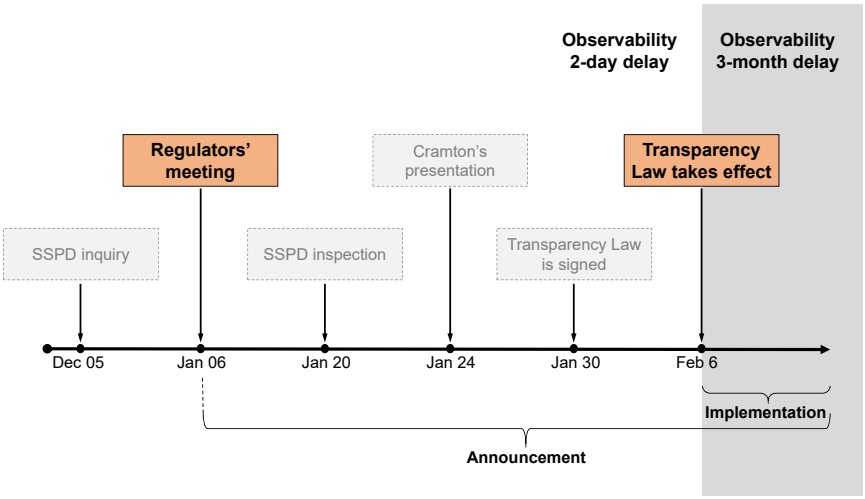

Figure 1: Timeline.

Note: January 6 and February 6 are referred to throughout the paper as the 'announcement' and the 'implementation' dates, respectively, and correspond to the two main exogenous shocks in our analysis. The SSPD initiatives on December 5 and January 20 are discussed in Section 4.4 to explore the role of enforcement threats.

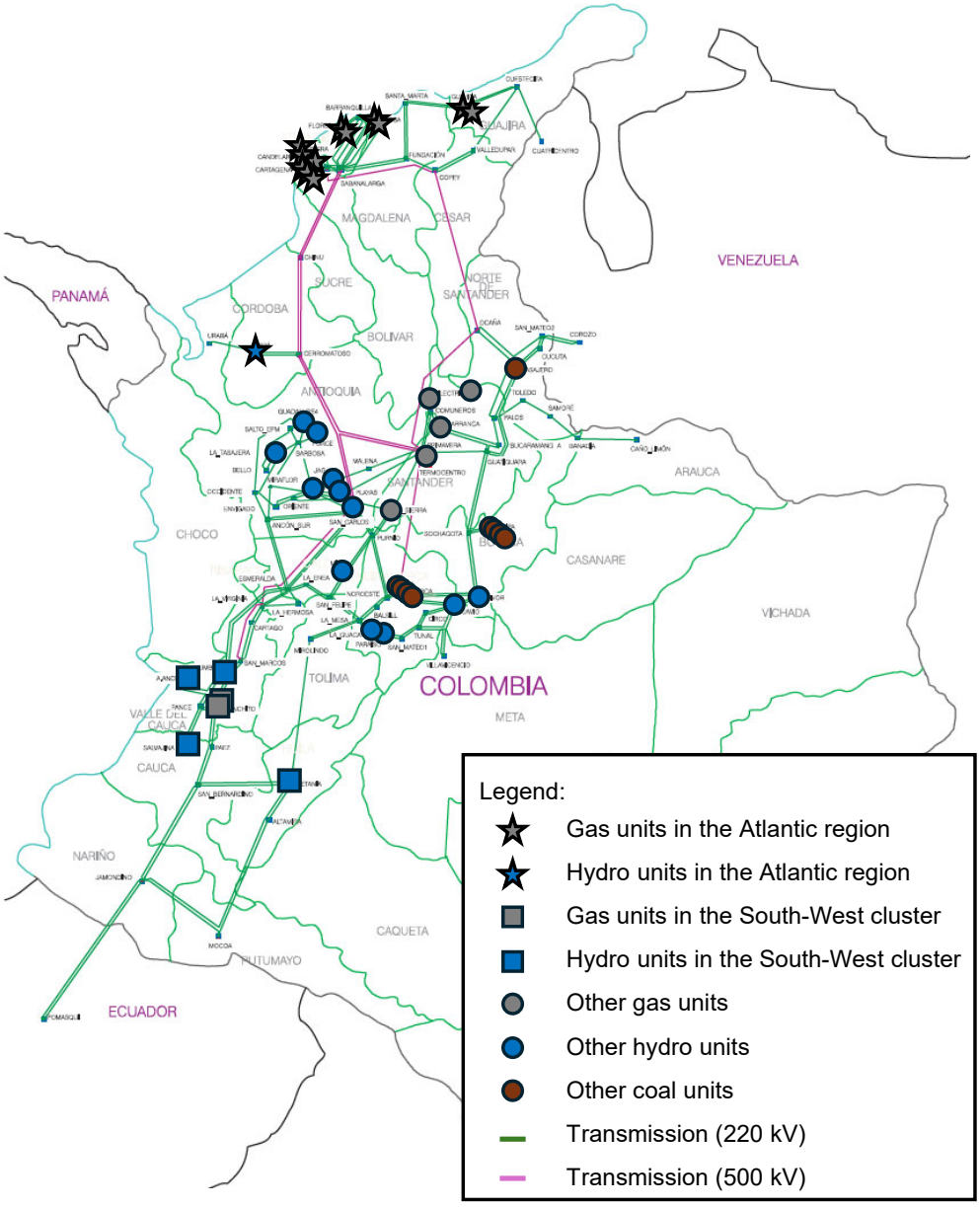

Figure 2: Map.

Note: The figure presents the map of Colombia, the location of the electricity generation units participating in the wholesale electricity market in 2008/2009, and the transmission network in 2008. The map also shows the division of the country in political units called "departamentos" with green lines. The map is provided by the Unidad de Planeación Minero Energética, and units are classified by the authors.

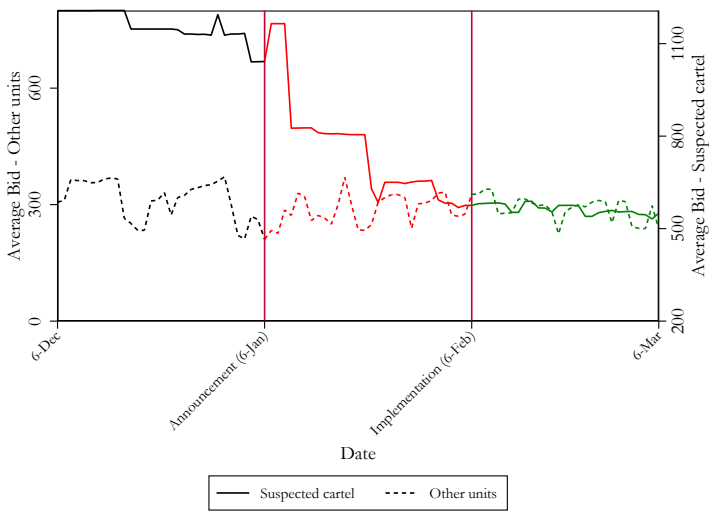

Figure 3: The main fact: Cartel and non-cartel bids.

Note: Average bid of the suspected cartel units (solid line, right axis) and other units (dashed line, left axis) over time. The vertical lines represent the announcement and implementation dates.

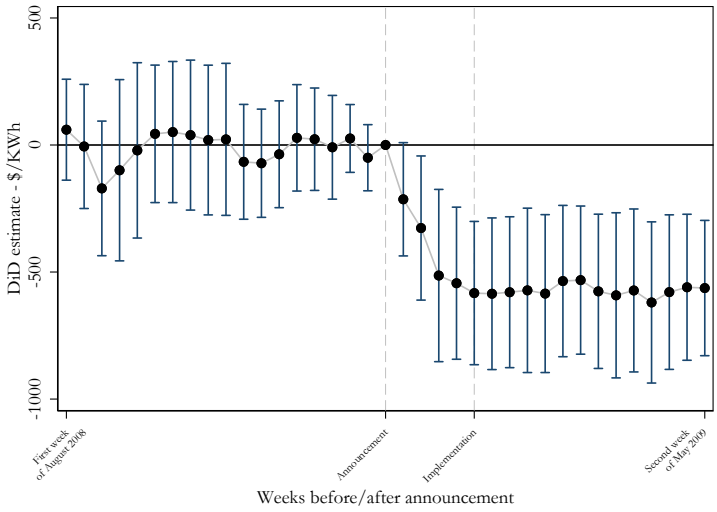

Figure 4: Event study estimates: Bids.

Note: Event study estimates using bid as the dependent variable, controlling for unit and date fixed effects. Robust s.e. are clustered by unit and date. The x-axis represents weeks around the 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. Dashed vertical lines refer to the week of the announcement and of the implementation.

