The Effects of Global Leniency Programs on Margins and Mergers

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Abstract

In a cross-country study, we investigate how staggered passage of national leniency programs from 1990-2012 has affected firms' margins and merger activity. We find that these programs, which give amnesty to cartel conspirators that cooperate with antitrust authorities, reduced the gross margins of the affected firms. However, firms reacted to new regulations by engaging in more mergers that had negative effects on downstream firms. Our results imply that although these programs were generally effective, their full potential was mitigated by mergers that substitute for cartels, and that a strong merger review process might be a prerequisite for strengthening anti-collusion enforcement.

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Researchers, policy makers, and media have recently raised concerns about potentially decreasing product market competition in the US and the rest of the world.[1] Reduced competition may come from increasing industry consolidation as well as collusion among market participants to retain their individual market shares. Recently, many countries, including the US, have sprung into action to combat anticompetitive misconduct by making the formation of cartels more difficult. However, whereas stronger enforcement has been shown to deter cartels and enhance detection (Miller, 2009), observations that product market competition has not intensified and in fact may even have weakened raise questions about the effectiveness and unintended consequences of antitrust enforcement.

One possible reason for such ineffectiveness could lie in the substitutability between cartels and mergers in achieving market power. We study this issue by focusing on the staggered passage of leniency programs around the world as a proxy for strengthening antitrust enforcement against cartel activities. We focus on firms from 63 countries and territories over 1990-2012 using data from the Compustat Global and North America databases. We first show that the global leniency programs were effective in general. They led to more cartel detections and lower margins of affected firms and thereby likely improved consumer welfare. However, we also find that when firms face new regulatory barriers to cartel formation, they acquire other firms – and these mergers have a negative effect on customer firms' stock prices. Given that breaking up cartels is justified as it encourages competition and protects consumer welfare, our findings suggest that although leniency programs are effective in general, their positive effects might be mitigated if firms redraw their boundaries in response to regulatory actions. Thus, anti-collusion enforcement may be effective only when coupled with a strong merger review process.

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[1] Grullon, Larkin, and Michaely (2016) find that 75% of US industries have become more concentrated over the last two decades. De Loecker and Eeckhout (2017) document the rise in average markups across US industries, primarily coming from the increased number of high-markup firms. The Economist (2016) notes that corporate profits have been rising over time, as does the Council of Economic Advisers (2016). Shapiro (2017) provides a critical review of the recent policy debates, media discussion, and academic evidence on the topic and calls for additional resources for cartel enforcement and tighter merger control (among other suggestions).

Leniency programs have been among the most important developments in cartel detection and deterrence (Chen and Rey, 2013). By reducing fines or even providing immunity for cartel members that collaborate in conviction cases, leniency programs are expected to increase the costs of forming cartels and the benefits of breaking them up. We exploit the fact that countries passed leniency laws at different points in time between 1993 and 2011 to establish their causal effect on firms' gross profit margins and merger activity. In particular, we compare the change in the margins and acquisition strategies of firms that are affected by the law to the contemporaneous change in the margins and acquisition strategies of control firms that are headquartered in countries that have not yet passed such a law.

We focus on a sample of 54,189 firms over the period of 1990-2012. We follow three difference-in-difference identification strategies: (a) an estimation based on a staggered passage of the laws, (b) a one-to-one firm matching, and (c) an identification based on a firm's exposure to the passage of laws in foreign countries (exposure is defined in terms of the industry's export share to a country or the firm's own operations there).

Using all three identification strategies, we show that leniency laws have indeed affected the margins of firms. This suggests that, by and large, leniency programs have been effective tools in dissolving existing collusive arrangements and/or preventing the formation of new ones. The effect is significant in economic terms. Our estimation based on a staggered passage of the laws suggests that leniency laws lead to a 14.8% drop in gross margins from the average sample gross margin before the leniency law passage. We further find that such restrictions on the ability to create a cartel increase incentives to engage in mergers. The passage of a leniency law raises the annual total dollar value of mergers from 0.6% to 1.3% of the lagged total assets. In other words, firms replace the market power provided by a cartel with the market power provided by a larger scale.

We obtain consistent results if we use a matched sample methodology. We match each country with a control country with the closest GDP per capita selected from the countries that had not passed the law by the time the treated country did. We then match firms in the treated country to the corresponding firms in the control country that are operating in the same industry and are closest in size. These firms are in the same industry, are similar in size, and are located in countries with similar states of economic development; therefore, in the absence of leniency laws, we expect them to have experienced similar changes in profitability and to have followed similar corporate policies. When we control for matched-pair*year and firm fixed effects, the results indicate that passage of the law has a greater effect on the margins and the merger activity of firms headquartered in the countries that passed the law than it does on the margins and the merger activity of their counterparts in countries that did not pass the law.

Our results are also robust to the third specification in which, instead of focusing on leniency laws passed in the country where the firm is headquartered (i.e., the country that the firm is presumably most exposed to), we exploit the passage of leniency laws in countries to which the firm's industry is exporting. Because these foreign countries represent the most likely markets of the firm's international operations, the firm is also likely affected by their leniency laws. At the same time, however, there is less concern about the spurious link between the unobservable trends in a firm's industry in its home market and the passage of the laws. This specification also addresses the concern that our firm-level observations may not be spread equally across countries. The analysis based on these alternative and more exogenous measures of a firm's exposure to leniency laws delivers consistent results.

We also investigate the economic effects of the mergers that follow leniency law passages. We start by demonstrating that firms that pursue mergers after the passage of leniency laws experience a smaller drop in profitability than (a) similar firms in their industry and country, and (b) similar firms in their industry and country that attempt but fail to complete mergers.

Finally, we distinguish between merging to improve efficiency and merging to increase market power by studying how downstream firms react to the cartel-busting-related mergers of suppliers. We examine the merger deal announcements and, using the OECD Input-Output tables, we compare the stock price reactions following these deals for firms that are more and less likely to be the downstream firms of the merging suppliers. This analysis allows us to control for deal fixed effects and any differences in unobservable and observable deal characteristics before and after leniency laws. We find a strong negative stock market reaction for downstream firms around the post-leniency-law merger announcements of supplier firms. This suggests that potential customer firms are expected to lose from the mergers initiated in the wake of new leniency laws. At least to a certain extent, then, these mergers act as a substitute for now harder-to-form explicit cartels, mitigating the effectiveness of the leniency laws.

I. Theoretical Background and Contribution to Literature

Leniency programs allow the courts and/or regulators to grant full or partial immunity to companies that participated in illegal cartels but cooperated in providing information about the cartel. The US was the first country to adopt such a program in 1973. However, this program remained largely ineffective until 1993 when it was strengthened by making the guarantee of amnesty clearer and broader. The revised law stipulates that if a cartel is not being investigated by the Department of Justice (DoJ) and the Federal Trade Commission (FTC) or if these antitrust authorities do not have sufficient evidence, the first self-reporting cartel member, including its managers, employees, and directors, will be granted amnesty. The revised law proved successful in destabilizing existing cartels and in deterring new cartel formation (Miller, 2009) and inspired several other countries to pass similar laws (Hammond, 2005).

Our article contributes to the literature on the effects of leniency programs (see Spagnolo and Marvão (2017) for an extensive recent summary of the literature). The theoretical literature, which started with

studies on how leniency can prevent collusion in hierarchical relationships (e.g., Kofman and Lawarrèe, 1996), has distinguished two main countervailing forces. On the one hand, leniency laws destabilize cartels as they reduce leniency applicant's costs of defection and potentially increase the costs of its rivals, who may have to pay fines imposed by the antitrust authority (Motta and Polo, 2003; Ellis and Wilson, 2003; Aubert, Kovacic, and Rey, 2006; Harrington, 2008). If a firm's rivals bear extra costs, the firm is likely to become more competitive in the product markets. If all firms anticipate this, the cartels become less stable. On the other hand, the anticipated costs of collusion could decrease if firms expect to be the first to apply for leniency, which would enable them to pay lower fines than in the case without leniency and impose extra costs on their rivals (e.g., the negative effects noted by McCutcheon, 1997; Spagnolo, 2000; Motta and Polo, 2003; Chen and Rey, 2013). This would stabilize existing cartels or even induce the formation of new ones. Thus, the overall effect of leniency laws on the costs of collusion is uncertain and depends on the size of fines and the other particulars of the leniency program (e.g., whether leniency is awarded only to the first applicant).

Most recent empirical research has found that leniency programs have a positive effect on competition. For example, Miller (2009) finds that the US leniency program has increased cartel deterrence and enhanced detection, whereas Borrell, Jiménez, and García (2014) show an improved managerial perception of competition in countries that have passed leniency programs. We contribute by establishing a link between the passage of leniency programs and firm margins that capture the decrease in collusion activity in a more comprehensive way than the number of leniency applications and convictions. In this way, we respond to Miller's (2009) two suggestions for further research: capturing undiscovered cartels and exploring the introduction of leniency programs by other antitrust authorities to avoid relying on a single time-series, as in the case of studying a single new leniency program.

In addition, we are the first, to the best of our knowledge, to demonstrate how firms react to leniency programs by changing their acquisition strategies.[2] In so doing, we provide empirical evidence to the theoretical literature on the corporate effects of collusion (e.g., Maksimovic, 1988; Spagnolo, 2001).

Our second contribution to the empirical literature on collusion focuses on the substitution between cartels and mergers. Early observations of such a relationship were made by Bittlingmayer (1985) and Mueller (1996), who show that the first great merger wave in the US followed the passage of the Sherman Act restricting cartels, as well as by Symeonidis (2000), who documents that cartel prohibition induced corporate restructuring in the UK. A few recent articles have systematically looked at the merger activity that follows cartel detections. For instance, Hüschelrath and Smuda (2013) and Davies, Ormosi, and Graffenberger (2015) study European Commission (EC) cases and find that merger activity rises after cartel convictions at the firm level and at the general worldwide industry level, respectively. Marx and Zhou (2015) also study EC cartel cases and find that merger rates increase following the passage of the EC leniency law in 1996.

Our article differs from these studies in that it focuses on a larger international sample. This allows us to analyze a more comprehensive set of firms. In addition, the observed leniency application and the follow-up merger might be subject to reverse causality. Many cartel cases are detected during the merger negotiations by the due diligence teams of the merging parties, which suggests that cartel detection might be endogenous to the merger activity. Moreover, whereas detected cases could provide important insights, they could also be endogenous to unobservable factors affecting the prevalence of both cartels and mergers. Such factors could include the industry's expected profits, whistleblowing by competitors or employees, and lobbying by consumer pressure groups. Thus, our identification strategy, which relies on

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[2] Sovinsky and Helland (2018) find that the US leniency law discouraged firms to participate in joint research ventures. Heim, Huschelrath, Laitenberger, and Spiegel (2019) study anticompetitive effects of leniency laws on minority share purchases.

using the staggered passage of leniency laws around the world to measure increasing costs of collusion, provides important complementary evidence to that of the detected cases, as it also captures the reaction of the members in the unobserved cartels. The staggered nature of leniency programs around the world also enables us to control for global or regional industry shocks that occur contemporaneously with the passage of leniency laws. Such shocks are difficult to control for when only a single jurisdiction is studied, as in Miller (2009) or Marx and Zhou (2015), for example.

Finally, by showing how a shock to upstream firms propagates to the stock prices of downstream firms, our results also contribute to retrospective studies on whether mergers result in higher market power or improve efficiency (e.g., Ashenfelter and Hosken, 2010; Ashenfelter, Hosken, and Weinberg, 2013, 2014; see Kwoka, 2014 for an extensive survey of the literature). We look at one particular type of mergers – those that arise because of higher costs of collusion – and find evidence consistent with the market power effects.

