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

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Weak institutions and concentrated markets may favour the emergence of cartels – informal arrangements in which firms coordinate to limit competition. Yet, evidence from developing countries remains scarce due to empirical challenges: under collusion, firms deviate from current profit maximization in anticipation of future rewards, but since profit maximization places few restrictions on firms' behavior, collusive conduct is hard to infer. In the Colombian wholesale electricity market, certain firms lowered prices, in an anticipatory and coordinated fashion, immediately after the *announcement*, and before the actual *implementation*, of a regulatory reform that would make coordination more difficult in the future. We complement this novel *announcement* design with a forensic analysis that uncovers a form of coordinated behavior among suspected cartel members that likely required explicit communication, and quantify the overcharges from the cartel to be at least 12%. Policy-makers should take dynamic enforcement considerations into account in their fight against collusion.

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JEL Codes: O13, L13, L14, L41.

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1 Introduction

Cartels – informal arrangements in which firms coordinate to limit competition – might be particularly widespread in developing countries (World Bank, 2016), where entry barriers and political connections protect incumbents (Djankov et al., 2002; Faccio and Zingales, 2022), competition authorities are weaker (Besley et al., 2020), and markets thinner and more concentrated (Mitton, 2008). Despite 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. One reason is that most empirical studies focus on cartels investigated by competition authorities and, as those are weaker in developing countries, fewer documented examples exist. Indeed, according to the Private International Cartel database (Connor, 2020), only 5% of proven cartels are in Africa (72% of those in South Africa), 7% in Latin America and 11% in Asia. Another reason is that 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 few restrictions on firms' behavior, making collusive conduct hard to infer from behavior alone.[1]

This paper uncovers collusion in the Colombian wholesale electricity market by showing that a subset of firms lowered prices immediately after news that coordination might become more difficult in the future. This "announcement" design exploits a key insight common to models of collusive behavior, identifying coordinated behavior under arguably limited assumptions.[2] Besides its intrinsic relevance (access to reliable and cheap energy is critical for development (Greenstone et al., 2014)) and detailed data, we take advantage of a regulatory change to implement our novel test of collusive behavior supported by future rewards. 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

[1] More generally, cartels are a form of relational contracts – informal arrangements sustained by the value of future interactions (Baker et al., 2002). These arrangements, which are particularly common in developing countries (Macchiavello, 2022), enable parties to cooperate but may harm other market participants.

[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.

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 arrangements, tacit or explicit, aimed at increasing 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 30−40% immediately after the *announcement* – and well before the actual *implementation* – of the regulatory change. Consistent with the key implications of models of collusion, some firms reacted to the announcement in an anticipatory and coordinated way. This strategy allows us to rule out several confounders, including the fact that changes in market transparency itself could alter firms' bidding behavior. We complement this evidence with a forensic analysis to uncover other forms of coordinated behavior, and with a structural exercise that further supports the reduced-form findings and quantifies the costs of collusion.

It is important to clarify what our test is – and is *not* – meant to accomplish. The test aims at uncovering the existence of coordinated, anti-competitive, conduct. Its logic doesn't rely on – and the evidence doesn't demonstrate – that firms participating in such an arrangement perfectly foresaw the actual change in transparency enacted by regulators and how that would make coordination unsustainable. The test requires that at least some of the suspected cartel members become sufficiently pessimistic about their ability to coordinate bids in the *future*. The test thus also does not assume, nor does it aim at identifying, the communication strategies – tacit or explicit – and the particular equilibrium used by firms to coordinate actions.[3]

Section 2 provides background information on the Colombian wholesale electricity market and the transparency reform. Like many other wholesale electricity spot markets, the Colombian one is organised as a uniform-price multi-unit procurement auction. In the market, generating units submit availability and bids to supply electricity to the grid the following day, the market regulator aggregates bids to obtain the market supply and then, given demand, determines the price that clears the market and the resulting allocation – the so called ideal dispatch. In the real dispatch the market regulator allocates production taking into account constraints to generation and transmission that arise after generating units have

[3]For example, while subgame perfect equilibrium (SPE) relies on players' anticipatory behavior (which is consistent with our evidence), it also assumes correct beliefs about future play both on- and off- equilibrium paths (about which, instead, our evidence is essentially mute).

submitted their bids. This is done through a process of positive and negative reconciliations which, as we later clarify, plays a critical role in our analysis.

Section 3 fixes ideas about the form of coordinated behavior we later uncover and illustrates the logic of our test. Chassang and Ortner (2023) elucidates the challenges involved in identifying collusive conduct in the data: e.g., in dynamic environments, pricing behavior can deviate from static profit maximization, firms might make mistakes, and so on. Collusive strategies are difficult to characterize in general and, perhaps, particularly so in our context in which capacity constraints, unilateral exercise of market power, participation in forward markets, and shocks to the transmission network and the resulting reconciliations significantly affect behavior in the uniform price auction even in the absence of coordination. Our test relies on an insight common to all repeated-games models of collusion – an *instantaneous* response in anticipation of *future* changes in the ability to sustain collusion – which arguably circumvents the challenges involved in modeling such a complex environment.

Section 4 implements the test, presents the main evidence, and rules out confounders. Unlike studies that rely on proven cartels, we do not know the identity of the firms participating in the arrangement – if one existed. There are two ways to "assign" firms to the suspected cartel. A first approach puts forward a priori hypotheses on which firms might be participating and tests for differential changes in 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 confirm its consistency with the second one.

We conjecture that thermal units in the Atlantic region coordinated bids to increase profits from the positive reconciliations market. The rationale for this choice is that thermal units have higher costs and can't make profits in the ideal dispatch. Because positive reconciliations occur when there are disruptions to transmission or generation, units are more likely to compete for positive reconciliation with nearby units – justifying our focus on a cluster of geographically nearby units despite the unique national market. This classification isolates a group of 14 units – henceforth, the *cartel*. Using both DID and more flexible event-study specifications, we show that the average bid for cartel units falls after the announcement, and before the implementation of the regulatory change, regardless of whether we use thermal units located in other regions, hydro units in the same region, or both, as the comparison group. Thermal units in the Atlantic region are also more likely to feature a structural break in bids between the announcement and the implementation date: a break

is detected for 11 out of the 14 units, but for only 2 of the remaining 33 units.[4,5]

Section 5 explores further whether thermal units in the Atlantic region coordinated behavior, possibly through explicit communication, before the announcement date. While the evidence of a sudden drop in bids following the announcement date is consistent with a game-theoretic definition of collusion, it does not meet a legal definition that typically requires evidence of express agreement and overt communication (Chassang and Ortner, 2023). While we have no pretense that our analysis establishes such evidence, we detect a particular type of coordination that arguably would have been difficult to achieve without communication. In particular, we show that suspected cartel units increase bids in the previous day's auction precisely when other members declare themselves unavailable immediately prior to the real operation. In other words, occasionally, suspected cartel members bid on a particular day "as if" they have information about the behavior of other members that is – in theory – only revealed in the future. This correlation in behavior only appears among thermal units in the Atlantic region and ceases after the reform. This particular form of coordination requires units to declare unavailability upon winning in the day-ahead auction process – a behavior that, if done too frequently, attracts the regulator's attention. This anomalous coordination was thus rare and did not play a quantitatively important role in generating extra profits, but it nevertheless lends further support to the hypothesis that suspected cartel members coordinated behavior.

This leaves open the question of whether such coordination was tacit or involved explicit communication. While we do not observe explicit communication, patterns of attendance at the meetings of the National Council of Operations (CNO) reveal strategic behavior consistent with such a hypothesis. CNO holds regular in-person meetings in Bogota to discuss engineering problems on the network. We downloaded the minutes of all the meetings in 2008 (i.e., before) and 2009 (i.e., after the announcement date). Within a DID framework, we find that after the reform, suspected cartel members stopped sending employees from commercial areas involved in setting bids to the meetings. The CNO meetings might have provided a venue that facilitated communication.

Section 6 presents a quantification exercise. Unlike the previous analysis, this requires committing to a specific model and, inevitably, simplifying assumptions. We focus on the reconciliations market and thermal units. We estimate best responses and associated profits exploiting detailed information on both units' production costs and residual demands typ-

[4]Interestingly, 2 of the 3 cartel units that fail the test are publicly managed, while the 2 that pass the test but are not in the baseline cartel belong to the only firm that owns both units in and out of the cartel. Results are robust to several alternative definitions of the cartel.

[5]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.

ically available in wholesale electricity markets. First, we compare actual bids with those that would unilaterally maximise profits. Before – but not after – the reform, thermal units in the Atlantic region display a bimodal distribution of the ratio of observed bids relative to profit-maximising bids, with peaks around one and four. In contrast, other thermal units – and thermal units in the Atlantic region after the reform – display a unimodal distribution with a single peak around one. In other words, suspected cartel units could have often increased their static profits by bidding significantly lower. Second, we embed the corresponding optimal deviations into the dynamic enforcement constraint. For a wide range of discount factors consistent with interest rates in Colombia during the sample period, such deviations are not incentive-compatible under the old transparency rule but become so under the new rule. While this doesn't prove that the change in transparency led to the cartel's demise, the estimates provide a sanity check that such an interpretation is quantitatively consistent with observed behavior.

Section 7 discusses the policy implications of our results.

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.[6]

[6]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 sup-

We contribute to the literature on firms in developing countries with evidence of coordinated pricing in the wholesale energy market. 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).[7] 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).[8]

Our primary contribution to the empirical literature on collusion is a novel test, based on a policy announcement design.[9] Chassang and Ortner (2019) study of procurement in Japan is particularly related to our paper. They argue, and find supporting evidence, that dynamic 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. To the best of our knowledge, this "announcement" design is novel, and complements studies of "investigation shocks" (see, e.g., Clark and Houde, 2014). Investigation shocks, however, tend to be announced and implemented simultaneously, affecting incentives to deviate from anti-competitive pricing arrangements via current *and* future profits. 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

ported 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. Delabastita and Rubens (2024) identifies employers' collusion among Belgian coal mines in the late nineteenth century.

[7] 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. Fioretti et al. (2024), Suárez (2022*a*,*b*), and Camelo et al. (2018) study the Colombian wholesale energy market but with a different focus.

[8] Collusive agreements are examples of *relational contracts* – informal agreements sustained by the value of future interactions. Such arrangements might be particularly relevant in developing countries (see, e.g., Macchiavello and Morjaria (2015), Blouin and Macchiavello (2019), Macchiavello and Morjaria (2021), Ghani and Reed (2022)). The key difficulty in testing models of relational contracting is that neither the *future* value of the relationship nor the *current* temptations to deviate are typically observed. Our empirical test could be used to study relational contexts in other contexts.

[9] 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.

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.