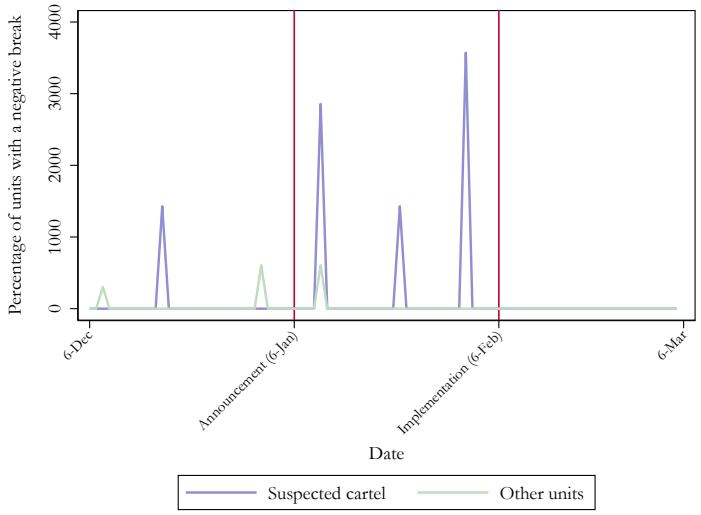

Figure 5: Test for structural breaks in the time series of bids.

Note: We use a QLR test to investigate whether a unit has a significant break in the intercept of the time series of bids (see Section 4.3). The figure reports the percentage of units for which the tests detects a significant break ($p < 0.01$) associated with a decrease in bids for the suspected cartel group and for the other units.

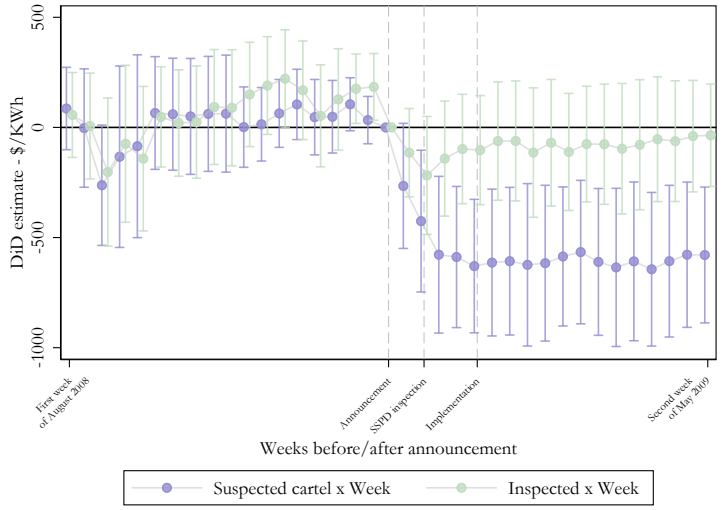

Figure 6: Event study estimates: SSPD inspection.

Note: On January 20, 2009, SSPD conducted inspections at the the four biggest firms (none with units in our suspected cartel definition, see Section 4.4 for details). We extend our event study model by adding an interaction between a dummy for being inspected with dummies for each week. Robust s.e. are clustered by unit and date.

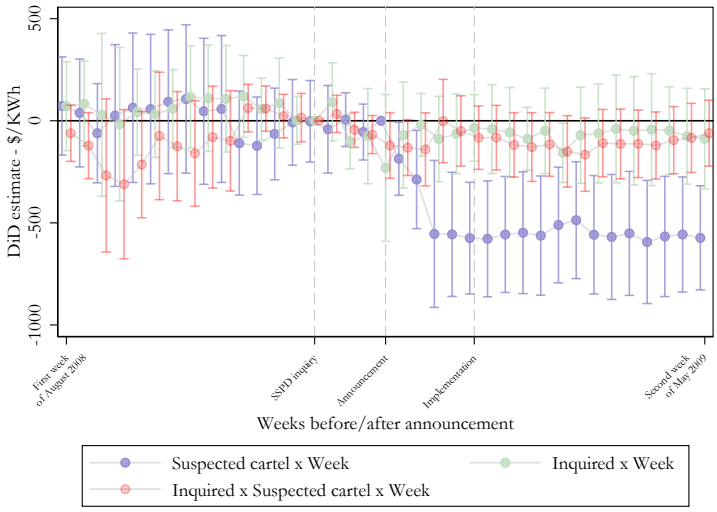

Figure 7: Event study estimates: SSPD inquiry.

Note: On December 5, 2008, SSPD summoned to its headquarters many firms, including some with units in our suspected cartel definition (see Section 4.4 for details). We extend our model by adding interactions for this inquiry for cartel and non-cartel units. Robust s.e. are clustered by unit and date.

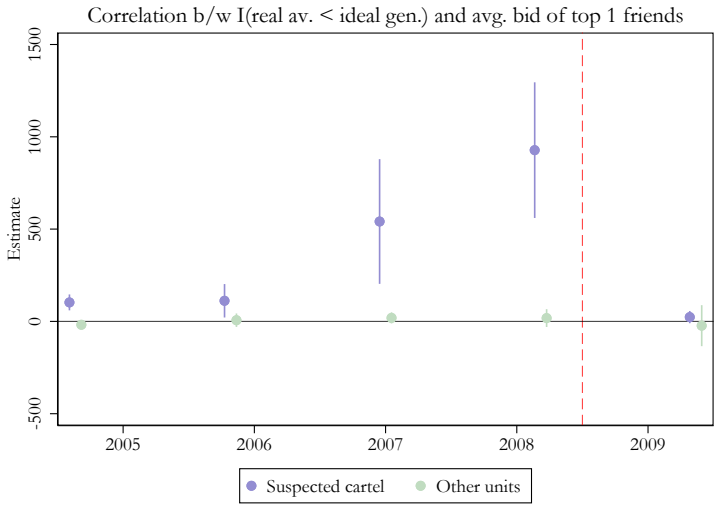

Figure 8: Bids coordination.

Note: We regress the (average) bid of the 'friend(s)' of unit i on an indicator for unit i declaring production availability below the ideal generation quantity it is awarded. We include in the explanatory dummy the 75% cases where the difference between availability and ideal generation is largest. We run separate regressions for cartel and others units and for each year. 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 (data is complete for other years). See Figure A12 for robustness.

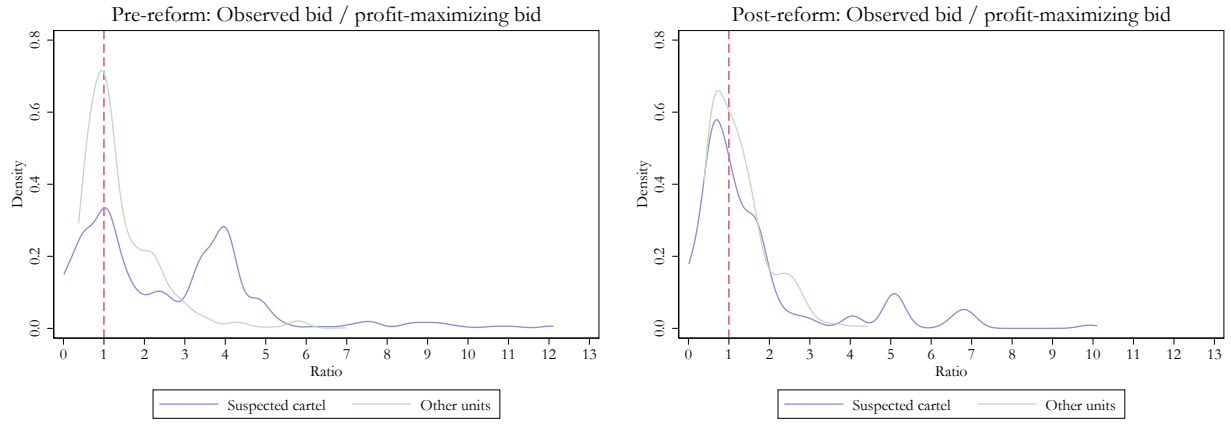

Figure 9: Distribution of the ratio of observed bids to profit-maximizing bids.

Note: We simulate counterfactual bids and the corresponding profits from positive reconciliations to compute the bid that maximizes static profits for each unit. 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.

