Lobbying and Cartel Enforcement*

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Abstract

This paper examines the effect of firms' lobbying activities on penalties received from public competition authorities. We show that lobbying expenditures are negatively associated with the amount of the cartel sanction, within cartel. Our estimates also report that cartel sanctions are more responsive to lobbying expenditures when (1) the head of the antitrust authority is aligned with the president and (2) the lobbying firm qualifies for the leniency program. Overall, our results appear consistent with the regulatory capture and the informational lobbying theories, with a stronger effect for the latter.

Keywords: Cartel, Lobbying, US Antitrust Division, Cartel Enforcement, Cartel Sanctions, Competition authorities
JEL Classification: D72, G28, K21

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1. Introduction

The political influence on competition authorities is an old topic that goes back to the origins of competition laws.[1] Few examples of lobbying influence on antitrust enforcement are the 2003 *physician* price fixing cases[2] in the US or the 2011 *consumer detergents*[3] case in the EU. In both cases, lobbying efforts succeed in changing antitrust outcomes and rules. Despite a large body of literature examining the causes and consequences of cartels, relatively little is known about how firms manage the antitrust prosecution process. Exploring several channels of explanation and addressing the self-selection issue, this paper is the first to document the link between political influence activities and the US antitrust agency. Considering firms prosecuted by the US antitrust authority between 1998 and 2018, we examine whether politically connected firms receive a more lenient treatment from the competition regulator compared with non-lobbying peers.

The United States Antitrust Division's view of corporate cartels is clear, "[cartels] have no legitimate purposes and serve only to rob consumers of the tangible blessings of competition" (Barnett et al., 2012). Driven by optimal sanction theories from the economic literature,[4] the US competition authority relies on detection tools, criminal penalties and financial sanctions to eliminate cartels. With a budget increase of roughly 76% between 1998 and 2018 it seems reasonable to conjecture an increase in time of the US antitrust enforcement level, or a least no dramatic decrease. Nevertheless, the last years level of enforcement activity were being questioned amid poor willingness of the Trump administration to prosecute cartel cases.[5] For comparison, during Barack Obama's second term as President (2013-2016), US antitrust authorities sanctioned 78 firms for a total amount of $6,299M. By contrast, only 37 firms for a total amount of $1,133M have been charged during Donald Trump' presidency (2017-2020).[6] This recent radical shift in enforcement activity leaves room for the existence of a link between the severity of the US Antitrust Division and the stance of policy-makers, which can be politically influenced.

The economic and political literatures agree that lobbying is one of the most powerful tools to avoid costs such as enforcement actions (Phillipon, 2019).[7] While the private-interests theory documents the legislators ability and incentives to influence the outcome of cartel enforcement (Weingast, 1984; Laffont and Tirole, 1991),[8] antitrust regulators might also be receptive from lobbying. Firstly, lobbyists can provide issue-specific information to the resources constrained agency, which in turn can hasten the enforcement process (Bertrand et al., 2014). Secondly, special interest groups can take

[1] Kris Dekeyzer (DG Comp) in the Trade associations, Lobbyist and Competition Law forum at Concurrences.com (https://www.concurrences.com/) mentions the case of UK State monopolies granted to guilds by the crown in the 16th Century.

[2] One of them being the *Surgical Specialists of Yakima, P.L.L.C* case (http://www.ftc.gov/os/2003/09/surgicaldo.pdf).

[3] EC website: https://ec.europa.eu/competition/antitrust/cases/dec_docs/39579/39579_2633_5.pdf.

[4] See Harrington (2017) for an extensive review.

[5] The Trump administration has prosecuted less cartel cases than any government in the last 50 years (Shubber, 2019).

[6] US Department of Justice: https://www.justice.gov/atr/criminal-enforcement-fine-and-jail-charts (Last accessed: 15 January 2020).

[7] Some lobbying firms proudly communicate that their extensive interactions with regulators enable them to alter agencies' enforcement actions. See for example the website of Lobbyit.com company: https://lobbyit.com/services.

[8] Legislator can influence the regulators' decisions using various monitoring and disciplining mechanisms or threats if budgetary cuts (Lipton and Protess, 2013).

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advantage from the society cost concern of the antitrust authority when it imposes the financial sanction (Werden et al., 2011). Thirdly, political ideology can affect the level of antitrust enforcement from the agency (Groll and McKinley, 2015). For cartel members facing a sanction from the antitrust authority that negatively impacts their profitability and reputation (Gunster and van Dijk, 2016), the fact that they might try to avoid or reduce cartel sanctions using political connections is not surprising.

In this paper, we propose an empirical set-up in a cross-sectional design to formally test whether lobbying activities impact the outcome of cartel enforcement. We create a unique data set combining the Private International Cartel data (PIC)(Connor, 2016) with lobbying expenditures data[9] of prosecuted firms by the US Antitrust Division over the 1998-2018 period. By adding data on firm characteristics and industries, five sets of results are derived from our empirical analysis.

We first examine the relationship between lobbying expenditures and cartel sanctions. The regressions examine the aggregate amount of lobbying expenditures, the status of lobbying at the Department of Justice and the Federal Trade Commission, and their multiplication in the year of the sanction. Because the cartel sanction occurs at a point in time, we run cross-sectional regressions to assess the effect of lobbying expenditures on the level of sanction within cartel. The resulting marginal effect from one specification of our main results indicates that 1% change in lobbying expenditures has a statistically significant negative effect of -0.036% change on the amount of the US cartel sanction. In a following set of results, we examine the impact of the lobbying status to the competition authority on the level of the sanction. Our estimates document that lobbying either the DoJ or the FTC (or both) during the year of the sanction decreases the log transformed amount of sanction by 0.911. This is equivalent to a decrease in the sanction by a factor of 2.42 ($e^{0.911}$).

Second, we adopt instrumental variable (IV) strategies to mitigate some of the endogeneity concerns, whereby, step 1 accounts for the determinants of firms' lobbying activities and step 2 estimates the influence of lobbying activities on cartel sanctions. Our instrument proxies the cost of lobbying (Kerr et al., 2014). The results indicate that 1% change in lobbying expenditures has a statistically significant negative effect of -0.071% change on the amount of the US cartel sanction. Compared to our main linear results (-0.036%), not treating the endogeneity selection gives us upward biased estimates (de Figueiredo and Richter, 2014). To avoid for the high correlation between firms' lobbying amounts across years, we run the same regressions considering a measure of lobbying increase as explanatory variable and find results consistent with the previous ones. These robustness checks confirm the conclusion from our main results. Taking an average firm in terms of lobbying ($0.68M) and cartel sanction ($14.07M), the result from our main specification translates into a reduction of $3.12 for each $1 invested in lobbying and corresponds to a lobbying return of 312%. This translation of the result should be interpreted with caution as it is highly dependent on the levels of lobbying and sanction expenditures included in the calculation.

[9]The two other politically influential tools are campaign contributions from political action committees (PACs) and revolving doors. PACs are contributions from individuals and corporations supporting or opposing political candidates (Milyo et al., 2000). Compared with lobbying expenditures, which can be related to informative signaling and legislative lobbying (e.g., legislative drafting), PACs can be perceived as a middle of exchange (vote buying) (Hall and Deardorff, 2006). They are also distinctly smaller in terms of spending than lobbying. The term "revolving doors" indicates that ex-regulators are hired by lobbyist agencies (see, Goldman et al., 2013).

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Third, to validate the relevance of our instrument, we investigate whether the lobbying strategy of firms changes with a change in distance between their headquarter and the antitrust authority's location. As an identification strategy, we take advantage of the adoption of a new competition regulation, which corresponds to the closure of four Antitrust Division offices on January 1, 2013. The impact of this decision triggers a decrease in antitrust enforcement for firms that were near to the closed offices and an increase in enforcement for firms headquartered near to the remaining regional offices (with higher resources) (Ha et al., 2022). We measure a firm's exposure to this exogenous shock by the change in the distance between the firm's headquarters and the nearest new DoJ regional office. Taking the increase in lobbying as the dependent variable, we perform a difference-in-difference panel analysis around the 1st January 2013 that includes the change in distance and an interaction term between the change in distance and a binary variable that indicates the year of the event. Our results indicate that increased resources for antitrust enforcement are positively associated with increased lobbying expenditures. In addition, the section on the instrument validation provides tests of exogeneity on observable variables.

We then perform a number of tests to further establish the robustness of our main results. In several sets of test, we assess whether the coefficient on our variable of interest is changed when we specify the sample differently and when we take different measures of lobbying expenditures. Overall, the results are consistent with our main conclusion. We perform an additional robustness test by taking the amount of fines from damage claims instead of the amount of the penalty imposed by the competition authority. This placebo test indicates that lobbying expenditures have no impact on the level of the private penalty resulting from a cartel suit, confirming the robustness of our main specification.

To better understand the channel through which lobbying might impact regulatory decisions, our final set of results includes heterogeneity in the relationship between lobbying expenditures and anti-cartel sanctions. First, we seek to disentangle the explanation for these results by including an interaction between lobbying expenditures and a binary variable equal to one if the head of the Antitrust Division is not aligned with a US president. By appointing the agency's Assistant Attorney General (AAG or Division chief), the ideology of the US president shapes decisions at the US Antitrust Division level. Whenever the US president is not aligned with the AAG, the lobbying influence on the president does not percolate towards the Antitrust Division. Our results indicate that the negative impact of lobbying expenditures on the sanction is weaker when the Division chief is not aligned with the US president. This result is consistent with the regulatory capture theory (Grossman and Helpman, 1994). Second, we leverage cartel members' need for information to qualify for the leniency program to study the explanatory channel of informational lobbying. Specifically, we include an interaction term between lobbying expenditures and a binary variable equal to one if the cartel member qualifies for the leniency program. Our results show stronger and statistically more significant effects. Consistent with the literature on informational lobbying, these estimates indicate that lobbying firms are more informed about the enforcement process and/or better inform the regulator. Overall, our results indicate a significant negative association between lobbying expenditures and cartel sanctions, although these results are consistent with regulatory capture theory, this phenomenon is primarily explained by the informational lobbying theory (Bombardini and Trebbi, 2020).

The empirical literature on political economy documents that lobbying activities might be valuable

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for firms. Several papers evaluate the impact of lobbying activities on policy-makers' decisions in favor of interest groups. For example, lobbying the US Congress impacts the US corporate tax system (Richter et al., 2009; Alexander et al., 2009) and the structure of US trade tariffs (Gawande et al., 2006; Blanga-Gubbay et al., 2020). Another strand of the literature documents the association between lobbying activities and the likelihood of enforcement actions from regulatory US federal agencies, such as the Federal Trade Commission (Holburn and Vanden Bergh, 2014) or the banking regulators (Correia, 2014; Lambert, 2019). These studies suggest that lobbying influence percolates from legislators to regulatory agencies. Other papers focus on the ability of prosecuted firms to influence the outcome of an enforcement action through lobbying activities. Faith et al. (1982) and Weingast and Moran (1983) investigate the link between districts of legislators and antitrust enforcement actions. They find that firms prosecuted by the US Antitrust Division are more likely to get their case dismissed if they have headquarters in the same districts as legislators. More recently, Yu and Yu (2011) indicate that fraudulent lobbying firms are less likely to be detected by authorities, Faccio and Zingales (2017), Phillipon (2019) and Cowgill et al. (2022) show the relationship between corporate political connections with the degree of market competition and Fidrmuc et al. (2018) and Mehta et al. (2020) document favorable M&A antitrust decisions for lobbying firms.

