A Field Experiment on Antitrust Compliance

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Abstract

We study the effectiveness of firms' compliance programs by conducting a field experiment in which we disclose to a random subset of Japanese firms evidence of illegal bid-rigging. We track the bidding behavior of the treated firms and how they compare with the control group. We find that firms change their bidding behavior: our test of bid-rigging fails to reject the null of competition when applied to the bidding data of the treated firms after the intervention. We find evidence that these changes are not the result of firms ceasing to collude, however. Our findings instead suggest that firms continue to collude even after the intervention and that the changes in the bidding behavior we document are the result of active concealment of evidence by cartelizing firms.

Keywords: Regulatory Compliance, Scoring Auctions, Collusion.

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1 Introduction

In many policy areas, there is an increasing trend towards delegating the day-to-day monitoring and enforcement of regulations to the the regulated firms themselves (Sigler and Murphy, 1988, Ayres and Braithwaite, 1995). This trend reflects the rapid growth in the scope, scale and complexity of government regulations without commensurate growth in regulatory resources.[1] While regulatory agencies retain the authority to investigate and intervene, this authority is exercised when firms exhibit repeated noncompliance or when firms' compliance functions are deemed ineffective. Thus, regulatory regimes often take the form of a hierarchical structure in which the firm's compliance function handles much of the routine regulatory violations, while the agencies, on the other hand, are primarily charged with overseeing the compliance function of the firms, taking direct control only under a limited set of circumstances.[2]

While the trend towards delegating monitoring and enforcement to firms have been especially salient for policy areas such as environmental protection, consumer protection, and investor protection, this trend is also starting to affect policy areas such as antitrust which have traditionally had a strong emphasis on external enforcement (Sokol, 2017). For example, in some recent antitrust violation cases, the U.S. DOJ has started "seeking court-supervised probation as a means of assuring that the company devises and implements an effective compliance program"(Assistant Attorney General Baer, 2014).[3] Compliance functions typically

[1]See e.g., Davis (2017) for a discussion of the rapid growth in government regulations in the U.S.

[2]The regulatory hierarchy is described in Ayres and Braithwaite (1995) as follows: "achievement of regulatory objectives is more likely when agencies display both a hierarchy of sanctions and a hierarchy of regulatory strategies of varying degrees of interventionism. The regulatory design requirement we describe is for agencies to display two enforcement pyramids with a range of interventions of ever-increasing intrusiveness (matched by ever-decreasing frequency of use). Regulators will do best by indicating a willingness to escalate intervention up those pyramids or to deregulate down the pyramids in response to the industry's performance in securing regulatory objectives."

[3]See, for example, the following remarks by Bill Baer, the Assistant Attorney General of the Antitrust Division on Sept 10, 2014.

> We also expect companies to take compliance seriously once they have pleaded guilty or have been convicted. Taking compliance seriously includes making an institutional commitment to change the culture of the company. Companies should be fostering a corporate culture that encourages ethical conduct and a commitment to compliance with the law.
>
> In such cases, the division will consider seeking court-supervised probation as a means of

include setting internal rules and procedures that are consistent with regulation, actively monitoring and reporting violations, ensuring that remedial actions are taken, etc. According to one estimate, regulatory compliance accounts for about 1.34% of the total wage bill of U.S. firms (Trebbi and Zhang, 2022).

Although compliance functions within firms have become an important part of antitrust enforcement, and of regulation of firms more generally, there is still relatively limited evidence on the effectiveness of within-firm compliance functions. In this paper, we provide empirical evidence on one aspect of regulatory compliance, specifically, the extent to which firms can take remedial action when confronted with evidence of illegal activity. How firms respond to evidence of regulatory violations – whether firms take steps to end wrongdoing, ignore evidence and continue to engage in wrongdoing, or actively seek to conceal incriminating evidence – should factor into the design of enforcement policies.

In order to study how firms respond to evidence of illegal activity, we conduct a field experiment in which we disclose to a set of construction firms in Japan evidence implicating them of illegal bid-rigging.[4] To do so, we first develop a statistical test of bid-rigging for scoring auctions. We then apply the test to bidding data from auctions let by the Ministry of Land Infrastructure and Transportation in Japan. We identify 242 firms whose bidding behavior is inconsistent with competitive bidding. We then subject a random subset of these firms to a treatment in which we send out a letter that explains the statistical test we ran and the outcome of the test (i.e., competition is rejected) for the firm in question. We compare the subsequent bidding behavior of the treated firms and that of the control firms to identify how firms respond to evidence implicating them of bid-rigging.

Our first finding is that firms do respond to the information provided to them in the letter that we send out. In particular, we find that our statistical test of bid-rigging fails to reject the null of competition when applied to the bidding data of the treated firms after the

> assuring that the company devises and implements an effective compliance program. We reserve the right to insist on probation, including the use of monitors, if doing so is necessary to ensure an effective compliance program and to prevent recidivism.

[4]Bid-rigging in procurement auctions is illegal in Japan and firms face fines of up to 10 % of all relevant sales in the affected market, in addition to the direct monetary gains that results from collusion.

intervention. For the control firms, our test continues to be effective at identifying bid-rigging behavior. Using Fisher's randomization test, we can reject (at the 1% significance level) the strong null hypothesis that the treatment induces no change in firm bidding behavior with respect to the ability of our statistical test to detect collusion.

Our second finding is that the change in firms' bidding behavior is likely to be the result of an adaptive response by the firms to evade detection without stopping collusion. First, we do not find significant changes in the level of bids or the quality of the proposals, which are often associated with a shift from collusion to competition. Moreover, we find a statistically significant decrease in the number of bidders submitting valid bids, or an increase in the proportion of bidders who submit bids above the reserve price conditional on participating. Because we study procurement auctions, bids above the reserve price have no chance of winning. These responses are consistent with continued collusive behavior. Finally, we document additional direct evidence of continued collusion using a test developed in Kawai et al. (2022). Our findings are consistent with concealment of incriminating evidence without stopping collusive behavior.

The results of our experiment suggest that, at least for the subset of construction companies participating in procurement auctions, existing levels of compliance capacity within colluding firms are unlikely to complement formal regulatory actions in achieving regulatory compliance.[5] Our findings are hence somewhat different from previous studies that document firms stopping potentially illegal behavior after allegations of wrongdoing are made (See, e.g., Christie et al., 1994 for the NASDAQ collusion case, and Monticini and Thornton, 2013, for the LIBOR manipulation case). Our findings suggest that punitive approaches may be needed in the absence of widespread publicity. Our findings also suggest that regulators should be cautious in sharing certain types of information with firms when putting pressure on them to change their behavior. If evidence of regulatory violations can be concealed with

[5]It should be noted that in Japan, there is no equivalent of *qui tam* law suits in which private parties are compensated for assisting governments recover damages from illegal activities. For example, in the U.S., the False Claims Act allows private parties to bring suit on behalf of the government and receive a portion of the damages recovered. See, e.g., Kovacic (2001), and Engstrom (2013) for analysis on *qui tam* law suits. On the topic of private enforcement of laws more generally, see, e.g., Landes and Posner (1975) and Polinsky (1980).

little cost without changing the underlying illegal conduct, regulators may wish to refrain from sharing detailed information with the firms.

Related literature. There is a large literature in law and organizational behavior that analyzes how within-firm compliance functions complement the work of regulatory agencies, for example, Braithwaite (1985), Ayres and Braithwaite (1995), Parker (2002).[6] There is also a small theoretical literature on regulatory compliance in economics that studies the trade-off between regulatory capture and efficient use of private information, e.g., Gehrig and Jost (1995) and Grajzl and Murrell (2007).[7] In these models, firms are better informed about the business environment than the regulators making it possible for firms to implement more efficient regulation. However, given the obvious conflict of interest, the regulator may not want to delegate rule-setting and enforcement completely to firms.[8]

Our paper is also related to papers that study firm adaptation in environments where regulatory agencies use screens to select the set of firms that go under scrutiny. For example, Wollmann (2019) and Cunningham et al. (2021) document evidence that firms adapt to the merger notification threshold set by the Hart-Scott-Rodino Act. The possibility of firm adaptation implies that the design of screens should account for the firms' equilibrium response. The importance of taking an equilibrium view of regulatory screening has been made previously by Cyrenne (1999), LaCasse (1995), Harrington (2004), Ortner et al. (2022), etc.

We study how firms react to evidence of incriminating evidence. In this sense, our paper is closely related to the studies of Christie et al. (1994) and Monticini and Thornton (2013). Christie et al. (1994) documents immediate changes in the quotes offered by market makers in the NASDAQ market after newspapers reported potential collusion by dealers. Monticini

[6] A number of papers by Sokol discusses how self-regulation can be applied to antitrust. See, e.g.,

[7] Relatedly, there is also a literature on self regulation, e.g., baron, Egorov, Harstad.

[8] Other related work include Innes (1999), who studies remediation activities that are offered voluntarily by violators and Kaplow and Shavell (1994), who study self-reports of violations by perpetrating firms. There is also a literature that studies the efficacy of leniency programs and the incentives they create for firms to report their involvement in collusion, e.g., Motta and Polo (2003), Aubert et al. (2006), Spagnolo (2005), Chen and Harrington (2007), Harrington (2008a) and Miller (2009).

and Thornton (2013) find evidence that banks stopped underreporting LIBOR rates after the Wall Street Journal reported potential manipulation of the rates. Presumably, for both the NASDAQ collusion case and the LIBOR manipulation case, the publicity created by the news reports were such that pressure to discontinue any illegal activities were unusually strong.