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), however, argue that transparency can hinder cartels by helping firms devise more profitable deviations and discuss examples in which that appears to have been the case. Similarly, Kawai et al. (2025) argues that privacy can facilitate collusion in auctions by making defection less attractive: a potential cheater can no longer be sure of winning after a deviation. We provide evidence consistent with the conventional view.[10] Furthermore, the "backward-looking" information sharing allows us to focus on the length of the secret price-cutting period, abstracting from the real-time information value of prices. However, 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.[11]

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 our sample period in 2008/9, 47 generation units produced an average daily $\approx$ 150 GWh of electricity. Among these units, 32 units were privately managed by 11 firms and accounted 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,

[10] This is the case for most of the empirical evidence based on policy changes in transparency. For example, Albæk et al. (1997) finds that the Danish antitrust authority's decision to publish firm-specific transaction prices of ready-mixed concrete increased prices, Byrne and De Roos (2019) finds that a requirement for petrol stations to commit to prices a day ahead and post those prices to a central, government-operated website, allowed stations to effectively monitor coordination. Byrne et al. (2023) finds that a firm losing access to high-frequency data about rivals' prices softened price competition. Luco (2019) finds that disclosure increased margins in the Chilean retail gasoline industry. The descriptive analysis in Bajari and Yeo (2009) also suggests that limited disclosure rules hindered firms' ability to collude in the US Spectrum Auctions.

[11] We also relate to the literature on cartels' duration (Levenstein and Suslow, 2011), but we do not seek to explain how firms started and built coordination (see, e.g., Byrne and De Roos, 2019), nor the role of firms' heterogeneity in such process (see, e.g., Clark and Houde, 2013; Clark et al., 2024).

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. 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 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*

[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.

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

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] 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 2007/2008, the average bid in the market increased significantly (see Figure A2). The authorities 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 government agency overseeing regulatory compliance by public utility service providers – summoned several generation 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, but did not raise concerns about price coordination.

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]

[16] During our sample period, there was no change in the rules used to calculate the cost-based regulated price (see the Appendix A.6 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.

[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

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 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 Conceptual Framework: A Test for Collusion

3.1 Conceptual Framework

In the next 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 earned from positive reconciliations. For expositional simplicity, we might refer to this behavior as "collusion" and to those units as the (suspected) "cartel", with the understanding that our analysis does *not* establish evidence that would conclusively prove explicit communication or a conspiracy

(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).

[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.

to fix prices in court.[26]

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.[27] 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 when shocks to the transmission network prevent units that won the ideal dispatch from supplying electricity. This implies that, despite the nationwide network for the ideal dispatch, units located nearby compete with each other for positive reconciliations as they are likely eligible simultaneously. Indeed, in 2008, Thermal units in the Atlantic region accounted for only 2.7% of the ideal dispatch but for 42.4% of positive reconciliations. This justifies our focus on the isolated cluster of thermal units in the Atlantic region.[28,29]

Characterizing collusive strategies within a formal repeated-game model is difficult in general and, perhaps, particularly so in our market, and therefore lies beyond the scope of our

[26] The Colombian antitrust law (Law 155/1959, Law 1340/2009, Decree 2153/1992) prohibits any tacit or explicit coordinated actions between competitors that limit market competition. The law does not use the term "collusion" in a formal definition, but rather refers to an exhaustive list of restrictive trade practices.

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

[28] 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.

[29] 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.

analysis. First, even abstracting from capacity constraints considerations and participation in forward markets – important features of the wholesale electricity market – uniform-price auctions display multiple pure-strategy equilibria (Fabra et al., 2006). The model would also need to consider shocks to the network and reconciliations, which, as explained above, play a crucial role in our analysis. Finally, colluding firms may try to devise strategies that reduce the risk of detection. Without making strong assumptions, it would be hard to say much about how firms might coordinate bids over time in our context.

A simple insight common to all repeated-games models, however, is sufficient to derive an intuitive test. We can thus postpone until later a stylized model to guide a "back-of-the-envelope" quantification exercise.

3.2 The Logic of the Test

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

$$\pi(a^c_{it}(\sigma_{it});\sigma_{it}) + \delta\pi(a^c_{it+1}(\sigma_{it+1});\sigma_{it+1}) + ... + \delta^n \mathbf{V}^c_{it+n} \geq \\ \geq \pi(a^d_{it}(\sigma_{it});\sigma_{it}) + \delta\pi(a^p_{it+1}(\sigma_{it+1});\sigma_{it+1}) + ... + \delta^n \mathbf{V}^p_{it+n}, \quad (1)$$

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 collusive behavior are built on a common insight: the *future* value of the relationship – the discounted (expected) difference in the payoffs from cooperation and defection $\mathbf{\Delta}_{it'} = \mathbf{V}^c_{it'} - \mathbf{V}^*_{it'}$ – deters *current* temptations to deviate – the difference in payoff between deviating from the agreement $\pi(a^d_{it}(\sigma_{it}))$ and sticking to it $\pi(a^c_{it}(\sigma_{it}))$. 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 $\pi(a^c_{it}(\sigma_{it}))$ to $\pi(a^d_{it}(\sigma_{it}))$, 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 Behaviour Supported by Future Rewards (Collusion): *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, Anticipatory Responses

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.

4.1 Defining Suspected Cartel Membership

Figure A2 shows a large drop in the average bid around the policy change. While this is consistent with the logic of the test, it is far from conclusive. We do not know the identity of the firms participating in the 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. We thus begin with the first approach and then validate it using the second one.

Our baseline definition assigns *thermal units located in the Atlantic region* to the cartel. The logic for this choice was described above. All but one of the 15 units in the Atlantic region are thermal.[30] The baseline definition yields 14 units in the cartel belonging to 5 firms.[31] 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-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

[30] 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).

[31] 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. Section 4.5 discusses differences in behavior between these different groups of firms.

only suggestive – we now subject our hypothesis to more rigorous testing.

4.2 Bidding behavior around the Announcement Date

Figure 3 shows a sharp drop in the average bid right after the *announcement* date, but *only* for thermal units in the Atlantic region. Their average bid falls by about 43% – 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. We distinguish the announcement and the implementation of the policy, controlling for time-invariant heterogeneity across units and heterogeneous time effects. The baseline specification is given by:

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

where $ln(b_{it})$ is the (log of the) bidding price of unit i at date t, the dummy variable $\mathbb{1}\{Cartel\}_i$ takes value one if i is a thermal unit in the Atlantic region and zero otherwise. The dummy variable $\mathbb{1}\{Announ\}_t$ takes value one if t is a date after the announcement date (January 6th, 2009) and zero otherwise, the dummy variable $\mathbb{1}\{Trnsp\}_t$ takes value one if t is a date after the implementation of the transparency policy (February 6th, 2009) and zero otherwise. λ_i are unit fixed effects and μ_t are date fixed effects, which control for common market conditions (such as demand and input prices). We also explore specifications in which date fixed effects μ_t vary by technology type – i.e., controlling for differential time shocks affecting hydro versus thermal units, or by region. Standard errors are two-way clustered by date and generation unit.

Table 2 presents the results. Across a variety of specifications, we find a statistically significant decrease in bidding prices of cartel units after the policy announcement. Depending on the specification, the estimates range between a drop of 47% and 30%. Column (1) reports results without including any fixed effect. Column (2) controls for unit and date fixed effects and finds identical results. Column (3) allows for the interaction of date-fixed effects with technology-fixed effects. Column (4) instead controls for the interaction of date-fixed effects with regional dummies.[32]

The differential drop in bids is not explained by a differential change in forward positions or production costs for cartel units. Figure A5 shows that there was no change in the ratio of forward contracts over total availability for cartel and non-cartel units around the event dates. Turning to production costs, Figure A6 shows an abrupt fall in the margin

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

($Bid - Mg.Cost$) for cartel units but not for other units. Using margins instead of bids provides qualitatively similar results in the DID analysis (see Table A2).

Interestingly, the coefficient for $\mathbb{1}\{Cartel\}_i \times \mathbb{1}\{Trnsp\}_t$ turns out to be small and statistically insignificant in specifications that control for more potential confounders (in columns (3) and (4)): market transparency did not further change bidding behavior differently between units in the cartel and units outside, once we let time effects vary across technologies. This is consistent with the idea that, by that point, coordinated bidding between thermal units in the Atlantic region had ceased, and their bidding was undistinguishable from that of other thermal units.

Figure 4 reports estimates from a more flexible event-study specification. We extend the baseline specification in equation (2) including interactions between weekly dummies for leads and lags relative to the announcement date and the $Cartel_i$ dummy. First, the specification rules out differences in pre-trends in bidding behavior between units assigned and not assigned to the alleged cartel. Second, the differential drop in bids right after the announcement remains persistent throughout the rest of the sample period.[33]

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, the profits from positive reconciliations (and also total profits) sharply decreased for thermal units in the Atlantic region after the announcement date. Furthermore, Figure A9 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.

4.3 A Validation: Structural Breaks 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

[33] Figure A7 replicates the event study looking at margins, and finds identical results. Figure A8 includes a (placebo) event study for the isolated cluster in the South-West. The estimates suggest that this cluster didn't change bidding 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.

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

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

It is worth emphasizing 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.

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 enforcement. 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 The Role of Public Units

Among thermal units in the Atlantic region at the time of the events, five were managed by the publicly owned firm GECELCA. This raises the question of whether these units also coordinated bids or whether they bid lower prices, disciplining the behavior of neighboring private units.

Table A4 shows that our result (for ease of comparison, in column 1) holds when restricting the cartel definition to include only private units (i.e., assigning all publicly managed units to the control group, in column 2), or when excluding publicly managed units from the sample (column 3). However, we also find a differential drop in bids for publicly managed thermal units in the Atlantic region, relative to other publicly managed thermal units (column 4). That is, publicly managed units also reacted to the announcement by reducing bids, which suggests that they likely also coordinated bids – albeit to a lesser extent.

Two considerations help in interpreting these results. First, Suárez (2022*a*) documents that public units also respond to market power incentives in the Colombian market, albeit less than private ones. Second, 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).

4.6 Discussion

We have shown that thermal units in the Atlantic region decreased bids in a coordinated way after the announcement date, but before the actual implementation of the transparency reform. Furthermore, Figures 6 and 7 suggest that this reaction is unlikely to have stemmed entirely from an anticipated threat of enforcement. It thus appears plausible that bidding behavior changed at least in part in anticipation of a reform that would have made coordination harder to sustain.

The evidence from our test does not identify the exact equilibrium played nor the type of communication – tacit or explicit – that sustained it. In particular, the evidence does not prove that suspected cartel members were playing a subgame perfect equilibrium (SPE) of a repeated game, nor that all units perfectly anticipated that, under the new transparency rules, the dynamic enforcement constraints would be violated. SPE relies on anticipatory behavior – which is in line with our evidence – but also on optimization and correct beliefs on- and off- the equilibrium path, about which our evidence is essentially mute. Our evidence reveals an anticipatory and coordinated response, suggesting that dynamic enforcement considerations can be taken quite seriously in the fight against anti-competitive conduct.