This study also relates to the literature on cartels, which originates from the industrial organization literature. There is abundant theoretical literature on cartels, from oligopolistic competition models (Stigler, 1964 and Friedman, 1971) to more recent analyses of collusion by software programs (Harrington, 2019). We can differentiate the empirical literature on cartels into descriptive and econometric analysis. Most empirical descriptive papers focus on the efficiency of cartels. There are various ways to assess cartel efficiency. For instance, some studies explore the determinants of cartel duration (Suslow, 2005; Levenstein and Suslow, 2011; Hyytinen et al., 2020), while others evaluate either cartel overcharges (Connor and Bolotova, 2006; Connor, 2007; Combe and Monnier, 2012; Allain et al., 2015) or the impact of antitrust authorities' investigations on the stock price response of firms (Aguzzoni et al., 2009; Gunster and van Dijk, 2016). To the best of our knowledge, only three papers examine the determinants of cartel sanctions. Cohen (1996), Bolotova and Connor (2008) and Connor and Miller (2013) investigate the determinants of sanctions for corporate crimes. Cohen (1996) considers all types of corporate crimes and finds evidence that sanctions are proportional to the relative level of harm, while Bolotova and Connor (2008) and Connor and Miller (2013) focus on sanction determinants for cartel cases. Bolotova and Connor (2008) conduct an estimation at the cartel-level and suggest that cartel sanctions are negatively related to price overcharges. Using a firm-level data set, Connor and Miller (2013) find a positive correlation between the amount of cartel fines and both price overcharges and the size of the affected market. While controlling for the determinants of classical sanctions, we extend the literature assessing the marginal impact of lobbying expenditures on cartel sanctions.

The contributions of this paper are twofold. Firstly, we extend the optimal deterrence theory of crime (Cohen, 1996) towards interplays between firms and regulators. A consensus in the industrial organization literature suggests that the amount of sanction imposed on cartels affects the risk-taking behavior of cartelists (Harrington, 2006). By showing that firms can rely on their influence through lobbying to mitigate the punishment by antitrust authorities, in theory, this study informs about the existence of a potential new determinant of cartels' stability. Secondly, while most of the empirical literature on lobbying and policy enforcement investigates the effect of lobbying on the risk of prosecution, we analyze the ability of prosecuted firms to mitigate the outcome of such enforcement action.

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The paper is organized as follows. Section 2 develops the hypothesis and Section 3 describes the data. The Section 4 presents the results and Section 5 concludes.

2. Hypothesis development

As regulatory agencies have limited resources, many competition authorities use mechanisms to hasten the enforcement process and save resources (OECD, 2008). In the United States, the vast majority of corporate cartel sanctions are the result of plea agreement negotiations between the prosecutor and the defendant, which happen behind closed doors, and are out of public eye (Connor and Miller, 2013). The Division attorneys have, under some standards, discretion regarding the enforcement outcome.[10] This discretionary power is mainly limited by two statutory schemes: the United States Sentencing Commission Guidelines (USSGs) and the Alternative Fines Act (18 USC paragraph 3571 d) (FCPA, 2012). However, since January 2005, the USSGs has adopted an advisory role (DoJ, 2020), which increases the discretionary power of regulators and gives more room for political influence. In this section we rely on the legal background, the enforcement process and the political connections literature to set the hypotheses of our research.

2.1. The US Antitrust Division enforcement process

In the United States, the enforcement of the Sherman Act (15 USC paragraph 1) is the responsibility of the Antitrust Division of the Department of Justice ("the Division", hereafter) and the Federal Trade Commission (FTC). Even if their authorities overlap, the Division is the only one that can impose criminal sanctions. USSGs promulgate detailed calculations of the corporate sanction for each convicted offender. The sanction is computed in three steps. First, a base fine is calculated from the volume of affected sales. The guidelines suggest that 20% of the volume of affected sales is a good estimation of the gain from the offense. Second, aggravating and mitigating factors are considered, and lead to the attribution of a culpability score for each offender. Third, the base fine is multiplied by the culpability score (ranged between 0,75 and 4). Theoretically, the final sanction must not exceed either twice the financial gain from the cartel or $100 million. Nevertheless, in the sample period we consider, some Sherman Act violations resulted in fines above $100 million. Under the Alternative Fines Act, the court may impose up to twice the gain that the offender obtained. As the gain from the offense is hard to estimate, the Division negotiates mutual agreement with some defendants to determine the sanction's calculation (Connor and Miller, 2013).

Along with the statutory schemes, two legal mechanisms have substantial impact on cartel corporate sanctions, namely, the Division's corporate leniency program and plea agreement. The corporate leniency program guarantees full immunity from federal prosecution to the first confessor with enough information about the cartel. To determine who was the first applicant, the Division gives a marker to hold the place of the applicant in line. The Front Office is in charge of verifying whether the applicant fulfills the conditions to obtain leniency and, in case of recommendation, the Assistant Attorney General takes the final decision (DoJ, 2020). The alternative criminal dispute resolution mechanism,

[10] Department of Justice notice from the Assistant Attorney General Brian A. Benczkowski (Ethics and Compliance Initiative 2019), https://www.justice.gov/opa/speech/assistant-attorney-general-brian-benczkowski (Last accessed: 30 January 2020).

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i.e. plea agreement, gives sanction discount for cartel members who cooperate with the investigation, and is granted after one member of the cartel gets full-immunity from the sanction. Plea agreement recommendations are almost always accepted, in that case the trial does not take place.[11]

In theory, members of the competition authority have substantial discretionary power in several stages of the enforcement process. Investigations can be triggered by various sources. Two examples are leniency applications and citizen complains. For criminal matters, like cartel infringements, a preliminary investigation is opened after the review of the appropriate Directors and the clearance of the FTC. If the latter decides to continue the investigation, the Division's staff submits a plan of investigation to the appropriate directors and the assigned Deputy Assistant Attorneys General (DAAG). The investigation ends when the Division's staff and the chief believe that the matter should not be investigated further. After the investigation and the filling of indictment, the Division's staff establishes recommendations for sentencing. The recommendation includes a proposed sanction, based on calculations from the USSGs. For instance, when making sentencing recommendation, the Division's staff should notify possible reduction or increase of the base fine following changes in the culpability score. The recommendation is reviewed by the Principal DAAG and sometimes by the Assistant Attorney General. For the cases of plea agreement, the staff should prepare plea agreement recommendations in accordance to the Division policy directives. Deviation from policy directives must be documented and approved by the Assistant Attorney General. As noted above, the nature of the USSGs is advisory since 2005, but any deviation from the USSGs still requires supervisory approval. Even if the Government's discretion in setting the sanction is limited by the USSGs, it can have a substantial impact on the sanction by filing a motion. For instance, the chief prosecutor can recommend a reassessment of the sanction on the basis of a defendant cooperation. The judge reviews the recommendation and has wide discretion in deciding whether to take it into account. When the case is ruled, the Appellate Section is contacted in the event of an appeal of the decision. The PIC data set does not consider appealed decisions, since appeals usually take five to ten years to be decided (Connor, 2016).

Insert Figure 1 here

Figure 1 displays the enforcement process of the Division from the cartel detection to the case resolution. Five regional offices are in charge of the investigation phase, two in Washington DC and the others in San Francisco, New-York and Chicago. Several other federal agencies are involved in the cartel enforcement process, the Federal Bureau of Investigation, the Department of Defense, the Department of Transportation, the Security Exchange Committee or the Federal Trade Commission, to name a few. The involvement of a particular regional office and other agencies mainly depends on the related industry.[12] As regional offices send their recommendations to the AAG (located in the main Division office in DC) and the majority of other agencies' headquarters are located in the US capital, the decision-making power is concentrated in Washington DC. Knowing that the majority of cases do not go to trial, our hypotheses will focus on the political influence done at the US Capitol through the total amount of lobbying expenditures. Results of the lobbying status specifically at the DoJ and FTC will also be presented (the amount of lobbying expenditures per agency is not available in the lobbying disclosure reports). Related to the timing of lobbying, we assume that cartel members

[11] See IIEA Youtube video of Scott Hammond: "Scott Hammond on A United States Perspective on Cartel Enforcement".

[12] DoJ Youtube video: "US Department of Justice: Recognizing Antitrust Conspiracies & Working with the Antitrust Division".

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do not have incentives to inform the authority before the detection, excepted if a member decides to apply for the leniency program (which triggers the investigation). Along the prosecution process our analysis focuses on lobbying expenditures at the year of the sanction. This decision is based on the fact that the approval for the amount of the sanction or the deviation from the USSG is taken by the head of the Division at the end of the process. Given the differences in the length of the enforcement process, we also check the robustness of our results using lobbying expenditures from years preceding the year of the sanction.

2.2. Theoretical framework

There is not one single way for corporations to influence regulatory decisions through lobbying activities. Firms can lobby directly the federal agencies (de Figueiredo and Tiller, 2001) or the senators and the representatives (Mehta et al., 2020) that oversight the regulatory agency in exchange of regulatory favors (Gordon and Hafer, 2005). In a less direct way, lobbying can be dedicated to influence the drafting of appropriation bills during the budgetary process (Lipton and Protess, 2013), with the intent to cut regulatory agencies' budget. For example, in response to the US vs. Microsoft (1990) case, Microsoft's officials urged congress members to cut Clinton's recommendation for the budget allocation of the Division by about $9 million (Morgan and Eilperin, 1999). Gordon and Hafer (2005) indicate that any lobbying expenditures observed by the agency could be used to signal the company's willingness to contest the regulatory agency's decision.

The literature on regulatory capture also assumes that bureaus are highly responsive to the policy preferences of Congress (McNollgast et al., 1999). Weingast and Moran (1983) establish three key factors of congressional dominance in the Division's behavior: the budgetary process, Congress oversight, and the appointment of decision-makers. First, in the budgetary process, agencies compete against each other to obtain budgetary favors from Congress. In particular, it has been argued that the creation of two authorities, i.e., the DoJ and the FTC, reveals the desire of antitrust agencies to increase their budget, despite a loss of efficiency (MacKay et al., 1987). Second, congressional sanctions may be imposed on the bureaucrats, which can have ex-post negative effects on staff careers (Zingales, 2013). Lindsay (1976) shows that the recidivism rate in the FTC is better explained by staff career goals, rather than the decrease in consumers' welfare loss. Finally, according to Weingast and Moran (1983), the appointment of the Division's decision makers is perhaps the most effective control factor of Congress on bureaucrat behavior. The Attorney General and Congress appoint the most senior officials on the Division. The President designates one Assistant Attorney General, who is confirmed by the Senate (DoJ, 2020). In short, while the views of bureaucrats have substantial impact on regulatory programs, the President and Congress determine which views are represented (Weingast, 1984).