Lastly, our paper is related to the literature on detecting cartels in auctions. Early seminal work includes Hendricks and Porter (1988), Baldwin et al. (1997) and Porter and Zona (1993, 1999). More recent work includes Bajari and Ye (2003), Abrantes-Metz et al. (2006), Athey et al. (2011), Conley and Decarolis (2016), Schurter (2017), Kawai and Nakabayashi (2022), Chassang et al. (2020), and Kawai et al. (2022).[9] The approach taken in the current paper is an extension of the approach proposed in Kawai et al. (2022) to scoring auctions.[10]

2 Institutional Background and Auction Format

Our paper analyzes the bidding behavior of firms that participate in auctions let by the Ministry of Land Infrastructure and Transportation (MLIT). This section provides a brief description of the institutional background and the auction format used by the MLIT.

MLIT is the largest procurement buyer in Japan, letting in each year, about 9,000 auctions, worth a combined total of about 1.7 trillion yen (about $17 billion USD). The range of projects let by the MLIT include road paving, building and repairing bridges, installation of electrical equipment and other machinery as well as civil engineering work.

Since around 2006-7, almost all MLIT auctions are let using a scoring auction. In a scoring auction, each bidder submits a proposal in addition to a price, which is assigned a quality measure. Allocation is determined by a comparison of each bidder's score, an index

[9]Other related work includes Pesendorfer (2000), who studies bidding rings with and without side-payments, and Asker (2010), who studies knockout auctions among cartel members. Ohashi (2009) and Chassang and Ortner (2019) document how changes in auction design can affect the ability of bidders to sustain collusion. Clark et al. (2018) analyze the breakdown of a cartel and its implications on prices. For a survey of the literature, see Porter (2005) and Harrington (2008b).

[10]See e.g., Che (1993), Asker and Cantillon (2008, 2010), etc. for analysis of competitive equilibrium in scoring auctions.

that combines both price and quality of the bidder. The MLIT scores each bid according to the rule

$$s = q/p,$$

where q is the quality measure and p is the price submitted by the bidder. Bidders submit sealed bids simultaneously and the project is allocated to the bidder with the highest score, subject to the secret reserve price.

How much room the bidder has to improve the quality component of the bid depends on the type of project. For simple projects, all bidders satisfying certain requirements are all given the same quality. For these auctions, q is essentially fixed and allocation is determined by price competition. For more complicated projects, however, the quality of the proposals play a much more important role in determining q. For simple projects, the maximum q is as low as 130 while for complex projects, it can be as high as $q = 190$. The worst possible quality a bidder can be assigned is $q = 100$ regardless of the complexity of the project.

One institutional detail that is worth mentioning is that when the price of bidder i exceeds the secret reserve price, quality of the bid is either not recorded entirely, or is assigned the lowest possible measure of 100. This fact explains certain features of the data pattern that we document in our analysis below.

3 Model and Test Statistic

This section specifies a model of scoring auctions and introduce the test statistic that we use to screen for non-competitive behavior.

Game form. A buyer procures a single item from a finite set N of potential suppliers. The procurement contract is allocated through a sealed-bid auction with a secret reserve price r, which is drawn from a distribution F_r. Each potential bidder $i \in N$ decides whether or not to participate in each auction. Conditional on participation, a bidder incurs participation cost $k > 0$, and submits a bid $\mathbf{b}$ consisting of a price-quality pair $\mathbf{b} = (p, q)$. Profit from non-participation is normalized to 0. A bid is valid if the price is below the reserve price, i.e., $p \leq r$. The bidder who submits a valid bid with the highest score is allocated the project, where the score is computed according to the formula $s = q/p$. Ties are broken with uniform probability. We denote by $\vee \mathbf{s}_t$ the highest score among participating bidders, by $\mathbf{s}_{-i,t} \equiv (s_{j,t})_{j \neq i}$ scores of firms other than i, and by $\vee \mathbf{s}_{-i,t} \equiv \max_{j \neq i} s_{j,t}$ the highest score among i's participating competitors. Let $\vee \mathbf{s}_{-i} \prec s_i$ denote the event that bidder i wins the contract, i.e. s_i is the highest score and possible ties are broken in favor of bidder i. Bids are publicly revealed at the end of each period.

The bidder's profit conditional on winning is given by $p - C(q, \theta)$, where θ is the cost type of the bidder. The function $C(\cdot, \theta)$ represents the cost of providing higher quality for each type θ. We assume that $C(\cdot, \theta)$ is increasing, convex and continuously differentiable for each θ.

Information. Each bidder i privately observes a signal $z_i \in Z_i$. We do not impose any assumptions on the distribution of signal profile $\mathbf{z}_t = (z_{i,t})_{i \in N} \in Z = \prod_{i \in N} Z_i$. Signals may be arbitrarily correlated. We denote by $F_Z(\cdot)$ the distribution of signals.

Cost types $\theta = (\theta_i)_{i \in N} \in \mathbb{R}^N$ are drawn independently conditional on each private signal z_i, i.e.,

$$\theta_i | z_i \sim \theta_i | \mathbf{z}_t, \theta_{-i}.$$

Bidder i's cost type does not provide information about the cost type of other bidders beyond the information already provided in the private signal z_i.[11] This class of information structures nests asymmetric independent private values, correlated values, and complete information. We denote by $F_C(\cdot | \mathbf{z}_t)$ the conditional distribution of the profile of cost types $\mathbf{c}$ given signals $\mathbf{z}$.

Indirect Profit Function Because any bid (p, q) that satisfies $p/q = s$ guarantees the bidder the same winning probability, optimal bidding behavior requires a bid (p, q) with

[11]Because the signals are allowed to be correlated, $z_{i,t}$ helps bidder i predict the cost of other bidders.

$p/q = s$ to be the solution to the following maximization problem:

$$\max_{p,q} \; p - \mathbb{E}_{z_i}[C(q,\theta)]$$
$$\text{s.t. } q/p = s,$$

where the expectation $\mathbb{E}_{z_i}$ is taken with respect to θ conditional on z_i. The objective function, $p - \mathbb{E}_{z_i}[C(q,\theta)]$, is the profit the firm obtains if it wins. We denote the solution of this problem as $\pi_i(s, z_i)$. The function $\pi_i(s, z_i)$ corresponds to the indirect profit function of the firm when it bids a score of s and its signal is z_i. Note that $\pi_i(s, z_i)$ is continuously differentiable in s.[12]

Competitive Bidding We now derive implications of Bayes Nash equilibrium that we use to construct our test of competitive bidding. Our first result is that, under competition, winning or losing is as-if-random conditional on close bids.

Proposition 1. *For any bidder i and for any positive number $\eta > 0$, there exists ε such that*

$$\mathsf{prob}(i \text{ wins} \mid |s_i - \vee \mathbf{s}_{-i}| < \varepsilon) \geq 1/2 - \eta.$$

Proof. Incentive compatibility of the bidder implies

$$D(s_i)\pi_i(s_i, z_i) \geq D(s_i + \varepsilon)\pi_i(s_i + \varepsilon, z_i)$$

and

$$D(s_i)\pi_i(s_i, z_i) \geq D(s_i - \varepsilon)\pi_i(s_i - \varepsilon, z_i),$$

where $D(s_i)$ denotes $\mathsf{prob}(s_i \succ \vee \mathbf{s}_{-i})$, the probability of winning the auction. Noting that

[12] Note that $\pi_i'(s, z_i) < 0$ for any s.

$D_i(s_i)$ must be continuous in equilibrium,[13]

$$
\begin{aligned}
\mathsf{prob}(i \text{ wins} \mid |s_i - \vee \mathbf{s}_{-i}| < \varepsilon) &= \frac{D_i(s_i) - D_i(s_i - \varepsilon)}{D_i(s_i + \varepsilon) - D_i(s_i - \varepsilon)} \\
&= \frac{1 - \frac{D_i(s_i - \varepsilon)}{D_i(s_i)}}{\frac{D_i(s_i + \varepsilon)}{D_i(s_i)} - \frac{D_i(s_i - \varepsilon)}{D_i(s_i)}} \\
&\geq \frac{1 - \frac{\pi_i(s_i, z)}{\pi_i(s_i - \varepsilon, z)}}{\frac{D_i(s_i + \varepsilon)}{D_i(s_i)} - \frac{\pi_i(s_i, z)}{\pi_i(s_i - \varepsilon, z)}} \\
&\geq \frac{1 - \frac{\pi_i(s_i, z)}{\pi_i(s_i - \varepsilon, z)}}{\frac{\pi_i(s_i, z)}{\pi_i(s_i + \varepsilon, z)} - \frac{\pi_i(s_i, z)}{\pi_i(s_i - \varepsilon, z)}} \\
&= \frac{\frac{1}{\pi_i(s_i, z)} - \frac{1}{\pi_i(s_i - \varepsilon, z)}}{\frac{1}{\pi_i(s_i + \varepsilon, z)} - \frac{1}{\pi_i(s_i - \varepsilon, z)}} \\
&= \frac{\frac{d}{ds}\left(\frac{1}{\pi_i(s_i, z)}\right) \times \varepsilon + o(\varepsilon)}{\frac{d}{ds}\left(\frac{1}{\pi_i(s_i, z)}\right) \times 2\varepsilon + o(\varepsilon)}
\end{aligned}
$$

As $\pi_i(s_i, z)$ is continuously differentiable and $\pi'(s_i, z) < 0$, the last term converges to 0.5 as $\varepsilon \to 0$. □

Proposition 1 shows that the winning probability must not be much lower than 0.5 conditional on close bids. Because at most one bidder can win, and because there are at least two close bidders conditional on the existence of close bids, it cannot be that firms' winning probability is frequently much larger than 1/2. The next proposition formalizes this argument. For any $\epsilon > 0$, let ϵ-close denote the event that the winning bid is within ϵ of the next highest score. For any $\varepsilon > 0$, let $\mathbb{E}[\cdot \mid \epsilon\text{-}\mathsf{close}]$ denote the expectation over z_i conditional on the event ϵ-close.