Collusive arrangements are complex: even when members can explicitly communicate, successful collusion requires a mutual understanding of many elements of the agreement (Harrington, 2008; Genesove and Mullin, 2001). It is thus highly implausible that, following the announcement, all units in the cartel immediately reacted in an *anticipatory* way to the uncertain prospect of a less transparent market regime in the future. More likely, many units might have reacted in an *adaptive* way to the unexpected behavior of those units that lowered their bids first (see Figure 5). Interestingly, the first units that decreased prices were those belonging to EMGESA – the largest firm among the collusive firms. This is potentially consistent with evidence from other contexts in which larger firms are more sophisticated bidders (Hortacsu et al., 2019) and/or tend to take on the role of leaders that coordinate in asymmetric cartels (see Byrne and De Roos, 2019; Clark et al., 2024, for examples). Other than this, we find no other characteristic that correlates with the ordering in which units decreased their bids.[34]

5 Further Evidence of Coordinated Bahaviour

While the evidence in the previous section is consistent with collusion, at least in a game-theoretic sense, successful prosecution typically requires evidence of explicit coordination and communication. This section provides suggestive evidence that thermal units in the Atlantic region might have indeed coordinated actions in a way that would have been difficult to achieve without communication, and some suggestive evidence that such communication might have taken place. These exercises do not, and are not meant to, establish hard evidence of explicit communication that would lead to conviction in a court. 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 sometimes increase bids in the day-ahead auction precisely when other cartel units declare unavailable immediately prior to the real operation after winning in the day-ahead auction. 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

[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).

revealed on day $t+1$. This correlation in behavior only appears among cartel units and vanishes after the cartel's demise. Note that we do not argue that this particular behavior played a quantitatively important role in generating extra profits for cartel members. On the contrary, this coordination must be quite rare as it requires units to declare unavailability 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. The anomalous behavior, 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 A10 shows that this is indeed the case. The top-right panel of the Figure then reports the likelihood that a unit declares unavailable upon winning in the ideal dispatch. This is larger for cartel units than non-cartel units – the second ingredient. Finally, the bottom panel shows that the probability that a cartel unit receives a positive reconciliation when another cartel unit wins in the ideal dispatch is much higher than when a non-cartel unit wins.

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

We regress the average bid of j's friend(s) on a dummy indicating whether unit j's production availability is below the ideal generation quantity awarded. We repeat the estimation separately for different years, and separately for j 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.

[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.

The similarly isolated cluster of units in the South-West – our placebo – does not feature coordinated behavior throughout the entire sample period.[36]

5.2 Suggestive Evidence of Communication

Suspected cartel members thus coordinated some actions in a way that might have required communication. Of course, the cartel might have also communicated to coordinate in other ways that we haven't detected. We now provide further evidence that is at least consistent with the hypothesis that communication might have occurred in practice.

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 framework, we test whether there was any differential change in attendance between the cartel and non-cartel firms before and after the policy change.[37]

Since firms typically send only one attendee per meeting (if any at all), we focus on two dependent variables: a dummy that takes the value of 1 if the firm sends someone to the meeting and a dummy that takes the value of 1 if the firm sends someone from the commercial area to the meeting. We also explore the composition of attendees conditional on sending someone to the meeting. We categorize participants as working in the commercial area if, at the time of the meeting, their CV (accessed through websites such as LinkedIn, newspapers and industry publications) mentioned the words *commercial* or *marketing* in their job titles.[38] 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

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

[37] 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.

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

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.

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

This section presents a quantification exercise. Unlike our previous analysis, this requires committing to a specific model and, inevitably, simplifying assumptions. We focus on thermal units and the reconciliations market and estimate 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 ($Rank_{it}^2$), time (δ_t), and unit (γ_i) fixed effects which capture Q_t^+ and a unit's location in the network. We estimate

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

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

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

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

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

We focus on a one-year period around the policy change and estimate Equations (4) and (5) separately for suspected cartel units and the comparison group to reflect the geographic

segmentation of the reconciliations market.[39] 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.[40] 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 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 – as a sanity check – 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.[41] 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

[39] Figure A12 shows that the model fits well the distribution of $q^*_{i,t}$ in both groups.

[40] 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.

[41] 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.

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 optimal deviation, and the competitive continuation.

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

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

To circumvent this challenge, we extrapolate unobserved bidding strategies using a regression approach. The approach is discussed in detail in Appendix A.4 but, in a nutshell, it works as follows. We regress collusive bids, $b^C_{i,t}$, on production costs, demand, and unit fixed effects using observations from the *pre-reform* period. Given these estimates, we extrapolate collusive bids using the value of the covariates from the *post-reform* period. With these bids at hand, we use (3), (4), and (5) to compute $\pi_{i,t}(b^C_{i,t}; b^C_{-i,t})$ and $\pi_{i,t}(b^D_{i,t}; b^C_{-i,t})$ for the post-reform dates. Symmetrically, we extrapolate $b^*_{i,t}$ from the *post-reform* period to the *pre-reform* one. Conditional on those, we use (3), (4), and (5) to compute $\pi_{i,t}(b^*_{i,t}; b^*_{-i,t})$ for the pre-reform dates.[42]

The left panel of Figure 10 presents the results. The Figure reports the *smallest* incentive to collude across cartel units. The lines report estimates using the baseline discount factor, calibrated to match the 15.1% interest rate. For robustness, the shaded areas present bounds

[42]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 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.[43] It is worth iterating that this exercise doesn't prove that the change in transparency rules led to the cartel's demise. However, coupled with Section 6.2, the results 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

Using the counterfactual estimates of bids and quantities described above, we provide a back-of-the-envelope estimate of the additional costs generated by the cartel. It is worth emphasizing that this estimate is not meant to reflect the cost associated with transparency, but rather the cost of the coordinated behavior observed in the data up to January 6, 2009.

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 that $\sum_i q^+_{i,t}(b^C_{i,t}, b^C_{-i,t}) = \sum_i q^+_{i,t}(b^*_{i,t}, b^*_{-i,t}) = Q^+_t$. It is worth noting that our estimate likely provides a lower bound (see Appendix A.5 for details). 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. 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. Finally, our calculation does not account for the downstream adverse effects of higher

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

electricity prices, e.g, on firms' output and productivity growth (Abeberese, 2017).

The cartel generated an additional cost of at least 11 billion COP per month. This corresponds to an increase of around 12% relative to the counterfactual scenario without the cartel (right panel of Figure 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%. Around 10 million households lived in Colombia in 2008. If all the energy allocated via positive reconciliations is bought by households, and assuming a full pass-through of the cost increase to consumers, the average household paid 1,100 COP in excess per month in the second semester of 2008 due to the collusive agreement (with many household living with less than a minimum wage of 461.500 COP).

7 Policy 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 and policy-makers fighting collusion.

Our analysis has policy implications for market design – including energy markets – particularly in developing countries. The Colombia case provides an interesting example because the country's energy sector was successfully reformed in the nineties and is generally considered one of the best-designed and regulated markets among developing countries (World Bank, 2019). Collusive behavior might be more widespread and induce larger distortions in less well-designed energy markets.

In our context, the Colombian regulator lacked sufficient evidence to open targeted investigations and attempt prosecution. This induced the regulator to instead alter the market design in the hope of hindering (potential) collusive practices. Changes in market design, however, can be costly. For example, in our context, market transparency facilitates the efficient inter-temporal allocation of scarce water resources. The fact that at least some suspected cartel members reacted in an anticipatory way raises the possibility that regulators might be able to strategically use *announcements* to induce behavioral responses and acquire sufficient evidence to open investigations and attempt prosecution (Chassang and Ortner, 2023). Prima facie, the general applicability of our test for collusion may appear limited by researchers' and regulators' ability to predict which policies shift future values when firms collude. Our testing strategy might thus be subject to a potentially large number of results where collusion is present, and yet the test fails to detect it. However, the applicability of our

test might significantly expand when combined with the logic of investigation shocks. For example, the announcement of market studies – that are in the powers of some competition authorities such as the CMA in the UK – could, at least in principle, lead to changes in behavior – e.g., participation in leniency programs – that could contribute to the acquisition of evidence to attempt prosecution. Investigating this possibility merits further theoretical and empirical scrutiny, in the spirit of Ortner et al. (2022).

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

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8 Figures

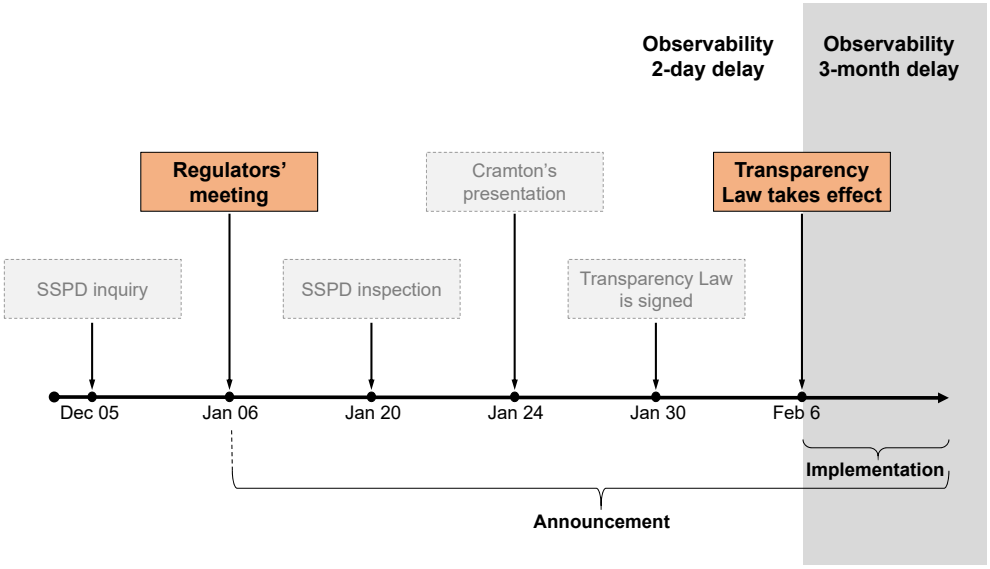

Figure 1: Timeline.

Note: 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.