The hypothesis we aim to test concerns the relationship between lobbying expenditures and cartel sanctions. The interest-group theory established by Stigler (1971) and Peltzman (1976) indicates that private interests are not independent from antitrust enforcement actions, including those of firms prosecuted by the antitrust authority. For example, Faith et al. (1982) show that firms headquartered in legislators' districts are more likely to avoid antitrust actions. The promotion of industries plays a significant role in the activities of committee members and percolates into the Antitrust Division's enforcement action (Posner, 1979). A politician representing a district is likely to try to attract federal funding (leading to the creation of new jobs), or avoid loss of wealth for the district's constituent firms (keeping the number of jobs stable) (Shughart II and McChesney, 2010). Related to market concen-

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tration, Fidrmuc et al. (2018) and Mehta et al. (2020) demonstrate that merging companies connected to members of the judiciary committee are more likely to receive a favorable answer from the antitrust authority. In this paper, we hypothesize that *lobbying expenditures are associated with lower amount of sanctions*.

To disentangle the complexity of our results, we link them with two potential channels of explanation documented by Groll and McKinley (2015). First, we take advantage of the US president choice for the appointment of the AAG. Under the regulatory capture theory, we posit that *the negative association between cartel members' lobbying and sanctions is weaker when the AAG is not aligned with the US president*. After a new elected president arrives at the White House, it takes roughly nine months for the new AAG to start her position. We assume that when the head of the Antitrust Division is not aligned with a US president, i.e. at the beginning of her presidency term, the influence of private-interests is higher. For example, President Trump appointed a former lobbyist as Division's Assistant Attorney General,[13] with a noticeable decrease in the number of cases prosecuted by the Division since, and antitrust sanctions in 2017 at a ten-year low (Higbee et al., 2020). While Donald Trump started his new position in January 2017, Makan Delrahim was nominated by the Congress in June 2017 and started his new position end of September 2017. This hypothesis is also in line with an increasing empirical literature on political influence and alignment of ideologies (Ellis and Groll, 2020). The second channel we explore relates to the informational lobbying theory. The theoretical literature on political connections assumes that lobbying is a way to inform the lawmakers about the economic consequences of their decisions in the presence of asymmetric information (Laffont and Tirole, 1991). This informational mechanism is similar to the one of the leniency program, under which the Division offers a sanction immunity in exchange of information. The sanction immunity is received by the firm reporting the cartel before the beginning of the investigation but under some conditions. One of these conditions is the ability to provide enough information on the cartel. For this last hypothesis, we posit that *the impact of lobbying is stronger when the cartel member qualifies for the leniency program*. Overall, we propose to formally test the hypothesis that regulatory agencies decisions are not independent from private interests, and if so, that the relationship between lobbying activities and antitrust sanctions is negative.

3. Data and descriptive statistics

3.1. Cartels

The data on cartels is drawn from the Private International Cartels (PIC) data set (Connor, 2016). The available version of the data set offers a wide range of information about convicted international cartels from January 1990 to September 2018. Further information about hundreds of cartels, along with a detailed discussion on both the collection strategy and a variables' description can be found in Bolotova and Connor (2008) and Connor and Miller (2013). The most recent case is the *ADRs Bid Rigging* cartel that occurred between 2012 and 2014 and for which nine international banks have been sanctioned by the DoJ in 2018. Some firms participated in several cartels, for example, the Japanese firm *Mitsuba corp.* (specialized in the car industry) is a multiple offender present in seven different cartel cases. Marvão (2021) documents the learning process of multiple offenders and its

[13] Open Secret, Makan Delrahim profile: https://www.opensecrets.org/federal-lobbying/summary (Last accessed: 30 January 2020).

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substantial impact on the cartel sanction. In our regressions we take into account this kind of behavior by controlling for recidivism. The sample contains information firm and cartel characteristics. For each specification we control for the time and industry fixed effects. We also provide estimates controlling for the cartel fixed effects. As the number of variables is high compared with the number of observations when we include all the fixed effects (when we study the relationship between lobbying expenditures and sanction in a cross-sectional way), our main specifications include firms and cartels characteristics controls (based on the model of Connor and Miller, 2013) without cartel fixed effects to avoid overfitting issues. The sample collected comprises 463 firms that were active in 109 cartels convicted by the US Department of Justice from 1998 to 2018. The sample period is determined by the lobbying expenditures data, which are available from 1998. When we control for firms and cartel characteristics (which are essential determinants to study the antitrust sanction) our specification suffers from missing variables. While the total number of observations for some variables equals 657, the number of observations drops to 422 when we include the total assets variable. Some cartel members are not listed companies and their financial information is not publicly available. For example, we don't find any information on the Taiwanese firm Tung Hat Fermentation that was active in the 2001 Nucleotide cartel case. The other variable with missing information is the affected sales variable, which is missing in the data set we use and is not available in all the cartel decision reports. Even when we do not include the controls (only the fixed effects) we end-up with 629 observations because of missing information (for few cartels) on the year of the sanction in the Connor (2016)'s data set. A detailed description of our variables is available in Table 1.

Insert Table 1 here

Descriptive statistics for the cartel sample are reported in Table 2 and Figure 2 displays the time distribution of DoJ fines with a peak level in 2015. The average fine per firm is about $14.07 million and the sum of fines between 1998 and 2018 is about $9,244 million. The average duration of a cartel in our sample is about 9.43 years. This information is often underestimated, as competition authorities base their estimation on both available evidence and the oldest proof they obtain, which can be more recent than the creation of the cartel. The average number of cartel participants per cartel is 13.05. Bid-rigging cartels represent 65% of the sample. These descriptive results are in line with the empirical antitrust literature (see, Miller, 2009 or Levenstein and Suslow, 2011).

Insert Table 2 and Figure 2 here

3.2. Lobbying expenditures

We use lobbying disclosure reports to collect information about lobbying. The Lobbying Disclosure Act (1995)[14] and the Open Government Act (2007) require lobbyist to register and report information on their activities to the Senate Office Public Records. Data are compiled by the Center for Responsive

[14]The LDA defines *lobbying activity* as follows: "lobbying contacts and efforts in support of such contacts, including preparation and planning activities [...] that is intended [...] for use in contacts, and coordination with the lobbying activities of others." The *lobbying contact* is "any [...] communication to a covered executive branch official or a covered legislative branch official that is made on behalf of a client with regard to (i) the formulation, modification, or adoption of Federal legislation (including legislative proposals); (ii) the formulation, modification, or adoption of a Federal rule, regulation, Executive order, or any other program, policy, or position of the United States Government; (iii) the administration or execution of a Federal program or policy (including the negotiation, award, or administration of a Federal contract, grant, loan, permit, or license); or (iv) the nomination or confirmation of a person for a position subject to confirmation by the Senate."

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Politics (CRP), a non-profit organization that promotes political transparency.[15] Lobby variables are constructed with the name of the cartel member, the corresponding lobbying expenditures and the related industrial sector.[16] Our analysis relies on the amount of lobbying a cartel member expends in a given year and for a specific sector. Similarly to the majority of empirical researches on lobbying, we consider lobbying activities at the mother institution level (Correia, 2014; Fidrmuc et al., 2018; Lambert, 2019). Indeed, subsidiaries benefit from the lobbying through parent companies without necessarily lobbying on their own (Miard, 2014). To control for the different lobbying practices across sectors, we collect the average lobbying expenditures in a given year for each industry related to the cartel member.[17] The taxonomy of industries is taken from CRP, which is based on 2-digits SIC codes.

Insert Figure 3 here

Figure 3 reports statistics on cartel members' lobbying expenditures per year. In line with the lobbying literature, we observe an overall increase of lobbying expenditures over the 1998-2018 period. Descriptive statistics show that almost 46% of our observations have lobbied at least once over the sample period. When we focus on our main specification (the year of the sanction), we find positive lobbying expenditures for 42% of our observations. The proportion of cartel members active in lobbying expenditures is fairly stable, the correlation between lobbying expenditures and its lagged value equals 94%. The highest amount of lobbying was spent by the US giant *General Electric Co.* in 2010 for a total of $39 million. For the same year, the average sectoral lobbying from the same industry was about $246,093. This disparity of lobbying within sectors is fairly stable over the sample years and well documented in the lobbying literature (see, Bombardini and Trebbi, 2012). To control for the size of firms, we also collect data from *Bloomberg* on cartel members' total assets. The sum of lobbying expenditures made by all detected cartel members in our sample corresponds to 17% of their aggregated total assets and the correlation between these two variables equals 7%. As presented in Figure 4, when we divide our sample in eight different groups, ranked from lowest to highest by the value of their total assets, we observe that lobbying firms receive lower sanction than non-lobbying firms for seven categories out of eight.

Insert Figure 4 here

[15] Center for Responsive Politics: https://www.opensecrets.org/federal-lobbying. (Last accessed 30 September 2020). Along with lobbying expenditures, the CPR also provides data on political contribution data. This research focuses on lobbying expenditures mainly because lobbying expenditures, compared with PACs, represent the bulk the lobbying activities (Ansolabehere et al., 2002 and Bombardini and Trebbi, 2012). Nevertheless, we control for campaign contributions from the cartel members to the house and senate judiciary committees (Cohen et al., 2011). To do so, we collect PACs data on each chairpersons and ranking members of both senate and house's judiciary antitrust subcommittees (which oversight both the DoJ and the FTC) during the 1998-2011 period. We control for the senators and the representatives in position of either chairman or ranking member during the sample period. To stick with Connor and Miller (2013)'s sanction determinants and avoid collinearity issues, we do not include these controls in our presented results. Nevertheless, we do not notice quantitative changes when we include them in our regressions.

[16] One firm can lobby several industries and/or be active in several industries. For a specific firm, we collect data on lobbying expenditures per industry. Controlling for related industries, we aggregate lobbying expenditures and link the firm with its main activity sector (this information is taken from CRP, in almost all cases, firms favour one industry in terms of lobbying expenditures).

[17] We also collected to which federal agency a cartel member is lobbying (data are available from CPR). We were interested in the direct lobbying toward the DoJ which directly oversight the Antitrust Division.