Proposition 2 (as-if random bids)**.** *Assume Z is finite valued. For all $\eta > 0$ there exists $\epsilon > 0$ small enough such that*

$$
\mathbb{E}\left[\left|\mathsf{prob}_\sigma(i \text{ wins} \mid z_i \text{ and } |s_i - \vee \mathbf{s}_{-i}| < \epsilon) - \frac{1}{2}\right| \;\middle|\; \epsilon\text{-}\mathsf{close}\right] \leq \eta. \tag{1}
$$

[13] Recall that participation costs k are strictly positive.

In words, winning is as-if random conditional on close bids under competition. This result motivates our regression discontinuity test of competition. The following Corollary forms the necessary link between equilibrium bidding and our regression discontinuity test.

Corollary 1. *Assume Z is finite valued.*[14] *Let $\Delta_i^s = s_i - \vee \mathbf{s}_{-i}$.*

$$\lim_{\epsilon \searrow 0^+} \left| \mathbb{E}\left[p_i \mid \Delta_i^s \in (0, \epsilon)\right] - \mathbb{E}\left[p_i \mid \Delta_i^s \in (-\epsilon, 0)\right] \right| = 0. \tag{2}$$

Corollary 1 guarantees that, under the null of competition, the conditional expectation of the price, p_i, when bidder i is a marginal winner, is the same as the conditional expectation of p_i when bidder i is a marginal loser. If the conditional expectation of p_i is different when bidder i is a marginal winner than when i is a marginal loser, we reject the null that bidder i is bidding competitively. We use Corollary 1 to test for competitive bidding.

Remarks on Corollary 1 We now make several remarks. In Corollary 1, the conditional expectation is taken with respect to p_i, the price component of bidder i's bid. An analogous expression holds for the quality component q_i of bidder i's bid as well, i.e.,

$$\lim_{\epsilon \searrow 0^+} \left| \mathbb{E}\left[q_i \mid \Delta_i^s \in (0, \epsilon)\right] - \mathbb{E}\left[q_i \mid \Delta_i^s \in (-\epsilon, 0)\right] \right| = 0.$$

In fact, if we let x_i be any covariate of bidder i (e.g., the amount of projects recently won), it is straightforward to show that an analogous expression holds for x_i as well. We show this fact in Appendix XXX. In Section XYZ, we exploit this fact by taking the bidder's backlog as the outcome x_i, and test for discontinuities in the backlog between marginal winners and marginal losers. We find that marginal winners tend to have lower backlog than marginal losers, suggesting that firms in the treated groups continue to collude even after the intervention.

Corollary 1 compares the conditional expectation of p_i when bidder i is a marginal winner to that when bidder i is a marginal loser. Because each bidder has 0.5 probability each of

[14] Finiteness of Z is assumed to guarantee uniform convergence over all realizarions of z.

being the marginal winner and the marginal loser, we can show that a modified version of Corollary 1 holds in which we compare bidder i's price, p_i to the winner's price, p^*:

$$\lim_{\epsilon \searrow 0^+} \left| \mathbb{E}\left[p_i - p^* \mid \Delta_i^s \in (-\epsilon, 0)\right] \right| = 0, . \tag{3}$$

Lastly, we note that in some cases, we can take the running variable to simply be the price difference between bidder i's price and the lowest price, i.e.,

$$p_i - p_{i(1)},$$

where $i(1)$ is the bidder with the lowest price and $p_{i(1)}$ is its price. The corresponding outcome variable is $s_i - s_{i(1)}$. This would be the case for auctions in which the quality component is essentially the same for everyone, for example. For these auctions, $\lim_{\Delta^s \searrow 0^+} \Leftrightarrow \lim_{p_i - p^{(1)} \searrow 0^+}$. Another example would be if the equilibrium distribution of $\{p_i\}_{i \in N}$ is smooth for every $z \in Z$. If the distribution of $\{p_i\}_{i \in N}$ is smooth, then Proposition 2 holds, and hence Corollary 1 must hold as well.

4 Identifying Firms That Do Not Bid Competitively

Illustration: Case of Nippo Corporation We now discuss how we test each firm for competitive bidding using the results obtained in the previous section. To illustrate how we implement our test, we first focus on bidding data from one firm, Nippo Corporation between 2012 to 2015. Although the period 2012-2015 is not within the time frame we focus on to identify non-competitive bidding, Nippo Corporation was involved in a bid-rigging scheme that was discovered and prosecuted in 2013 by the Japanese competition authority. Because Nippo Corporation is one of the largest construction firms in Japan, the collusion case was widely publicized. We also included, in the letter that we sent out to the treated firms, the analysis of Nippo Corporation to illustrate the mechanics of our test.

Because the firm's bidding behavior may change before and after the investigation, we test the firm's conduct separately. Let $T_k,\ k \in \{\text{Before, After}\}$ denote the partition of the sample to those that took place before and after the investigation. For each auction $t \in T_k$, let $i^*(t)$ be the winner of the auction, i.e., the bidder who had the highest score. For each bidder $i \neq i^*(t)$ who participated in auction t, we construct $\Delta_i^s = s_i - s_{i^*(t)}$ and $\Delta_i^p = p_i - p_{i^*(t)}$. The variable Δ_i^s is the margin of defeat for bidder i and Δ_i^p is the price difference between bidder i and the winner. We then test whether or not $\lim_{\epsilon \searrow 0^+} \mathbb{E}\left[\Delta_i^p | \Delta_i^s \in (-\epsilon, 0)\right] = 0$.

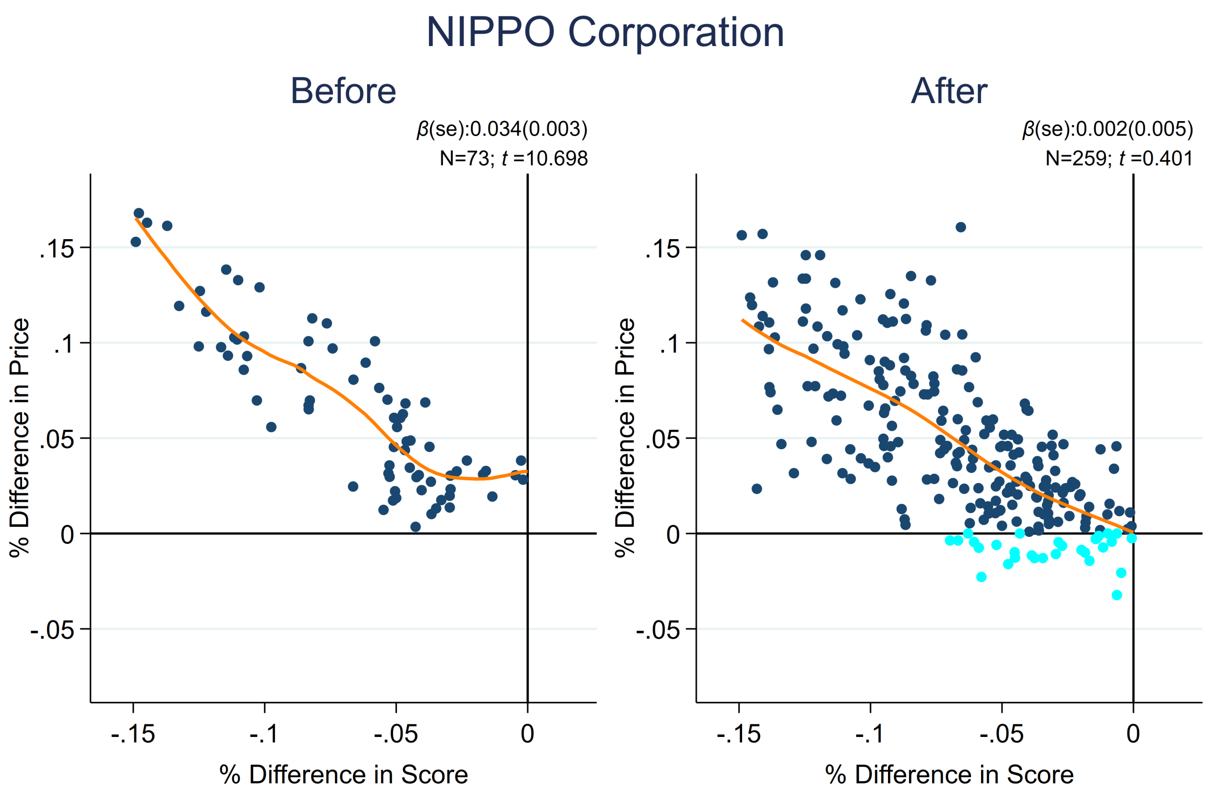

Figure 1: Nippo

Figure 1 illustrates graphically the mechanics of our test. Figure **??** is a binned scatter plot of (Δ_i^s, Δ_i^p) for T_{Before} (left panel) and T_{After} (right panel). The curve in the figure corresponds to the nonparametric regression, $\mathbb{E}\left[\Delta_i^p | \Delta_i^s\right]$. Our test corresponds to whether or not the regression line passes through the origin, $(0, 0)$

Panel (A) of Table 1 reports our estimates of $\mathbb{E}\left[\Delta_i^p | \Delta_i^s\right]$ at $\Delta_i^s = 0$ for T_{Before} (first column) and T_{After} (second column). Our estimates are obtained using a local linear regression with a coverage error rate optimal bandwidth and a triangular kernel with a bias correction procedure as proposed in Calonico et al. (2014). Standard errors are clustered at the auction level. We find that for the sample of auctions during which the firm was involved in a bidding ring, the intercept of $\mathbb{E}\left[\Delta_i^p | \Delta_i^s\right]$ at $\Delta_i^s = 0$ is estimated to be xyz and statistically different from 0 at the 95% confidence level. On the other hand, our estimate of the intercept is statistically indistinguishable from 0 for the sample of auctions after the investigation.