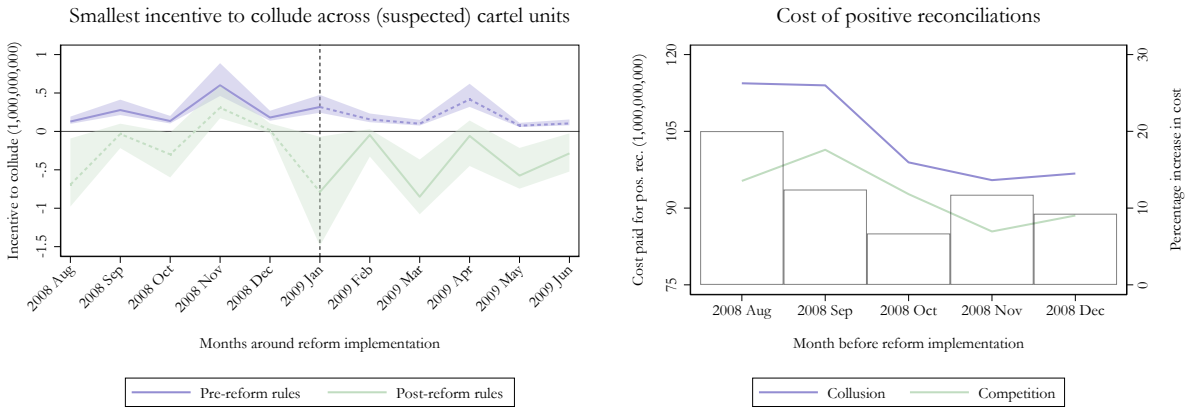

Figure 10: Incentives to collude and the cost of the cartel.

Note: The left figure presents the smallest incentive to collude across cartel units over time (see Section 6.3 for details). For the purple (green) line, we assume that a unit can unilaterally deviate for 2 (90) days before triggering competition. Solid lines refers to the actual transparency regime, and dashed lines to the counterfactual one. The lines represent estimates for our baseline discount factor, calibrated to match the relevant 15.1% interest rate in the period. The shaded area presents bounds to our estimates when we assume discount factors equivalent to 10% or 20% interest rates.

The right figure presents the estimated cost of the cartel for consumers. 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 with respect to the competitive scenario.

9 Tables

Panel A		Time-invariant	characteristics				
	Suspected cartel			Other units			
Variable(Unit level)	Obs	Mean	SD	Obs	Mean	SD	T-Test
Coal(percentage)	14	0.00	0.00	33	0.27	0.45	-44.21
Gas(percentage)	14	1.00	0.00	33	0.24	0.44	127.63
Hydro(percentage)	14	0.00	0.00	33	0.48	0.51	-70.05
Sec. fuel oil(percentage)	14	0.36	0.50	17	0.06	0.24	26.74
Sec. fuel coal(percentage)	14	0.14	0.36	17	0.00	0.00	19.20
Sec. fuel ACPM(percentage)	14	0.07	0.27	17	0.06	0.24	1.77
Sec. fuel JET A1(percentage)	14	0.00	0.00	17	0.06	0.24	-12.95
Sec. fuel none(percentage)	14	0.43	0.51	17	0.82	0.39	-30.76
Simple cycle(percentage)	14	0.50	0.52	17	0.76	0.44	-19.73
Combined cycle(percentage)	14	0.36	0.50	17	0.24	0.44	9.32
STIG cycle(percentage)	14	0.14	0.36	17	0.00	0.00	19.20
Capacity(KWh)	14	3554822.55	4577997.74	33	7581883.50	7684849.96	-28.63
Panel B		Before	06/01/2009				
	Suspected cartel			Other units			
Variable(Unit-date level)	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
Panel C		After	06/01/2009				
	Suspected cartel			Other units			
Variable(Unit-date level)	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: Descriptive statistics for the suspected cartel (columns 2 to 4) and other units (columns 5 to 7). Panel A reports time-invariant characteristics: (i) the share of units by primary input (coal, gas, or water); (ii) for thermal units, the share by secondary fuel (oil, coal, ACPM, Jet 1, or none); (iii) for thermal units, the share by cycle type (simple, combined, or STIG – Steam Injected Gas Turbine); (iv) capacity. Panel B reports the value of time-varying outcomes or characteristics in the period before the announcement date, and Panel C for the period afterwards. Column 8 reports the value of the t-test for the difference between the means in columns 3 and 6. Panel C reports the difference between the mean in the post and pre-period and the associated t-test. The pre-period includes observations from August 1, 2008 to January 5, 2009, and the post-period from January 6, 2009 to July 31, 2009.

VARIABLES	(1) LnBid	(2) LnBid	(3) LnBid	(4) LnBid	(5) LnBid	(6) LnBid	(7) Bid	(8) Bid	(9) Bid	(10) Bid	(11) Bid	(12) Bid
Suspected cartel x Announcement	-0.54***	-0.54***	-0.48***	-0.29*	-0.34**	-0.49***	-374.66***	-374.66***	-347.95***	-251.24**	-277.61*	-418.70***
	(0.13)	(0.14)	(0.13)	(0.15)	(0.15)	(0.13)	(120.54)	(124.55)	(123.52)	(97.07)	(157.22)	(117.78)
Suspected cartel x Implementation	-0.18**	-0.18*	-0.15*	-0.27***	-0.29***	0.15	-216.61***	-216.61***	-102.89	-230.99***	-112.33	24.08
	(0.08)	(0.10)	(0.08)	(0.08)	(0.09)	(0.11)	(47.47)	(55.77)	(70.02)	(59.15)	(76.22)	(86.35)
Announcement	-0.01						-76.73***					
	(0.06)						(26.51)					
Implementation	-0.12						4.39					
	(0.08)						(26.39)					
Observations	17,155	17,155	16,955	16,590	16,955	16,955	17,155	17,155	16,955	16,590	16,955	16,955
R-squared	0.29	0.82	0.83	0.85	0.84	0.85	0.24	0.81	0.82	0.84	0.83	0.83
Unit FE	NO	YES	YES	YES	YES	YES	NO	YES	YES	YES	YES	YES
Date FE	NO	YES	YES	N/A	N/A	N/A	NO	YES	YES	N/A	N/A	N/A
Date x Fuel FE	NO	NO	NO	YES	NO	NO	NO	NO	NO	YES	NO	NO
Date x Sec. fuel FE	NO	NO	NO	YES	NO	NO	NO	NO	NO	YES	NO	NO
Date x Cycle FE	NO	NO	NO	NO	YES	NO	NO	NO	NO	NO	YES	NO
Date x Region FE	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO	NO	YES
Marginal cost	NO	NO	YES	YES	YES	YES	NO	NO	YES	YES	YES	YES
Forward contracts	NO	NO	YES	YES	YES	YES	NO	NO	YES	YES	YES	YES
Input levels x Fuel dummies	NO	NO	YES	N/A	YES	YES	NO	NO	YES	N/A	YES	YES

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

Table 2: Difference-in-differences estimates for bids.

Note: The table presents estimates from the difference-in-differences model described in equation (2) using the logarithm of bids as the dependent variable (columns 1-6). In column 2 we control for Unit and Date FE. In column 3 we further control for marginal cost, forward contracts over total capacity, and a vector of Input levels x Fuel dummies consisting of coal prices interacted with fuel type indicators, gas prices interacted with fuel type indicators, and water intake and water reserves, each interacted with fuel type indicators. In column 4, 5 and 6 we alternatively control for Date × Fuel FE and Date × Secondary fuel FE, for Date × Cycle FE or Date × Region FE. Fuel can take three distinct values: hydro, gas, or coal. Secondary fuel can take five distinct values (for thermal units): oil, coal, ACPM, Jet 1, or none. Cycle can take three distinct values (for thermal units): simple, combined, or STIG. Regions are Atlantic, North-West, Central and South-West. Robust s.e. clustered by unit and date in parenthesis. Columns 7 to 12 repeat the same analyses using bid (in level) as the outcome.

VARIABLES	(1) Dummy for PR	(2) Profits from PR	(3) Total profits
Suspected cartel x 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

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
Suspected cartel	3	11	14
%	21	79	100
Other units	31	2	33
%	94	6	100

Table 4: Cartel definition and structural breaks.

Note: The table presents the number (and percentage) of units in our baseline (suspected) cartel and non-cartel groups 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
Suspected 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 difference-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 on 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.5 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 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 behavior

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 behavior. We therefore need an instrument for (the probability of) a positive reconciliation for unit i at date t.

[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).

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 A15 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 A15 presents the results and shows two main patterns. First, excluding or includ-

ing 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 behavior 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 behavior 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 behavior during 2008.

4. **Cartel 5**: *Three dummies: Atlantic, Thermal, Private , and two continuous variable: Forward Contracts, and Bidding behavior 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 A5 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 A6 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 A16 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

[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.

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.

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

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 A7 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 A17 plots the distributions of in-sample predictions versus observed bids.

A.5 Marginal Costs and Cost-Based Positive Reconciliation Prices

Calculation of 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, 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].