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4. Empirical results

4.1. The lobbying impact on cartel sanction

This paper aims to determine whether lobbying expenditures affect antitrust sanctions. More precisely, we examine the impact of lobbying expenditures on the cartel sanction around the year of the sanction. We conjecture that the major impact of the lobby on the sanction takes place at the end of the enforcement process and not during the (mechanical) investigations of the antitrust regional offices. In addition, (see Section 2) the main decision related to the level of sanction is taken at Washington DC, and not at the regional offices. As we have lobbying yearly data, we examine the effect of lobbying expenditures (at Washington DC) during the year of the sanction to reduce the risk of confunding effects. Results with the lagged transformation of our lobbying measure will also be presented in the robustness checks section. To study our main hypothesis, we rely on cross-sectional regressions at the year of the sanction with a baseline model inspired by Connor and Miller (2013), which estimates the determinants of the Division's cartel sanction:

$$LN(Sanction_i) = \sum_{i=1} \beta_0 X_i + \beta_1 LN(Lobby_i) + u_i$$

where X_i contains the control variables and u_i is the error term. Control variables (X_i) are divided in two main categories, *Firm Controls* and *Cartel Controls*. The category *Firm Controls* contains information on firm characteristics: the size, the number of cartels in which the firm was involved, whether it received the leniency program and the other penalties the firm received. Based on the model of Connor and Miller (2013), *Cartel Controls* contains the variables on cartel characteristics (see Table 1). Our three main specifications use alternatively *Firm Controls*, *Cartel Controls* and controls for cartel fixed effects. Even though controlling for cartel fixed effects absorbs all the between cartels variation, this option is very demanding for our predictions as it uses a lot of variable compared with the number of observations. Using *Cartel Controls* instead of controls for cartel fixed effects allows us to have a higher explanatory power (when we focus on the adjusted R^2), while controlling for cartel characteristics. In a third specification we drop *Firm Controls* and *Cartel Controls* to avoid missing values (as discussed in Section 3) and get as much observations as possible. The main downside of this specification is that it does not control for firm differences inside a cartel. Not controlling for firms heterogeneity might lead to biased estimates as the predictor would be correlated with the error term. Nevertheless, we limit this potential issue by implementing cartel fixed effects (controlling for variation between cartels) and including firms characteristics controls. The inclusion of firm fixed effects is not possible as the sanction is in a point in time, which allows only for cross-sectional regressions with a limited amount of firm variations. The $LN(Sanction_i)$ variable refers to the logarithmic transformation of one plus the amount (in \$US) of sanction a cartel member gets from the US competition authority the year of the sanction. The explanatory variable, $LN(Lobby_i)$, is the logarithmic transformation of one plus the lobbying expenditures (in \$US) the year of the sanction. As the sanction occurs in a point in time, we do not have longitudinal data and therefore decide to compare the impact of the amount of lobbying the year of the sanction on the dependent variable within cartel. In a following section we will also test the robustness of our results when we take other transformation of the lobbying measure such as the lagged value (during the investigation process) and the change in lobbying. The change in lobbying from one year to another is interesting as it allows to compare an additional variation between the observations that is more likely to be related to the antitrust sanction situation. Nevertheless, because of the cross-sectional strategy, we might lose

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information by using this transformation. For example, whether a firm never lobbies or lobbies the same positive amount each year the change in lobbying amount will be equal to zero. As a result, the following set of results will focus on a cross-firm analysis.

Insert Table 3 here

Table 3 reports OLS regression results of our main model taking the logarithmic transformation of one plus the amount of the US cartel sanction as the dependent variable and the logarithmic transformation of one plus the lobbying expenditures the year the sanction as the explanatory variable. We present two sets of results, the first one taking the entire sample period (1998-2018), while the other focuses on the period after the Antitrust Criminal Penalty Enhancement and Reform Act of 2004 that entered into force in January 2005 (2005-2018). This decision is a substantial change for the US criminal law as it modifies the foundation of federal sentencing that existed for almost two decades before it. Aiming at incentivise wrongdoers to cooperate more with the authority, the new Act (i) lets the sentences' decisions largely to the discretion of the sentencing Antitrust Division and (ii) allows for more lenient treatments (e.g. with an increasing use of plea agreements). We conjecture that this change gives more room for lobbyists to influence the regulatory decisions and we expect to see stronger effects for the second set of results as a robustness from our main specifications that encompass the entire sample period.

The Table 3's results from columns 1 to 3 focus on the entire sample period, while specifications 4 to 6 display the results from the 2005-2018 sample period. Except for the third specification, all Models document a negative association between lobbying expenditures and the level of the cartel sanction. In addition, we observe that the results are stronger and more statistically significant when we focus on the 2005-2018 sample, which might indicates a stronger influence from lobbying expenditures on regulatory decisions when the Division has more discretionary power. To accurately answer this question we would have to compare the results from the 1998-2004 and 2005-2018 sample periods, unfortunately we do not have enough observations in the 1998-2004 sample period to predict estimates. If we focus on the Model 4, the marginal effect indicates that 1% change in lobbying expenditures has a statistically significant negative effect of -0.04% change on the amount of the US sanction. The negative association between our dependent and explanatory variables holds also within cartel.

Insert Table 4 here

Table 4 presents the results from linear regressions, taking as explanatory variable either a binary variable equal to one if the firm lobbied the DoJ, the FTC or both (lobbying status DoJ/FTC) the year of the sanction or the multiplication between that binary variable and the logarithmic transformation of one plus the amount of lobbying expenditures the year of the sanction. The dependent variable is similar to the one from Table 3, which is the logarithmic transformation of one plus the amount of sanction imposed by the Antitrust Division. We do not have the precise amount of lobby dedicated to one federal agency. The information on the lobbying disclosure reports indicates the names of the lobbied agencies for a certain amount lobby but not how the expenditures are distributed between agencies. The second measure is then a proxy of the lobbying expenditures to the competition US federal agencies, which is equal to the expenditures times one if the firm did lobby the DoJ, the FTC or both (lobbying status DoJ/FTC). Even though the FTC does not handle criminal cases, the two agencies have strong ties as they might work on the same cases, share opinions on competition laws

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and (at least for the higher managers) go to the same conferences, workshops and meetings.[18] Taking either the DoJ/FTC dummy or the multiplication between that binary variable and the lobbying expenditures allows us to increase the precision of our results. As lobbying expenditures might be use for a wide range of reasons, by narrowing (even if the measure remains not perfect) the explanatory variable to expenditures related to competition agencies we get rid of a substantial amount of lobbying amounts that are less likely to connects with antitrust matter. Specifically for the targeted federal agency the data are not available before 2008 (Blanga-Gubbay et al., 2021). The reason is mainly because lobbying reports filed prior 2008 are not available in scannable pdf format, and some of them are digital versions of handwritten documents. Starting from 2008, following the Honest Leadership and Open Government Act of 2007, lobbying reports are filed electronically. This issue concerns only the information on the federal agencies and the nature of the lobbied bills.

All the estimates are consistent with the results from Table 3. The first specification reports that lobbying either the DoJ or the FTC (or both) during the year of the sanction decreases the amount of the sanction by 88.7%. In terms of lobbying expenditures targeting partially the competition agencies, the column (4) indicates that one percent of change in lobbying expenditures decreases the amount of the sanction by 0.063%. If we consider the average amount of sanction ($14.07M) and the average lobbying expenditures ($0.68M), the result from the fourth column can be interpreted as a sanction decrease of $1.3 for each dollar invested in lobbying the DoJ or the FTC (or both) during the year of the sanction $\left(\frac{\partial sanction*lobby}{\partial lobby*sanction} = -0.063 * \frac{14.07}{0.68} = -1.30\right)$.

4.2. Addressing endogeneity

The political connections literature extensively documents that lobbying expenditures is an endogenous variable. For example, it can be possible that cartel members lobby massively because they expect severe sanctions. This problem may cause correlation between our lobbying measures and the error term, which would result in biased estimates. In our case, if we assume that higher potential sanction will incentivize the firm to lobby more, not controlling for the selection issue will lead to an underestimated negative effect. A standard approach to tackle this issue in economics consists of applying an instrumental approach in a two-stage estimation (Akay et al., 2014; Quigley et al., 2019). As usual, the cornerstone of the two-stage approach lies in the choice of instruments. Compelling instruments should satisfy both the exogeneity and relevance conditions. The following section discusses the estimation procedure and the selected instruments.

The literature documents the difficulty of identifying appropriate instrumental variables in empirical applications extensively. It is necessary to satisfy the following two conditions for a valid instrumental variable: (i) a reasonably high level of correlation of the instrument with the variable that is instrumented (relevance condition) and (ii) the independence of the instrument from the error term of the main equation (exclusion restriction). According to the theory, the ideal instrument should capture the variation in a cartel member's lobbying expenditures that is exogenous to the antitrust sanction. Our instrument is a geographical distance, which is a classical instrument in the political economy and financial literature (Igan et al., 2012; Houston et al., 2014; Unsal et al., 2016; Cao et al.,

[18] Few examples are the Bruxelles CRA conference (May 2022) (https://www.youtube.com/watch?v=7MNIBgMco6k) where both Jonathan Kanter (AAG of the Division) and Lina Khan (Chair of the FTC) were present or the December 2021 joint statement of the agencies concerning the CMA (https://www.ftc.gov/news-events/news/press-releases/2021/12/joint-statement-ftc-doj-antitrust-division-uk-competition-markets-authority).

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2018; Lambert, 2019). The advantage of this instrument is its predetermined nature, which implies the non-existence of influence with the variable of interest. The financial literature emphasizes that the distance impacts companies' financial decisions either within (Petersen and Rajan, 2002) or across countries (Mian, 2006). Igan et al. (2012) hypothesize that the cost of lobbying is an increasing function of the distance between firms' headquarters and the political hub. Building on a similar argument, we consider that firms geographically far from Washington DC will lobby less as it is more costly to signal themselves. One anecdotal evidence is the 2018 Amazon move of headquarters to Washington DC to strengthen their relationship with the Capitol.[19] The decision to change headquarters is exogenous to a punctual cartel sanction, but reflects the cost of building political connections in Washington DC.

To document the relationship between the antitrust sanction and lobbying expenditures, we first adopt a linear IV strategy to estimate the causal effect (see, Angrist and Pischkle, 2009). The first stage of the equation represents the firm's decision to lobby. In the second stage, we regress our variable of interest (antitrust sanction) on the fitted value of the explanatory variable. To tackle the possible endogeneity of our variable of interest while controlling for the left censored sample issue, we use probit-probit and probit-Tobit estimations (Angrist, 2001). The first-stage probit estimation allows us to obtain the inverse of the Mill's ratio. We display the first stage as firm i's decision to lobby. The second stage of the probit-probit specification displays the outcome equation and focuses on the determinants of cartel sanctions. Consistent with Greene (2002), the inverse Mill's ratio is introduced to correct the endogeneity and is computed similarly for the two-stage specifications. The outcome equation estimates are computed using the maximum likelihood estimation method.

Insert Table 5 here

Table 5 reports the results of the first and second stage of regressions from the two-stage IV approaches. Columns 1 refers to the OLS regression from the first specification of Table 3. We include it as a benchmark for the results of the second column. Columns 2 to 5 display the 2SLS IV regressions. For each of these four Models, the instrumental variable is the logarithmic transformation of the distance between firms' headquarter and the Capitol (Washington DC). The non-variation in time of our instrument is not an issue in terms of relevance as our regression strategy is cross-sectional. From column 2 to 5, we evaluate two measures of lobbying expenditures and, as robustness, their lagged transformation. *Lobbying expenditures* is the logarithmic transformation of one plus the lobbying expenditures the year of the sanction. *Lobbying expenditures*$_{t-1}$ is the logarithmic transformation of one plus the lobbying expenditures the year before the sanction (during the investigation, on average). Δ*Lobbying expenditures* refers to the logarithmic transformation of one plus the increase in lobbying expenditures from the year before to the year of the sanction. Δ*Lobbying expenditures*$_{t-1}$ refers to the logarithmic transformation of one plus the increase in lobbying expenditures from two years before to the year of the sanction (the change before and after the start of investigations, on average). Even though we might lose information by differentiating lobbying expenditures, this transformation allows to include time variation in the analysis by focusing on the increase of lobby between two years. Columns 6 and 7 present the probit-probit and probit-Tobit regressions. For these columns we consider the left-censored issue and take the *Lobbying status* variable (equal to one if a firm lobby the year of the sanction) as explanatory variable.