II. Data on Leniency Programs and Other Main Variables

A. Leniency Programs

We study the passage of leniency programs around the world as our primary source of variation in the anti-cartel enforcement. We collect data on the passage of leniency programs in 63 large countries and territories from Cartel Regulation 2013 published by Getting the Deal Through.[3] We manually check this

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[3] In some cases, most notably in the US, leniency laws were *strengthened* rather than *introduced* during this period. For the sake of brevity, throughout the article we use these words interchangeably. In addition, leniency laws could have coincided with other legal changes that strengthened enforcement against cartels. In that case, one should interpret the passage of leniency laws as a proxy for generally stronger enforcement against product market collusion. We choose to focus on the introduction of leniency laws, as opposed to penalty increases or changes in investigative powers, as leniency laws are more comparable across countries. In addition, unlike some other cartel policy changes, leniency programs have never been reversed and could have been considered nearly permanent, which reduces concerns that the leniency treatment was assumed to be temporary (Hennesy and Strebulaev, 2015). For a subset of countries, we collect data on other legislative changes of cartel enforcement and control for them in Table 8.

information and complement it with press releases and news articles. Table 1 reports the years when leniency programs were passed.

We carefully read online discussions and press announcements to make sure that the passage of these programs is not confounded with a particular trend in economic or political conditions. If that were the case, our identification strategy might be capturing these conditions rather than the effect of the leniency programs. We find that some countries passed their leniency programs after prominent collusion cases.[4] Other countries passed leniency programs after significant pressure from the US, the EU, or supranational organizations (Lipsky, 2009).[5] Moreover, the EU encourages its member states to adopt leniency programs and often seeks similar provisions in its bilateral associations and trade agreements, whereas the IMF and the World Bank request the overhaul of antitrust laws as a condition for loans and other funding (Bradford, 2012). The economic conditions around the passage of leniency programs also varied. For instance, Taiwan passed its law in response to general concerns about rising consumer prices, whereas Korea passed it after the financial crisis.

In the Internet Appendix, we report the estimation in which we attempt to predict the timing of leniency program passage as a function of a country's economic characteristics as well as of the political orientation of its government. The most consistent variable in predicting the law is the GDP per capita, which suggests that more advanced countries passed the law first. We therefore control for this variable in all of our regressions, in addition to other macroeconomic characteristics. More importantly, neither GDP

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[4] The US strengthened its leniency program after the vitamin cartel, and Hungary did so after facing significant criticism of its competition investigation against mobile telephone operators. Switzerland strengthened its competition law, including leniency provisions, in 2003 after it failed to prosecute firms involved in a vitamin cartel.
[5] Mexico passed the law in 2006 following the general recommendations of an OECD Peers Review on Competition Law and Policy in Mexico. The review reported that Mexico's antitrust authority needed better investigative tools, including the ability to give leniency to whistleblowers who reveal secret cartel conduct. Similarly, the US bargained for the strengthening of Singapore's antitrust laws in its negotiations for a bilateral free trade agreement.

growth nor the unemployment rate is related to the timing of the passage, which suggests that worsening (or improving) economic conditions are unlikely to have been the main driver of leniency law passage.

B. Data on Margins and Mergers

Our main response variables in this article are margins, captured by accounting gross profitability margins, and firm merger activity. The data on gross profitability margins and other accounting information come from the Compustat Global and North America datasets. We consider all the publicly listed firms in these datasets over the period of 1990-2012.[6]

The data on mergers come from the Securities Data Corporation (SDC) Platinum Database, from which we extract all merger transactions in the world from 1990-2012. We measure the annual merger activity of a firm by the total dollar value of mergers and acquisitions over the year, scaled by lagged assets. Our initial sample covers 54,189 firms and 543,737 firm-years. Table 1 reports the number of firms from each country in our main dataset and the number of firm-year observations that they contribute to our sample, and Table 2 presents the summary statistics of the key variables. As the sample is based on publicly listed firms, the firms are relatively large, with average assets of $2.9bn and average gross margins of 26%. The mean dollar value of acquisitions, scaled by assets, is 1.4%. In the period before the passage of leniency laws, the mean gross margin is 34%, and the mean dollar value of acquisitions is 0.6%. On the other hand, in the period after the passage of leniency laws, the mean gross margin is 20%, and the mean dollar value of acquisitions is 1.9%.

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[6] The Compustat Global sample includes only publicly listed firms, so we do not observe the product portfolios of the firms. Still, this sample has some advantages. First, it allows us to perform a wide-scale study spanning different industries and countries, providing insights into the global effects of leniency programs. Second, our approach to identifying how the average leniency program has affected international firms relies on an assumption that any contemporaneous policy changes are not correlated with the passage of leniency programs in a systematic fashion. It is more difficult to satisfy such a condition in a single-country study. Third, having public firm data allows us to look at the stock price reaction and assess how the valuations of customer firms change after the merger transactions that followed leniency program passage.

To illustrate this comparison, we plot the effect of leniency laws for the period from the two years before to the two years after the passage of the law. We display the average gross profitability margins for firms that were affected by leniency laws as well as for a sample of control firms. The control sample consists of firms that did not face the introduction of leniency laws over the same five-year period as the treated firms. The plots depicted in Figure 1 illustrate that although the pre-trends for the treatment and the control groups are similar, there is a drop in the gross margin of the treated firms. The upper plot shows the separate trends for treated and control firms whereas the lower plot shows the 95% confidence intervals around the difference between the averages of treated and control firms. We see that the difference between the gross margins becomes statistically significant from zero after the passage of leniency laws.

We do the same for our measure of merger activity, i.e., total dollar value of assets scaled by lagged assets. The plots depicted in Figure 2 illustrate that although the pre-existing trends of the treatment and the control groups are similar, there is an increase in the merger activity of the treated firms and such difference is statistically significant from zero.

We formally compare the affected firms and the control firms in Table 3. We follow the same procedure as in Figures 1 and 2, comparing the average gross profitability margins as well as the average merger activity for firms that were affected by leniency laws and for control firms that were not affected by leniency laws. We compare the margins one year before the leniency program was passed for the treated firms. The control sample consists of firms that did not face the introduction of leniency laws over the same five-year period as the treated firms. We estimate averages for each country and industry. We do not find statistically significant differences, which suggests that right before the passage of the leniency law, the margins of the firms in the treated countries were not much different from the margins of the firms in the control countries.

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C. Other Data

Although we focus on the effect of leniency programs on firm profitability and merger activity, we also provide evidence on cartel detections. The data on detected cartels come from the Private International Cartel dataset on cartel sanctions created by John Connor and described in detail in Connor (2014). This hand-collected dataset covers all of the major private international[7] cartels discovered, disclosed, and sanctioned by regulators around the world since January 1986. We use the year 2012 edition, which contains 746 cartels involving 7,496 firms (some firms are recidivists and, as such, are members of multiple cartels). The median (mean) cartel involves five (ten) companies and lasts five (seven) years before it is discovered by a regulator. The estimated median (mean) dollar value of the market size that is affected by a cartel is $2bn ($26.7bn). In addition, we use the data on detected cartels to estimate the probability that a firm belongs to a cartel based on the observable firm, industry, and country characteristics. The mean probability of a firm being indicted in a collusion case in a given year is 0.4%.

Table 1 reports the number of the detected (convicted or suspected) cartel cases in each country in our sample, as well as the number of firms in our sample. With a few exceptions, the larger economies have more firms and more firm-year observations in our sample, as well as more detected cartel cases. The correlation between the number of firms in the Compustat Global and North America dataset and the number of detected cartels is 0.86, which suggests that larger economies prosecute collusive activities more (likely in proportion to the sizes of their economies). This correlation also makes a case for using firm-level specifications that add more weight to larger economies that also have more cartels.

In addition, we employ an identification strategy in which we look at the effects of leniency law passage in foreign countries. Our proxy for a firm's exposure to a foreign country's leniency law is the

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[7] One drawback of this dataset that it covers only international cartels, i.e., those involving firms that come from at least two different countries.

trade activity in the firm's industry between its home country and the foreign country. To capture the trade activity, we use export data from the CEPII TradeProd database, which contains bilateral trade flows for more than 200 countries at the three-digit ISIC industry level from 1980-2006. We match them to the three-digit SIC codes. If multiple three-digit ISIC codes match the three-digit SIC codes, we take the average of the respective values within the three-digit SIC. Because the CEPII TradeProd database is available only at the three-digit industry level, to maintain consistency we adopt this level of granularity throughout the whole article. For a limited number of firms, we also collect data on their actual operations in foreign countries. The data regarding individual firm operations around the world come from the Lexis Nexis Corporate Affiliations database that we manually name-match to Compustat data.

III. Leniency Law Effects on Cartel Detections and Margins

Miller (2009) shows that the US leniency law had a significant effect on cartel deterrence in the US. We start by extending his analysis to the international context and examine whether these laws were effective globally.

We first study the (observed) effect of the staggered passage of leniency laws on cartel detections at the country level. Then, we move to our main variable of interest and investigate the (observed and unobserved) effect of the passage of leniency laws on firm profitability. In studying effects on margins, we perform the analysis at the firm level and follow three flavors of differences-in-differences estimation strategies. First, we estimate a country-level specification relying on the staggered passage of leniency laws. Second, we perform a matched firm analysis by matching firms in leniency-law-passing countries to firms that are in the same industry but located in non-leniency-law-passing countries. Finally, we apply an identification strategy in which we estimate the effect of leniency laws passed in other countries that are a firm's potential export markets or where the firm has international operations.

A. Effects on Cartel Detections

We first investigate whether the passage of a leniency law in a country predicts the conviction of cartels in that country. We estimate a country-level panel specification from 1990-2012. We assign the lead jurisdiction to be the relevant country, as reported in the Connor (2014) dataset. Because the total number of convicted or suspected cartels in a country is a count variable that takes a value of zero with high frequency (82% of the observations are zero), we assume that the expected number of detections follows an exponential function of the leniency law treatment and estimate a Poisson model (e.g., Hausmann, Hall, and Griliches, 1984). Specifically, we use the method of quasi-maximum likelihood, which provides consistent estimates as long as the conditional mean is correctly specified even if the true underlying distribution is not a Poisson distribution (Wooldridge, 1999). To control for generic differential characteristics between countries, we control for country fixed effects and cluster the standard errors at the country level. We also control for time fixed effects. Our specification is then:

$$Detections_{kt} = \alpha + \beta Leniency\ Law_{kt} + \gamma X_{kt} + u_k + v_t + \varepsilon_{kt}, \tag{1}$$

where k indexes countries, and t indexes years. $Detections_{kt}$ is the number of convicted and suspected cartels in country k in year t as captured by Connor (2014). $Leniency\ Law_{kt}$ is our treatment of the passage of a leniency law, X_{kt} represents a vector of control variables, and u_k and v_t are country and year fixed effects, respectively.

We report the results in Table 4. In Column (1), we find that the passage of a leniency law more than doubles the number of detected cartels, increasing them by 154%.[8] This result is robust to controlling for the country's time-varying macroeconomic conditions (Column 2). In Column (3), the dependent variable

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[8] The economic effect from the Poisson model is estimated to be 154%=exp(0.935)-1. The results are robust to estimation by an OLS regression with country and year fixed effects.

is the number of firms detected in the cartel cases. We find that the number of detected cartel members is higher by a factor of eight.

The economic significance is larger than in the study of Miller (2009), who finds a 61% increase in cartel detections over a short six-month period following the introduction of US leniency program. The single time series in Miller (2009) makes it difficult to capture the longer-term effects due to potential confounding factors. In this study, we rely on the assumption that potential confounding unobserved factors are uncorrelated across countries. Our findings thus suggest that longer-term effects of leniency program introductions might be even more effective than suggested in Miller (2009).

B. Effects on Margins: Difference-in-Difference Estimates

Next, we perform firm-level analysis and focus on firm margins, proxied by gross profitability margins. By showing the link between cartel enforcement and profitability, we can also capture the effects of the unobservable breakups of existing cartels and the reduced probability of their formation. These unobservable effects cannot be detected by looking at actual cartel detections alone. Indeed, only about 10% to 30% of all cartel conspiracies are discovered (Connor, 2014), and it is likely that leniency laws affect not only the probability of discovery, but also the stability of the undiscovered cartels. In fact, one could argue that the passage of leniency laws leads to more cartels in general and/or greater cartel stability (Spagnolo, 2000; Motta and Polo, 2003; Chen and Rey, 2013). More cartel detections could simply reflect the same detection rate but a larger number of cartels in the economy. Thus, the gross margin would capture these unobservable effects of the undetected cartels.[9]

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[9] This effect on gross margins is net of any firm-level adjustments in response to cartel enforcement, such as the merger activities we study later in this article.