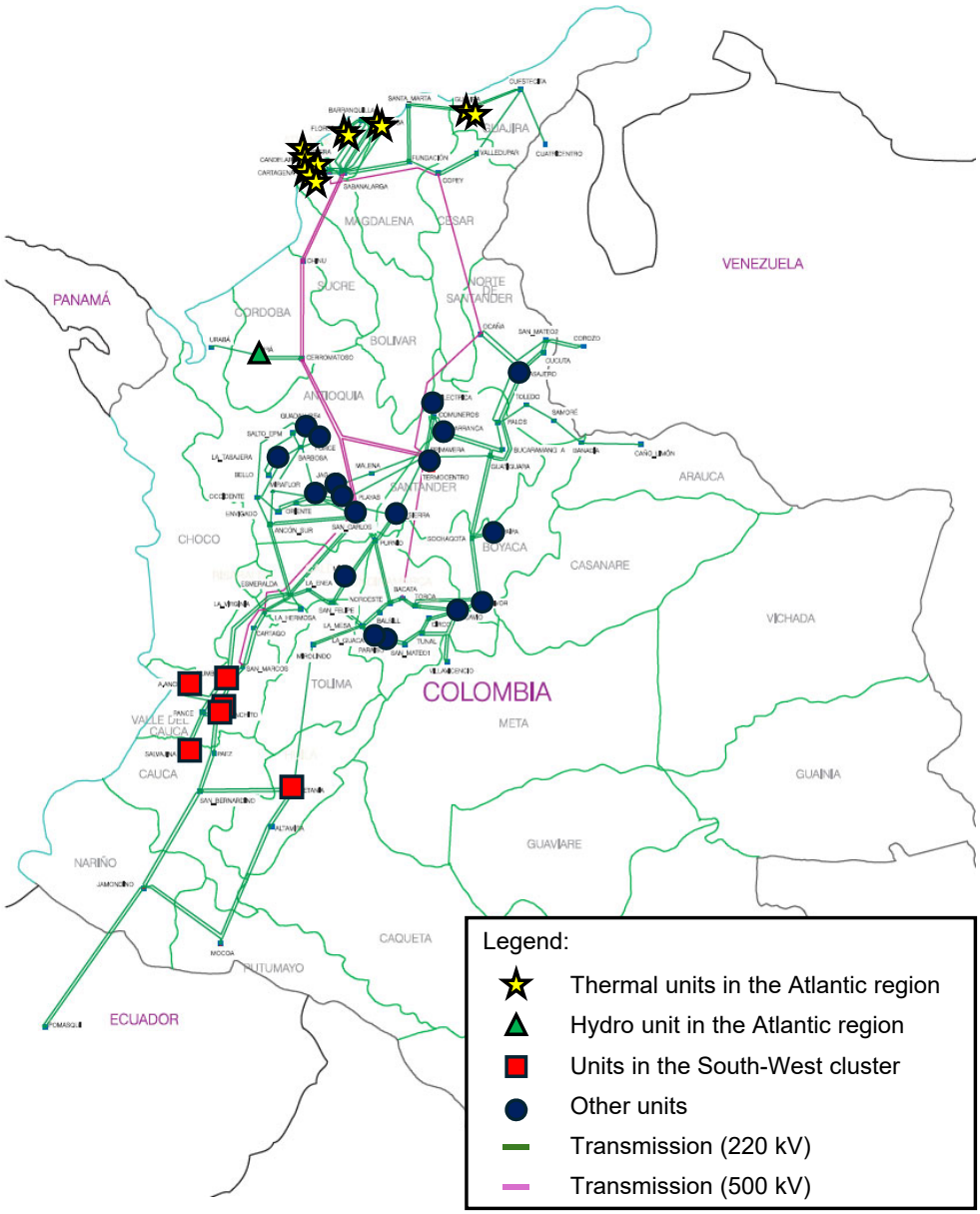

Figure 2: Map.

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.

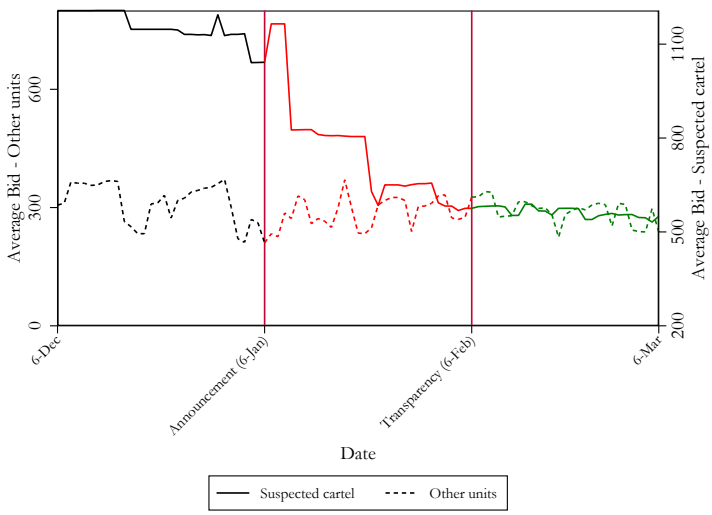

Figure 3: The main fact: Cartel and non-cartel bids.

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.

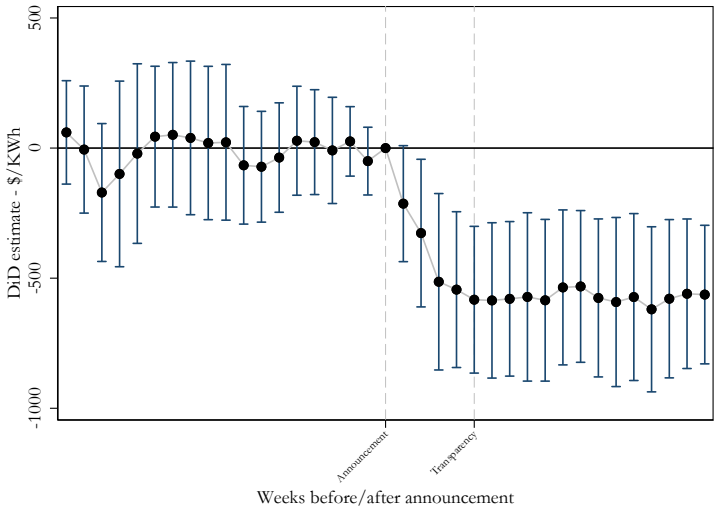

Figure 4: Event study estimates: Bids.

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.

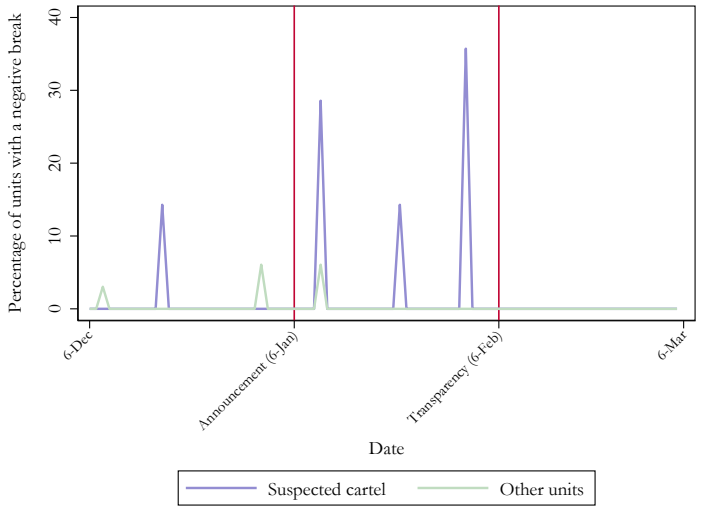

Figure 5: Test for structural breaks in the time series of bids.

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.

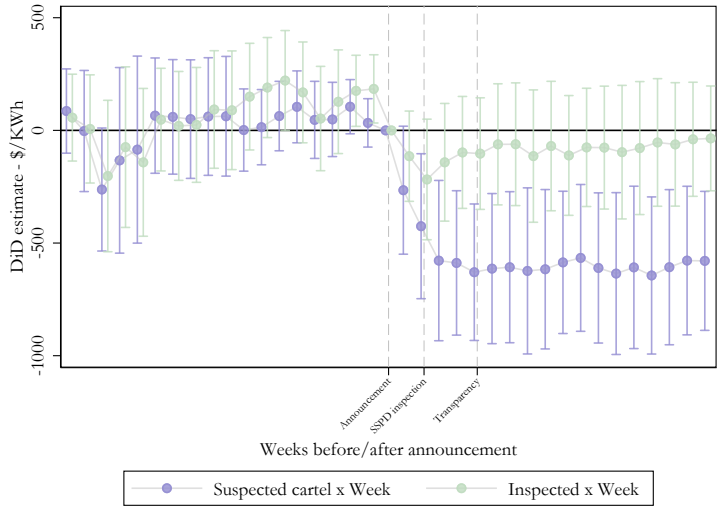

Figure 6: Event study estimates: SSPD inspection.

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. The x-axis represents weeks around the main events. 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, of the SSPD inspection, and of the implementation.

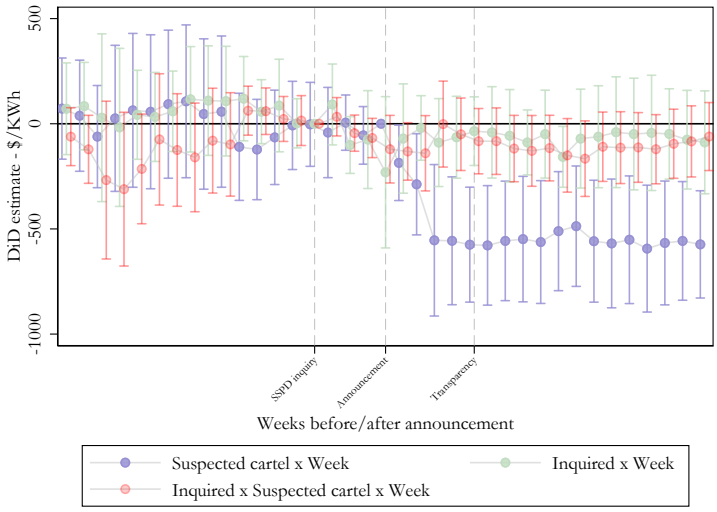

Figure 7: Event study estimates: SSPD inquiry.

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 event study model by adding an event interaction for this inquiry, split between cartel and non-cartel units. Robust s.e. are clustered by unit and date. The x-axis represents weeks around the main events. 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 SSPD inquiry, the announcement, and of the implementation.

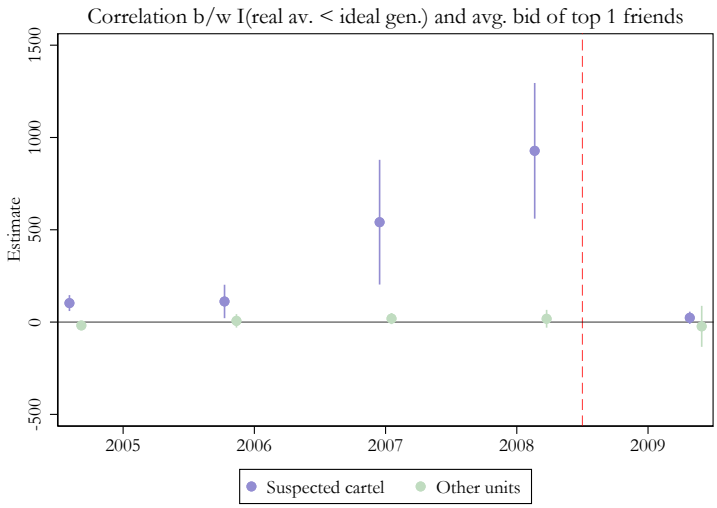

Figure 8: Bids coordination.

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 only include in the explanatory dummy the 75% cases where the difference between availability and ideal generation is the largest. We run separate regressions for the two groups (cartel, others) 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.