[19] Open Secret website: https://www.opensecrets.org/news/2018/11/amazon-makes-move-to-dc-metro-area/.

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For the columns 2 to 5, the $Wald\ F$-statistics larger than 10, which validates the relevance condition. Except for the fifth specification, all the estimates are consistent with our results from the Table 3. When we focus on the marginal effect from the second Model of Table 5 (-0.071), we can translate our results into return on lobby investment by taking two concrete cases from our sample. The first, *Bridgestone Corp.*, is interesting, as its level of sanction corresponds to the median value for firms having experienced a sanction. While the sanction of *Bridgestone Corp.* was about \$28M, the lobbying spending the year of the sanction (2008) was equal to \$1.2M. Considering these figures, our model predicts that an additional 1\$ invested in lobbying activity by *Bridgestone Corp.* leads to a drop in sanction by \$1.61 $\left(\frac{\partial sanction*lobby}{\partial lobby*sanction} = -0.071 * \frac{28}{1.2} = -1.61\right)$, which corresponds to a 161% return on lobbying.[20] This result is an upper bound as we do not address the OLS underestimation of left-censored samples. The second case concerns the firm situated in the middle of the ranked vector of lobbying expenditures, namely, *ABB Asea Brown Boveri*.[21] This case is interesting, as it mirrors the median explained variable's example with that of the median (from positive sanctions only) independent variable of interest. At the end of the enforcement process, *ABB Asea Brown Boveri* was punished for a total of \$53M and lobbied \$0.16M the year of the sanction (2000). Our estimates translate these figures into a sanction reduction of \$23.51 for \$1 invested in lobbying $\left(\frac{\partial sanction*lobby}{\partial lobby*sanction} = -0.071 * \frac{53}{0.16} = -23.51\right)$.[22] Nevertheless, these examples are case specific and driven by a large difference between cartel sanctions and lobbying expenditures. Column 3 indicates that the amount of lobby during the year of investigation also negatively impact the amount of sanction. The measure of an increase in lobbying from the year before to the year of the sanction (column 4) shows that the change in lobbying is negatively associated with the amount of the antitrust sanction, which is not the case for its lagged transformation (the increase in lobbying between two years before the year of the sanction and the year before the sanction).

4.3. Instrument validation

This section aims at validating the relevance and the independence of our instrument. The first part focuses on the relevance and the following one on the exclusion restriction. We study whether the lobbying behavior of firms changes when they are far from the competition authority's offices. Based on Ha et al. (2022), the identification strategy we use exploits the passage of a new competition regulation that impacts the enforcement level in some US areas. In October 2011, the DoJ announced the closure of four Antitrust Division field offices located in Atlanta, Cleveland, Dallas and Philadelphia. The goal of the this decision was to consolidate the resources into the four remaining field offices (San Francisco, New-York, Chicago and Washington) and the headquarter (Washington). This decision came to force the 1st of January 2013. The DoJ announced that this cost-cutting measure would save more than \$130 Million. This change of resources distribution created a decrease in antitrust enforcement for firms near to the closed regional offices and an increase of enforcement level for firms situated near to

[20] As both variables are expressed in natural logs in our model (the log-log regression coefficient approach is in line with the Connor and Miller (2013)'s procedure), we use the elasticity formula to recover the partial effect. Our conclusion regarding the impact of lobbying also holds with a level-level regression coefficient approach.

[21] When we take the median from the sample with only positive sanctions.

[22] While the coefficient of the lobbying impact from our regression is fairly small, the high returns on lobbying are driven by an average of US sanctions more than twenty times bigger than the average of lobbying expenditures. As mentioned by Gordon and Hafer (2005), even a small amount of lobbying expenditures is sufficient for firms to signal themselves to the regulator. Another argument explaining the size of lobby are its potential side effects, like reputational costs (Haque et al., 2013).

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the remaining offices (Ha et al., 2022).[23]

The estimates focus only on US firms, which are more likely to be affected by the event, and over a three-year period around the shock (Borisov et al., 2016). We measure a firm's exposure to this event by the change in distance between the firm's headquarters and the nearest new DoJ regional office. The treated group consists of firms that experienced an increase in distance, while the control group consists of firms that did not experience a change in distance. For example, if a firm's headquarters was located in Washington DC prior to the event, the closure of the regional office has no impact on the change in distance to that firm's nearest antitrust regional office. If a firm's headquarters was close to the Dallas office (closed after January 1, 2013), after the event, the increase in distance from that firm's headquarters to the nearest new regional antitrust office is positive. Therefore, this observation is in the control group. The sample consists of 159 US firms that were members of at least one cartel during the period 1998-2018. Considering the three-year window around the peer (2010 to 2015), the number of observations equals 1,086 when we do not include the control variables, and 415 with firm size control (this change is due to missing accounting data for private firms). From the 1,086 observations, almost equally distributed before and after the event, only 5% were under investigation during the three-year window. The average increase in distance is equal to 147.9 KM, including those that did not experience the change.

To measure the lobbying sensitivity we consider the logarithmic transformation of one plus the lobbying expenditures. We estimate the following difference-in-difference specification:

$$LN(Lobbyingexpenditures_{i,t}) = \beta_1 \Delta Distance_i \times Post_t + K_{i,t} + X_{i,t} + \Theta_t + \alpha_i + \epsilon_{i,t}$$

where $LN(Lobbyingexpenditures_{i,t})$ refers to the logarithmic transformation of one plus the lobbying expenditures. $Post_t$ is a binary variable equal to one if the year is on or after 2013. $\Delta Distance_i$ is the increase in distance in 100-KM between a firm's headquarter and an antitrust office after the event. $K_{i,t}$ refers to $\Delta Distance_i$ and $Post_t$. $X_{i,t}$ contains the control variables, which are the size of the firm (measured with the total assets) and a dummy variable equal to one if the firm is a recidivist. Θ_t, α_i and $\epsilon_{i,t}$ refer to the year fixed effects, the firm fixed effects and the error term, respectively.

Insert Table 6 here

Table 6 displays the estimation results. For all the specifications the explanatory variable (Δ $Distance_i \times Post_t$) shows negative and statistically significant results. These estimates indicate a decrease in lobbying expenditures when the distance between the firm's headquarter and the regional office of the competition authority increases. This result validates the relevance of our instrument as we assume that the cost of lobbying is higher when the distance between firm's headquarter and the antitrust authority is bigger. This result is robust to different measures in change of lobbying expenditures (growth rate or variance) and is in line with the Ha et al. (2022)'s results that reports a change in firms' behavior when the distance between the firm and the antitrust authority increases.

Concerning the exclusion restriction, we are unable to report that the instrument is independent of unobserved firm characteristics. Nevertheless, it is possible to test whether the IV is correlated

[23] For more anecdotal evidence see the Appendix A of Ha et al. (2022).

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with observed firm and cartel characteristics (De Groote and Declercq, 2021). Table 7 displays the results when we regress the IV on the control variables. Except for one control (firm size), we do not find any evidence that indicates a correlation between the IV and the observed firm and cartel characteristics. The second test simply runs the 2SLS specifications (see, Table 5 column 2) without controls. When we drop firm characteristics controls, the coefficient we obtain for our explanatory variable is slightly bigger (-0.093) than the one from the Table 5's column 2 (-0.071), with a similar sign and a result statistically significant. Running the same regression without any controls gives us a coefficient equal to -0.102 but this time not statistically significant (S.E. clustered at the industry level = 0.102). Overall, we find few evidence of correlation between the IV and the observed variables. These findings undermine the likelihood of correlations between our instrument and unobserved variables.[24]

Insert Table 7 here

4.4. Additional robustness checks

In this section we assess whether the coefficient of our variable of interest is modified when we specify the sample or the model differently. We also evaluate the robustness of our results when there is a change in the agency's resources. In Table 8, we present the specification we use in our main table (Table 3) by using the same sample size for each specifications. This allows us to compare the results in a better way and also validates the results of our main specification (controlling for the determinants of the sanction, see Connor and Miller, 2013). For each column our results are consistent with Table 3. Unfortunately, compared to the third and sixth columns of Table 3 because of missing values for firms and cartel characteristics we are not able to have as much observations as we would like. Table 9 reports linear regressions' estimates for different sub-samples. Model 1 indicates the results when we remove the bottom and top 10% of firms' total assets of the sample. Columns 2 and 3 display the results excluding top and bottom quartiles of firms' total assets, respectively. The estimates indicate that the impact of lobbying is stronger for bigger firms. This result can be explained by the higher level of sanction for bigger firms accompanied by small variations of positive lobbying expenditures between firms. All the estimates are in line with our main results from Table 3.

Insert Table 8 and Table 9 here

In Table 10, we explore the long-term lobby relationship taking the number of time a firm lobbied over our sample period 1998-2018 as explanatory variable (Hill et al., 2013). The negative sign of the coefficient combined with a statistically significant result from specifications 1 is consistent with the Groll and McKinley (2015) theory of long-term lobbying relationship. Nevertheless, we do not find any effect from the two following specifications, which indicates that the lobbying around the year of the sanction have more impact on the Division's final decision. To support this assumption, we check for the effect of an increment of lobby from one year to another. In Table 11 we take the logarithmic transformation of one plus the increase in lobbying expenditures from one year to the following one, focusing on the year of the sanction and the year before. Out of six specifications, four Models document a negative association (statistically significant) between an increase in lobbying and the amount of the antitrust sanction. As we mentioned in the previous section, this methodology allows to take into account the high correlation of lobbying from one year to another. The downside is the lost of information if the firm lobbies the same amount each year. The results we find are

[24] Results from the second test available upon request.

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consistent with Table 5. We also examine whether lobbying expenditures from the years before the year of the sanction have an influence on the level of the sanction. Focusing on the Model with cartel controls (in line with the model of Connor and Miller, 2013), Table 12 documents the effect on the cartel sanction from lobbying expenditures the year of the sanction (similarly to the first specification of our main results in Table 3), and from lobbying expenditures one and two years before the year of the sanction. The results show that lobbying expenditures from years before the year of the sanction have a negative effect on the cartel sanction and the magnitude of the impact is decreasing over the years. This result tends to support our assumption about the timing of lobbying related to the agency decision. As documented in Table 6, there is a change in lobbying when firms are threaten by competition authorities and the influence of the lobbying efforts on the antitrust sanction tends to be higher when the case is near to the end of the process.