Following previous studies (see, e.g., Green and Newbery (1992) and Wolfram (1999)), we included the transportation cost in the marginal cost computation. For gas-fired units, we take as transportation costs the sum of the usage fees for each segment of the gas transmission network necessary to take the gas from Guajira to the respective generation units. These fees are regulated by the CREG and are published in regulatory acts (CREG, 003a,b).

For coal-fired units, we use as reference price 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

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

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

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

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), 2009).[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]

Cost-Based Maximum Price for Positive Reconciliations According to the regulations in place during the period we analyzed, the maximum regulated price for positive reconciliation prices was defined differently depending on whether the generation unit was thermal or hydroelectric. The cap price for thermal plants was calculated with a formula that took into account engineering estimates about the different costs of a thermal plant, such as the cost of fuel, transportation, maintenance, and start-up. The price cap for hydroelectric units was calculated with a formula that considered an indicative supply function, which was based on the reservoir's filling percentage, technical characteristics of the reservoir, and the positive reconciliation prices for thermal plants. See resolution 34 CREG (2001) for more details.

References

Barros, F. and Modesto, L. (1999). Portuguese Banking Sector: a Mixed Oligopoly? *International Journal of Industrial Organization*, 17(6):869–886.

CREG (2001). Resolución No. 034: Por la cual se dictan normas sobre funcionamiento del Mercado Mayorista de Energía. Technical report, Comisión de Regulación de Energía y Gas.

CREG (2003a). Decisón sobre la solicitud de revisión de Cargos Regulados del Sistema de Transporte de PROMIGAS S.A. E.S.P. Technical report, Comisión de Regulación de Energía y Gas, Resolución 70.

CREG (2003b). Por la cual se resuelven los Recursos de Reposición interpuestos contra la Resolución CREG-013 de 2003. Technical report, Comisión de Regulación de Energía y GasResolución 125.

CREG (2005). Sustitución del artículo 3° de la Resolución CREG 023 de 2000. Technical report, Comisión de Regulación de Energía y Gas, Resolución 119.

[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).

Fabra, N. and Reguant, M. (2014). Pass-through of emissions costs in electricity markets. *American Economic Review*, 104(9):2872–99.

Green, R. and Newbery, D. M. (1992). Competition in the British Electricity Spot Market. *Journal of Political Economy*, 100(5):929–53.

Ishii, R. (2009). Favor exchange in collusion: Empirical study of repeated procurement auctions in Japan. *International Journal of Industrial Organization*, 27(2):137–144.

Ministerio de Minas y Energía (MME) (2009). Resolución MME 180507 de 2003.

Porter, R. and Zona, J. (1993). Detection of Bid Rigging in Procurement Auctions. *Journal of Political Economy*, 101(3):518–38.

Superintendencia Delegada para Energía y Gas (2008). Resumen Acciones MEM Diciembre de 2008. Technical report, Superintendencia de Servicios Publicos.

Wolak, F. (2000). An Empirical Analysis of the Impact of Hedge Contracts on Bidding Behavior in a Competitive Electricity Market. *International Economic Journal*, 14(2):1–39.

Wolfram, C. D. (1998). Strategic bidding in a multiunit auction: An empirical analysis of bids to supply electricity in england and wales. *RAND Journal of Economics*, 29(4):703–725.

Wolfram, C. D. (1999). Measuring Duopoly Power in the British Electricity Spot Market. *American Economic Review*, 89(4):805–826.

A.6 Appendix Figures

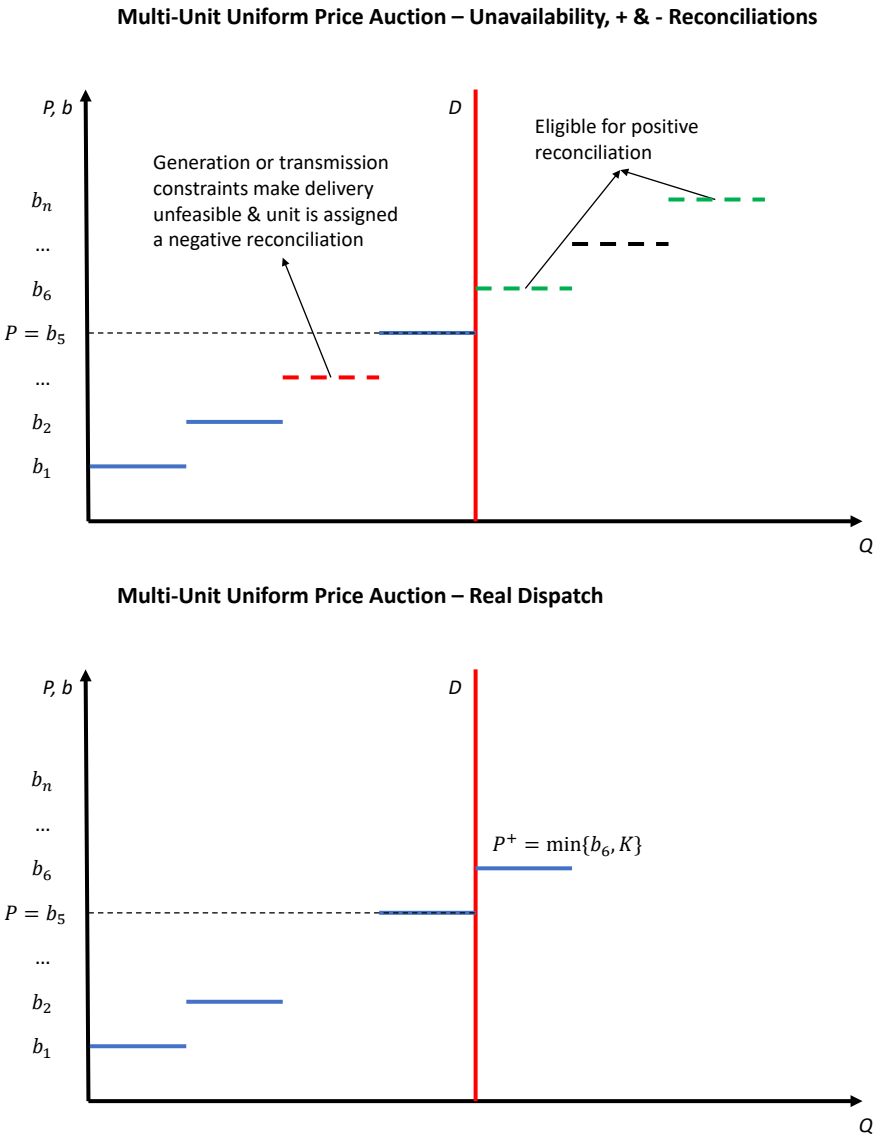

Figure A1: Uniform price auction, ideal dispatch, and reconciliations.

Note: The system operator arranges bids in increasing order $(b_1, b_2, \ldots, b_n)$ to fulfill 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).

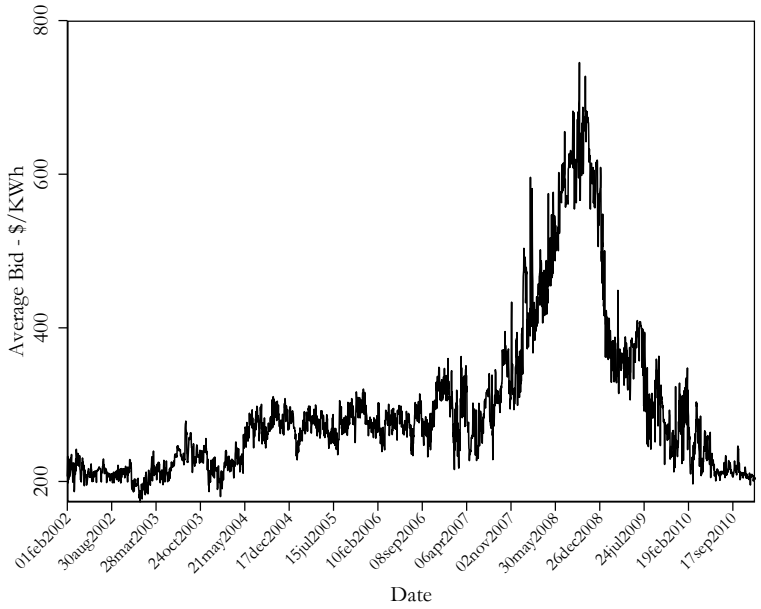

Figure A2: Average bid time series.
Note: Daily average bid from 2002 to 2010 in the Colombian wholesale electricity market.

Colombia Spot Market and Interconnections

Peter Cramton

24 January 2009

Professor of Economics, University of Maryland
Chairman, Market Design Inc.