As we discussed at the end of Section 3, it is possible to construct an alternative test of competition by taking the running variable to be the price difference, $p_i - p_{i(1)}$, and the outcome variable to be $s_i - s_{i(1)}$, where $i(1)$ denotes the bidder who submits the lowest price, $p_{i(1)} \equiv \wedge \mathbf{p}_i$. Figure 2 is a binned scatter plot of $(p_i - p_{i(1)}, s_i - s_{i(1)})$ for all bidders $i \neq i(1)$ for T_{before} (left panel) and T_{before} (right panel). The left panel clearly shows that bidders who marginally submit higher bids than $i(1)$ have much lower score than $i(1)$. This also means that these bidders also have much lower quality than $i(1)$. The right panel corresponds to the set of auctions after collusion. We find that the bidders who bid marginally higher than $i(1)$ can have higher or lower score. The curves in the figure correspond to the nonparametric regression of $\mathbb{E}\left[s_i - s_{i(1)} | p_i - p_{i(1)}\right]$.

The bottom panel of Table 1 reports the estimate of $\mathbb{E}\left[s_i - s_{i(1)} | p_i - p_{i(1)}\right]$ at $p_i - p_{i(1)} = 0$. We find that the estimate is negative and statistically significant for T_{Before} while it is statistically indistinguishable from 0 for T_{After}.

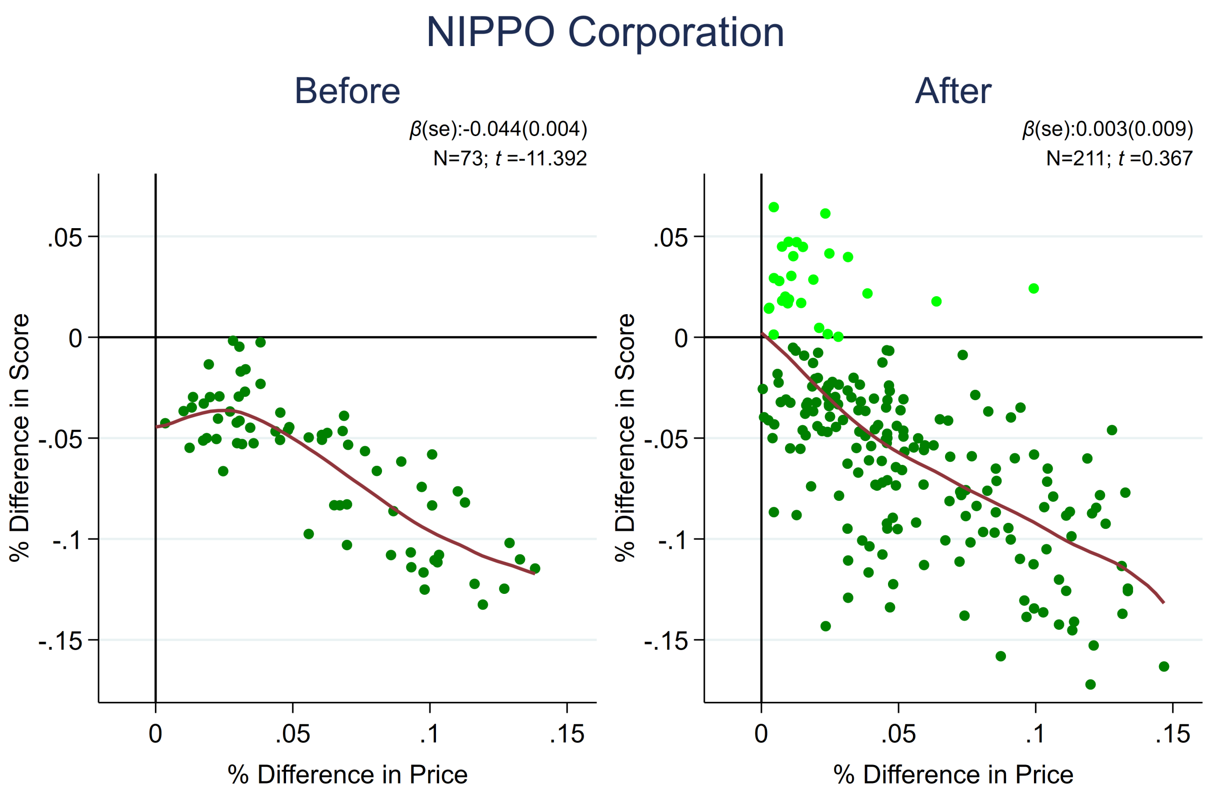

Figure 2: Nippo

	(1) Before	(2) After
Panel (A) : $\mathbb{E}\left[\Delta_i^p \| \Delta_i^s = 0\right]$		
$\hat{\beta}$	0.034^{***}	0.002
	(0.003)	(0.005)
h	0.026	0.037
Obs.	73	259
Panel (B) : $\mathbb{E}\left[s_i - s_{i(1)} \| p_i - p_{i(1)} = 0\right]$		
$\hat{\beta}$	-0.044^{***}	-0.003
	(0.004)	(0.009)
h	0.016	0.044
Obs.	73	211

Panel (A) corresponds to the sample of auctions in. *, **, and *** respectively denote significance at the 10%, 5%, and 1% levels.

Table 1: Intercept of Partial Linear Regression, NIPPO Corporation.

Screening for non competitive bidders For each firm that participated in MLIT auctions between April 2015 and March 2017, we test

$$\mathbb{E}\left[\Delta_i^p | \Delta_i^s = 0\right] = 0$$

and

$$\mathbb{E}\left[s_i - s_{i(1)} | p_i - p_{i(1)} = 0\right] = 0$$

during that time period. Based on the two tests, we initially select 1,143 firms that fail either of the tests at 10% significance.[15] We employ a relatively permissive significance level because there are a number of firms that only marginally fail the tests, yet exhibit bidding patterns that clearly resemble those illustrated in the left panels of Figures 1 and 2.

Among the initially selected firms, we further screen them by visually inspecting the nonparametric regression line. The point of the additional screening was to minimize, as much as possible, making type I errors, i.e., including competitive firms in the baseline sample. Ultimately, we end up with 240 firms which we were confident classifying as noncompetitive.[16] Because the selection of the 240 firms are based on somewhat subjective criteria, we make sure that all of the subsequent econometric analysis are robust to the subjective nature of the sample selection process.

5 Experimental Design

Assignment of Treatment The firms that we identify in the previous section often bid on the same auctions and hence we expect some communication to take place among these bidders. In order to contain potential treatment spillovers across units, we partition the firms, grouping those that frequently bid together using a clustering algorithm.[17]. The resulting partition has the property that firms within each group bid on the same auction frequently

[15] Of these firms, 635 firms failed the first test, 601 firms failed the second test, and 93 failed both.

[16] We initially select 242 firms, but we end up dropping 2 firms after performing the clustering procedure we discuss next section. See Online Appendix Section **??** for details.

[17] In particular, we use a hierarchical agglomerative linkage procedure. We provide the details in Online Appendix Section **??**

while firms in different groups rarely do. We end up with 26 groups of firms. We then construct 13 matched pairs based on the group's geographical location, type of work (e.g., landscaping, paving, etc.), and the number of firms in the group.

Finally, we assigned treatment status with rerandomization to achieve balance in the mean winning bid, t-statistics of the RD tests, and the sample size between the treatment and the control (Morgan and Rubin, 2012). We take the effect of rerandomization into account when conducting our statistical tests below.

Treatment We send out physical letters to 13 groups, or a total of 106 firms on Feb 12, 2019. We send the letters to the firms' addresses recorded in the MLIT's registry. In the letter, we first explain that we are developing a screen for bid rigging and that we are exploring its usefulness and applicability. We explain the mechanics of our test by walking through Figures 1 and 2 of Nippo Corporation. We then include corresponding figures for the firm in question and discuss the similarities between the firm's bidding patterns with those of Nippo Corporation before the investigation, i.e., the regression lines do not pass through the origin. We also include a list of auctions used for the analysis of the firm's bidding pattern to emphasize that our analysis is specific to the firm. Finally, we ask the firm whether the firm is aware of various screening methods to detect noncomepetitive bidding, and whether such screens can help the firm improve antitrust compliance. We include a return envelope for the firm to send back its reply, asking them to do so by March 15, 2019. The letter sent to the firms in the treatment groups are in the Online Appendix.

Baseline Summary Statistics Table 2 and 3 report the summary statistics of the auctions and the bidders separately by treatment status. The average reserve price of the auctions is about 128 million yen, or about \$1.3 million. The winning bid is about 94% of the reserve price and the average number of bidders is 4.81. The fact that there are very small differences in the means of these variables between the treatment and the control is by design. We assigned treatment by a matched pairs design and, moreover, we rerandomized the assignment to achieve balance for $\mathsf{WinBid/Reserve}$ and the overall sample size.