Insert Table 10, Table 11 and Table 12 here

The US antitrust law allows a private party to bring a civil suit for any injury that results from an antitrust violation. For example, customers of a cartel may sue convicted wrongdoers for the losses they suffered in civil lawsuits (Bodnar et al., 2021). We collect this information from the Connor (2016) data set and use these data to conduct a placebo test. We conjecture that lobbying expenditures have no impact on the damages actions or damages class actions. In Table 13, we take the logarithmic transformation of one plus the amount of the private sanction as dependent variable. The explanatory variables are the one from our main Model (Table 3) and its lagged transformation. We find no evidence of lobbying influence on the amount of the damages actions.

Insert Table 13 here

4.5. Exploring the channels

In this section we investigate channels of explanation related to regulatory capture and informational lobbying. In Table 14 we explore more heterogeneity in the relationship between agency's decision and political connections including an interaction term between lobbying expenditures and a dummy variable equal to one if the AAG (who is appointed by the US president) is not aligned with the US president. Our sample is almost equally divided between Republican and Democrat presidency of years: Clinton (98-00), Bush (01-08), Obama (09-16) and Trump (17-18), which results in 10 years for the Republicans and 11 years for the Democrats. We assume here that the AAG is not aligned with the president when the positions of a new president and a former AAG overlap. This situation occurs during the first year of the presidency term. In our sample we have three years that correspond to this situation, 2001 (Bush), 2009 (Obama) and 2017 (Trump). The results from all the specifications indicate that the effectiveness of lobbying cartel members is relatively weaker when the AAG is not aligned with the US president. Under this background, one interpretation would consider that the lobbying expenditures effect on the cartel sanction is weaker because the lobbying at the legislative level does not percolate to the regulatory level when the AAG is not the one appointed by the current US president. This result complements the empirical literature showing differences in government interventionism between more and less ideologies alignment (Wehner and Gerson, 2014; Igan et al., 2021). The change of enforcement outcome related to the alignment of the AAG is in line with the regulatory capture theory (Peltzman, 1976) and the legislative subsidy theory (alignment of ideologies) (Ellis and Groll, 2020).

Insert Table 14 here

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To qualify for the leniency program, the cartel member must report the unlawful behavior with enough information on the cartel before the beginning of the investigations. With the estimates from Table 15, we explore the channel of informational lobbying by assuming that lobbying is a way to be better informed about the regulatory process or to better inform the regulator. Including an interaction term between lobbying expenditures and a binary variable equal to one if the cartel member received the leniency program, we conjecture that the lobbying effect is higher when the firm gets the lenient treatment. Because the reception of the leniency program implies a sanction equal to zero, the effect of our interaction term mainly operates at the extensive margin. As a result, Table 15 presents estimates from Tobit regressions. The interaction term indicates a statistically significant negative effect whenever we use firm controls (two specifications out of three), which corresponds to the models with higher explanatory power and in line with Connor and Miller (2013). When we compute the marginal impact from specification 1, the total impact of lobbying equals -0.033 + -0.096 = -0.129, which translates into a decrease of the sanction about 0.129% for 1% change in lobbying. This result indicates that lobbying firms are more likely to qualify for the leniency program, which has a meaningful impact on the sanction and is in line with the informational lobbying literature (Ellis and Groll, 2020).

Insert Table 15 here

5. Conclusion

These last two decades have seen the success of firms' lobbying expenditures in reducing the US market competition with the effect of driving profits higher, depressing wages and limiting opportunities for investment, innovation, and growth (Phillipon, 2019). While the objective of antitrust laws is to protect fair competition in the marketplace, this article documents to what extent firms' lobbying expenditures affect the cartel enforcement outcome of the US Antitrust Division.

Using a unique data set on cartel sanctions and lobbying expenditures from firms prosecuted by the US Antitrust Division over the 1998-2018 period, we show that lobbying expenditures are negatively related to cartel sanctions. This paper also documents that the negative association between lobbying expenditures on cartel sanctions is stronger when (1) the head of the Antitrust Division is aligned with the US president and (2) the cartel member qualifies for the leniency program. The first effect is broadly consistent with the regulatory capture view of regulation, while the second and stronger effect suggests the domination of the informational theory to explain our predictions.

Our results have implications for the effectiveness of antitrust laws. Given that an optimal sanction must theoretically take the profit out of the collusion (Ginsburg and Wright, 2010), the significant influence of prosecuted firms might impact the optimal amount of sanction, and hence, the cartel stability. Examining the effects of lobbying expenditures on optimal fine models could be an interesting avenue for future research. Moreover, whether lobbying activities influence other channels related to the profitability of cartel members, for example, stock prices, other penalties from non-US competition authorities, or legal costs, is an open research question.

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6. Tables and Figures

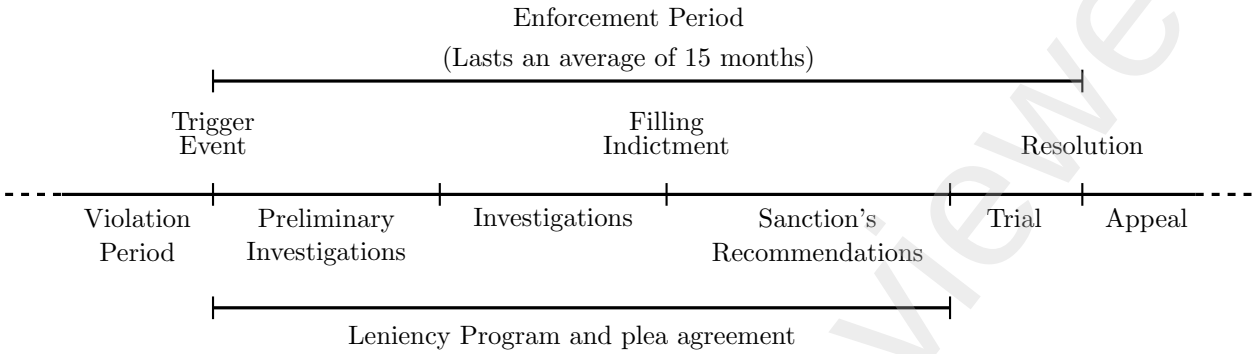

Figure 1: Timeline of the enforcement action (prepared by the authors, according to the USSGs, DoJ, 2020 and Connor, 2006).

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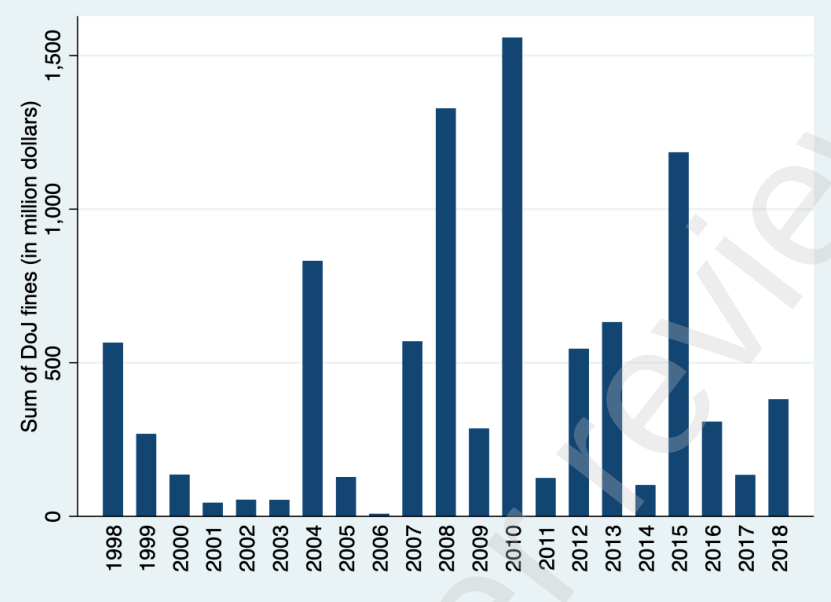

Figure 2: Sum of DoJ fines per year

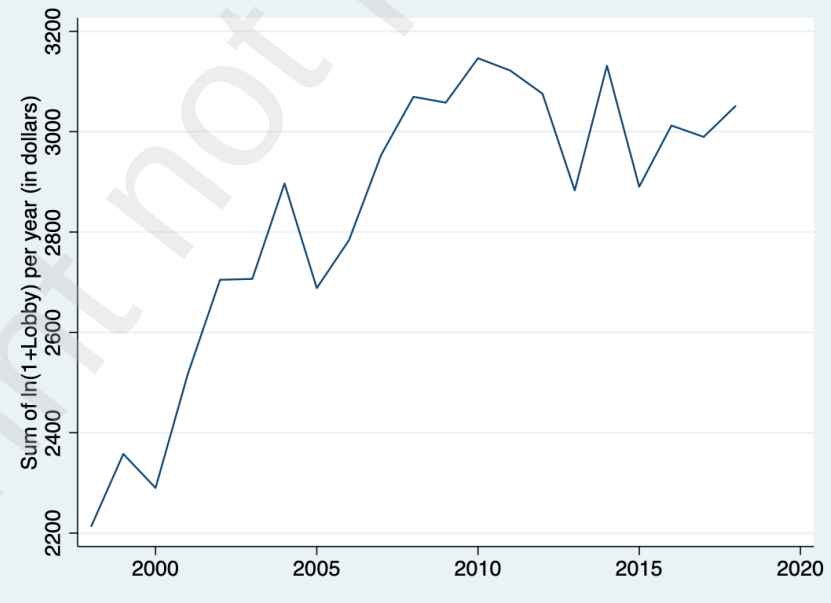

Figure 3: Sum of ln(1+Lobby) per year

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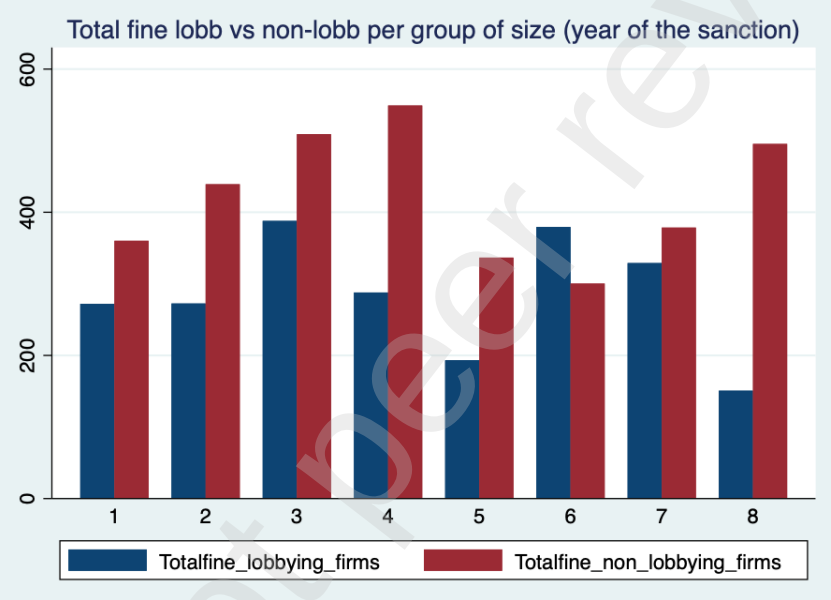

Figure 4: Sanctions for lobbying and non-lobbying firms.