Improved information policy

- Colombia currently has full transparency
  - All bid information revealed after bidding period
- Allows day-by-day punishment for deviation from tacit collusion
  - Much easier to support tacit collusion
- Anonymous bids would be better
- Then reveal all bids after 90 days
  - Has benefits of full transparency
  - But does not allow immediate punishment of defectors from tacit agreement

Figure A3: Cramton's CREG Presentation.

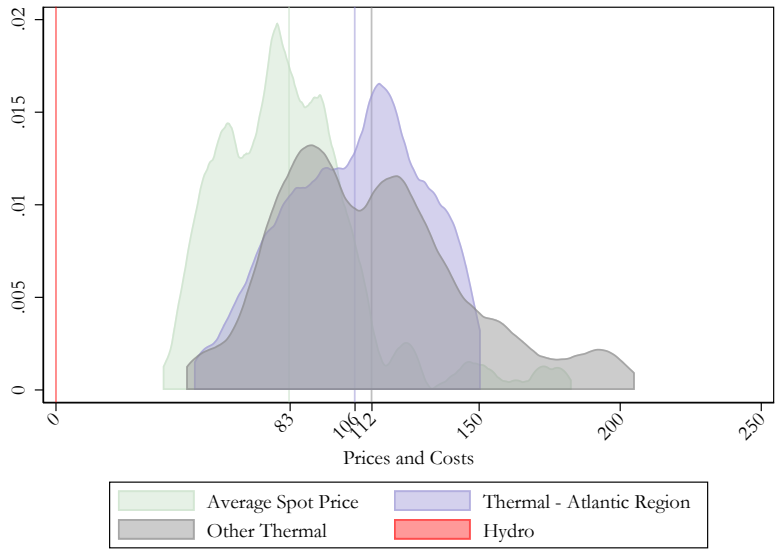

Figure A4: Spot price and production costs.

Note: The figure presents the distribution of the 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.

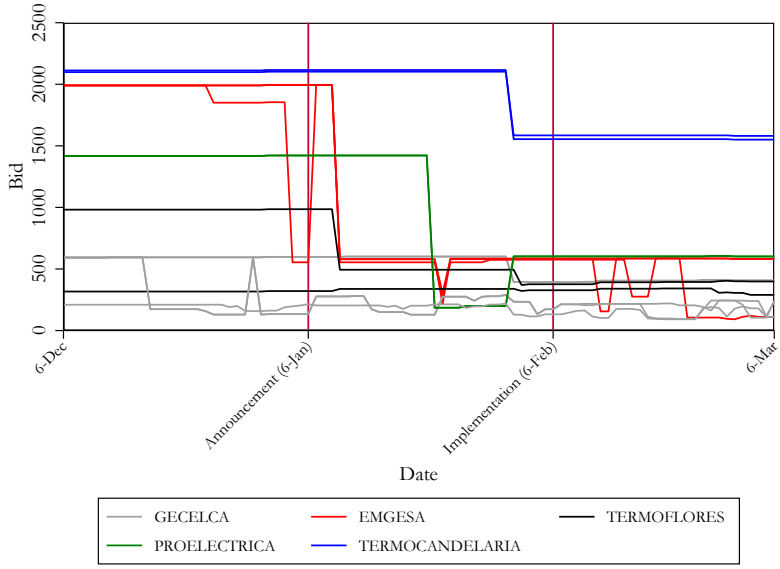

Figure A5: Bids of suspected cartel units.

Note: The figure shows the time series of bids for each suspected cartel unit separately. We use the same color for units belonging to the same firm (see legend).

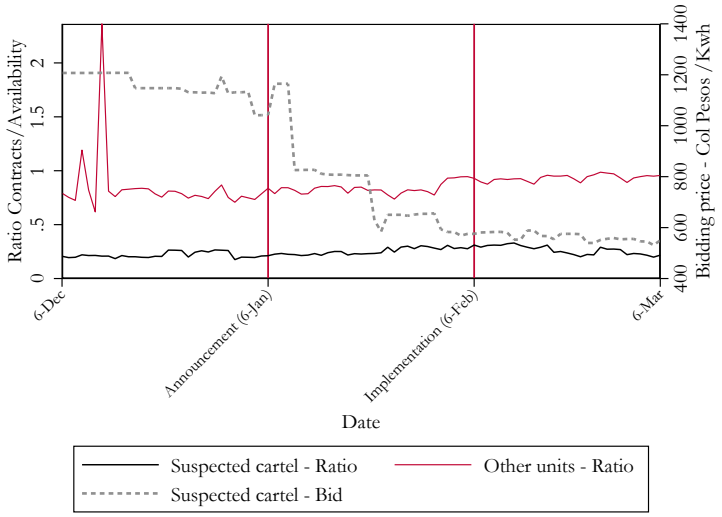

Figure A6: Forward contracts.

Note: Solid lines (right axis) present the average ratio between the amount of electricity sold through forward contracts and production capacity for units in the suspected cartel and for other units. The dashed line presents the average bid for cartel units (left axis).

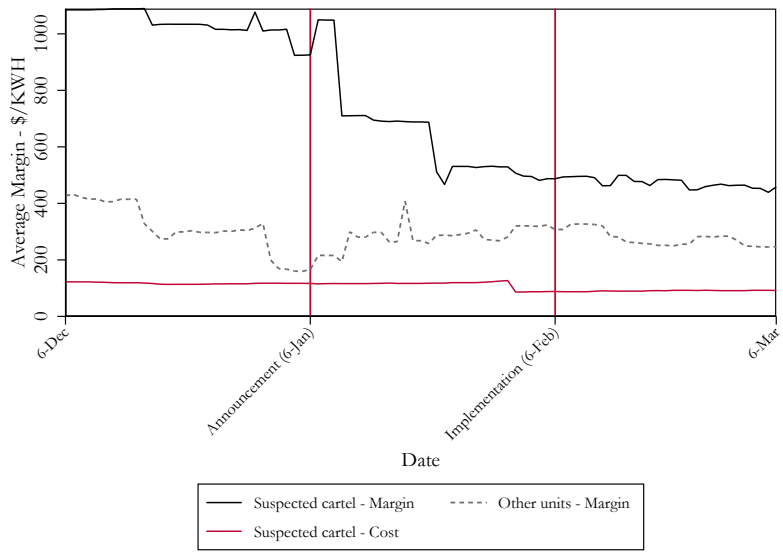

Figure A7: Average margin.

Note: The figure presents the average margin (bid minus cost) of the suspected cartel units (solid black line), the margin of the other thermal units (dotted grey line), and the average marginal production cost of cartel units (solid red line) over time.

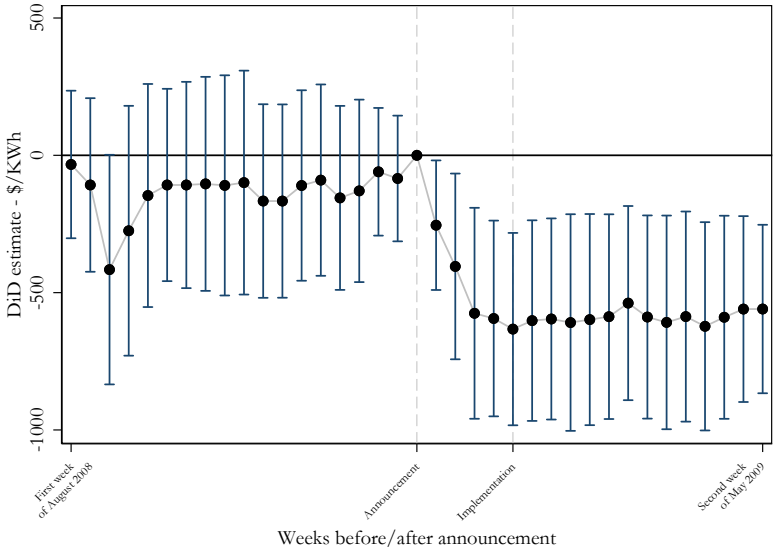

Figure A8: Event study estimates for margin.

Note: Event study estimates using margin (bid minus cost) as the dependent variable, controlling for unit and date fixed effects. The sample only includes thermal units. 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. Dashed vertical lines refer to the week of the announcement and of the implementation.