	Auctions			
	Treatment		Control	
	Mean	Obs.	Mean	Obs.
Reserve	128.273 (72.387)	1,301	128.701 (72.470)	1,290
WinBid / Reserve	0.939 (0.034)	1,301	0.939 (0.034)	1,290
Bid / Reserve	0.942 (0.039)	1,301	0.942 (0.039)	1,290
Quality	155.722 (6.836)	1,300	155.746 (6.839)	1,289
Number of Bidders	4.813 (2.877)	1,301	4.814 (2.885)	1,290

Note: The table shows the summary statistics of auctions that take place from fiscal year 2015 through 2017. Reserve is reported in millions of yen. Standard errors are in parenthesis.

Table 2: Summary Statistics (Auctions)

Table 3 reports the summary statistics of the bidders separately by treatment status. We report the mean annual sales, profits, the number of engineers employed by the firm, and the t-statistic corresponding to our RD estimate. The first three variables are obtained from the registry maintained by the MLIT. Although the total number of employees is not recorded in the registry, the ratio of engineers to the total number of employees is usually about 1:2 to 1:3, based on the subset of firms that report the number of employees on their web pages. The t-statistics that we report in the table correspond to the t-statistics of our estimates of $\mathbb{E}\left[\Delta^p|\Delta^s\right]$ and $\mathbb{E}\left[s_i - s_{i(1)}|p_i - p_{i(1)} = 0\right]$ for each firm before the intervention.

6 Results

Changes in the bidding behavior We first document changes in the firms' bidding behavior in relation to the ability of our test to screen for collusion. Figures 3 and 4 plot

	Firms Treatment	Control
Annual Sales	2,087.12	2,109.32
	(2,278.15)	(3,612.07)
Annual Profits	146.52	136.87
	(192.54)	(296.93)
# Engineers	26.75	27.49
	(18.97)	(34.19)
t-value (Score)	3.45	3.55
	(4.11)	(8.42)
t-value (Price)	-3.06	-3.52
	(4.38)	(4.98)
N	106	133

Note: Sales and profits are reported in million Yens. There are 240 firms in our sample. We could not get the data for one firm in the control group.

Table 3: Summary Statistics (Firms)

(Δ^p, Δ^s) for non-winners before and after the intervention.[18] Figure 3 corresponds to the treatment group and 4 corresponds to the control group. The figures provide suggestive evidence that bidding behavior changed after the intervention for the treated firms. Comparing the left and right panels of Figure 3, we find that the regression line seems to intersect the y-axis close to the origin in the right panel. This suggests that marginal losers stop bidding substantially higher than the marginal winner in terms of prices after the intervention. The t-statistic of the intercept estimate falls from about 8.25 before the intervention to about 1.56 after. For the control group, the regression line seems to intersect the y-axis above the origin even after the intervention. The t-statistic remains largely unchanged from 5.80 before the intervention to 5.66 after.

In order to ascertain that these changes are unlikely to be the result of chance, we run a Fisher's test of the strong null hypothesis (See, e.g., Imbens and Rubin (2015) for a textbook

[18]The sample used for the Before panel consist of those let between XYZ and Feb 15, 2019, the date we sent out the letters. The sample for the After panel consist of those let after March 15, 2019, the date by which we asked the firms to respond to our survey.

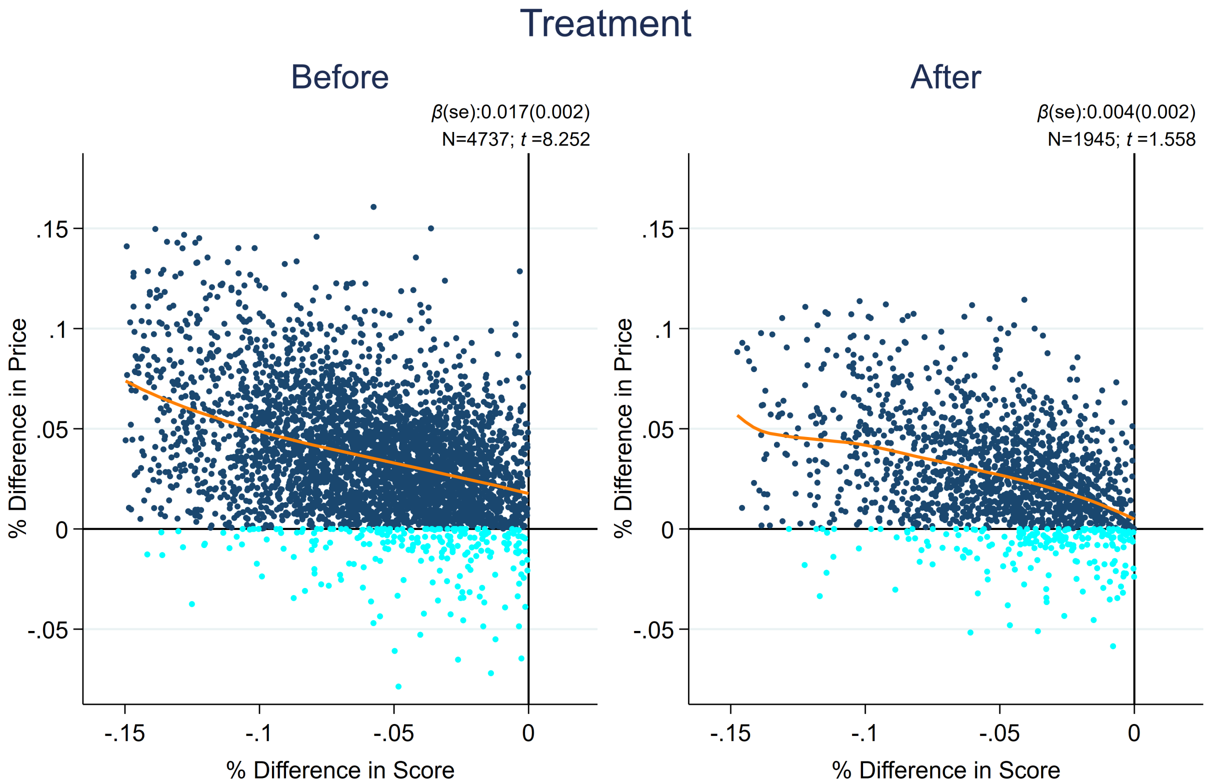

Figure 3: $\mathbb{E}[\Delta^p \mid \Delta^s]$ for Treatment Firms

treatment). Specifically, for each group $g \in \{1, \cdots, 26\}$, we compute the change in the t-statistic before and after the intervention,

$$Y_g = t_g^{\text{After}} - t_g^{\text{Before}},$$

where t_g^{Before} and t_g^{After} are the t-statistic of the estimated intercept ($\mathbb{E}[\Delta^p \mid \Delta^s]$ at $\Delta^s = 0$) computed using the sample before the intervention and after the intervention. Now, for any partition of the groups G and G', consider the difference in the average Y_g between groups in G and groups in G':

$$\tau_{G-G'} = \frac{1}{\mid G \mid} \sum_{g \in G} Y_g - \frac{1}{\mid G' \mid} \sum_{g \in G'} Y_g.$$

The statistic τ_{G-G} corresponds to a measure of the extra decline in the t- statistic exhibited by groups in G relative to G'. If we set $G = G_T$ and $G' = G_C$, where G_T and G_C are the

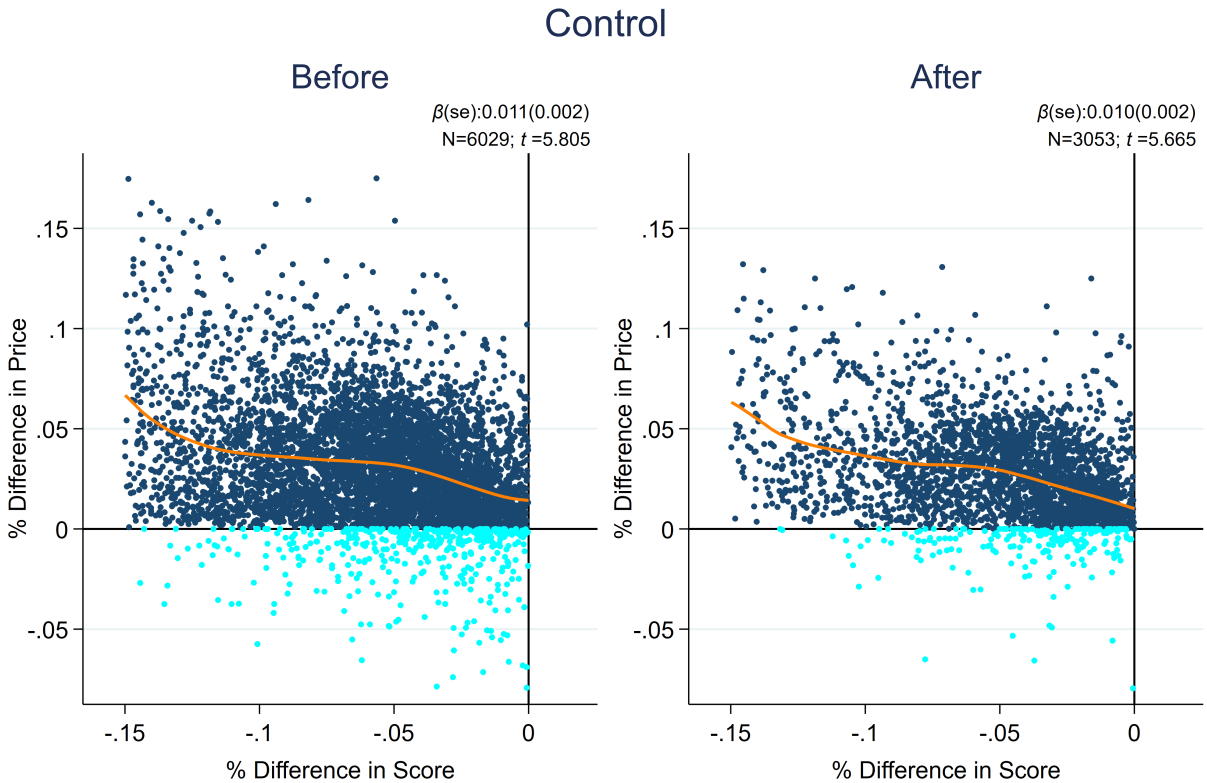

Figure 4: $\mathbb{E}[\Delta^p \mid \Delta^s]$ for Control Firms

set of treated groups and control groups, $\tau_{G_T-G_C}$ corresponds to the extra decrease in the t-statistic for the treatment groups relative to the control groups. Note, however, that we can compute $\tau_{G-G'}$ for arbitrary partition G and G', not just for G_T and G_C.