Firms are ranked from lowest to highest by the value of their total assets. N = 422, which refers to the number of observations we have on cartel members the year of the sanction when we include the total assets information (some cartel members are not listed companies and their financial information is not publicly available, e.g., the Taiwanese firm Tung Hat Fermentation that was active in the 2001 Nucleotide cartel case). Each group is distributed equally, the firms lobbying the year of the sanction represent 43% of the observations.

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Table 1: Definitions of variables

Variable	Definition
Dependent variables	
Sanction	the amount of the sanction from the Antitrust Division in $US
Private sanction	the amount of the fines from private damages claims $US
Explanatory variables	
Lobbying expenditures	Amount of lobbying expenditures the year of the sanction $US
ΔLobbying expenditures	increase in lobbying from one year to another (negative changes are set to zero) $US
Lobbying status	= 1 if the firm did lobby the year of the sanction
Lobbying status DoJ/FTC	= 1 if the firm did lobby the DoJ or the FTC (or both) the year of the sanction
Long-term lobbying	Count the number of years a firm did lobby over the sample period
Firm controls	
Leniency	= 1 if the cartel member qualifies for leniency
Othpen	Other penalties (from other countries or private suits)
Prison	Months of prison time assigned to cartel executives
Recidivist	Count the number of cartels a firm is involved in
Total assets	Total assets the year of the sanction
Cartel controls	
Affected sales*	The volume of US affected sales for each cartel
Bidrig	= 1 if bid-rigging cartel
Gov buy	= 1 if a government is the major buyer
Duration	Cartel duration in months
Many buy	= 1 if more than 100 buyers
Members	Number of members in the cartel
Third party	= 1 if a third party (a non-competing firm) is included in the cartel
Instrument	
DistDC	The distance (in km) between the firm headquarter and the Capitol
ΔDistance	The change in distance (in km) from the firm's headquarter to the new nearest DoJ regional office
Interactions	
Post	= 1 for the years after 2012
Republican	= 1 if the AAG is Republican

* The PIC dataset contains also the affected sales information per firms. Nevertheless, because of a substantial amount of missing variables (more than half of our observations) we proxy this variable with the total amount of affected sales by cartel.

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Table 2: Descriptive statistics of firms prosecuted by the Division over the 1998-2018 period at the year of the sanction

Variables	Unit	Mean	Median	SD	Max	Min	Observations
Dependent variables							
Sanction	US$M	14.07	0	51.57	579	0	657
Private sanction	US$M	19.39	0	58.27	688	0	657
Explanatory variables							
Lobbying expenditures	US$M	0.68	0	0.21	39.00	0	657
ΔLobbying expenditures	US$M	0.14	0	0.41	3.30	0	639
Lobbying status	Dummy	0.31	0	0.46	1	0	657
Lobbying status DoJ/FTC	Dummy	0.036	0	0.18	1	0	657
Long-term lobbying	Number	6.34	0	8.47	21	0	657
Firm controls							
Leniency	Dummy	0.06	0	0.23	1	0	657
Othpen	US$M	28.04	1.35	80.22	774.30	0	657
Prison	Months	5.62	0	20.21	168	0	657
Recidivist	Number	3.36	1	4.20	18	1	595
Total assets	US$B	6.36	0.15	22.10	240.00	29×10^{-6}	422
Cartel controls							
Affected sales	US$M	0.17	0.006	0.52	2	0	577
Bidrig	Dummy	0.65	0	0.47	1	0	657
Duration	Months	113.15	85.32	86.42	348	5	657
Gov buy	Dummy	0.19	0	0.39	1	0	657
Many buy	Dummy	0.44	0	0.49	1	0	657
Members	Number	13.05	10	11.21	44	2	657
Third party	Dummy	0.05	0	0.22	1	0	657
Instrument							
DistDC	KM	6,811	6,559	4,513	16,833	27.04	632
ΔDistance	KM	71.70	243.78	0	1,285	0	632
Interactions							
Post	Dummy	0.37	0	0.48	1	0	657
Republican	Dummy	0.35	0	0.47	1	0	657

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Table 3: Cartel sanctions and lobbying expenditures the year of the sanction

Sanction	(1)	(2)	(3)	(4)	(5)	(6)
	1998 - 2018 Sample			2005 - 2018 Sample		
Lobbying expenditures	-0.036***	-0.028**	-0.004	-0.040***	-0.032**	-0.018*
	(0.011)	(0.012)	(0.010)	(0.012)	(0.013)	(0.011)
Firm controls						
Leniency	-1.318***	-1.588***		-1.371***	-1.608***	
	(0.338)	(0.347)		(0.372)	(0.373)	
Othpen	0.004***	0.005***		0.004***	0.005***	
	(0.000)	(0.000)		(0.000)	(0.000)	
Prison	0.037***	0.036***		0.040***	0.038***	
	(0.005)	(0.006)		(0.005)	(0.006)	
Recidivist	-0.159***	0.010		-0.208***	0.007	
	(0.050)	(0.018)		(0.056)	(0.019)	
Total assets	0.062**	0.049*		0.080***	0.064**	
	(0.027)	(0.030)		(0.029)	(0.032)	
Cartel controls						
Affected sales	-0.036			-0.029		
	(0.064)			(0.069)		
Bidrig	-0.379			-0.471*		
	(0.273)			(0.282)		
Duration	0.221*			0.236*		
	(0.123)			(0.128)		
Gov buy	0.258			-0.101		
	(0.445)			(0.511)		
Many buy	-0.346			-0.114		
	(0.322)			(0.338)		
Members	-0.025*			-0.030**		
	(0.014)			(0.014)		
Third party	-1.140**			-1.453***		
	(0.504)			(0.545)		
Cartel FE	no	yes	yes	no	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
R^2	0.52	0.59	0.42	0.50	0.58	0.45
Observations	312	375	629	288	346	503

This table contains the estimates from linear regressions. For all the specifications, we take the value of the logarithmic transformation of one plus lobbying expenditures as explanatory variable and the logarithmic transformation of one plus the cartel sanction as dependant variable. Models 1 to 3 are estimated for the 1998-2018 sample period, while Models 4 to 6 are estimated for the 2005-2018 sample period. All variables are defined in Table 1. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 4: Cartel sanctions and lobbying at the DoJ and the FTC (over the 2008-2018 period)

Sanction	(1)	(2)	(3)	(4)	(5)	(6)
Lobbying status DoJ/FTC	-0.887***	-0.911***	-0.506**			
	(0.284)	(0.331)	(0.260)			
Lobbying status DoJ/FTC × lobbying xp.				-0.063***	-0.064***	-0.027**
				(0.019)	(0.022)	(0.016)
Firm controls						
Leniency	-1.675***	-1.569***		-1.619***	-1.573***	
	(0.284)	(0.358)		(0.310)	(0.022)	
Othpen	0.005***	0.005***		0.005***	0.005***	
	(0.001)	(0.001)		(0.001)	(0.001)	
Prison	0.047***	0.55***		0.044***	0.050***	
	(0.009)	(0.015)		(0.009)	(0.012)	
Recidivist	-0.126**	0.839		-0.203**	0.854	
	(0.057)	(0.757)		(0.059)	(0.777)	
Total assets	0.071**	0.067**		0.046*	0.062**	
	(0.026)	(0.029)		(0.021)	(0.029)	
Cartel controls						
Affected sales	-0.060			-0.003		
	(0.079)			(0.083)		
Bidrig	-0.919***			-0.976***		
	(0.298)			(0.346)		
Duration	0.365***			0.289**		
	(0.146)			(0.143)		
Gov buy	-0.731			-0.543		
	(0.536)			(0.692)		
Many buy	-0.776*			-1.586***		
	(0.491)			(0.555)		
Members	0.018			0.045		
	(0.028)			(0.031)		
Third party	-1.080*			-0.574		
	(0.682)			(0.776)		
Cartel FE	no	yes	yes	no	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
R^2	0.52	0.59	0.47	0.51	0.59	0.47
Observations	261	319	439	261	319	439

This table contains the estimates from linear regressions. For all the specifications, we take the logarithmic transformation of one plus the cartel sanction as dependant variable and either a binary variable equal to one if the firm lobbied at the either the DoJ or the FTC (or both) or the multiplication of that variable with the logarithmic transformation of one plus the amount of lobbying expenditures as explanatory variable. All the specifications are estimated between 2008-2018 because the information on the lobbied agency is not available before 2008 (Blanga-Gubbay et al., 2021). The DoJ and the FTC are the main competition federal agencies in the US. Even though the FTC does not handle criminal cases, the two agencies have strong ties as they might work on the same cases, share opinions on competition laws and (at least for the higher managers) go to the same conferences and meetings. Robust standard errors are the results in parentheses. All variables are defined in Table 1. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 5: Cartel sanctions and lobbying expenditures: addressing endogeneity

	OLS	2SLS IV	2SLS IV	2SLS IV	2SLS IV	Probit-probit	Probit-Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lobbying expenditures				**Stage 1**			
DistDC		-2.314***	-2.251***	-1.418***	-2.155***	-0.161***	-0.161***
		(0.390)	(0.370)	(0.348)	(0.347)	(0.022)	(0.022)
R^2		0.33	0.28	0.33	0.27	0.21	0.21
Sanction				**Stage 2**			
Lobbying expenditures	-0.036***	-0.071**					
	(0.011)	(0.031)					
Lobbying expenditures$_{t-1}$			-0.057*				
			(0.032)				
ΔLobbying expenditures				-0.110*			
				(0.060)			
ΔLobbying expenditures$_{t-1}$					-0.041		
					(0.038)		
Lobbying status						-0.082*	-0.035**
						(0.045)	(0.493)
Inv. Mill's Ratio						0.142**	0.411*
						(0.073)	(0.247)
Firm Controls	yes	yes	yes	yes	yes	yes	yes
Cartel Controls	yes	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes
R^2 or Pseudo R^2	0.52	0.47	0.46	0.41	0.39	0.41	0.21
Number of observations	312	312	312	285	281	312	312
Cragg-Donald Wald F stat		35.1	36.9	16.57	21.77		
Log likelihood						-131.636	-336.601

This table represents estimates from IV, probit-probit and probit-Tobit regressions explaining the impact of lobbying expenditures on US cartel sanctions. Columns 1 refers to the OLS regression from the first specification of Table 3. Columns 2 to 5 report the 2SLS IV regressions. For each of these four Models, the instrumental variable is the logarithmic transformation of the distance between firms' headquarter and the Capitol (Washington DC). We evaluate a different measure of lobbying expenditures in each column (2 to 5). *Lobbying expenditures* is the logarithmic transformation of one plus the lobbying expenditures the year of the sanction. *Lobbying expenditures*$_{t-1}$ is the logarithmic transformation of one plus the lobbying expenditures the year before the sanction (during the investigation, on average). Δ*Lobbying expenditures* refers to the logarithmic transformation of one plus the increase in lobbying expenditures from the year before to the year of the sanction. Δ*Lobbying expenditures*$_{t-1}$ refers to the logarithmic transformation of one plus the increase in lobbying expenditures from two years before to the year of the sanction (the change before and after the start of investigations, on average). Columns 6 and 7 present the probit-probit and probit-Tobit regressions. For these columns we consider the left-censored issue and take the *Lobbying status* variable (equal to one if a firm lobby the year of the sanction) as explanatory variable. All models are estimated over the 1998-2018 period and all variables are defined in Table 1. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 6: Change in lobbying expenditures when US firms are investigated (3-years window).