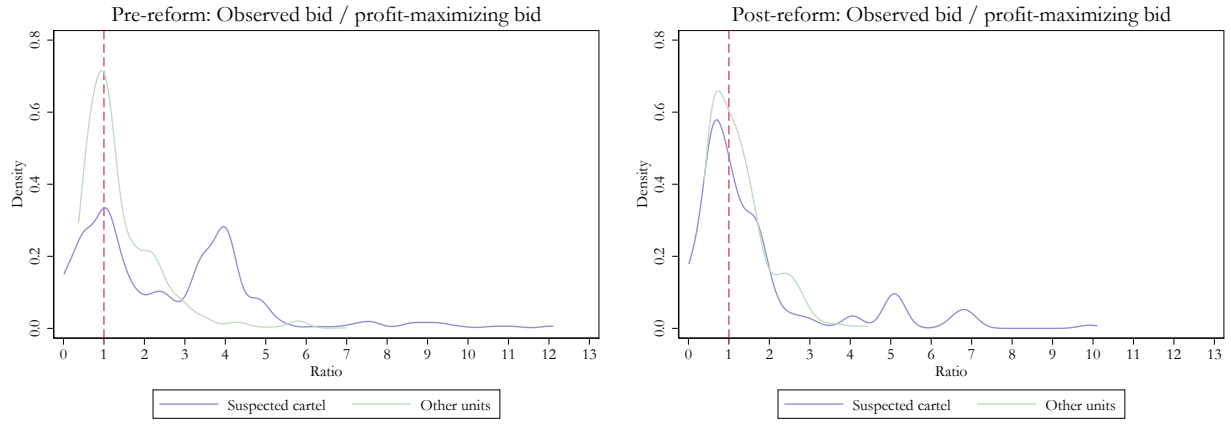

Figure 9: Distribution of the ratio of observed bids to profit-maximizing bids.

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.

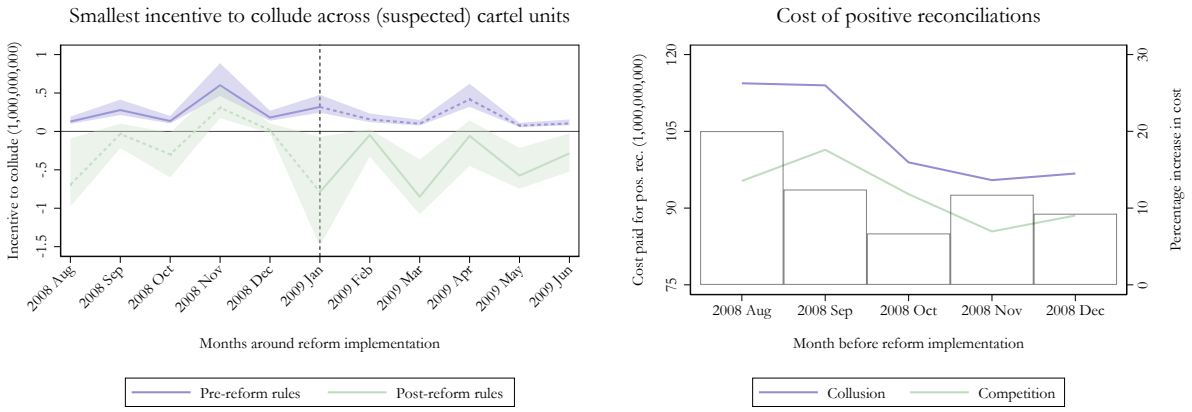

Figure 10: Incentives to collude and the cost of the cartel.

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

			Before 06/01/2009				
	Suspected cartel			Other units			
Variable(Unit)	Obs	Mean	SD	Obs	Mean	SD	T-Test
Bid(COP/KWh)	2212	1213.57	714.17	5214	362.06	557.36	49.99
Ratio forward contracts/availability(Percentage)	2212	0.27	0.25	5046	0.67	1.17	-23.11
Probability positive reconciliation(probability)	2212	0.13	0.31	5214	0.24	0.34	-13.50
Average Positive reconciliation(KWh)	2212	22702.29	76145.57	5214	10127.97	29856.41	7.53
Revenue from Positive reconciliation(Millions COP)	2212	107.76	347.30	5214	17.87	53.33	12.11
Average Availability(KW)	2212	126946.42	164209.28	5214	282285.07	299716.71	-28.64
Estimated Marginal Cost(COP/KWh)	2212	113.22	19.07	5214	60.55	63.09	54.69
			After 06/01/2009				
	Suspected cartel			Other units			
Variable(Unit)	Obs	Difference	T-Test	Obs	Difference	T-Test	T-Test
Bid(COP/KWh)	2898	-631.84	35.70	6831	-73.00	7.65	27.38
Ratio forward contracts/availability(Percentage)	2898	0.01	-1.35	6799	0.26	-9.43	-29.03
Probability positive reconciliation(probability)	2898	0.01	-1.39	6831	0.04	-5.47	-18.23
Average Positive reconciliation(KWh)	2898	-3157.89	1.51	6831	2263.29	-3.64	5.12
Revenue from Positive reconciliation(Millions COP)	2898	-36.15	4.10	6831	5.94	-5.17	9.75
Average Availability(KW)	2898	8199.63	-1.71	6831	731.54	-0.13	-30.20
Estimated Marginal Cost(COP/KWh)	2898	-27.14	50.93	6831	-7.95	7.22	44.44

Table 1: Descriptive statistics.

Note: Descriptive statistics for the suspected cartel (columns 2 to 4) and non-cartel units (columns 5 to 7) for the periods before (top panel) and after the announcement date (bottom panel). The pre-period includes observations from August 1, 2008 to January 6, 2009, and the post-period from January 6, 2009 to July 31, 2009.

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

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

Table 2: Difference-in-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. In columns 3-4 we further control for forward contracts over total capacity and alternatively for Date × Technology FE or for Date × Region FE. Regions are Atlantic, North-West, Central, and South-West. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) Dummy for PR	(2) Profits from PR	(3) Total profits
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
Date x Technology FE	NO	NO	NO
Forward Contracts	NO	NO	NO

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

Table 3: Profits after the announcement.

Note: The table presents differences-in-differences estimates for various outcomes controlling for unit and time-fixed effects, where the Post period refers to the period after the policy announcement. Column 1 presents the estimates for the probability of receiving positive reconciliations. Column 2 presents the estimates for the profits from positive reconciliations, conditional on receiving some positive reconciliations. Column 3 presents the estimates for the total profits (unconditional). Profits are measured in 1,000,000 of Colombian Peso. Robust s.e. clustered by unit and date in parenthesis.

	No break	Break	Total
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.

	(1)	(2)	(3)
VARIABLES	Someone	Someone Commercial	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 to sending someone to the meetings. Robust s.e. clustered by firm and date in parenthesis.

A For-Online-Publication Appendix

A.1 Data

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

The second dataset provides time-varying marginal costs for each generation unit. To construct them, we follow a standard engineering methodology (Green and Newbery, 1992; Wolfram, 1998, 1999; Wolak, 2000; Fabra and Reguant, 2014) that uses technical specifications of each generation unit (i.e. heat rate), fuel prices and transportation costs (see Appendix A.6 for details about calculations and data sources).

Finally, we hand-collected data from the minutes of the meetings of the Association of Generating Units (CNO in Spanish).[1] We first download the minutes and type the name of each attendee in an excel file. Then, we give the excel file to two different RAs to complete the occupation. They searched for the CV of the attendees of these meetings through Linkedin and other web sources. We were particularly interested to know if attendees had a job position in the commercial area, and therefore were likely to be directly involved in setting bids at the time of the meeting. The great majority of information collected was uniform across RAs. In case of discrepancies, the authors took 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 A14 shows that results are robust if we *ex*clude EMGESA units, and if we *in*clude all EMGESA units. It further shows that our results are unlikely to be driven by chance (Placebo). To conduct the placebo exercise, we randomly allocate some of the units to the placebo cartel and the rest to the control group. In doing so, we keep the same proportion of cartel and non-cartel units as is in our baseline definition (14/47). We repeat this procedure 1,000 times and report the mean of the effect across repetitions along with confidence intervals constructed with the standard deviation across repetitions.

Figure A14 presents the results and shows two main patterns. First, excluding or 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 A15 shows the event study for these four definitions. For all of them, the level of the coefficients after the announcement is lower than before the announcement. In particular, for all definitions, there is a sharp and discontinuous drop in the coefficients right after the announcement date.

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

A.4 Details on Extrapolating Bids

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

[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 $t-1$ $\ln\left(Q^{+}_{t-1}\right)$, and the logarithm of the demand of ideal generation $\ln\left(Q_t\right)$ – the two exogenous quantities known at the time of submitting bids.

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

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 A16 plots the distributions of in-sample predictions versus observed bids.

A.5 Details on the Cost of the Cartel

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

In practice, we multiply the bids and amounts constructed to calibrate the enforcement constraints and then sum over units. We aggregate costs at the monthly level and present the results in the right panel of Figure 10.

A.6 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

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

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, 2003a,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 *MBTU* units, multiplying for a calorific value of the Colombian thermal coal of 1,370 btu per pound GAR (Source: regulation 2009 180507 Colombian Ministry of Energy and Mines (Ministerio de Minas y Energía , MME).[6] For computing the coal transportation costs, an importation parity approach is adopted. According to this criteria, we estimate it as the road freight transportation fee from the closest importation port to the respective location of the generation unit. These fees were extracted from the system of information of efficient costs for road freight transportation provided by the Transportation Ministry of Colombia.[7]

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

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

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

[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).

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.