In our estimation, we rely on the staggered nature of the passage of leniency programs to identify their causal effect on firm margins. We follow the standard approach used in the literature, which relies on the staggered passage of laws in different geographic regions (e.g., the business combination laws across the US as in Bertrand and Mullainathan, 2003). This allows us to compare the change in the margins of firms that were affected by the law to the contemporaneous change in the margins of the control firms that were headquartered in countries that had not yet passed such a law.[10] We consider all of the Compustat Global and North America firms within our sample period and perform our baseline estimation:

$$Margin_{ikjt} = \alpha + \beta Leniency\ Law_{kt} + \gamma X_{ikt} + u_i + v_{jt} + \varepsilon_{ikjt}, \tag{2}$$

where *i* indexes firms headquartered in country *k*, *k* indexes countries, *j* indexes three-digit SIC industries, and *t* indexes years. $Margin_{ikt}$ is firm *i*'s gross profitability margin in year *t*. $Leniency\ Law_{kt}$ corresponds to the passage of a leniency law, X_{ikt} represents a vector of control variables, and u_i and v_{jt} are firm and industry-year fixed effects, respectively. Because the identification is at the country level, we cluster standard errors at the country level.

The use of firm fixed effects allows us to fully control for non-time-varying differences between firms (and also implicitly for non-time-varying differences between countries in which they are headquartered). For example, let us consider Germany, which passed a leniency law in 2000. We examine how average gross margins changed for the average German firm as compared to the firms in other countries (e.g., the average Italian firm). Controlling for firm fixed effects assumes that the average Italian firm is identical to the average German firm except that it was exposed to a leniency law on a

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[10] Although we consider the firm's headquarters country as the country where the firm conducts most of its business activities, we realize that most firms have international operations. We address this potential concern by adopting an alternative identification strategy in Section III.C in which we explicitly take into account the geography of firms' international operations.

different date (2007 as opposed to 2000). Firm-level analysis also allows us to control for any compositional effects, i.e., firm entry and exit.

Table 5 reports our baseline differences-in-differences estimations. We control for firm fixed effects and time fixed effects in Column (1) and additionally for firm and country characteristics in Column (2). Specifically, we control for asset size, leverage, GDP per capita, unemployment, and imports as a percentage of GDP.[11] In Column (3), we additionally control for industry*year fixed effects. We consider industries at the three-digit SIC level. The use of industry*year fixed effects removes any industry trend that could affect our results, such as a drop in the profitability of certain industries that could have coincided with legislative changes in competition law or potential spurious effects, such as contemporaneous global events affecting the industry.

As we adopt the latter specification as our main one, we choose it to interpret the economic significance of our estimates. We find that the passage of leniency laws lowers gross margins by 5 percentage points. This represents a 14.5% drop with respect to the average sample gross margin of 34.5% before the passage of leniency laws. These results provide evidence that leniency laws have a sizable negative effect on the profitability of the average firm.[12]

We further control for geography-specific time trends, given that leniency program passage can be correlated with general economic cycles. Because our identification comes from the country*year variation, we are not able to include country-specific time trends on an annual basis because that would be collinear with our leniency program variable. Instead, we construct time buckets at three-year intervals, starting with year 1990 (i.e., the first bucket spans years 1990-1992, the second spans years 1993-1995,

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[11] We control for a country's levels of trade as free trade agreements and antitrust policies might have similar effects on market structure.

[12] We also perform the same estimation separately for every SIC3 industry. One concern is that the negative effect could be driven by a few cartelized industries. In line with the general sentiment cited in the introduction that cartel activity is widespread, we find that for 199 of 283 SIC3 industries (i.e., 70% of them), the effect of leniency laws on profitability is negative.

etc.). In this way, we can control for country-specific trends at three-year intervals without invalidating our estimation strategy. We report these tests in Columns (4). After saturating the model with these country trends (and thus focusing on the shorter-term effects), we find that the economic effect is a 7.2% drop relative to the pre-leniency law average.

In Column (5), we further saturate the model by adding region-specific time trends (by adding region*year) fixed effects, in addition to industry*year fixed effects and country-specific trends at three-year intervals. We consider seven geographic regions: North America, Latin America, Western Europe, Central and Eastern Europe, Asia, Africa, and Oceania. Simultaneously controlling for region, country, and industry trends reduces the power of the tests, and the statistical significance of our effect is lower than in the previous tests. We find that the economic effect is a 3.8% drop in margins, relative to the pre-leniency law passage average.[13]

These results show that leniency laws have reduced the profitability of the affected firms, which suggests the effectiveness of the global passage of leniency laws.

C. Effects on Margins: Matched Sample

One major concern with comparing the policy changes in the tests based on the staggered passage of laws is that the treated group of countries – those that passed the laws earlier – might be different from the control group of countries – those that passed the laws later. For instance, more developed countries might pass the laws earlier, as we see from the predictions of law passage timing reported in the Internet

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[13] We provide additional robustness tests in Section VII, where we control for contemporaneous economic policy changes, and in the Internet Appendix. In the Internet Appendix, we implement matched difference-in-differences estimators as in Gormley and Matsa (2011, 2016) to control for treatment-time fixed effects that are common to both treated and control firms in our analysis. In the Internet Appendix, we also demonstrate the robustness of the estimation of standard errors by clustering them by industry*country, double-clustering by country and year, double-clustering by industry and year, and double-clustering by country and industry. Our results are also robust if we exclude observations with a Cook's D statistic of greater than 4/N; thus, it is unlikely that our estimates are driven by outliers.

Appendix. In addition, the industrial composition of the two sets of countries might differ. We address these concerns using a matched sample technique. The goal is to assess the impact of the passage of leniency programs on firms that differ as to when they face the passage of the law but are otherwise similar.

More specifically, we match firms to their peers headquartered in countries without a leniency program. We assume that these matched firm pairs face similar challenges in their product markets and would have operated similarly if not for the leniency laws. We define a peer firm as a firm that is in the same three-digit SIC industry and similar in terms of capital stock three years prior to leniency law passage. For every treated country, we find a control country with the closest GDP per capita to that of the treated firm's country. The control country must not have passed a leniency law before the treated country did. It also must not have passed the law in the next three years. If multiple similar control countries exist, we select the one that is closest geographically. For instance, for Germany, the control country is Belgium, for Greece – Cyprus, and for Hungary – Croatia.

In the next step, for each firm in a treated country, we find the corresponding firms in the control country that operate in the same three-digit SIC industry. If there are multiple matches, we focus on firms that have the closest capital stock, as proxied by the book value of property, plant, and equipment, three years prior to the passage of the law. We discard any match where the capital stock differs by more than 100%. We limit the analysis to three years before and three years after the passage of a leniency law in the treated country.

We add firm fixed effects and matched-pair*year fixed effects to control for any unobservable trends between the matched pairs. This set of fixed effects allows us to estimate the effect of the passage of the law within each pair: how much profitability changed for a firm in the country that passed the law compared to the firm in the country that did not pass the law. We argue that as these firms are in the same

industry, are similar in size, and are located in countries with similar states of economic development, in the absence of leniency laws they would have experienced similar changes in profitability and followed similar corporate policies.

The use of matched-pair*year fixed effects also allows us to control for any aggregate (industrial) trends that could have affected these firms in a particular year (e.g., technological shocks that could have similarly affected these firms). One might argue that industrial changes could have changed incentives to collude, and heightened collusion could have led to the enactment of leniency laws. The use of matched-pair*year fixed effects allows us to control for such industrial trends.

Moreover, this test goes beyond the previous specifications in which we already controlled for industry*year fixed effects by also controlling for firms that are of similar size and located in similarly developed economies. Thus, their reaction functions to any industrial shocks are likely to be similar.

For each treated firm, we require at least one control firm in a similar country, in the same industry, of similar capital stock, that has six years of data around the passage of the leniency program in the treated firm's country, resulting in a smaller sample. Table 6 presents the results. In Column (1), we find that the passage of a law has a higher negative effect on the margins of firms headquartered in countries that passed a leniency law than on the margins of their counterparts in countries that did not pass such a law. Relative to our baseline estimates presented in Table 5, the economic effect in this smaller sample is of a higher magnitude, equivalent to an 11.7 percentage point drop in gross margin.

In Column (2), we focus on firms that have a non-zero estimated probability of being detected in a cartel case, estimated using a prediction model based on time-varying firm characteristics (asset size, leverage, and ROA) and country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. The results are consistent with our previous ones and suggest that

when we examine similar firms with high probability of cartel incidence, we find consistent statistical and economic results.

The underlying assumption in these tests is that two firms in the same industry in two closely related countries would follow parallel trends in the absence of the passage of the leniency law in one of the countries. In Column (3), we assume that the *trends* in margins are more likely to be parallel if the starting *levels* of margins are similar. Thus, we match the treated and control firms based on the margins when we observe multiple matches in the same industry in two countries. We find consistent results.

D. Effects on Margins: Identification Based on Foreign Laws

A possible critique of an identification based on the passage of leniency programs is that it may not be fully exogenous to the political and economic conditions of a firm's country. In addition, many firms operate across borders, and most prosecuted cartels are international (Ghosal and Sokol, 2014). A firm that colludes with other firms in a foreign product market is subject to that country's antitrust laws. Foreign leniency law passage is thus likely to affect firms that operate across borders, not least because antitrust authorities coordinate their actions in prosecuting similar cartels.[14] Even if international antitrust authorities do not share information as per the leniency applicant's request, the information on the actual decision by the antitrust authority is released publicly, and other antitrust authorities can take advantage of that by investigating similar conduct in their own jurisdictions. We thus follow an additional identification strategy based on the passage of leniency laws in foreign countries.

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[14] The DoJ's Deputy Assistant Attorney General Brent Snyder was quoted as saying that the US's "Corporate Leniency Program revolutionized cartel enforcement, led to the successful prosecution of many long-running and egregious international cartels, and served as a model for leniency programs subsequently adopted in dozens of jurisdictions around the world." He asserted that "leniency is more valuable than it has ever been because the consequences of participating in a cartel and not securing leniency are increasing: more jurisdictions than ever before are effectively investigating and seriously punishing cartel offenses" (Jun 8, 2015).

Ideally, we would like to measure a firm's exposure to foreign laws by the firm's country-level export shares in those jurisdictions. Absent such data, we measure firm exposure to international laws in two ways. First, we rely on the industry's exposure and define the treatment based on a firm's exposure to the passage of leniency programs in countries to which the firm's *industry* sends a significant fraction of its exports. This variable accounts for the fact that the passage of a leniency program in a foreign country that is likely to be a firm's product market also increases the costs of collusion, because it is more difficult to form international cartels with industry peers in a foreign country where it is easier to apply for leniency. At the same time, this variable is less subject to the criticism that leniency programs are passed in response to certain political and economic conditions of the firm that also affect the firm's operations through other channels. That is, the continuous variable we call "Export Market Leniency Law" is even more exogenous to the political and economic conditions in a firm's country.[15] Also, our variation in the treatment variable now comes at the country*industry*year level. In this way, we address the concern that some countries may have a disproportionally large number of observations in our data, in which case country-level identification could lack precision.

We estimate Export Market Leniency Laws as the weighted average of the passage of leniency programs in all other countries excluding the country in which the firm is headquartered:

$$(\text{Export Market Leniency Law})_{jkt} = \sum_{\hat{k}} w_{\hat{k}j} L_{\hat{k}t},$$

where $\hat{k}$ denotes any country other than country k, j denotes a three-digit SIC industry, and t denotes the year. $w_{\hat{k}j}$ is the share of the three-digit SIC industry j's exports from country k to any other country $\hat{k}$ out of all of the exports from industry j in country k in 1990. $L_{\hat{k}t}$ is an indicator variable that takes a value of one if country $\hat{k}$ passed a leniency program by year t, and zero otherwise. To avoid any endogeneity of the

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[15] It is unlikely that powerful firms in the US and other western countries contributed to stalling the introduction of antitrust legislation in foreign countries. If anything, large foreign countries could even have pressured smaller countries to pass this antitrust legislation (as in the US-Singapore Free Trade Agreement).

industry structure, we base the weights on the data in year 1990. The variable ranges from zero when none of the foreign countries that receive any exports from the firm's industry have passed a law to one when all of the foreign countries with exports from the firm's industry have passed leniency programs.