Lobbying expenditures	(1)	(2)	(3)	(4)
ΔDistance×Post	-0.050**	-0.051**	-0.050*	-0.051*
	(0.020)	(0.022)	(0.029)	(0.030)
ΔDistance	0.002***	0.001***	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Post	0.282*	0.337	0.058	0.133
	(0.167)	(0.381)	(0.127)	(0.250)
Total assets	1.733***	1.722***		
	(0.594)	(0.601)		
Recidivist	0.000	0.000		
	(0.011)	(0.104)		
Firm FE	yes	yes	yes	yes
Year FE	no	yes	no	yes
R^2	0.94	0.94	0.94	0.94
Number of observations	517	517	1,086	1,086

This table contains the estimates from the difference-in-difference strategy. For all the specifications, we take the value of the logarithmic transformation of one plus the lobbying expenditures as dependant variable. Post is a dummy variable which is one if the year is on or after 2013 or zero otherwise. 2013 is the year corresponding to the US Antitrust Division decision to close four of its regional offices (Atlanta, Cleveland, Dallas and Philadelphia). ΔDistance is the increase in geographical distance between firms headquarters and a regional antitrust office after the closure of the four regional offices in 100 KM. We control for the size of the firm (Total assets), which is a main determinant of lobbying (Grossman and Helpman, 2001). We also add the Recidivist variable to control whether firms already active in former cartels consider lobbying in their colluding strategy (dummy variable). The sample is restricted to US firms (impacted by the exogeneous event) and to a 3-years window around the event (Borisov et al., 2016). Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 7: IV exogeneity tests

Distance headquarters - Washington DC	Coefficient	Standard Error
Leniency	-0.230	(0.160)
Othpen	0.000	(0.000)
Prison	0.000	(0.004)
Recidivist	-0.017	(0.027)
Total assets	0.138***	(0.027)
Affected sales	-0.069	(0.045)
Bidrig	-0.323	(0.203)
Duration	0.093	(0.085)
Gov buy	-0.571	(0.345)
Many buy	-0.479	(0.364)
Members	0.037	(0.017)
Third party	0.430	(0.526)

This table reports the OLS regression of the instrumental variable on the control variables. The regression includes cartel and year fixed effects. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

Table 8: Cartel sanctions and lobbying expenditures the year of the sanction

Sanction	(1)	(2)	(3)	(4)	(5)	(6)
		1998 - 2018 Sample			2005 - 2018 Sample	
Lobbying expenditures	-0.036***	-0.023**	-0.016	-0.040***	-0.033***	-0.025*
	(0.011)	(0.011)	(0.015)	(0.012)	(0.012)	(0.015)
Firms Controls	yes	yes	no	yes	yes	no
Cartel Controls	yes	no	no	yes	no	no
Cartel FE	no	yes	yes	no	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
R^2	0.52	0.61	0.50	0.50	0.63	0.50
Observations	312	312	312	288	288	288

This table contains the estimates from linear regressions. For all the specifications, we take the value of the logarithmic transformation of one plus lobbying expenditures as explanatory variable and the logarithmic transformation of one plus the cartel sanction as dependant variable. Models 1 to 3 are estimated for the 1998-2018 sample period, while Models 4 to 6 are estimated for the 2005-2018 sample period. The specifications are the same as Table 3 except that we constraint the sample size to be the same in each Model, as a robustness for our main regressions. All variables are defined in Table 1. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 9: Cartel sanctions and lobbying expenditures: robustness checks

Sanction	(1)	(2)	(3)
	Exclude bottom & top 10%	Exclude bottom 25%	Exclude top 25%
Lobbying expenditures	-0.030**	-0.051***	-0.024*
	(0.014)	(0.016)	(0.014)
Firm Controls	yes	yes	yes
Cartel Controls	yes	yes	yes
Cartel FE	no	no	no
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
R^2 or Pseudo-R^2	0.46	0.42	0.51
Observations	292	266	267

This table contains the estimates from linear regressions. For all the specifications, we take the value of the logarithmic transformation of one plus lobbying expenditures as explanatory variable and the logarithmic transformation of one plus the cartel sanction as dependant variable. Model 1 indicates the results when we remove the bottom and top 10% of firms' total assets of the sample. Columns 2 and 3 display the results excluding top and bottom quartiles of firms' total assets, respectively. Models 1 to 3 are estimated for the 1998-2018 sample period. All variables are defined in Table 1. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

Table 10: Cartel sanctions and long-term lobbying

Sanction	(1)	(2)	(3)
Long-term lobbying	-0.018***	-0.005	-0.008
	(0.009)	(0.010)	(0.007)
Firm Controls	yes	yes	no
Cartel Controls	yes	no	no
Cartel FE	no	yes	yes
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
Pseudo-R^2	0.51	0.59	0.42
Observations	312	375	629

This table contains the estimates from linear regressions. We take the value of the logarithmic transformation of one plus the cartel sanction as dependant variable and the number of lobbying years over the all sample period as measure for long-term lobbying. Models 1 to 3 are estimated for the 1998-2018 sample period. All variables are defined in Table 1. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 11: Cartel sanctions and the difference in lobbying expenditures the year of the sanction

Sanction	(1)	(2)	(3)	(4)	(5)	(6)
ΔLobbying expenditures	-0.032**	-0.017	-0.006			
	(0.013)	(0.017)	(0.010)			
ΔLobbying expenditures$_{t-1}$				-0.037***	-0.038**	-0.039***
				(0.013)	(0.017)	(0.014)
Firm Controls	yes	yes	no	yes	yes	no
Cartel Controls	yes	no	no	yes	no	no
Cartel FE	no	yes	yes	no	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
R^2	0.54	0.57	0.41	0.54	0.56	0.45
Observations	254	299	517	232	273	419

This table contains the estimates from linear regressions. For all the specifications, we take the value of the logarithmic transformation of one plus the difference in lobbying expenditures before and during the year of the sanction (on average) as explanatory variable and the logarithmic transformation of one plus the cartel sanction as dependant variable. All Models are estimated for the 1998-2018 sample period. All variables are defined in Table 1. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

Table 12: Cartel sanctions and the increment of lobby

Sanction	(1)	(2)	(3)
Lobbying expenditures	-0.036***		
	(0.011)		
Lobbying expenditures in t-1		-0.025**	
		(0.012)	
Lobbying expenditures in t-2			-0.021*
			(0.012)
Firm Controls	yes	yes	yes
Cartel Controls	yes	yes	yes
Cartel FE	no	no	no
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
Pseudo-R^2	0.52	0.51	0.50
Observations	312	312	307

This table contains the estimates from linear regressions. We take the value of the logarithmic transformation of one plus the cartel sanction as dependant variable. Model 1 corresponds to the first specification in Table 3. Models 2 and 3 take the amount of lobbying expenditures one year before the year of the sanction (*lobbying expenditures in t-1*) and two years before the year of the sanction (*lobbying expenditures in t-2*), respectively. Models 1 to 3 are estimated for the 1998-2018 sample period. All variables are defined in Table 1. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 13: Private damages and lobbying expenditures

Private sanction	(1)	(2)	(3)	(4)	(5)	(6)
Lobbying expenditures	-0.014	-0.002	0.012			
	(0.012)	(0.018)	(0.017)			
Lobbying expenditures$_{t-1}$				-0.008	-0.008	0.014
				(0.013)	(0.015)	(0.017)
Firm Controls	yes	yes	no	yes	yes	no
Cartel Controls	yes	no	no	yes	no	no
Cartel FE	no	yes	yes	no	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
R^2	0.57	0.69	0.51	0.50	0.55	0.51
Observations	356	419	629	312	375	612

This table contains the estimates from linear regressions. We take the value of the logarithmic transformation of one plus the lobbying expenditures as explanatory variable (and its lagged transformation for the specifications 4 to 6) and the logarithmic transformation of one plus the private cartel sanction as dependant variable. As defined in the Connor (2016)'s data set, a private sanction comes from a private damage claim, for example, customers of a cartel may sue convicted wrongdoers for the losses they suffered in civil lawsuits (Bodnar et al., 2021). As a placebo test, we posit that private damages should not be influenced by lobbying expenditures. We run similar tests by taking lobbying expenditures one, two and three years after the DoJ sanction, without finding any effect. All Models are estimated for the 1998-2018 sample period. All variables are defined in Table 1. Robust standard errors are the results in parentheses. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 14: Cartel sanctions and lobbying expenditures: AAG and president alignment

Sanction	(1)	(2)	(3)
Lobbying expenditures×Non-alignment	0.821***	0.837***	1.001***
	(0.070)	(0.067)	(0.003)
Lobbying expenditures	-0.035***	-0.011	-0.003
	(0.012)	(0.012)	(0.012)
Non-alignment	-0.840***	0.365***	0.012
	(0.164)	(0.115)	(0.057)
Firm Controls	yes	yes	no
Cartel Controls	yes	no	no
Cartel FE	no	yes	yes
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
R^2	0.40	0.57	0.43
Observations	368	363	655

This table contains the estimates from linear regressions. We take the value of the logarithmic transformation of one plus the cartel sanction as dependant variable. The explanatory variable is the interaction between lobbying expenditures and a binary variable equal to one if the AAG of the antitrust authority is not aligned with the US president. $Non-alignment$ is a binary variable equal to one if the AAG is not aligned with the president. Models 1 to 3 are estimated for the 1998-2018 sample period. All variables are defined in Table 1. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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Table 15: Cartel sanctions and lobbying expenditures: the leniency program

Sanction	(1)	(2)	(3)
Lobbying expenditures×Leniency	-0.096**	-0.064*	-0.009
	(0.043)	(0.040)	(0.034)
Lobbying expenditures	-0.033***	-0.020*	-0.002
	(0.012)	(0.011)	(0.009)
Leniency	-0.657**	-0.761**	-1.228***
	(0.293)	(0.331)	(0.277)
Firm Controls	yes	yes	no
Cartel Controls	yes	no	no
Cartel FE	no	yes	yes
Industry FE	yes	yes	yes
Year FE	yes	yes	yes
Pseudo-R^2	0.25	0.32	0.23
Observations	312	375	629

This table contains the estimates from Tobit regressions. We take the value of the logarithmic transformation of one plus the cartel sanction as dependant variable. The explanatory variable is the interaction between lobbying expenditures and a dummy variable equal to one if the firm received the leniency program. Models 1 to 3 are estimated for the 1998-2018 sample period. All variables are defined in Table 1. P-value<0.01***, p-value<0.05**, p-value<0.1*.

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