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A.7 Appendix Figures

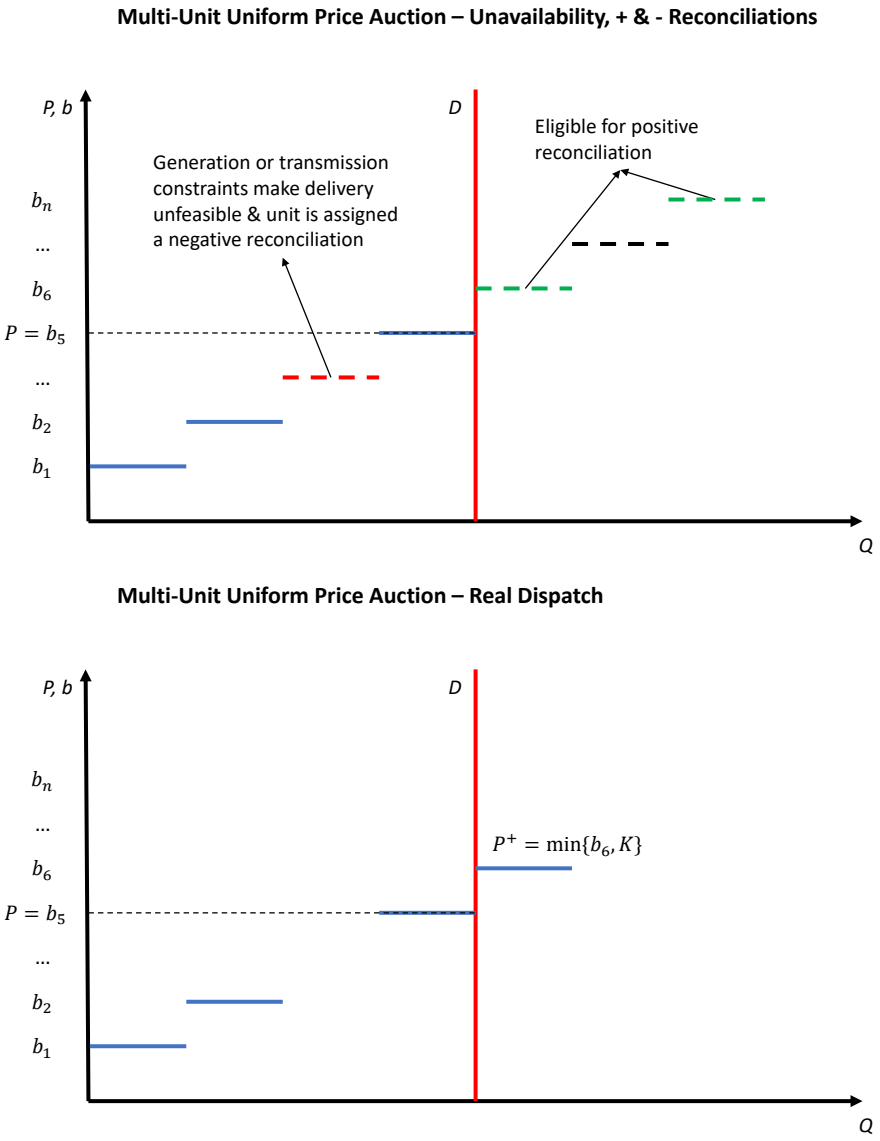

Figure A1: Uniform price auction, ideal dispatch, and reconciliations.

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

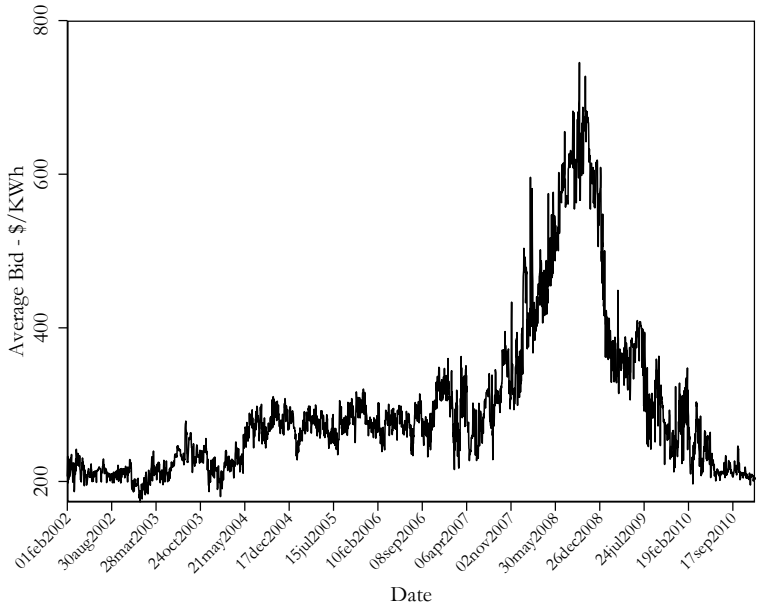

Figure A2: Average bid time series.
Note: Daily average bid from 2002 to 2010 in the Colombian wholesale electricity market.

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.

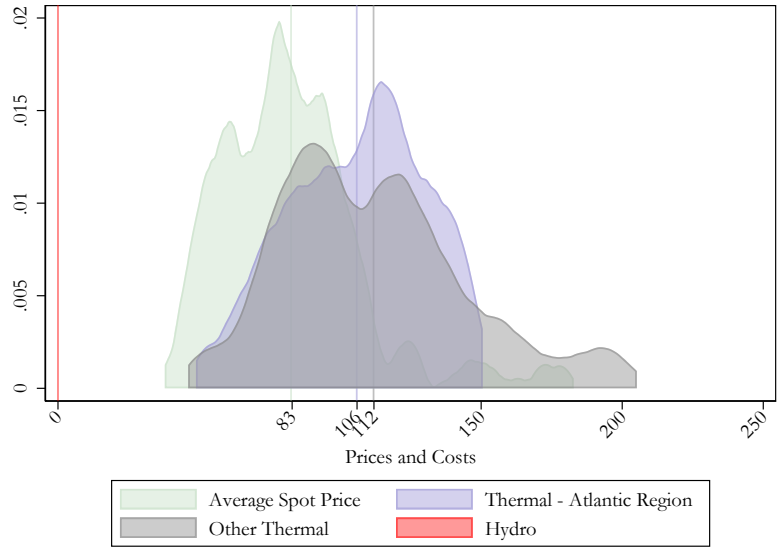

Figure A4: Spot price and production costs.

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.

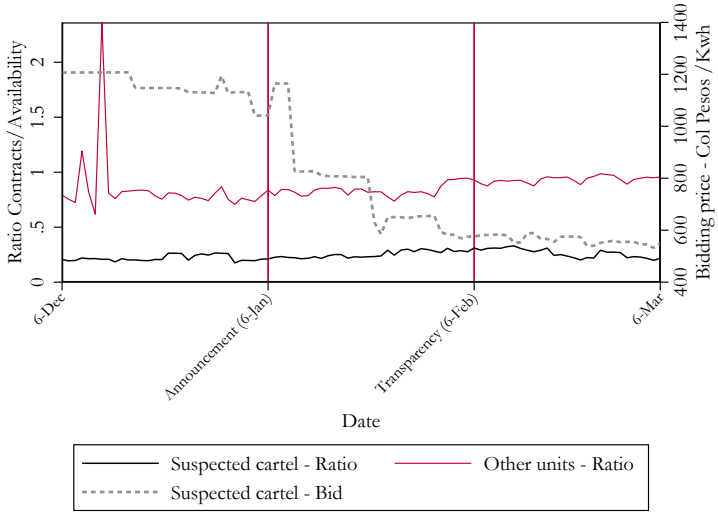

Figure A5: Forward contracts.

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).

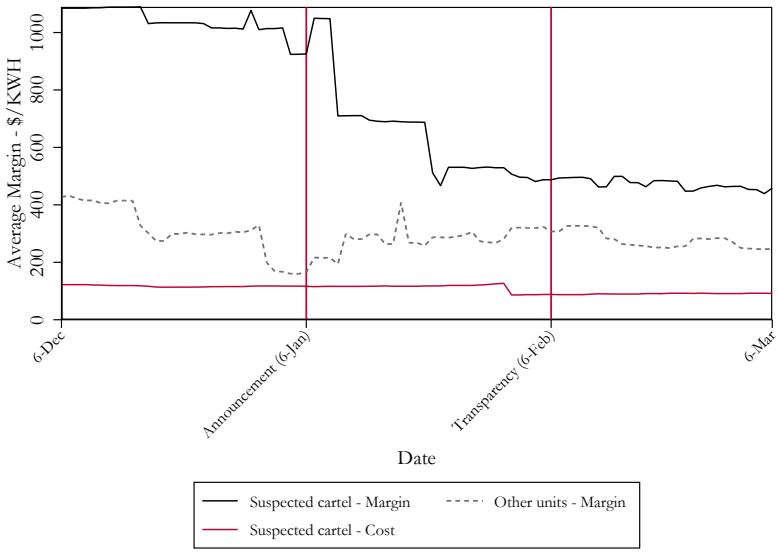

Figure A6: Average margin.

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

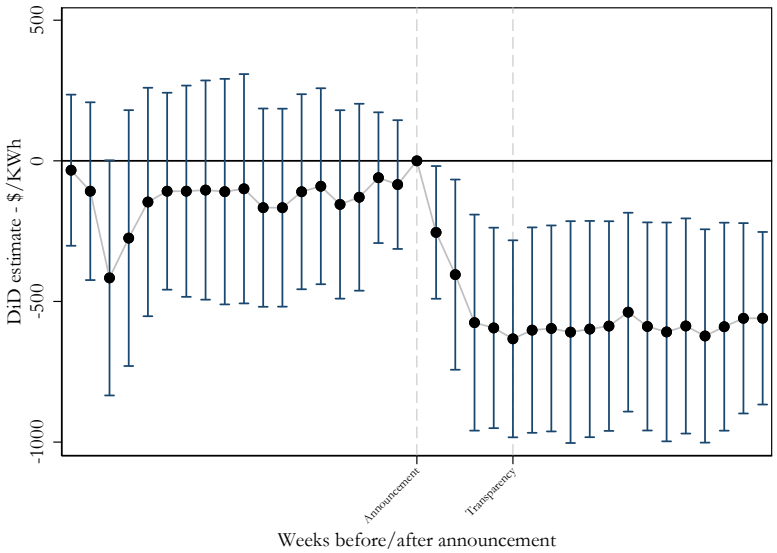

Figure A7: Event study estimates for margin.

Note: Event study estimates using margin (bid minus cost) as the dependent variable, controlling for unit and date fixed effects. 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.

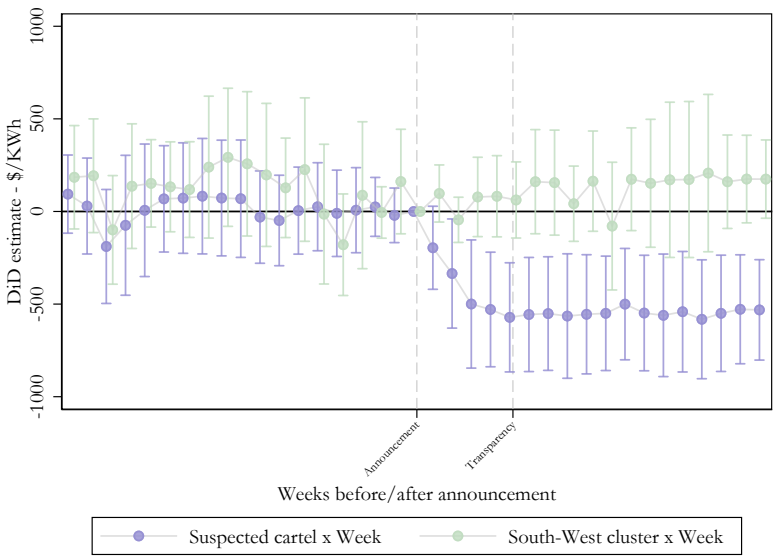

Figure A8: Event study estimates for units in the South-West cluster.

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.