Our specification is then as follows:

$$Margin_{ijkt} = \alpha + \beta(\text{Export Market Leniency Law})_{jkt} + \gamma X_{ikt} + u_i + v_{jt} + \varepsilon_{ijkt}. \quad (3)$$

Unlike in equations (1)-(2), in equation (3), a firm is considered as "treated" if at least one country to which its industry is exporting has passed a leniency program. The intensity of treatment changes as more of the countries to which the industry exports adopt leniency programs.

Table 7 reports the findings. In Column (1), we find that the passage of laws in these other countries has a significant negative effect on profitability. The results are robust to controlling for firm and country characteristics (Column (2)) and three-digit SIC industry*year fixed effects (Column (3)). In the latter case, we control for general industry trends at the global level. Thus, our identification provides a comparison within the three-digit SIC industry. Specifically, we compare the effects for firms in a three-digit SIC industry with a large export market in a foreign country that passes a leniency program to the effects for firms in the same two-digit SIC industry, but a different three-digit SIC industry with smaller or no export markets in foreign countries that pass a leniency program in that year.

As expected, the economic significance in this test is lower than the economic significance when we rely on the passage of the leniency law in the firm's headquarter country. If for every industry in each firm, we took the maximum exposure to the foreign country, i.e., the largest weight $w_{\widehat{k}j}$, and considered only those leniency laws, every such foreign leniency law contributed to lowering margins by 3.8 percentage points, which is lower than what we found for own country leniency laws in Table 5. The result is still economically meaningful, however, as it corresponds to an 11.2% drop of gross margins from pre-leniency levels.

The second way to measure a firm's exposure relies even more directly on the international nature of firm operations. For a subset of firms, we collect data on their international subsidiaries. This allows us to test whether the passage of laws in other countries in which they operate also has a significant effect. Specifically, we measure a firm's exposure to leniency laws by looking at the distribution of its subsidiaries around the world as recorded in the Lexis-Nexis Corporate Affiliations dataset.

Our proxy of exposure to leniency program changes is based on the proportion of firm activity that takes place in the country experiencing the program change. To illustrate, consider two firms, A and B, both headquartered in Italy. Firm A has 75% of its subsidiaries in Germany and 25% in France; Firm B has 25% of its subsidiaries in Germany and 75% in France. Hence, when Germany introduced the leniency program in 2000, Firm A was affected more than Firm B.

We call this exposure variable "Subsidiary Based Leniency Law." It is again estimated as the weighted average of the passage of laws in the other countries, excluding the one in which the firm is headquartered. However, unlike the previous variable, this one is defined at the firm level. It is:

$$(\text{Subsidiary Based Leniency Law})_{ikt} = \sum_{\hat{k}} w_{\hat{k}it} L_{\hat{k}t},$$

where $\hat{k}$ denotes any country other than country k, i denotes a firm, and t denotes a year. $w_{\hat{k}i}$ is the ratio of subsidiaries that firm i (from country k) has in any other country $\hat{k}$ of all firm i's foreign subsidiaries. $L_{\hat{k}t}$ is an indicator variable that takes a value of one if country $\hat{k}$ passed a leniency law by year t, and zero otherwise. The variable ranges from zero (when none of the foreign countries in which the firm has subsidiaries have passed leniency laws) to one (when all of the foreign countries in which the firm has subsidiaries have passed leniency laws). Our specification is then as follows:

$$Margin_{ikjt} = \alpha + \beta(\text{Subsidiary Based Leniency Law})_{ikt} + \gamma X_{ikt} + u_i + v_{jt} + \varepsilon_{ijkt}. \quad (4)$$

We present the results using this specification in the last three columns of Table 7.[16] We perform tests similar to those of the Export Market Leniency Law variable. In Column (4), we find that the passage of leniency programs in the countries where the firm has subsidiaries significantly reduces profitability as the firm becomes exposed to stronger antitrust enforcement in its foreign operations. The results are consistent when we control for firm and country characteristics (Column (5)) as well as for three-digit SIC industry*year fixed effects (Column (6)). The average firm in this sample has 1.6 foreign subsidiaries, which suggests that if the country of one of the subsidiaries passed the leniency program, the firm's gross margin decreased by 3.3 percentage points.

E. Effect on Margins: Heterogeneous Responses

If our hypothesis is correct, we should find that the impact of the passage of leniency laws differs across affected firms in predictable ways. Leniency laws are likely to affect primarily those firms that are engaged in collusion or have the potential to form cartels in the future. We conduct a set of tests of the heterogeneity of the effect and report the results in Table 8.

First, we estimate the likelihood that a firm will be detected in a cartel case. We use a prediction model based on time-varying firm characteristics (asset size, leverage, and ROA) and country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. Both industry and country characteristics are important determinants of the potential for cartelization. We fit the prediction model using only pre-leniency observations and predict the probability that a firm will be detected in a cartel case after the passage of a leniency law. Column (1) reports the results of the estimation in which we interact leniency law with the predicted probability of detection. We find that firms that are more likely to be detected in a cartel case experience a larger drop in margins.

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[16] Given the limited subsidiary data, our sample is significantly reduced.

Given that the average predicted probability of detection in our sample is 0.004, a firm with an average probability of detection based on the observables experiences an additional drop in the gross margins of 0.3 percentage points. This holds true even if the firm was never convicted during our sample period, which suggests that leniency laws also had deterrence effects on collusive actions.

Second, we look at the actual cartel detections as reported in Connor (2014) and interact a recent cartel investigation with the passage of a leniency law. In Column (2), we interact leniency law passage with the dummy if the firm was detected in the past three years, whereas in Column (3) we interact leniency law passage with the dummy if the firm was detected in the past five years. We find that the margins drop more for the firms that were recently detected in a cartel case. The effect is economically significant. A recent cartel detection contributed to the leniency law effect by an additional 3.6-4.2 percentage point drop in the gross margins, and the overall effect of leniency law passage on the firms that had recently been convicted was 8.2-8.6 percentage points – up to a 25% drop from the unconditional average of pre-leniency law gross margins.

Third, we explore differences across industries. In Column (4), we sort the firms according to the Herfindahl-Hirschman index (HHI) of the firm's industry in its country in the year of leniency law adoption. For each industry and country, we estimate HHI based on the number of employees of firms in that industry and country as reported in the Compustat Global and North America datasets.[17] Firms in concentrated industries with fewer symmetric players might find it easier to collude: Motta (2004) argues that this is the most important cross-sectional factor empirically predicting collusion, and Huck, Normann, and Oeschssler (2004) provide supporting experimental evidence. We then interact leniency law passages with HHI. We find that our results on margins are stronger for more concentrated industries.

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[17] HHI, especially if estimated based on the sales of firms in the industry, itself is likely to be endogenous to leniency law passage. We thus use a static measure of HHI in the year of leniency law adoption and base our estimate on the number of employees rather than sales (our results are robust if we use sales rather than employees).

The average HHI in our sample is 0.33, with the top quartile being 0.50 and the bottom quartile being 0.08. These interquartile range estimates suggest that the effect is small in economic magnitude and not statistically significant from zero at conventional levels for firms in industries with low levels of concentration, but the effect is as high as 9 percentage points for firms in industries with high levels of concentration.

Next, we recognize that the measures of HHI estimated based on solely publicly listed firm data might not reflect the actual degree of concentration in the industries (Ali, Klasa, and Yeung, 2009), especially in the sample of international firms. However, the industry is more likely to be oligopolistic if it exhibits increasing returns to scale. We thus classify the firms according to whether the industry in which they operate was experiencing increasing or decreasing returns to scale in 1996. We measure this by estimating a two-factor Cobb-Douglas production function for each two-digit SIC industry in year 1996, using all the Compustat Global and North America firms. We proxy for the firm's output by its sales, for the firm's labor by the number of its employees, and for the firm's capital by the value of firms' property, plant and equipment. We then add the coefficients for the proxies for labor and capital, and we define those industries in which the sum of the point estimates is higher than one as having increasing returns to scale, and those in which the sum of the point estimates is lower than one as having decreasing returns to scale. Both the median and mean of this sum in our sample are in fact equal to one. In Column (5), we show that the results are strongest in the industries that were experiencing increasing returns to scale.[18] Again looking at the interquartile range of this proxy of the returns to scale, we find that the effect of the leniency law varies from 4.3 percentage points to 5.9 percentage points.

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[18] Our results are consistent if we instead adopt the semi-parametric methodology of Olley and Pakes (1996). The Olley and Pakes (1996) approach also controls for the selection and simultaneity biases by allowing for firm-specific productivity differences and endogenizing the firm's liquidation decision. In line with Olley and Pakes (1996), we proxy for observable firm-level productivity using a firm's investment decisions – i.e., the changes in its property, plant and equipment and intangible

In Column (6), we sort industries according to their international exposure. We argue that higher international exposure would reduce the leniency law effect. First, if collusion is easier to sustain with domestic players than with international players, then higher import competition and thus rivalry from international players would make collusion between domestic players less likely. In addition, if a firm's industry is a large exporter, it would make the domestic leniency law less important, as the industry relies less on the domestic market for its sales. Thus, both exports and imports would reduce the importance of domestic leniency programs.

In particular, we estimate international exposure based on the country and industry trade flows as reported in the CEPII TradeProd database, which covers 26 industrial sectors from 1980 to 2006. As with the HHI measure, we fix trade flows in the year when the leniency law is passed. As we report in Column (6), the leniency law effect is weaker if the industry in the country is characterized by large trade flows. For instance, a firm at the top quartile of trade flows experienced a gross margin drop of 2.6 percentage points as compared to the 4.3 percentage point drop by the firm at the bottom quartile of trade flows.

Finally, we look at recent industry growth. In general, a high-growth industry is associated with less collusion (Ivaldi, Jullien, Rey, Seabright, and Tirole, 2003). First, high growth encourages new entry, and the industry is expected to become less profitable in the future. In this scenario, the loss of future profits from being punished by rival firms if cooperation breaks down would be lower than the gain from cheating today. Second, if the recent high growth is associated with a (temporary) upturn in a cyclical industry, the gain from deviation today would also outweigh the loss from punishment in the future, and collusion could be more difficult to sustain (Rotemberg and Saloner, 1986). We use the industry median of sales growth in the firm's country as the proxy for the industry's maturity. As Column (7) shows, we

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assets. Our results are also consistent if we consider only industries for which the sum of the point estimates is statistically significant from one at least at the 10% level.

find that the effect on margins is higher when interacted with this cross-sectional characteristic. If we look at the interquartile range of the median growth (3% to 13%), we find that the effect of the leniency law varies from 4.5 percentage points to 5.8 percentage points.

IV. The Effects of Leniency Laws on Mergers

We now investigate how firms adapt their merger activity after the passage of leniency laws. We posit that the increased cost of maintaining or starting new collusive links with competitors induces a change in one of the main corporate policies: merger activities. We analyze this using the SDC Platinum database, which we merge with the Compustat Global and North America datasets for the 1990-2012 period.[19]

A. Difference-in-Difference Estimates

We then apply a differences-in-differences methodology, as specified in Section IV.B, and estimate the effect of the passage of leniency laws on a firm's decision to engage in mergers:

$$Mergers_{ijkt} = \alpha + \beta Leniency\ Law_{kt} + \gamma X_{ikt} + u_i + v_{jt} + \varepsilon_{ijkt}, \tag{5}$$

where i indexes firms headquartered in country k, k indexes countries, and t indexes years. $Mergers_{ijkt}$ is measured by the total dollar value of mergers and acquisitions over the year, scaled by lagged assets.[20] $Leniency\ Law_{kt}$ is our treatment of the passage of a leniency law, X_{ikt} represents a vector of control variables, and u_i and v_{jt} are firm and industry-year fixed effects, respectively. We cluster standard errors at the country level.