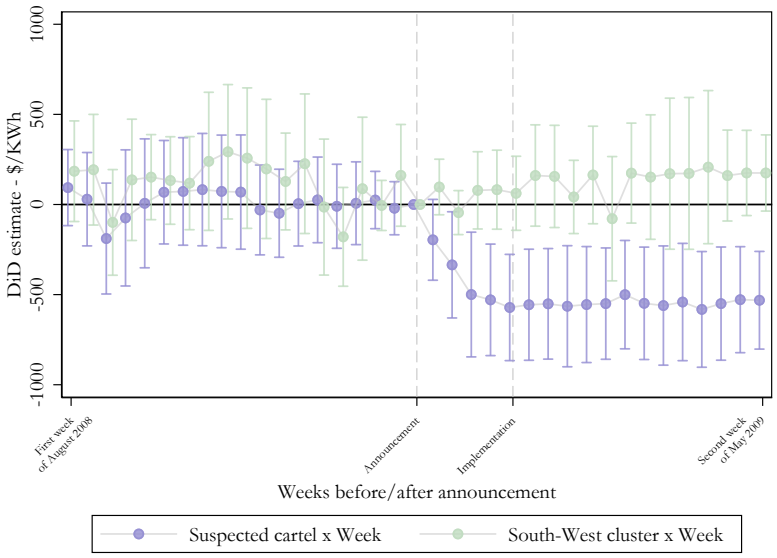

Figure A9: Event study estimates for units in the South-West cluster.

Note: Event study estimates for bids, controlling for unit and date fixed effects. Robust s.e. are clustered by unit and date. The x-axis represents weeks around the 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. Dashed vertical lines refer to the week of the announcement and of the implementation.

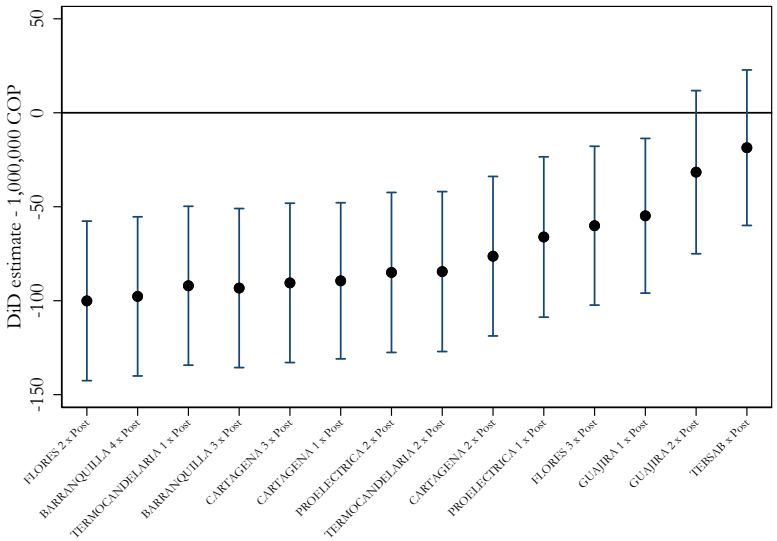

Figure A10: Profits of each unit in the suspected cartel.

Note: Estimates from a difference-in-differences model for 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.

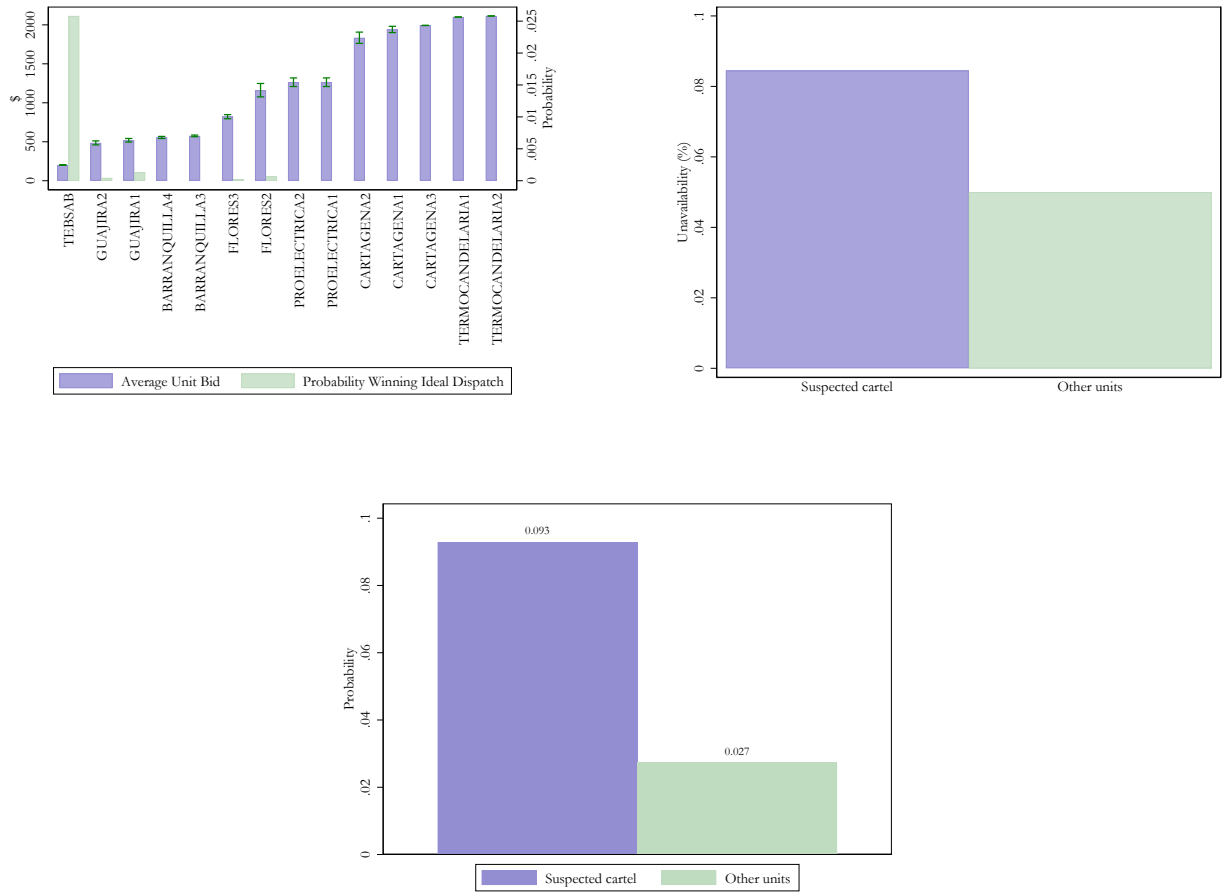

Figure A11: Probability of winning, of unavailability, and of reconciliations.

Note: The top left figure shows the average prices for the suspected 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 the suspected cartel units and other 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.