Under the strong null that our intervention had no effect on any of the treated groups whatsoever, the partition of the groups into the set of treated groups, G_T, and the control groups, G_C, have no special meaning. In other words, under the strong null of no effect, the distribution $\tau_{G_T-G_C}$ should have exactly the same distribution as $\tau_{G-G'}$ for arbitrary partition G and G'. Fisher's randomization test compares the distribution of $\tau_{G-G'}$ for arbitrary partition G and G' and compares it to the value of $\tau_{G_T-G_C}$.

Figure 5 reports the distribution of $\tau_{G-G'}$ for all possible partitions of G and G' as well as the value of $\tau_{G_T-G_C}$. The number of possible partitions are a subset of $2^{13}(=8,192)$

that satisfy the rerandomization criteria.[19] The left panel corresponds to the distribution of τ_{G-G} in which the underlying t- statistic is computed using Δ^s as the running variable and Δ^p as the outcome variable. The realization of $\tau_{G_T-G_C}$ is marked as a vertical line in the figure. Since the realization of $\tau_{G_T-G_C}$ is very far off from realizations of $\tau_{G-G'}$ for other possible partitions, we can reject the null (at the 1%) that the intervention had no effect on the treated groups. We plot the corresponding figure for the case in which the underlying t-statistic is computed using Δ^p as the running variable and Δ^s as the outcome variable. For reasons noted in the previous section, we do not find that the value of $\tau_{G_T-G_C}$ is different from realizations of $\tau_{G-G'}$ for other partitions. Hence, we cannot reject the null that firms changed the bidding behavior relative to the power of the second test to detect collusion.

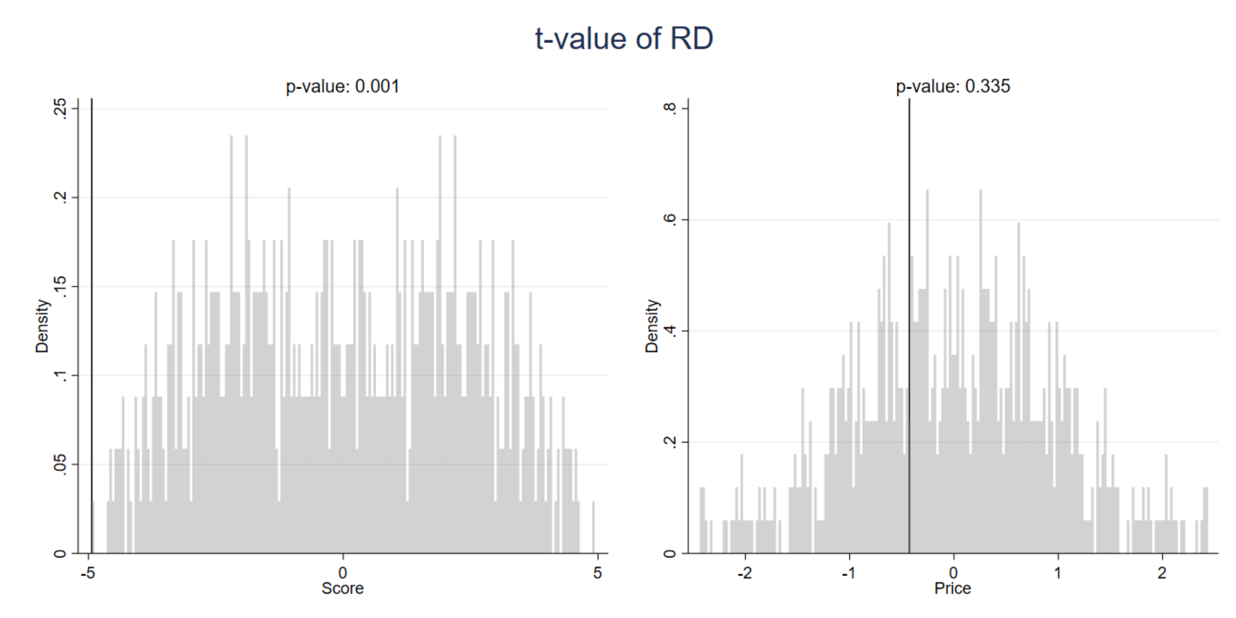

Figure 5: t-stats of RD

Changes to other outcome variables We now explore how the intervention changed other outcome variables of the auction as well as whether the changes in the bidding behavior

[19]Recall that we have a matched pair design in which one of the pair is treated and the other is not. The number of all possible treatment assignments is 2^{13}. Because we rerandomize to maintain balance between the treated and the control groups, we have a smaller number of possible assignments ().

we document in the previous section resulted from discontinuation of collusive behavior.

Figure plots the time series average of the winning bid (Left panel) and the average losing bids (Right panel) separately by treatment and control groups. The blue line corresponds to the control group, the red line corresponds to the treatment group, and the solid vertical line corresponds to the date of the intervention. We find that both for the treatment and the control groups, the bids are quite stable across time. In particular, we do not see any breaks in the bids for the treatment group around the time of the intervention. Since breakdowns in cartels typically result in significant drops in the price, Figure suggests that the firms in the treatment groups are unlikely to have ended collusion after the intervention.

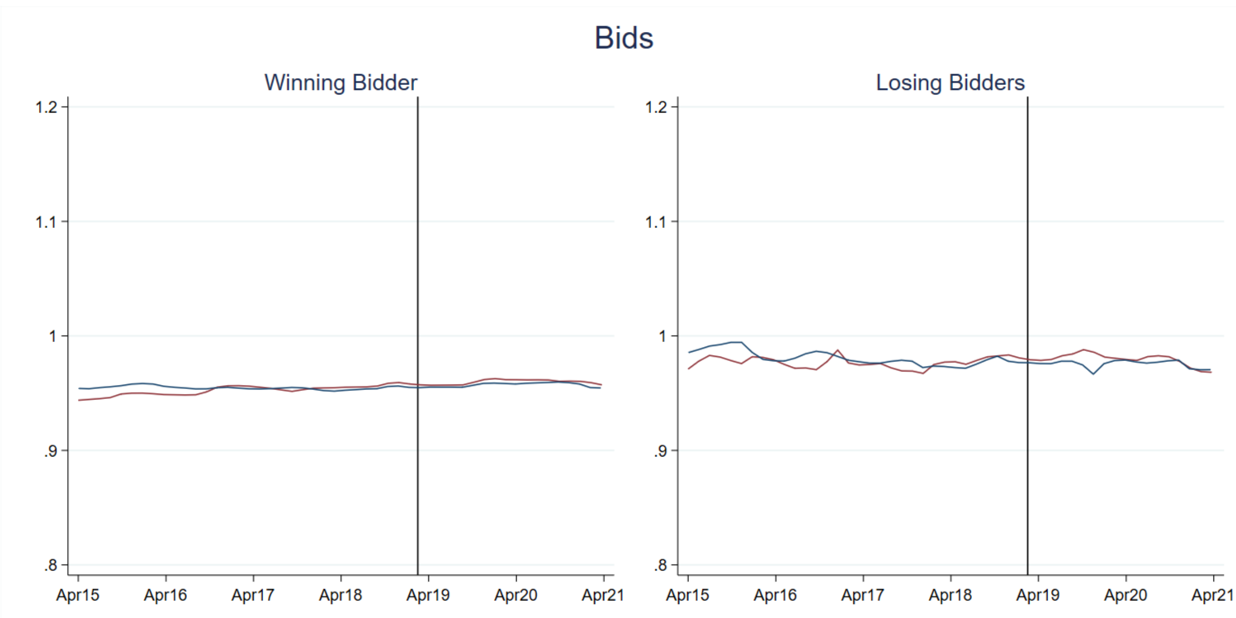

Figure 6: Bids

Figure 7 plots the time series average of the quality measures of the winning bidder (Left panel) and the losing bidders (Right panel). The red lines correspond to the treatment and the blue lines correspond to the control. We find that for the losing bidders, there is a substantial drop in the quality for the treatment group right around the time of the intervention.

As we mentioned in Section 2, firms that submit bids exceeding the secret reserve price are assigned a quality of 100 points, which is the lowest possible points attainable. In order to study the extent to which the changes in the right panel of Table 7 are driven by bids that exceed the reserve price, the left panel of 8 plots time series average of the probability that a losing bidder submits a bid that exceeds the reserve price.[20] The right panel of 8 plots the time series average of the number of bidders that submit a bid below the reserve price. The left panel of the figure suggests that there is a large increase in the probability of submitting a bid above the reserve price for the treatment group after the intervention. The right panel of the figure suggests that the number of bidders that submit bids below the reserve price falls for the treatment group after the intervention.