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[19] In this section, we follow the specifications described in Section III. For the sake of brevity, we refer the reader to Section III for details on the methodological choices.

[20] In the Internet Appendix, we provide the estimates based on the alternative data source of the merger activity. In addition, we provide results where we redefine the merger activity measure to capture only horizontal mergers, i.e., those that involve firms in the same industry and the same country.

We report the results in Table 9. Column (1) indicates that the passage of leniency laws increases the total dollar value of mergers by 0.7% where the mean prior to leniency laws is 0.6%, thereby doubling the pre-leniency law merger activity. The results are robust to controlling for firm and country characteristics (Column (2)), industry*year fixed effects (Column (3)), country*three-year-interval fixed effects (Column (4)), and – in the most saturated model – industry*year fixed effects, country*three-year-interval fixed effects, and region*year fixed effects (Column (5)). Both statistical and economic estimates are similar after we add these controls.

B. Matched Sample

Our next analysis is based on a matched sample technique, following the methodology described in Section III.C. We provide the results in Table 10, where we report the results on the total dollar value of the mergers and acquisitions, scaled by lagged assets. In Column (1), we find that the passage of a law has a higher positive effect on the merger activity of firms headquartered in countries that passed a leniency law than on the merger activity of their counterparts in countries that did not pass such a law. Specifically, we find that the passage of a leniency law increases the dollar value of a merger by 0.9 percentage points, which is a 50% higher effect than in our baseline estimations using the full sample.

In Column (2), we focus on firms that have a non-zero estimated probability of being detected in a cartel case, estimated as in the previous section using a prediction model based on time-varying firm characteristics (asset size, leverage, and ROA) and country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. The effect based on this sample, in which cartel incidence is more likely, is larger than in the sample of all matched firms. In Column (3), we match the firms based on the gross margins in case there are multiple matches in the same industry in treated and control countries, and we find consistent results.

C. Identification Based on Foreign Laws

Further, we study the passage of foreign leniency laws. Following the methodology described in Section IV.D, we estimate the effect of foreign laws on firms' merger activity:

$$Mergers_{ijkt} = \alpha + \beta(\text{Export Market Leniency Law})_{jkt} + \gamma X_{ikt} + u_i + v_{jt} + \varepsilon_{ijkt}. \quad (6)$$

In Table 11, we report that the adoption of foreign leniency programs has led to higher merger activity (Columns (1)-(3)). To assess the economic significance, we interpret the results in the same way we did for the margins. If the country to which the industry is the most exposed passed the law, the merger activity rises by 0.004, or a 65% rise relative to the pre-leniency average. We obtain results with similar economic and statistical magnitudes when we estimate an alternative specification based on where firm subsidiaries are located, as we report in Columns (4)-(6):

$$Mergers_{ijkt} = \alpha + \beta(\text{Subsidiary Based Leniency Law})_{ikt} + \gamma X_{ikt} + u_i + v_{jt} + \varepsilon_{ijkt}, \quad (7)$$

Taken together, the findings in this section indicate that on the margin, firms prefer to collude, but when collusion becomes more difficult, they pursue merger activities.[21]

V. Economic Effects of Leniency-Law-Induced Mergers

Our findings thus far have indicated that leniency laws lead to higher number of cartel detections (Table 3) and lower margins (Tables 5-8), and they induce firms to pursue more mergers (Table 9-11). However, we have not determined whether firms that experience a drop in profitability and pursue mergers enjoy better performance in the future than firms that experience a drop in profitability but do not pursue mergers. It could be that firms that are affected by a drop in profitability due to leniency laws and pursue

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[21] As we have data on the detected cartels, we investigate whether firms merge with their former partners of the busted cartels. After manually inspecting the names indicted for collusion, we find only a handful of cases in which former cartel members, private or publicly listed, merged with each other after being detected in a cartel case. It could be that firms may have expected that the antitrust authorities' concerns would be too strong to risk pursuing a merger.

mergers are different from the firms that do not pursue mergers. Demonstrating that these effects occur simultaneously is challenging. To address this issue, we estimate the effects industry by industry and determine whether industries where profitability dropped also exhibit an increase in merger activity. For each three-digit SIC industry, we separately estimate our baseline difference-in-difference specifications, reported in Table 5, Column (2) and in Table 9, Column (2). We obtain point estimates of the leniency law effect for 282 industries for both gross margins and mergers. We find that for 136 of these industries, the coefficient on gross margins was negative, and the coefficient on merger activity was positive. These findings suggest that in almost half of the industries, these effects occur simultaneously.[22]

In Internet Appendix, we also plot the performance of the firms that are affected by leniency laws and subsequently pursue mergers against the performance of the firms that are in the same country and three-digit SIC industry but do not pursue mergers following the leniency law. We show that acquirers are in fact able to improve their profitability, thereby negating the effect of the leniency laws. In an additional analysis, we also show that similar trends persist once we account for the self-selection bias, i.e. that firms that pursue mergers could be different from those that do not (Li and Prabhala, 2010; Kwoka, 2014). In the spirit of Savor and Lu (2009) and Seru (2014), we limit the control group to firms that announced mergers in the first two years following leniency law passage but then failed to complete the attempted mergers for some reason. We find that the profitability of firms with successful mergers suffered less than the profitability of firms with failed mergers following the implementation of leniency laws.

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[22] In the other cases, profitability decreased and merger activity decreased in 62 industries, profitability increased and merger activity increased in 56 industries, and profitability increased and merger activity decreased in 28 industries. We report all coefficients for these industry regressions in the Internet Appendix.

VI. Drivers of Leniency-Law-Induced M&As

Finally, we explore the drivers of leniency-law-induced mergers by focusing on the customer firms' reactions to merger deals that follow leniency law passage. For instance, the laws that make collusion more difficult reduce prices and margins in the industry, so firms must increase their output to cover fixed costs. Mergers are thus one way of improving efficiency and realizing the required economies of scale. Alternatively, mergers could be pursued to mitigate the negative effect of leniency laws on market power. In the first case, customer firms are unaffected or may even benefit if some of the cost savings are passed on. In the second case, customer firms are worse off due to these mergers.

We now distinguish between efficiency and market power explanations. We focus on the stock price reaction of the customers of firms involved in mergers following a recent passage of leniency laws. If these merger transactions were motivated by efficiency reasons, the stock prices of the customer firms would not respond (or they would respond more positively if some of the cost savings were passed on to them). If, on the contrary, these merger transactions were motivated by the market power, then the stock prices of customer firms would respond more negatively.[23]

We choose to rely on stock price analysis rather than operating performance such as margins for two reasons. First, the effect of the mergers on the margins of downstream industries might take time to appear, so we choose instead to capture it by the market's anticipation of the long-term effects on the firm's future operating performance. Second, the merger activity in the supplier industries that follows leniency laws might be endogenous to other industry events that might simultaneously affect the gross

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[23] As Eckbo (1983) suggests, "In principle, one could discriminate between the collusion and efficiency theories by examining the abnormal returns to the merging firms' corporate customers and suppliers of inputs." Such mergers might also have effects on rivals (e.g., Davies, Ormosi, and Graffenberger, 2015). We focus exclusively on the reaction to customer firms, as the predictions on other industry players are less straightforward. In the case of suppliers, the efficiency improvements might involve optimizing supply chains and have negative effects on some suppliers. In the case of rivals, a positive effect on rival stock prices might capture not only the concentration effect but also the increase in the expected probability that the rivals will be taken over.

margins of customers. If so, looking at the interaction between leniency law and supplier mergers on margins might introduce more interpretation concerns than looking at short-term stock price reaction around merger announcements.

We identify the supplier-customer relationships from the OECD Input-Output tables for year 2002.[24] We then examine the cumulative abnormal returns over a three-day [0,2] window to determine whether they differ as to whether a leniency law was passed in the country in the last five years. Because horizontal mergers are more likely than other mergers to have a negative effect on consumer welfare, and in order to make the analysis more tractable, in this set of analyses we consider only horizontal mergers (i.e., mergers in which both the acquirer and the target are in the same three-digit SIC industry and country).

We expect, ceteris paribus, that the abnormal returns will be more negative for customers (who would be more negatively affected if their suppliers merged) than for non-customers. We measure this differential effect by the fraction of inputs that the firm's OECD industry category gets from the merging firms' OECD industry category, out of the total domestic value of inputs. A higher fraction of inputs corresponds to a stronger relationship. We call this variable "Intensity of Relationship" (i.e., a greater likelihood that the firm is in a customer industry of the merging firms). Our tests help to assess whether the market value of firms with a high value of Intensity of Relationship (i.e., customers) react differently from the market value of firms with a low value of Intensity of Relationship (i.e., non-customers). That is, we can compare firms in the industry that obtain 30% of their supplies from the merging firm industry to firms in the industry that obtain 20% of their supplies from the merging firm industry. We assume that the stock price reaction among possible customer firms would be linear in this share of supplies.

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[24] Due to data limitations, we capture fewer countries in this set of tests: Australia, Brazil, Canada, China, Czech Republic, Denmark, France, Germany, Greece, Hungary, Italy, Japan, Korea, the Netherlands, Norway, Poland, Spain, and the United Kingdom.

As we are specifically interested in the changing costs of collusion, we dig further and examine whether this differential reaction between customer firms and non-customer firms further differs when suppliers are exposed to the recent passage of a leniency law. Our primary variable of interest is the interaction between Intensity of Relationship and the dummy for the passage of a leniency law within the past five years.

In this analysis, we exploit the cross-sectional power coming from the supplier-customer relationships for each deal. This allows us to add deal fixed effects to control for any differences between deals, including any deal characteristics as well as differences in the unobserved effects of these deals on the wider economy. That is, our estimation compares how, *for the same deal*, the market price reaction differed across downstream firms that were not involved in the deal. In particular, we investigate how the market price reaction differed between firms that had stronger customer links with firms involved in the deal and firms that had weaker customer links with the firms involved in the deal. In fact, this estimation strategy assumes that all other firms could be affected by the deal, but the intensity of the effect is driven by the strength of the supplier-customer relationships. Deal fixed effects allow us to control for the possible concern that the types of deals could have changed after leniency law passage, and the changed composition of the deals could explain the differences in customer firm stock reaction. Our methodology with deal fixed effects allows us control for these possible changes in observed and unobserved deal characteristics, assuming that the overall stock price reaction function to deal announcements has not changed after the passage of leniency laws and has remained linear in customer industries' share of supplies.

We report the results in Table 12. We winsorize the abnormal returns at the 1% level and cluster the standard errors at the customer firm's three-digit SIC level. In the baseline specifications in Column (1), we find that if the merging firms were recently exposed to a leniency law passage, the degree of industrial

connectedness (i.e., industry links) between the supplier and the customer industries affects customer abnormal returns negatively. Given that average connectedness in our sample is 0.18, this suggests that for a downstream firm in the average industry that has 0.18 of its supplies from the industry of the upstream merging firms, such a merger has a negative stock price reaction of 0.1%. These findings suggest that merger activity following a rise in the costs of collusion is harmful to downstream firms, hinting that these merger transactions could be driven by market power reasons. In contrast, previous researchers (see, e.g., Fee and Thomas, 2004; Shahrur, 2005) have found statistically insignificant or positive effects of mergers on customer firms and claimed that mergers are generally driven by efficiency gains.

In Column (2), we control for downstream firm and country characteristics. In Column (3), we cluster standard errors at the deal level. In Columns (4) and (5), we report the estimates of the pooled regression without deal fixed effects but with year fixed effects. In these specifications we see that when not interacted with the leniency law, the intensity relationship by itself is negatively related to the downstream firm returns (Column (4)) but such level effect disappears when interaction term is included and thus negative returns are primarily present after the leniency law passage (Column (5)). In Column (6), we examine an even more recent passage of leniency laws, i.e., within the past three years. In all of these specifications, the main result holds. The only time the results differ is when we look at the passage of a leniency law at any time in the past (Column (7)). In this case, we do not find a statistically significant effect, which suggests that only the *recent* passage of leniency laws that sparks merger activity in the upstream industry has a differential effect on customer firm stock price reaction.