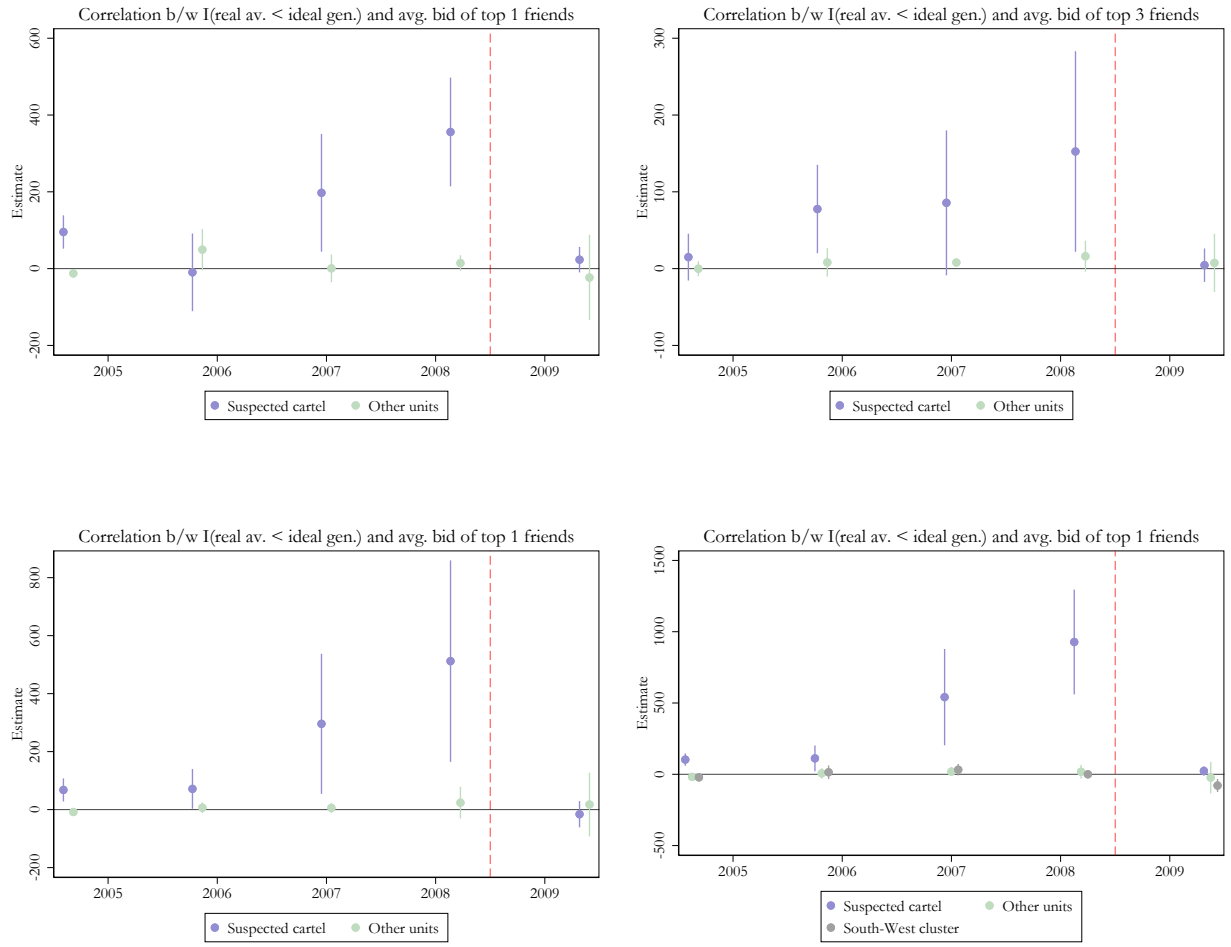

Figure A12: Robustness: Bids coordination.

Note: Estimates from regressions where the outcome variable is the (average) bid of the 'friend(s)' 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 (suspected cartel, others, South-West) and repeat for years 2005 to 2009. Compared to the baseline analysis in Figure 8, we perform four robustness exercises. (i) In the top left panel, we still consider 'top 1' friend 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.

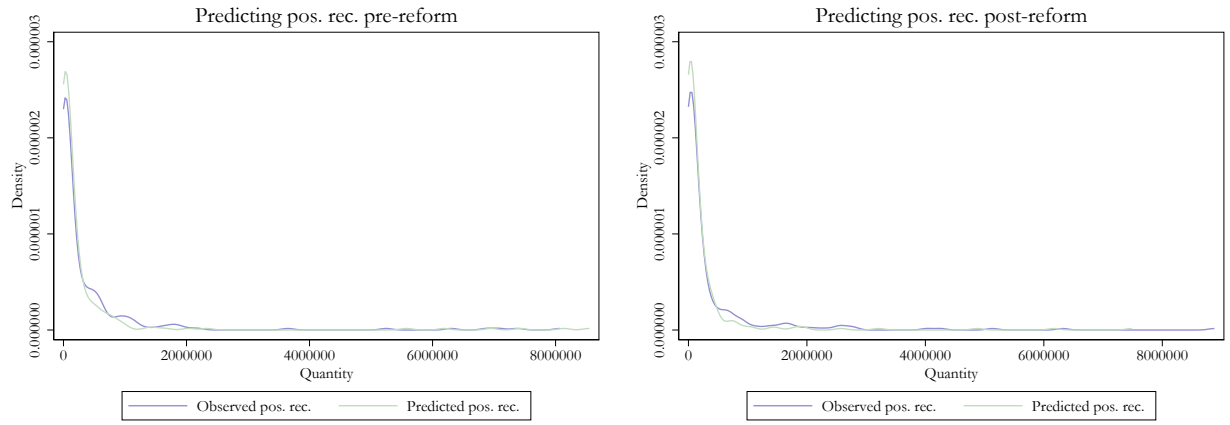

Figure A13: Distribution of observed quantities of positive reconciliations and in-sample predictions.

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 both suspected cartel and other 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.

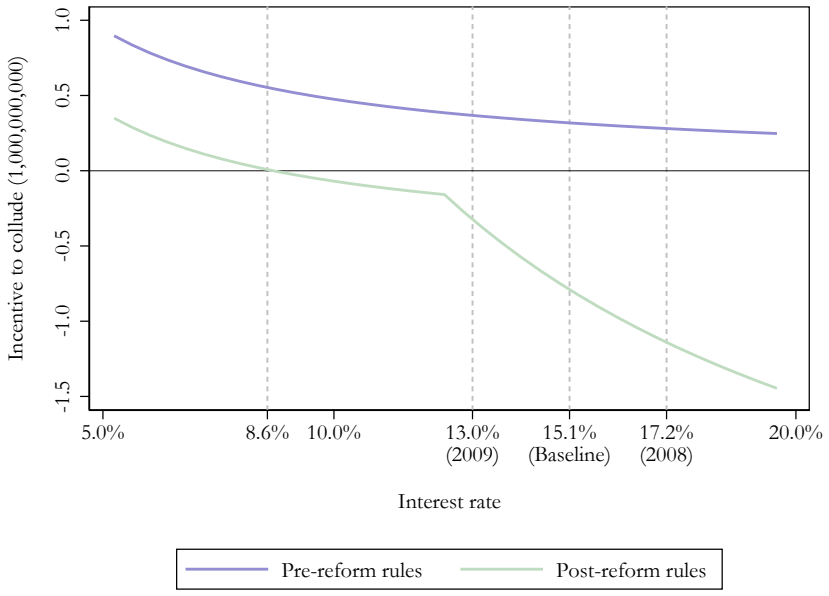

Figure A14: Cartel sustainability for different interest rates.

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 10), the ones observed in 2008 and 2009 (17.2% and 13%), and the critical one for cartel sustainability under the new regime (8.6%).

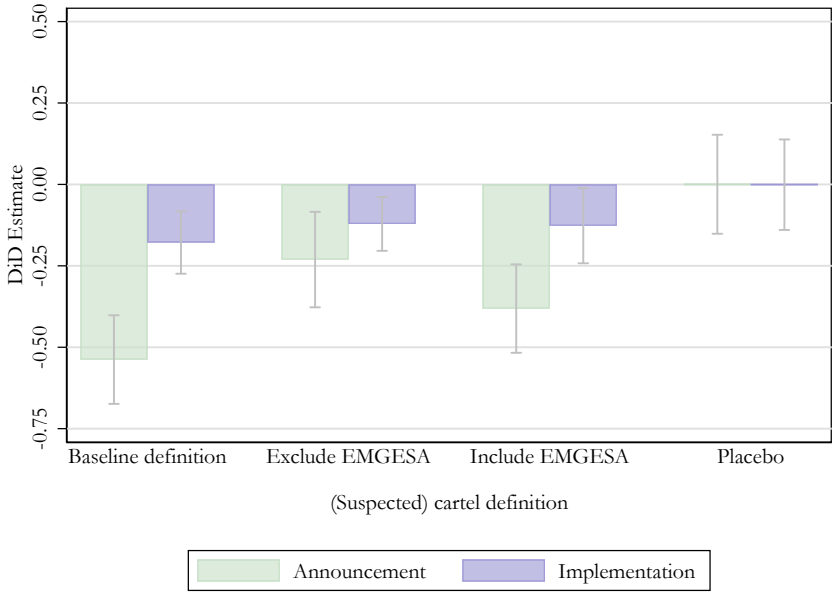

Figure A15: Robustness of cartel definition and placebo.

Note: The figure shows estimates of the 'announcement' and 'implementation' parameters from different DiD regressions. 'Baseline definition' 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.