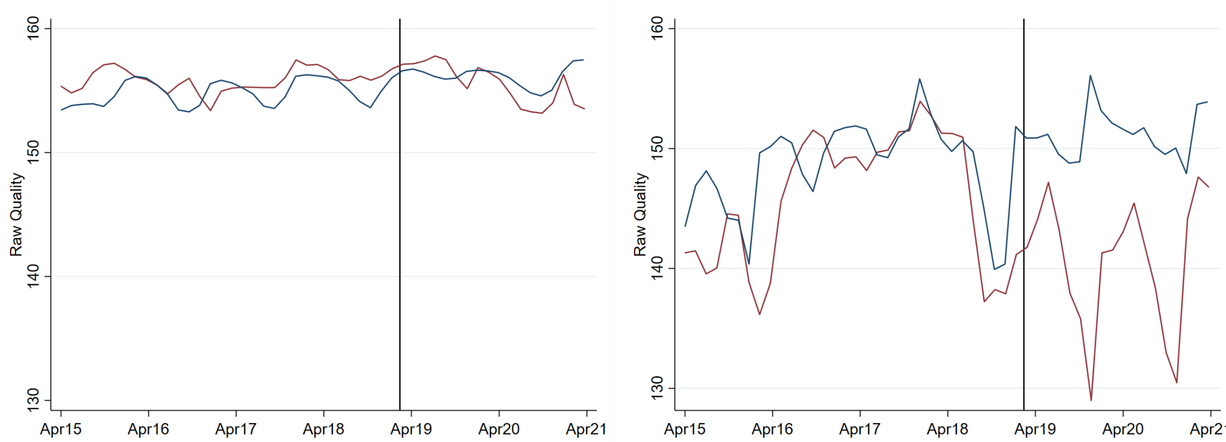

Figure 7: Winner's Raw Quality (Left) and Losers Raw Quality (Right)

The red line on the left panel represents the raw quality of the winning bid in the auction in which one or more treatment firms participate. The blue line represents the raw quality of the winning bid for the auction in which one more more control firms participate. The red line in the right panel represents the raw quality of the losing bid in the auction in which one or more treatment firms participate. The blue line represents the raw quality of the losing bid in the auction in which one more more control firms participate.

In order to assess the statistical significance of these findings, we again conduct Fisher's randomization test for each outcome. In particular, for each outcome variable Y_g, construct $\tau_{G-G'}$ as before for all possible partitions G and G', and compare the value of $\tau_{G_T-G_C}$ against the distribution. The left panel of Figure 9 corresponds to the winning bid and the right panel

[20] The probability that the winner's bid exceeds the secret reserve price is very close to zero, so we do not report the corresponding figure for the winner.

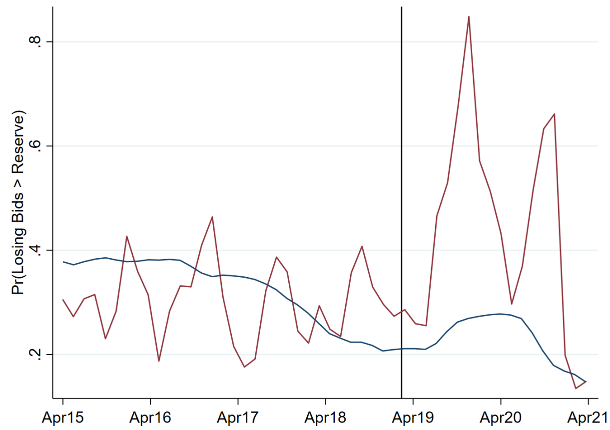

Figure 8: Losing Bids Above Reserve (Left) and Number of Valid Bids (Right)

The red line on the left panel represents the probability that the losing bid is above the reserve price in the auction in which one or more treatment firms participate. The blue line represents the probability for the auction in which one more more control firms participate. The red line in the right panel represents the probability that the losing bid is above the reserve price in the auction in which one or more treatment firms participate. The blue line represents the probability for the auction in which one more more control firms participate.

corresponds to the losing bids. We find that we cannot reject the null that the intervention had an impact of the winning bid.

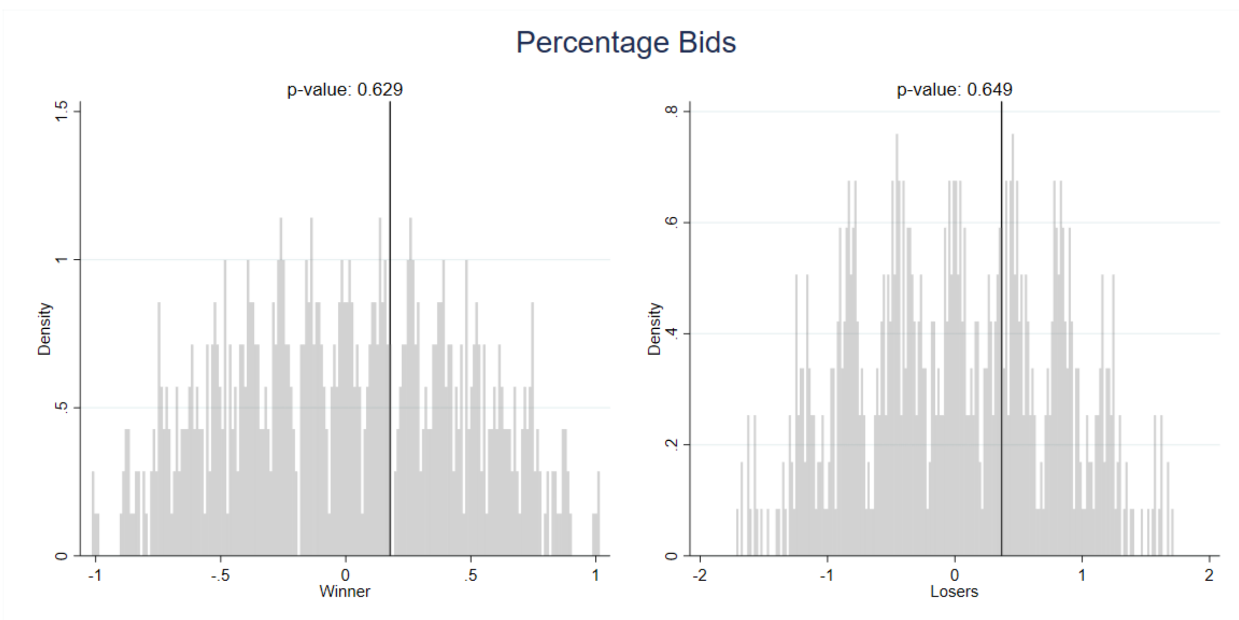

Figure 9: Percentage Bids

Figure 10 plots the histogram for quality. The left panel corresponds to the winner and the right panel corresponds to the losers. While we cannot reject the null that the winner's quality is not affected by the treatment, we can reject the null that the treatment has no effect on the losers' quality at 10% significance.

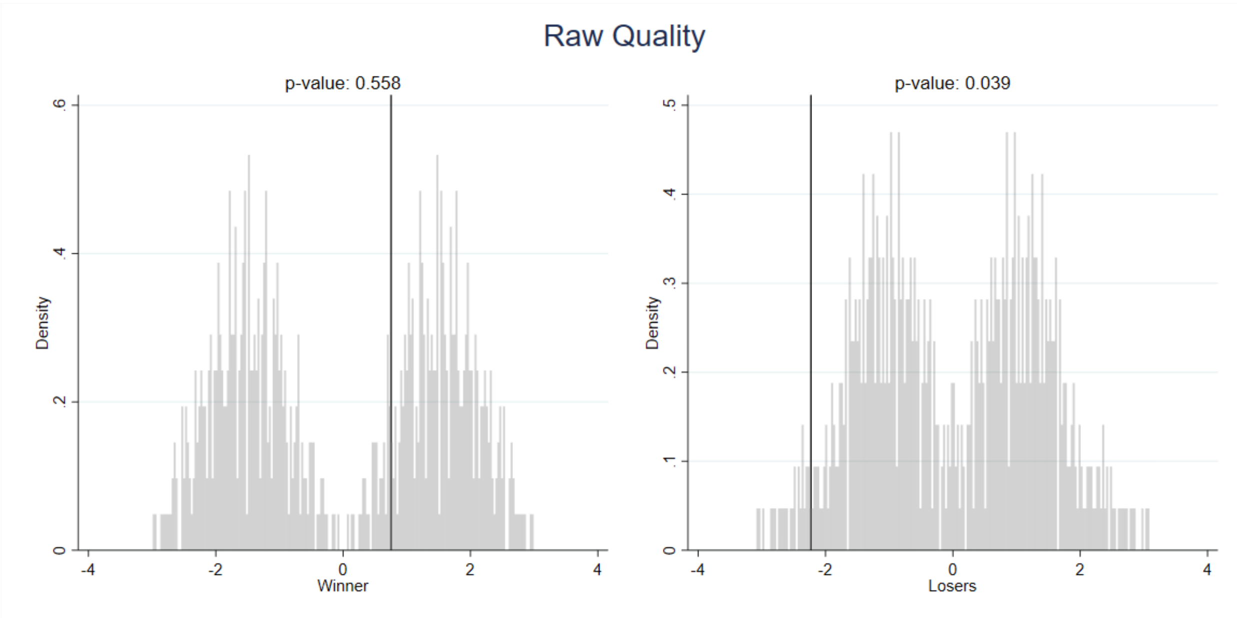

Figure 10: Quality

We can conduct analogous exercises for the probability that one of the losers submits a bid above the reserve price, and the number of bidders that submit bids below the reserve price. Figures 11 and 12 correspond to the Fisher's test. We reject the null at 10% significance.