It is worth noting that although these mergers seem to be harmful for downstream firms, this does not necessarily mean that they are harmful for the end consumers as these downstream firms might absorb the squeezed margins.

In addition, although one interpretation of our results could be that firms merge as a way of replacing cartels, it could well be that as collusion costs rise, firms merge to reduce the number of existing players (e.g., Miller and Weinberg, 2017) or reduce the asymmetry in size between the remaining market participants (Bos and Harrington, 2010). Fewer players and more symmetric industry structure could help to facilitate collusion in the future. In that case, merger activity would complement rather than substitute cartel activity in the future. Either way, this is consistent with our thesis that antitrust legislation against cartels fosters merger activity that harms downstream firms.

VII. Robustness to Confounding Policies and Economic Trends

One common concern in studies that evaluate policy responses using data from different countries is that the passage of the laws that are being evaluated is correlated with other types of legislation or economic conditions in general. Although it is hardly possible to control for *all* other laws, in this section we aim to demonstrate the robustness of our findings by controlling for the most likely confounding legislation. We perform the same specifications for margin analysis as in Table 5, Column (3), and the same specifications for merger analysis as in Table 9, Column (3). The results appear in Table 13, Panels A and B, respectively. Our finding on the effect of leniency programs is robust to these controls.

In Column (1), we control for the introduction of a country's first competition law that outlaws price fixing and other collusive arrangements (leniency laws only improve the enforcement of these general competition laws). Some countries (e.g., Denmark) introduced competition laws during our study period. As expected, we find that the first competition law also positively affects merger activity, but its impact does not render the effect of leniency laws insignificant.

In Column (2), we control for the degree of a country's financial development. Indeed, a country could simultaneously pass a leniency law and adopt policies that strengthen the rule of law and make the

country more attractive for investment. This could lead to the development of the financial markets and consequently to a wave of mergers when capital becomes more available. We use the Chinn-Ito index of a country's degree of capital account openness (Chinn and Ito, 2006), the country's stock market capitalization as a percentage of GDP, and the country's private credit as a percentage of GDP (retrieved from the 2013 update of the database of Beck, Demirgüç-Kunt, and Levine (2000)). Controlling for these financial market characteristics does not affect our estimates.

We acknowledge that other changes in antitrust law occurred during our study period. In particular, there have been significant increases in penalties, changes in what constitutes cartel conduct, and increased investigative powers of antitrust authorities. We remind readers that we focus on leniency laws because the passage of a leniency law is a clearly identifiable and measurable event, whereas the other provisions could occur multiple times in each country (e.g., changes in penalties[25]) or could have unclear effects on cartel conduct (e.g., changes in violation provisions). Even if there were correlated changes in anti-cartel provisions, our identification based on leniency laws would then be a proxy for a general strengthening of the anti-cartel provisions and would be informative of a general enforcement effect. However, our estimates could be biased if we incorrectly attribute the gradual strengthening of anti-cartel provisions to one particular year (i.e., when the leniency law was passed). In order to address this issue, for some countries, we collect data on other provisions (e.g., the first time the penalty was increased in our sample period or the first time the definition of what constitutes cartel conduct was changed). As noted in Column (3), these controls do not significantly alter our estimates, which provides confidence that the passage of leniency laws has been a crucial measure in fighting cartels. Interestingly, an increase in penalties reduces merger activity.

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[25] The market could have anticipated the size of the measurable changes, such as changes in penalties, and a binary treatment could overshoot or undershoot the impact, depending upon the market's expectations (Hennessy and Strebulaev, 2015).

Moreover, by adopting policies (e.g., leniency laws) that promote fair competition in an economy, the country also strengthens its rule of law, which makes it more attractive for investment and innovation. In Column (4), we control for measures of the rule of law, regulatory quality, and the quality of the judicial system based on WorldBank Worldwide Governance Indicators. This does not affect our estimates.

Further, in Column (5), we control for the changes of takeover legislation that might have coincided with the anti-cartel enforcement legislation if the countries changed multiple policies at the same time. We draw the list of merger legislation changes from Lel and Miller (2015), and we control for it in our estimations. The confounding legislation does not alter the effect of leniency laws on margins or on merger activity.

Finally, in Column (6), we report results from the specifications where we exclude firms located in the US. These firms constitute a significant share of our original sample, but excluding them does not significantly alter our results. Even though the economic effect on the gross margin is smaller, the effect on M&A activity is of similar magnitude.

VIII. Discussion and Conclusion

Stigler (1950) suggests that one reason mergers are preferable to cartels is the illegality of the latter. In line with this, we provide evidence that when antitrust authorities have better tools to prosecute cartels, firms switch to mergers, thereby mitigating the effectiveness of the antitrust policies and suggesting that anti-collusion enforcement must be coupled with a strong merger review process.

In particular, we look at the staggered passage of leniency programs around the world. As these programs were passed at different times, we can control for general economic and industry shocks that affect firms. We show that leniency programs have been effective, reducing the average margins of firms headquartered in the countries that adopt leniency programs, as well as the margins of the firms that trade

with them. Moreover, following the passage of leniency programs, which increase the costs of cartel stability, firms reorganize their activities by pursuing more merger transactions. These mergers generate a more negative stock price reaction for customer firms around their announcements than other merger transactions do. With these findings, we demonstrate that firms sometimes prefer weaker integration in the form of cartels to stronger integration through merging, and they resort to the latter only when collusion costs increase.

Our results have important policy implications. In general, recent antitrust legislation such as leniency laws has been successful in combating product market collusion, but we find that firms switch from explicitly colluding to pursuing more horizontal mergers once the costs of price fixing rise. This has significant implications for the goals of antitrust policy. In fact, if the goal of such policy is to benefit the customers of the cartels, the merger reorganization that takes place after the passage of the laws makes the goal more difficult to achieve. In this respect, our findings suggest the need for closer integration between the merger review and horizontal restraint arms of antitrust authorities.

Indeed, the decision to pursue enforcement against cartel behavior is usually made by considering cartels' potentially negative effect on consumer welfare, as compared to consumer welfare under oligopolistic competition between the same number of formerly colluding firms. However, an accurate counterfactual must take into account how firms reorganize themselves after collusion costs increase.

The results on merge activity raise an interesting question: if a bigger scale is optimal, why didn't the cartel participants merge even before the leniency laws were passed? As colluding firms might deviate from the collusive agreement, a cartel cannot fully replicate a merged firm, but a merged firm might be able to achieve what a cartel does. Though fully addressing this issue is outside of the scope of this article, we offer four possible explanations.

First, managers may not be willing to give up the private benefits of control. Indeed, cartels allow members to achieve coordination and outcomes not dissimilar from those of a merged entity by *preserving the independence of the firm* and allowing the executives to run two independent firms. These private benefits could be lost if one firm is acquired. However, as sustaining the cartel becomes more difficult, it becomes more attractive to pursue a merger. Moreover, such private benefits of control could be greater in more profitable firms – i.e., the ones in which the "quiet life" is more appealing to managers (Bertrand and Mullainathan, 2003). Once profitability drops after the passage of leniency laws, it becomes less attractive to maintain private benefits of control and to resist mergers. If this were the case, government enforcement against cartels would actually induce firms to overcome the inefficiencies in principal-agent relationships in which CEOs have different objectives than the shareholders. These mergers could actually harm consumers even more than the original cartels do.

Alternatively, firms may seek the pretense of competitive markets. Indeed, customers may resist high prices if they do not discern the policing force of competition (Kumar, Marshall, Marx, and Samkharadze, 2015). In this case, a clandestine cartel may be able to take advantage of customer beliefs that competition is still in place, and firms could enjoy lower buyer resistance, especially in procurement contracts. In that case, the need for the appearance of competition in the eyes of their customers could induce firms to pursue collusion instead of mergers, even when mergers are more efficient.

A third possibility is that mergers may involve transaction costs (e.g., fixed integration costs) or create agency problems (e.g., empire building), and, on the margin, some firms may prefer collusion.

Finally, a likely reason could be directly related to antitrust enforcement. The firms might prefer illegal collusion to legal mergers if the mergers that they would consider in lieu of collusion would not pass the scrutiny of competition authorities (whereas all of the firms could potentially participate in covert collusion). Dominant firms that would not be allowed to merge might consider illegal collusion among

themselves as a preferred alternative to other choices they might face (e.g., acquiring smaller rivals), even after they take into account the possible fines resulting from such collusion.

All of these alternative explanations are in line with our general empirical findings that when collusion becomes less feasible, firms move to the next best alternative: mergers. Further research could analyze the reasons mergers do not precede stronger antitrust enforcement and whether tighter anti-collusion policy and merger control are (strategic) complements or substitutes.

This also implies that having a lax merger control policy could mitigate or completely wipe out the beneficial competition-inducing effects of otherwise effective leniency laws, suggesting that the two policies might need to go hand-in-hand. In fact, we find that leniency laws have a smaller effect on the margins of non-US firms, whereas the economic effect on merger activity is similar for US and non-US firms. One might conclude from these findings that leniency laws have been less effective in reducing gross margins outside of the US because these countries have weaker merger review enforcement.

We acknowledge the following limitations in our analysis. First, no new legislation is passed in a vacuum. Although we control for contemporaneous law passage and changing economic conditions, future research could further analyze the political economy of increasing cartel enforcement to shed light on any pre-existing trends that might affect such analysis. In addition, the Compustat sample includes only publicly listed firms and does not allow us observe the firms' product portfolios. Better data could clarify the channels through which leniency programs reshape the industrial structure of the economies. Finally, our analysis is limited to partial equilibrium adjustments. It would be interesting to see how leniency laws have affected entry and exit dynamics, capital and labor mix, and wage dispersion, especially for firms that conducted follow-up mergers. This might provide a better understanding of the general welfare implications of these laws.

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Table 1. Leniency programs

This table reports leniency program passage by country. Our primary source of information is Cartel Regulation 2013, published by Getting the Deal Through. We complement this dataset using press releases and news articles. We also report the number of unique firms from each country in our Compustat Global and North America sample, the number of firm-year observations from each country, and the number of detected cartels that are covered in Connor (2014).

Country	Year	Firms	Obs.	Cartels	Country	Year	Firms	Obs.	Cartels
Argentina	None	73	925	3	Lithuania	2008	34	305	3
Australia	2003	2,075	20,082	11	Luxembourg	2004	44	416	1
Austria	2006	132	1,472	5	Malaysia	2010	1,035	12,848	1
Belgium	2004	162	1,843	11	Mexico	2006	146	1,681	6
Brazil	2000	384	4,492	19	Netherlands	2002	244	2,884	24
Bulgaria	2003	17	126	4	New Zealand	2004	157	1,623	4
Canada	2000	3,701	28,176	22	Nigeria	None	55	500	0
Chile	2009	164	2,124	4	Norway	2005	340	3,063	6
China	2008	2,478	27,416	0	Oman	None	58	604	0
Colombia	2009	38	349	1	Pakistan	2007	205	2,232	13
Croatia	2010	30	255	0	Peru	2005	77	931	0
Cyprus	2011	34	275	2	Philippines	2009	174	2,154	0
Czech Republic	2001	34	274	6	Poland	2004	402	3,454	6
Denmark	2007	196	2,335	0	Portugal	2006	80	838	7
Ecuador	2011	2	17	0	Romania	2004	47	357	6
Estonia	2002	17	181	1	Russia	2007	191	1,647	1
Finland	2004	158	2,060	1	Singapore	2006	738	8,066	0
France	2001	1,026	11,219	35	Slovakia	2001	10	75	5
Germany	2000	1,012	11,508	42	Slovenia	2010	23	253	0
Greece	2006	234	2,223	3	South Africa	2004	370	3,857	39
Hong Kong	None	364	3,654	0	Spain	2008	188	2,293	28
Hungary	2003	32	306	28	Sweden	2002	554	5,694	11
Iceland	2005	10	80	1	Switzerland	2004	270	3,596	9
India	2009	2,113	24,173	8	Taiwan	2012	1,688	14,395	6
Indonesia	None	338	3,775	7	Thailand	None	496	5,723	0
Ireland	2001	108	1,209	1	Turkey	2009	157	1,538	8
Israel	2005	285	2,601	8	Ukraine	2012	6	38	0
Italy	2007	340	3,643	53	UK	1998	2,832	28,168	19
Japan	2005	3,877	53,694	10	USA	1993	22,498	213,914	179
Jordan	None	103	828	0	Venezuela	None	21	222	0
Korea	1997	1,475	8,701	50	Zambia	None	9	81	0
Latvia	2004	28	270	1					

Table 2. Summary statistics

This table reports the summary statistics for the main variables used in the subsequent analysis. The summary statistics are reported at the firm level, except for intensity of relationship, which is reported at the firm-pair level. Both the gross profitability margin and the merger activity (which is measured by the total dollar value of mergers and acquisitions over the year, scaled by lagged assets) are winsorized at 1%.