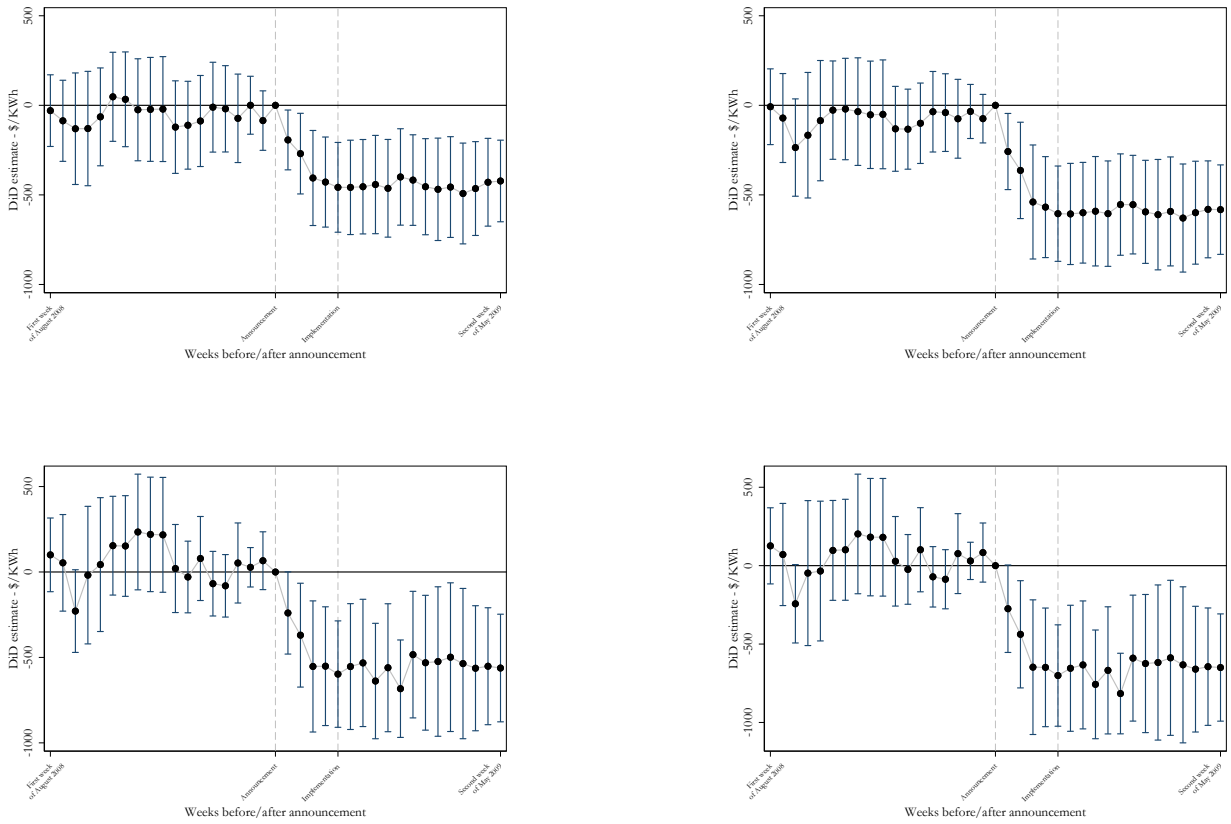

Figure A16: Event study estimates for alternative cartel definitions.

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.

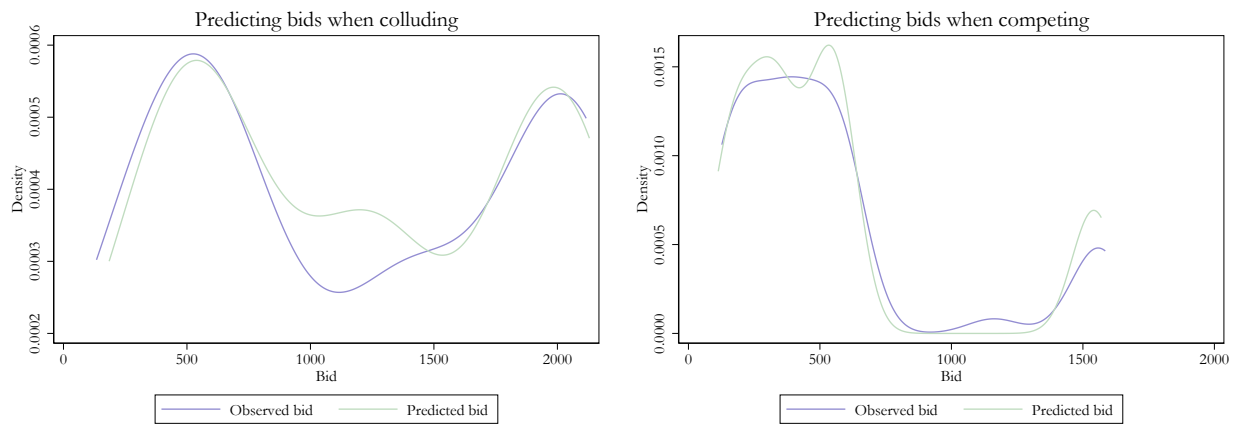

Figure A17: Distribution of observed bids and in-sample predictions.

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.7 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 analysis for the logarithm of bids on the lagged probability of positive reconciliation using observations from the year 2008. Column 1 presents the OLS estimate. Column 2 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. Column 3 presents the second stage of the IV estimation (the negative R-squared is common with IV estimation and many fixed effects). Column 4 presents the reduced-form estimate. All the columns control for 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	(5) Margin	(6) Margin
Suspected cartel x Announcement	-375.26***	-375.26***	-347.95***	-251.24**	-277.61*	-418.70***
	(120.48)	(124.41)	(123.52)	(97.07)	(157.22)	(117.78)
Suspected cartel x Implementation	-193.33***	-193.33***	-102.89	-230.99***	-112.33	24.08
	(47.05)	(55.26)	(70.02)	(59.15)	(76.22)	(86.35)
Announcement	-76.09***					
	(26.68)					
Implementation	12.98					
	(26.41)					
Observations	17,155	17,155	16,955	16,590	16,955	16,955
R-squared	0.22	0.80	0.81	0.83	0.82	0.82
Unit FE	NO	YES	YES	YES	YES	YES
Date FE	NO	YES	YES	N/A	N/A	N/A
Date x Fuel FE	NO	NO	NO	YES	NO	NO
Date x Sec. fuel FE	NO	NO	NO	YES	NO	NO
Date x Cycle FE	NO	NO	NO	NO	YES	NO
Date x Region FE	NO	NO	NO	NO	NO	YES
Marginal cost	NO	NO	YES	YES	YES	YES
Forward contracts	NO	NO	YES	YES	YES	YES
Input levels x Fuel dummies	NO	NO	YES	N/A	YES	YES

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

Table A2: Difference-in-differences estimates: Margin.

Note: DID estimates for margin (bid minus cost). We progressively add controls as in Table 2. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) LnBid	(2) LnBid	(3) LnBid	(4) LnBid	(5) LnBid
Suspected cartel x Announcement	-0.39***	-0.39***	-0.39***	-0.29*	-0.34**
	(0.12)	(0.13)	(0.14)	(0.15)	(0.15)
Suspected cartel x Implementation	-0.03	-0.03	-0.22**	-0.26***	-0.29***
	(0.12)	(0.13)	(0.09)	(0.08)	(0.09)
Announcement	-0.16***				
	(0.05)				
Implementation	-0.27**				
	(0.11)				
Observations	11,315	11,315	11,115	10,750	11,115
R-squared	0.25	0.83	0.83	0.86	0.84
Unit FE	NO	YES	YES	YES	YES
Date FE	NO	YES	YES	N/A	N/A
Date x Fuel FE	NO	NO	NO	YES	NO
Date x Sec. fuel FE	NO	NO	NO	YES	NO
Date x Cycle FE	NO	NO	NO	NO	YES
Date x Region FE	NO	NO	NO	NO	NO
Marginal cost	NO	NO	YES	YES	YES
Forward contracts	NO	NO	YES	YES	YES
Input levels x Fuel dummies	NO	NO	YES	N/A	YES

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

Table A3: Difference-in-differences estimates for thermal units.

Note: DID estimates when excluding hydro units from the sample of analysis. We progressively add controls as in Table 2. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) LnBid	(2) LnBid	(3) LnBid	(4) LnBid
Suspected cartel x Announcement	-0.54***	-0.63***	-0.53***	-0.41**
	(0.14)	(0.15)	(0.17)	(0.19)
Suspected cartel x Implementation	-0.18*	-0.14	-0.31**	-0.08
	(0.10)	(0.10)	(0.13)	(0.12)
Observations	17,155	17,155	8,760	8,395
R-squared	0.82	0.82	0.81	0.82
Unit FE	YES	YES	YES	YES
Date FE	YES	YES	YES	YES
Sample	All units	All units	Only private units	Only public units
Cartel definition	Thermal-Atlantic	Private-Thermal-Atlantic	Thermal-Atlantic	Thermal-Atlantic

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

Table A4: Difference-in-differences estimates for private and public units.

Note: The table presents our baseline DID estimates (column 1) along with the results when we restrict the cartel group to include only privately managed units (column 2), when we only use the sample of privately managed units (column 3), and when we only use the sample of publicly managed units (column 4).

	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 A5: 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
Suspected cartel x Announcement	-0.54***	-0.48***	-0.49***	-0.63***	-0.67***
	(0.14)	(0.12)	(0.14)	(0.13)	(0.14)
Suspected cartel x Implementation	-0.18*	-0.15	-0.18*	0.03	0.02
	(0.10)	(0.11)	(0.10)	(0.13)	(0.15)
Observations	17,155	17,155	17,155	17,155	17,155
R-squared	0.82	0.82	0.82	0.81	0.81
Unit FE	YES	YES	YES	YES	YES
Date FE	YES	YES	YES	YES	YES
Cartel Definition	1	2	3	4	5

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

Table A6: DiD estimates with alternative cartel definitions.

Note: Difference-in-differences estimates for the logarithm bids. 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 A7: 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.