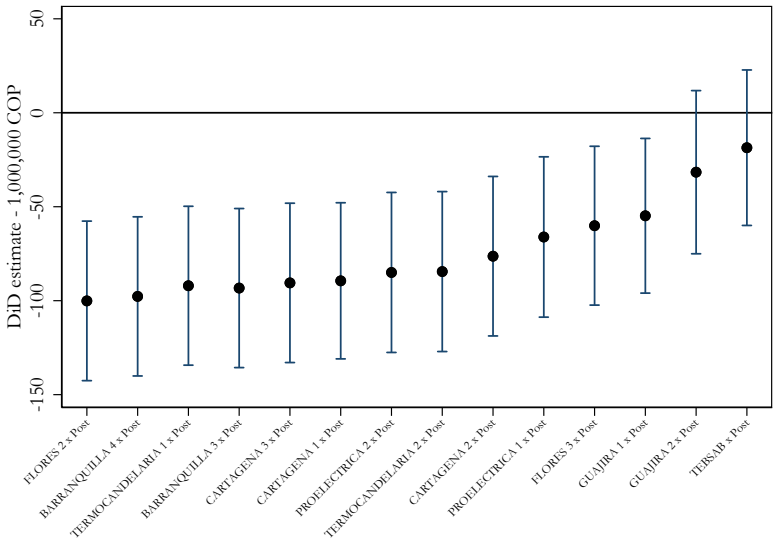

Figure A9: Profits of each unit in the suspected cartel.

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.

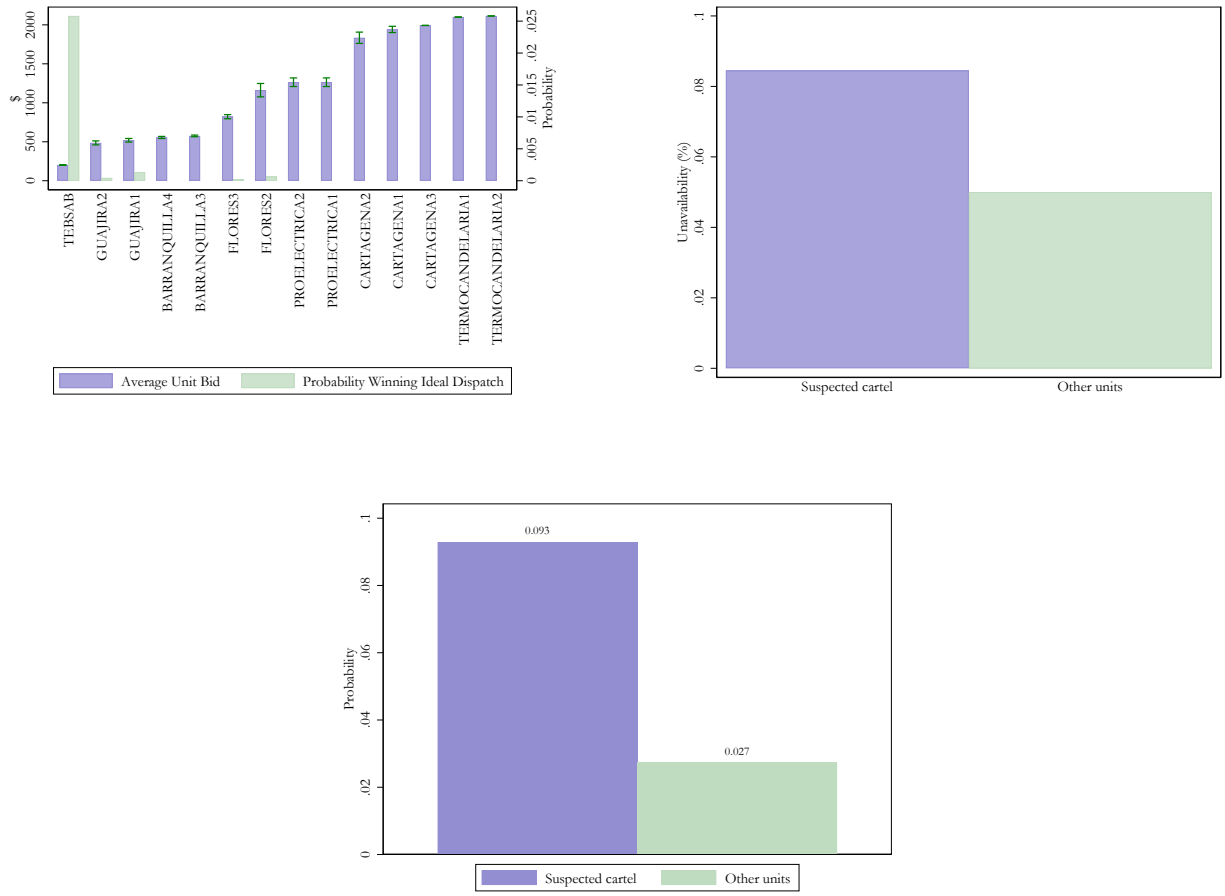

Figure A10: Probability of winning, of unavailability, and of reconciliations.

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.

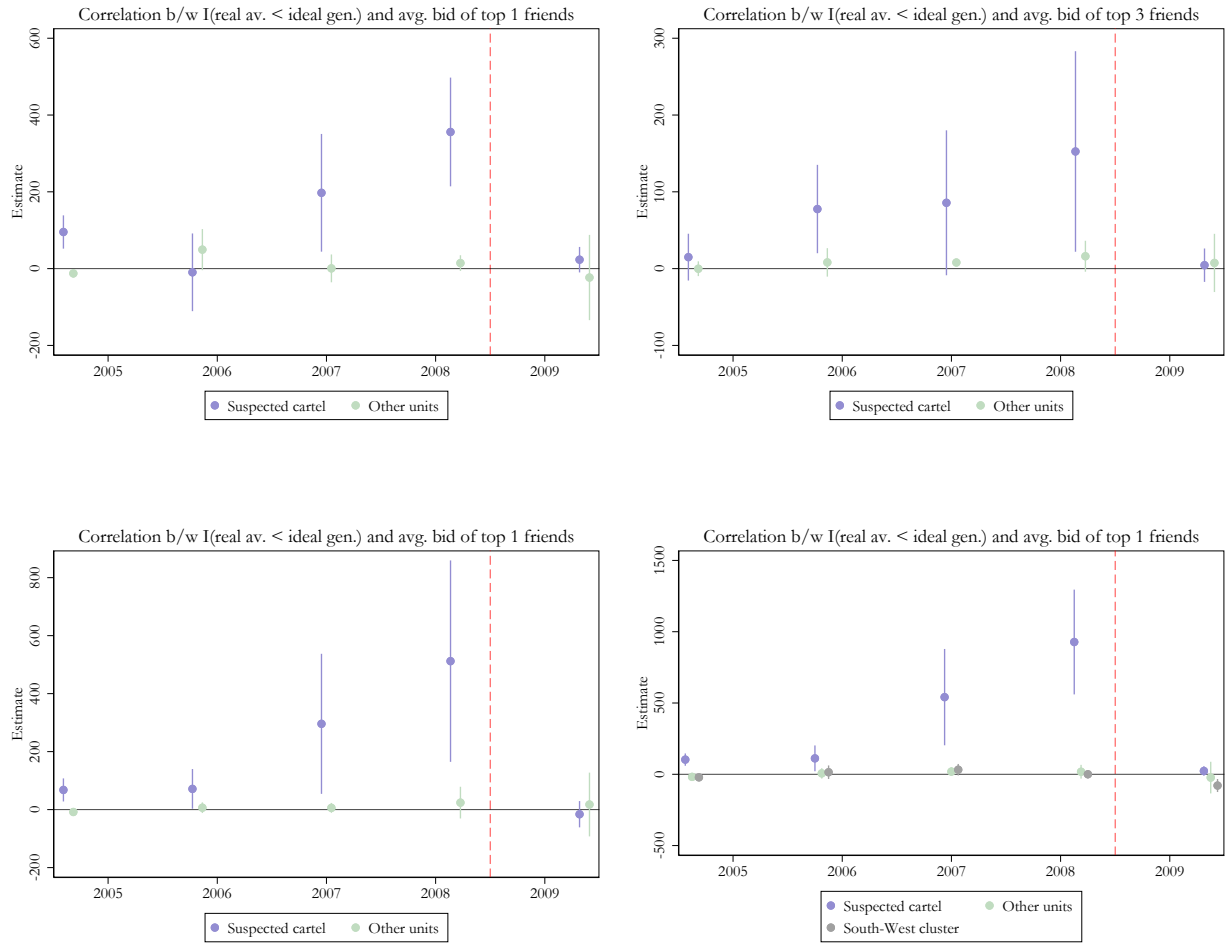

Figure A11: Robustness: Bids coordination.

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.

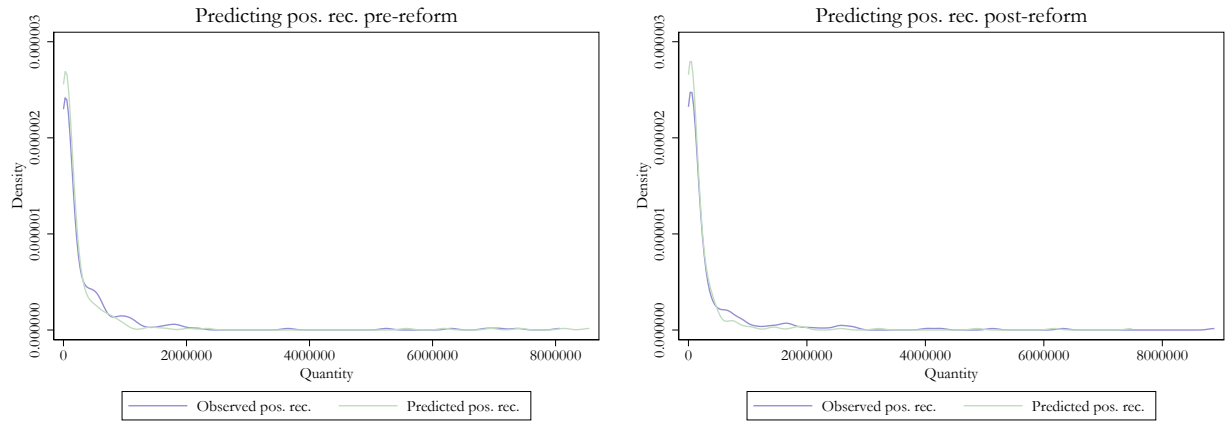

Figure A12: Distribution of observed quantities of positive reconciliations and in-sample predictions.

Note: We estimate how the quantity of positive reconciliation awarded to a unit depends on the rank of its bid, as in (4) and (5). We use 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.