Variable	Source	N	Mean	Median	St. Dev.
Key outcome variables					
Gross margin	Compustat	473,369	0.257	0.325	0.980
Dollar value of mergers and acquisitions over lagged assets	SDC Platinum	543,737	0.014	0.000	0.079
Key firm-level control variables					
Assets (m)	Compustat	507,354	2,886.19	130.75	37,700.46
Leverage (debt over book equity)	Compustat	501,357	0.828	0.366	2.151
Key identification variables					
Leniency law	Table 1	543,737	0.614	1.000	0.487
Export market leniency laws	Table 1 and CEPII TradeProd	216,649	0.579	0.790	0.381
Heterogeneity variables					
Predicted cartel probability	Connor (2014) and Compustat	399,921	0.005	0.000	0.023
Recent cartel detection (past 3 years)	Connor (2014)	404,048	0.007	0.000	0.084
HHI	Compustat	387,130	0.304	0.178	0.306
Returns to scale	Compustat	402,984	0.998	1.000	0.091
Industry growth	Compustat	386,366	1.091	1.083	0.104
Trade volume	CEPII TradeProd	156,626	0.072	0.052	0.085
Intensity of relationship	OECD Input-Output tables	5,770,510	0.180	0.138	0.100

Table 3. Comparison of treated and control firms

This table compares the average gross margins, winsorized at the 1% level (Column (1)), and the average total dollar value of mergers and acquisitions over the year, scaled by lagged assets and winsorized at 1% (Column (2)), of the firms in the countries that passed leniency programs to the contemporaneous average gross margins and the average total dollar value of mergers and acquisitions over the year, scaled by lagged assets, of the firms in the countries that did not pass leniency programs.

	(1)	(2)
	Margins	**Mergers**
Treated firms	0.289	0.0125
Control firms	0.294	0.0131
Difference	-0.005	-0.0006
	(-1.000)	(-1.416)
N	25,775	29,069

Table 4. Detected cartels

This table reports the Poisson quasi-maximum likelihood regressions. All regressions include country fixed effects and time fixed effects. Standard errors are clustered at the country level. The dependent variable is the number of cartels detected in the country in a particular year.

Our main variable of interest is the leniency law dummy. Column (1) provides the baseline specification. Column (2) controls for the country's macroeconomic conditions. In Column (3), the dependent variable is the number of firms detected in cartel cases. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Leniency law	0.935***	0.869***	2.274***
	(2.987)	(3.383)	(3.565)
GDP per capita		1.102**	
		(2.069)	
Unemployment		-0.025	
		(-0.584)	
Imports as a % of GDP		0.031*	
		(1.947)	
Country f.e.	Y	Y	Y
Year f.e.	Y	Y	Y
R-squared			
N	1,449	837	1,449

* p<0.10, ** p<0.05, *** p<0.01

Table 5. Margins: Differences-in-differences

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the gross margin, winsorized at the 1% level. All regressions include firm fixed effects and time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is the leniency law dummy. In Column (1), we test its effect without any additional controls. In Column (2), we control for firm and country characteristics: assets, leverage, GDP per capita, unemployment, and imports as % of GDP. In Column (3), we control for industry (three-digit SIC)*year fixed effects. In Column (4), we control for country*three-year-interval fixed effects. In Column (5), we control for industry (two-digit SIC)*year fixed effects, country*three-year-interval fixed effects, and geographic region*year fixed effects. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	-0.062**	-0.051**	-0.050**	-0.025***	-0.013*
	(-2.218)	(-2.100)	(-2.381)	(-2.667)	(-1.673)
Controls	N	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	N	N
Industry*year f.e.	N	N	Y	N	Y
Country*three-year f..e.	N	N	N	Y	Y
Region*year f.e.	N	N	N	N	Y
R-squared	0.516	0.525	0.525	0.528	0.528
N	471,452	401,580	401,461	401,567	401,447

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 6. Margins: Matched firms

We create a matched sample. We find a control country with the closest GDP per capita to that of the treated firm's country. The control country must not have passed a leniency law by the time the treated country did and must not have done so in the next three years. For firms in the treated country, we match corresponding firms in the control country, operating in the same three-digit SIC industry. In the case of multiple matches, in Columns (1)-(2), we select the one that has the closest property, plant, and equipment (but within 100% difference) three years prior to the leniency law passage in the treated country. We limit the analysis to three years before and three years after the passage of the law in the treated firm's country.

This table reports the regressions, where the dependent variable is the gross margin, winsorized at the 1% level. Column (1) reports the results for the full sample, whereas Column (2) present the results for those cases where the predicted probability of being detected in the cartel case for the treated firm exceeds zero. We use a prediction model based on time-varying firm characteristics (asset size, leverage, and ROA) and country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. We fit the prediction model using only pre-leniency observations and predict the probability that the firm is detected in a cartel case in the year after the passage of the leniency law. In Column (3), in the case of multiple matches in the same industry between treated and control countries, we pick a control firm that has the closest gross margins (but within 100% difference) three years prior to the leniency law passage in the treated country.

Our main variable of interest is the leniency law dummy. All of the regressions include firm and matched-pair*year fixed effects. Standard errors are clustered at the country level. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Leniency law	-0.117***	-0.058**	-0.140***
	(-9.325)	(-2.178)	(-6.462)
Firm f.e.	Y	Y	Y
Matched pair*year f.e.	Y	Y	Y
R-squared	0.722	0.719	0.612
N	59,067	36,552	62,597

* p<0.10, ** p<0.05, *** p<0.01

Table 7. Margins: Identification based on foreign laws

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the gross margin winsorized at the 1% level. All of the regressions include firm fixed effects and time fixed effects. Standard errors are clustered at the country level.

Our main variables of interest are a continuous variable of laws passed in other countries weighted by three-digit SIC exports from the firm's country (Columns 1-3) and a continuous variable of laws passed in other countries, weighted by the presence of a firm's subsidiaries (Columns 4-6). In Columns (1) and (4), we test their effect without any additional controls. In Columns (2) and (5), we control for firm and country characteristics: assets, leverage, GDP per capita, unemployment, and imports as % of GDP. In Columns (3) and (6), we control for industry (three-digit SIC)*year fixed effects. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	-0.106**	-0.092***	-0.067***			
	(-2.195)	(-3.097)	(-2.697)			
Subsidiary-based leniency laws				-0.075***	-0.051**	-0.053**
				(-2.886)	(-2.297)	(-2.596)
Controls	N	Y	Y	N	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	Y	Y	N
Industry*year f.e.	N	N	Y	N	N	Y
R-squared	0.557	0.563	0.562	0.68	0.68	0.674
N	201,845	163,385	163,376	98,360	79,992	79,532

* p<0.10, ** p<0.05, *** p<0.01

Table 8. Margins: Heterogeneity

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the gross margin, winsorized at the 1% level. All of the regressions include firm fixed effects and industry (three-digit SIC)*year fixed effects as well as controls for firm and country characteristics: assets, leverage, GDP per capita, unemployment, and imports as % of GDP. Standard errors are clustered at the country level.

In Column (1), our main variable of interest is the interaction between the passage of leniency laws and the likelihood that the market is cartelized. We use a prediction model based on time-varying firm characteristics (asset size, leverage, and ROA) and country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. We fit the prediction model using only pre-leniency observations and predict the probability that the firm is detected in a cartel case in the year after the passage of the leniency law. In Column (2), we interact leniency law passage with the dummy if the firm was detected in the past 3 years, whereas in Column (3) we interact leniency law passage with the dummy if the firm was detected in the past 5 years. In Column (4), we interact leniency law passage with the Herfindahl-Hirschman index of the firm's three-digit SIC industry in its country in the year of leniency law passage, where the shares are estimated by the number of employees in each firm. In Column (5), we interact leniency law passage with the measure capturing returns to scale in the firm's industry. We measure the latter by estimating a two-factor Cobb-Douglas production function for each industry in year 1996, using all the Compustat Global and North America firms. We proxy for the firm's output by its sales, for the firm's labor by the number of its employees, and for the firm's capital by the firm's property, plant and equipment. We then add the coefficients for the proxies for labor and capital, and we define industries in which the sum of coefficients is higher than one as having increasing returns to scale, and those in which the sum of the coefficients is lower than one as having decreasing returns to scale. In Column (6), we interact leniency law passage with the total trade flows of the firm's three-digit SIC industry in its country in the year of leniency law passage. In Column (7), we interact leniency law passage with industry growth, defined as the industry median of sales growth in the firm's country.

We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Leniency law	-0.042*	-0.050**	-0.050**	-0.007	0.137	-0.045**	0.095
	(-1.787)	(-2.365)	(-2.360)	(-0.282)	(1.572)	(-2.333)	(1.557)
Leniency law x Predicted cartel probability	-0.821***						
	(-7.544)						
Leniency law x Recent cartel detection		-0.036**	-0.042***				
		(-2.523)	(-2.720)				
Leniency law x HHI				-0.181**			
				(-2.230)			
Leniency law x Returns to scale					-0.189**		
					(-2.263)		
Leniency law x Trade volume						0.234*	
						(1.952)	
Leniency law x Industry growth							-0.135**
							(-2.636)
Recent cartel detection		-0.001	-0.004				
		(-0.128)	(-0.303)				
Industry growth							0.094
							(1.631)
Controls	Y	Y	Y	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	Y	Y
Industry*year f.e.	Y	Y	Y	Y	Y	Y	Y
R-squared	0.538	0.525	0.525	0.525	0.525	0.561	0.53
N	354,271	401,461	401,461	384,644	400,411	155,655	383,819

* p<0.10, ** p<0.05, *** p<0.01

Table 9. Mergers: Differences-in-differences

We consider all the Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the total dollar value of mergers and acquisitions over the year, scaled by lagged assets and winsorized at 1%. All of the regressions include firm fixed effects and time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is the leniency law dummy. In Column (1), we test its effect without any additional controls. In Column (2), we control for firm and country characteristics: assets, leverage, GDP per capita, unemployment, and imports as % of GDP. In Column (3), we control for industry (three-digit SIC)*year fixed effects. In Column (4), we control for country*three-year-interval fixed effects. In Column (5), we control for industry (two-digit SIC)*year fixed effects, country*three-year-interval fixed effects, and geographic region*year fixed effects. We report t-statistics in parentheses.*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	0.007***	0.006***	0.006***	0.004*	0.006**
	(4.005)	(3.515)	(3.455)	(1.952)	(2.530)
Controls	N	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	N	N
Industry*year f.e.	N	N	Y	N	Y
Country*three-year f..e.	N	N	N	Y	Y
Region*year f.e.	N	N	N	N	Y
R-squared	0.112	0.122	0.124	0.124	0.127
N	541,940	432,447	432,330	432,433	432,316

* p<0.10, ** p<0.05, *** p<0.01

Table 10. Mergers: Matched firms

We create a matched sample. We find a control country with the closest GDP per capita to that of the treated firm's country. The control country must not have passed a leniency law by the time the treated country did and must not have done so in the next three years. For firms in the treated country, we match corresponding firms in the control country that operate in the same three-digit SIC industry. In the case of multiple matches, in Columns (1)-(2), we select the one that has the closest property, plant, and equipment (but within 100% difference) three years prior to the leniency law passage in the treated country. We limit the analysis to three years before and three years after the passage of the law in the treated firm's country.