It is difficult to rule out completely the possibility that the changes in the bidding behavior of the treatment firms captured by Figures 3 and 5 result from breakdown of collusion. However, the fact that the bids do not decrease and the quality of the losers *decrease* for treated firms suggest that this is unlikely. Typically, increased competition is associated with decreases in bids and increases in quality and number of bids. Our results are most consistent with the hypothesis that bidders continued to collude, but adapted to our screen by changing how they bid.

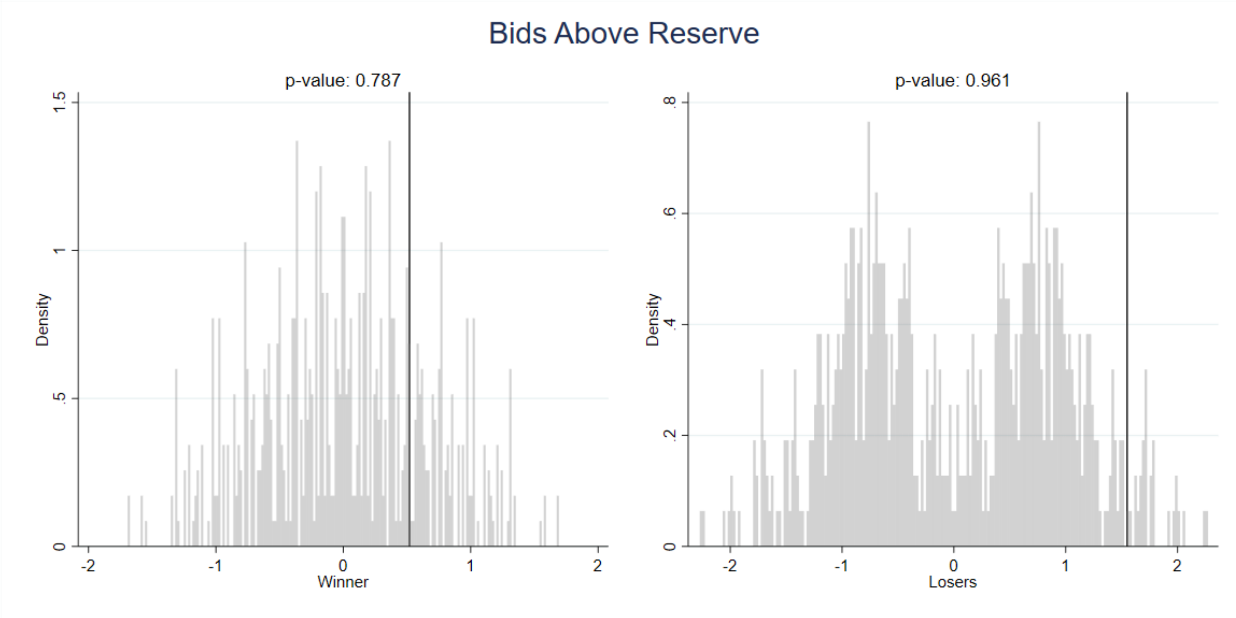

Figure 11: Bids Above Reserve

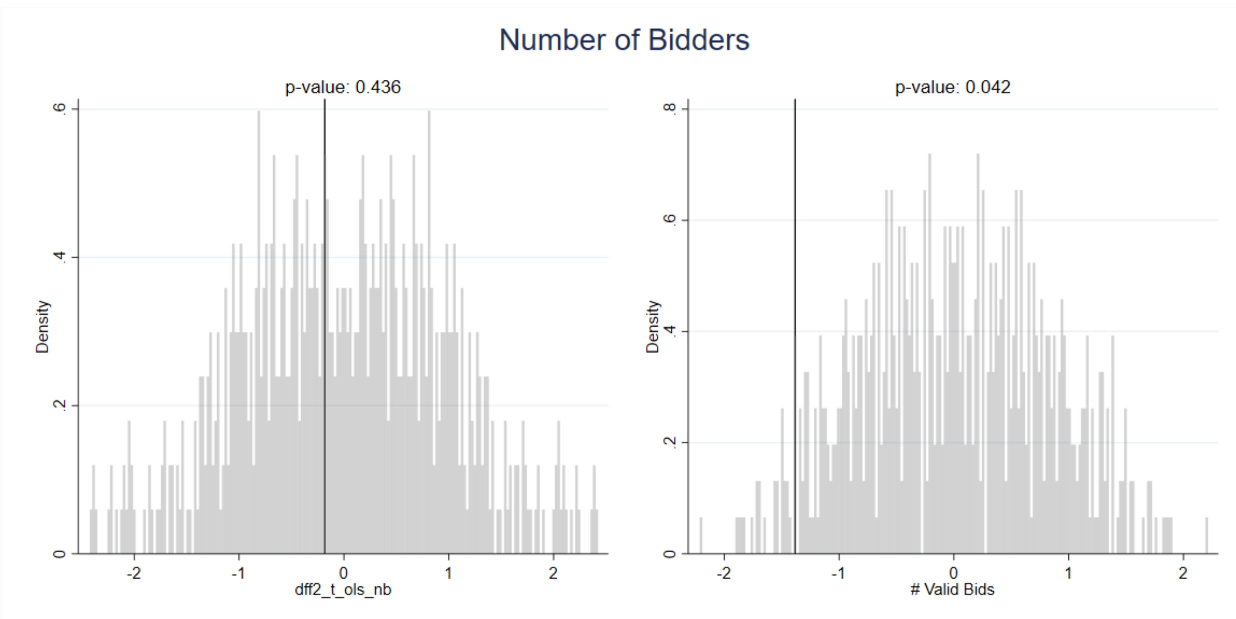

Figure 12: Number of valid Bids

Additional evidence of collusion Lastly, we provide additional direct evidence that bidders in the treated group continued to collude even after the intervention. In order to do so, we apply the test developed in Kawai et al. (2022) directly to the sample of treated firms. In Kawai et al. (2022), we construct a test that compares the backlog (i.e., amount of recently awarded projects) of marginal winners and marginal losers. Under the null of competition, any bidder is just as likely to be the marginal winner as the marginal loser. This implies that marginal winners and losers should, on average, have the same amount of backlog under the null.

Figure 13 corresponds to the bin scatter plot of backlog measures. We take Δ^s on the horizontal axis. Backlog is constructed by summing up the value of auctions won by each firm in a T-day window before the auction. In the figure, we use T equal to 30, 45, 60, 90, 120 and 150. For each T, there are two bin scatter plots, one corresponding to the period before the intervention and the period after. We find that there is a visible discontinuity for $T = 45, 60$ and 90 both for the period before and after the intervention. These results suggest that the treated firms continued to collude even after the intervention.

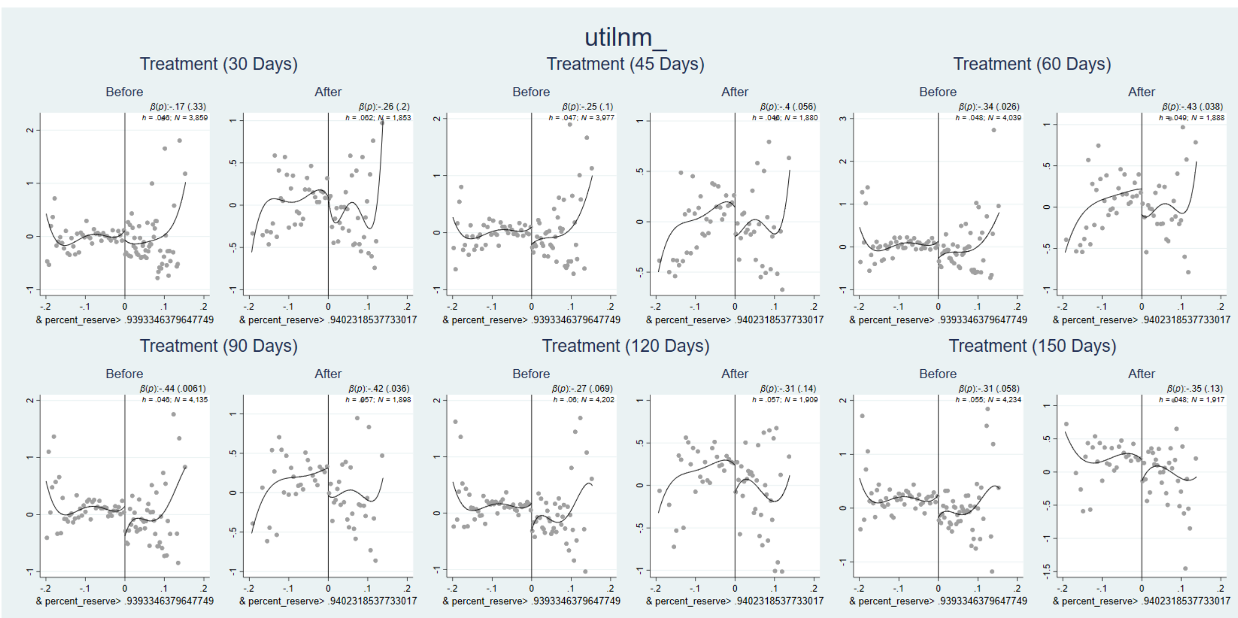

Figure 13: Binned scatter plot of backlog

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