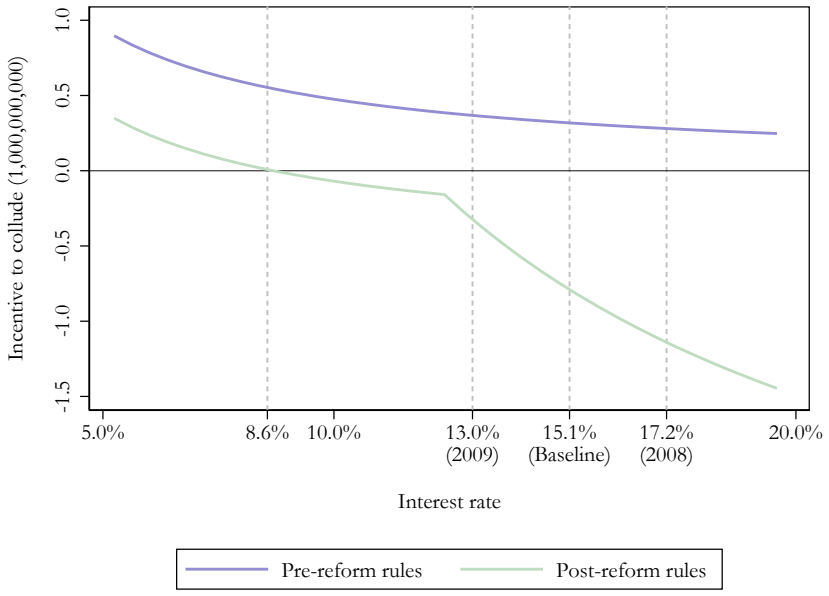

Figure A13: Cartel sustainability for different interest rates.

Note: The figure reports the smallest incentive to collude across cartel units, as computed in January 2009, for different values of the interest rate (and thus of the discount factor). The vertical lines refer to the interest rate used in our baseline exercise (15.1%, see Figure 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%).

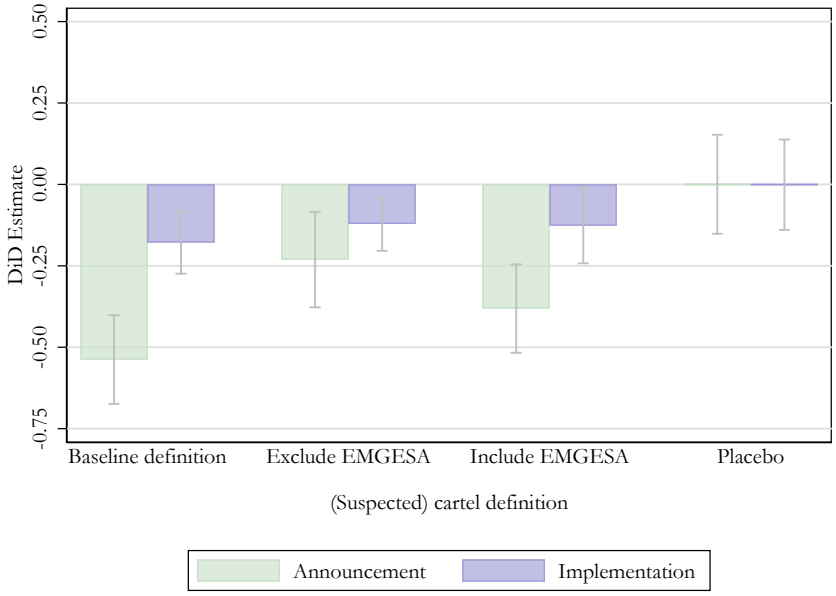

Figure A14: Robustness of cartel definition and placebo.

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.

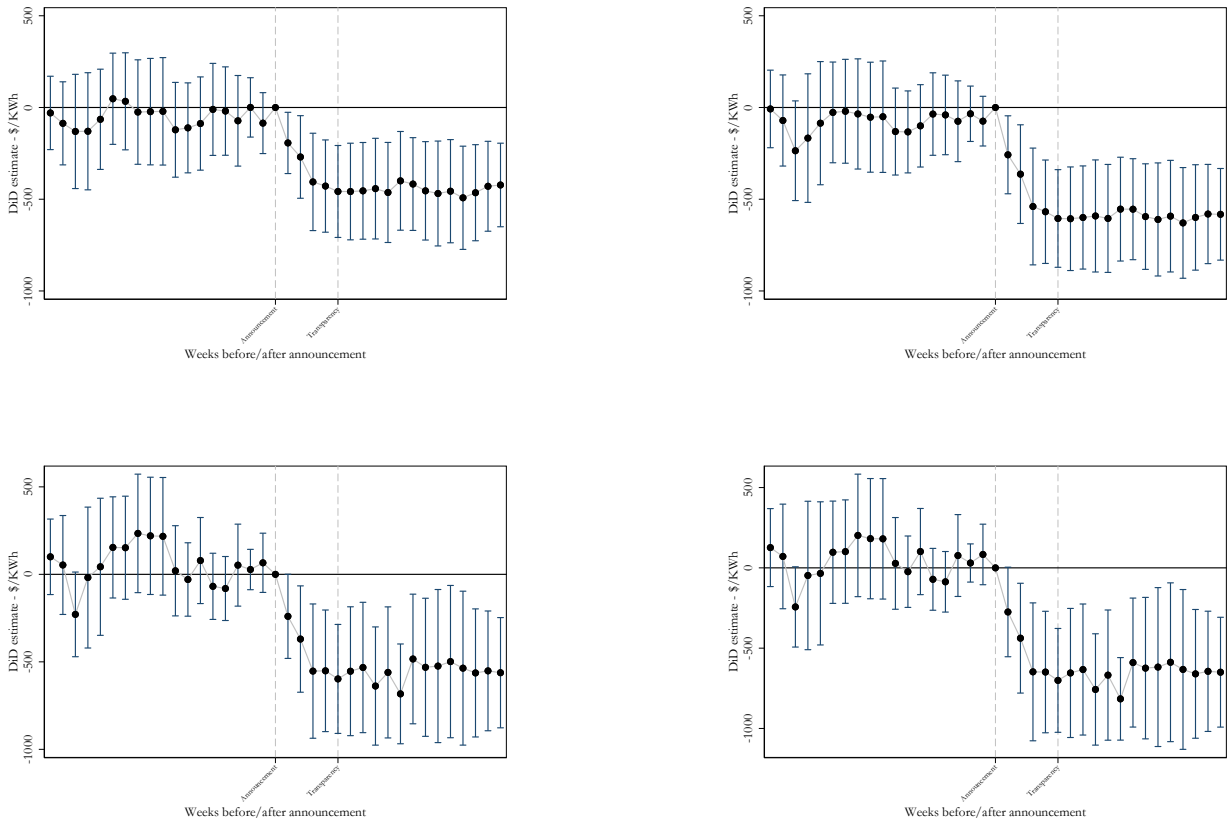

Figure A15: Event study estimates for alternative cartel definitions.

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

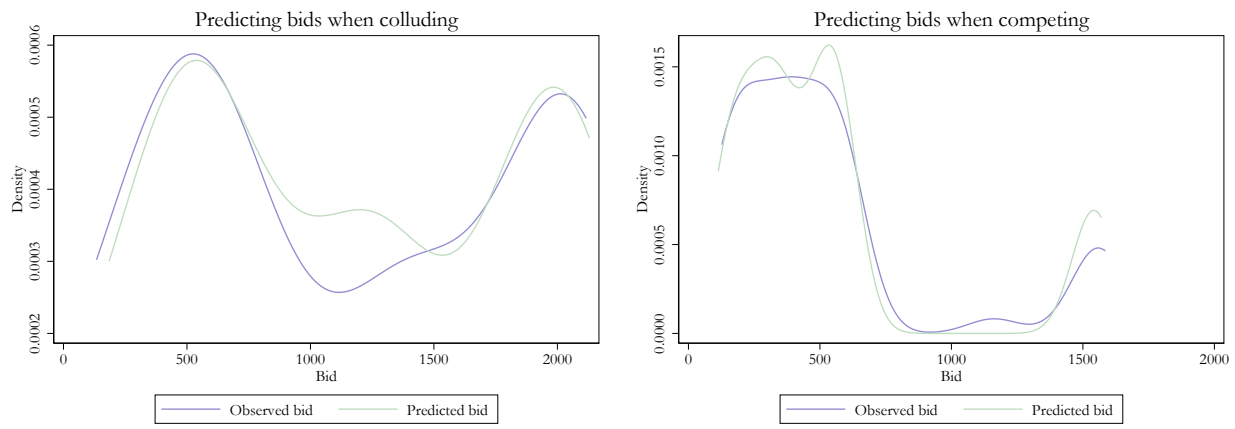

Figure A16: Distribution of observed bids and in-sample predictions.

Note: We estimate how cartel units set bids by regressing bids on costs, the lagged logarithm of the total amount of positive reconciliations, and the logarithm of the ideal generation quantity as in (A1). We use the resulting estimates to make in-sample prediction and average at the monthly level for each unit. In the figure, we compare the density of the average predicted bid (green line) with the density of the average observed one (purple line). The left (right) figure refers to observations from the six months before (after) the reform.

A.8 Appendix Tables

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

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

Note: The table presents the instrumental variables 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. 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
Suspected cartel x Announcement	-320.52**	-320.52**	-308.62**	-454.11***
	(125.83)	(130.48)	(130.39)	(116.92)
Suspected cartel x Implementation	-146.57***	-146.57**	-146.40**	-145.18***
	(47.66)	(57.51)	(56.76)	(35.37)
Announcement	-130.83***			
	(41.96)			
Implementation	-33.78			
	(24.93)			
Observations	11,315	11,315	16,955	16,955
R-squared	0.23	0.82	0.81	0.82
Unit FE	NO	YES	YES	YES
Date FE	NO	YES	N/A	N/A
Date x Technology FE	NO	NO	YES	NO
Date x Region FE	NO	NO	NO	YES
Forward Contracts	NO	NO	YES	YES

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

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

Note: Difference-in-differences estimates for margin (bid minus cost). Only thermal units are included in the sample. In columns 3 and 4 we further control for forward contracts over total capacity and alternatively for Date × Technology FE or for Date × Region FE. Regions are Atlantic, North-West, Central and South-West. Robust s.e. clustered by unit and date in parenthesis.

VARIABLES	(1) LnBid	(2) LnBid	(3) LnBid
Suspected cartel x Announcement	-0.39***	-0.39***	-0.36**
	(0.12)	(0.13)	(0.14)
Suspected cartel x Implementation	-0.03	-0.03	-0.03
	(0.12)	(0.13)	(0.13)
Announcement	-0.16***		
	(0.05)		
Implementation	-0.27**		
	(0.11)		
Observations	11,315	11,315	11,115
R-squared	0.25	0.83	0.83
Unit FE	NO	YES	YES
Date FE	NO	YES	N/A
Date x Technology FE	NO	NO	YES
Date x Region FE	NO	NO	NO
Forward Contracts	NO	NO	YES
Sample	Thermal units	Thermal units	Thermal units

Robust standard errors in parentheses

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

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

Note: The table presents the estimation results when we exclude hydro units from the sample of analysis. Note that we cannot estimate the relevant coefficients if we include Date × Region FE and exclude hydro units, because the cartel definition would coincide with being in the Atltantic region.

	(1)	(2)	(3)	(4)
VARIABLES	LnBid	LnBid	LnBid	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
Date x Technology FE	NO	NO	NO	NO
Date x Region FE	NO	NO	NO	NO
Forward Contracts	NO	NO	NO	NO
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.

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

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

Table A6: DiD estimates with alternative cartel definitions.

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

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

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

Table 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.