This table reports the regressions, where the dependent variable is the total dollar value of mergers and acquisitions, scaled by lagged assets, winsorized at the 1% level. Column (1) reports the results for the full sample, whereas Column (2) presents the results for those cases where the predicted probability of being detected in a cartel case for the treated firm exceeds zero. We use a prediction model based on time-varying firm characteristics (asset size, leverage, and ROA) and country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. We fit the prediction model using only pre-leniency observations and predict the probability that the firm is detected in a cartel case in the year after the passage of the leniency law. In Column (3), in the case of multiple matches in the same industry between treated and control countries, we pick a control firm that has the closest gross margins (but within 100% difference) three years prior to the leniency law passage in the treated country.

Our main variable of interest is the leniency law dummy. All of the regressions include firm and matched-pair*year fixed effects. Standard errors are clustered at the country level. We report t-statistics in parentheses.*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Leniency law	0.009**	0.017***	0.009**
	(2.038)	(3.591)	(2.657)
Firm f.e.	Y	Y	Y
Matched pair*year f.e.	Y	Y	Y
R-squared	0.148	0.187	0.120
N	59,067	36,552	62,597

* p<0.10, ** p<0.05, *** p<0.01

Table 11. Mergers: Identification based on foreign laws

We consider all Compustat Global and North America firms over 1990-2012. This table reports OLS regressions, where the dependent variable is the total dollar value of mergers and acquisitions over the year, scaled by lagged assets and winsorized at 1%. All of the regressions include firm fixed effects and time fixed effects. Standard errors are clustered at the country*industry (three-digit SIC) level.

Our main variables of interest are a continuous variable of laws passed in other countries, weighted by SIC3 exports from a firm's country (Columns 1-3) and a continuous variable of laws passed in other countries, weighted by the presence of a firm's subsidiaries (Columns 4-6). In Columns (1) and (4), we test their effect without any additional controls. In Columns (2) and (5), we control for firm and country characteristics: assets, leverage, GDP per capita, unemployment, and imports as % of GDP. In Columns (3) and (6), we control for industry (two-digit SIC)*year fixed effects. We report t-statistics in parentheses.*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	0.008***	0.007***	0.007***			
	(4.308)	(4.755)	(4.263)			
Subsidiary-based leniency laws				0.007***	0.007***	0.006***
				(3.744)	(4.273)	(4.408)
Controls	N	Y	Y	N	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	Y	Y	N
Industry*year f.e.	N	N	Y	N	N	Y
R-squared	0.088	0.092	0.092	0.108	0.117	0.118
N	216,099	171,515	171,507	102,287	82,234	81,774

* p<0.10, ** p<0.05, *** p<0.01

Table 12. Customer firm reaction to merger announcements

This table reports regressions in which the dependent variable is the cumulative abnormal returns on the stock of a firm in the customer industry of the acquirer and target industry after the merger announcement estimated over a three-day [0,2] window, and winsorized at 1%, where the expected returns are estimated using domestic country and global market returns over a 180-day estimation period. Data for non-US firms for years 1998-2010 are used in this analysis. Only transactions where the acquirer and the target are in the same three-digit SIC industry are considered. Customer industries are defined according to Input-Output tables. Baseline regressions include deal fixed effects. In the baseline specifications, the standard errors are clustered at the three-digit SIC industry level of the customer industry. All regressions include controls for firm and country characteristics: assets, leverage, profitability, and investment.

Our main variables of interest are the interactions between the intensity of the relationship (i.e., the fraction of inputs that the customer sources from supplier industries) and the recent passage of a leniency law. In Columns (1)-(5), the leniency law dummy takes a value of one if a leniency law was passed in the past five years. Column (1) reports the baseline specifications without any controls. In Column (2), we control for firm and country characteristics. In Column (3), we cluster standard errors at the deal level. In Columns (4) and (5), we report the estimates of the pooled regression without deal fixed effects, but with year fixed effects. In Column (6), the leniency law dummy takes a value of one if a leniency law was passed in the past three years. In Column (7), the leniency law dummy takes a value of one if a leniency law was passed at any time in the past. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Recent leniency law * Intensity of relationship	-0.006***	-0.008***	-0.008***		-0.009***	-0.007**	-0.005
	(-4.409)	(-3.239)	(-3.916)		(-3.393)	(-2.498)	(-1.188)
Intensity of relationship	0.000	0.000	0.000	-0.003**	0.001	-0.002*	0.002
	(0.164)	(-0.309)	(-0.530)	(-2.200)	(0.409)	(-1.950)	(0.447)
Recent leniency law				0.002***	0.003***		
				(4.933)	(5.893)		
Controls	N	Y	Y	Y	Y	Y	Y
Deal f.e.	Y	Y	Y	N	N	Y	Y
Year f.e.	N	N	N	Y	Y	N	N
R-squared	0.049	0.061	0.061	0.014	0.014	0.061	0.061
N	5,770,510	2,607,544	2,607,544	2,607,544	2,607,544	2,607,544	2,607,544

* p<0.10, ** p<0.05, *** p<0.01

Table 13. Robustness tests

Panel A. Profitability

We consider all Compustat Global and North America firms over 1990-2012. This table reports OLS regressions, in which the dependent variable is the gross margin, winsorized at the 1% level. All of the regressions include firm fixed effects, industry (three-digit SIC)*time fixed effects, and controls for firm and country characteristics: assets, leverage, GDP per capita, unemployment, and imports as % of GDP. Standard errors are clustered at the country level.

Our main variable of interest is the leniency law dummy. In Column (1), we control for the introduction of a competition law. In Column (2), we control for a country's financial market development. In Column (3), we control for other ways in which anti-cartel legislation was strengthened. In Column (4), we control for the country's rule of law and regulatory effectiveness. In Column (5), we control for the takeover laws. In Column (6), we exclude US firms. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	-0.050**	-0.047**	-0.055***	-0.066***	-0.043**	-0.024***
	(-2.374)	(-2.355)	(-4.134)	(-3.131)	(-2.257)	(-2.807)
Competition law	-0.03					
	(-1.272)					
Chinn-Ito index		0.065				
		(0.728)				
Stock market capitalization to GDP		0.000				
		(1.332)				
Private credit to GDP		0.000				
		(0.462)				
Increase in penalties			0.079*			
			(1.989)			
Change in investigative powers			0.068**			
			(2.276)			
Change in cartel definitions			-0.039			
			(-0.961)			
Other cartel laws			0.014			
			(0.206)			
Regulatory quality				-0.140**		
				(-2.419)		
Rule of law				0.115		
				(1.372)		

(Table continued on the next page.)

Government effectiveness					0.188***	
					(2.675)	
Takeover laws						-0.162***
						(-3.362)
Controls	Y	Y	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	Y
Industry*year f.e.	Y	Y	Y	Y	Y	Y
R-squared	0.525	0.522	0.528	0.533	0.525	0.515
N	401,461	391,254	345,946	330,835	397,631	235,494

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Panel B. Merger activity

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the total dollar value of mergers and acquisitions over the year, scaled by lagged assets and winsorized at 1%. All of the regressions include firm fixed effects and time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is the leniency law dummy. In Column (1), we control for the introduction of competition law. In Column (2), we control for a country's financial market development. In Column (3), we control for other ways in which anti-cartel legislation was strengthened. In Column (4), we control for the country's rule of law and regulatory effectiveness. In Column (5), we control for the takeover laws. In Column (6), we exclude US firms. We report t-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.006***	0.005***	0.007***	0.006**	0.006**	0.005***
	(3.427)	(2.953)	(3.872)	(2.366)	(2.573)	(5.473)
Competition law	0.004*					
	(1.988)					
Chinn-Ito index		-0.008				
		(-1.040)				
Stock market capitalization to GDP		0.000***				
		(3.387)				
Private credit to GDP		-0.000**				
		(-2.200)				
Increase in penalties			-0.006***			
			(-3.820)			
Change in investigative powers			-0.002			
			(-1.376)			
Change in cartel definitions			-0.004*			
			(-1.927)			
Other cartel laws			0.010***			
			(4.242)			
Regulatory quality				0.003		
				(1.535)		
Rule of law				0.005		
				(1.061)		
Government effectiveness				-0.008		
				(-1.372)		

(Table continued on the next page.)

Takeover laws					0.005*	
					(1.893)	
Controls	Y	Y	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	Y
Industry*year f.e.	Y	Y	Y	Y	Y	Y
R-squared	0.124	0.125	0.126	0.139	0.128	0.131
N	432,330	420,816	369,348	351,714	357,194	259,327

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Figure 1. Margin trends around leniency laws

We plot the gross margins, winsorized at 1% and averaged at the industry (three-digit SIC) level, for the period from two years prior to two years after the leniency law passage. The treated group consists of the firms that were affected by leniency laws. The control sample consists of firms that did not face the introduction of a leniency law over the same period as the treated firm (i.e., firms that did not have a leniency law introduced in the two years before or two years after the introduction of a leniency law for the treated firm). The above plot presents the trends for treated and control firms separately whereas the below plot presents the difference between treated and control firms with 95% confidence intervals.
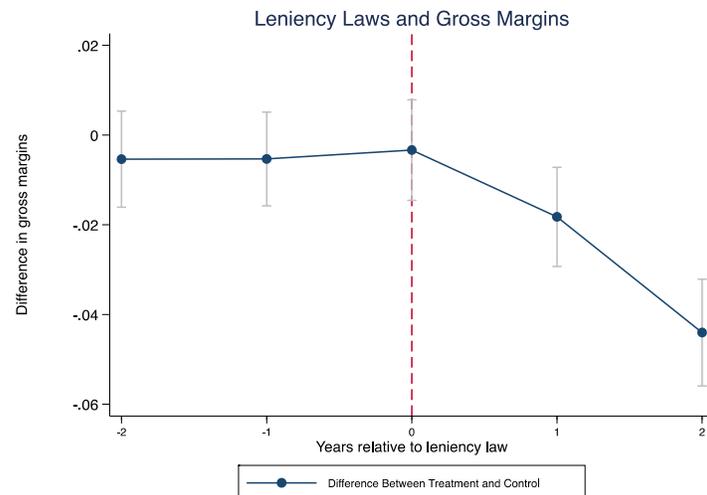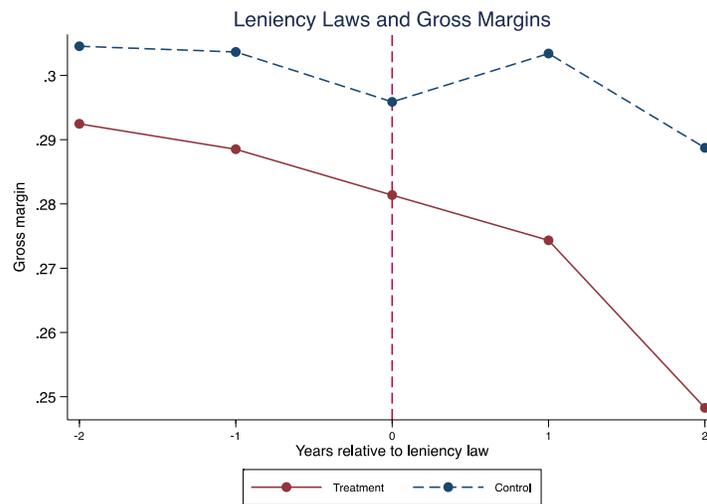
Figure 2. Merger trends around leniency laws

We plot the total dollar value of mergers and acquisitions over the year scaled by lagged assets, winsorized at 1%, and averaged at the industry (three-digit SIC) level, for the period from two years prior to two years after the leniency law passage. The treated group consists of the firms that were affected by leniency laws for the period from two years prior to two years after the leniency law passage. The control sample consists of firms that did not face the introduction of a leniency law over the same period as the treated firm (i.e., firms that did not have a leniency law introduced in the two years before or two years after the introduction of a leniency law for the treated firm). The above plot presents the trends for treated and control firms separately whereas the below plot presents the difference between treated and control firms with 95% confidence intervals.