

The Effects of Global Leniency Programs on Markups and Mergers

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Abstract

In a cross-country study, we investigate how the increasing costs of collusion affect merger activity by examining the staggered passage of national leniency laws from 1990-2012. These laws, giving amnesty to cartel conspirators that cooperate with antitrust authorities, lead to more cartel detections, and have generally increased the costs of collusion by reducing the average gross margins of the affected firms. We find that firms react to new restrictive regulations by engaging in more mergers. These mergers have negative effects for the downstream firms suggesting that mergers substitute cartel activity.

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Researchers, policy makers, and media have recently raised concerns about the 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 retaining 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, while stronger enforcement has been shown to lead to cartel deterrence and enhanced detection (Miller, 2009), the observations that product market competition has not intensified, and in fact may have instead weakened, raises questions about the effectiveness of antitrust enforcement and its potential unintended consequences.

One possible reason for such ineffectiveness could lie in the substitutability between cartels and the concentration through mergers in achieving the market power. We study this issue by focusing on the staggered passage of leniency programs around the world as proxies of strengthening antitrust enforcement against cartel activities. We focus on firms from 63 countries and territories over 1990-2012 using the data from Compustat Global and North America databases. Our empirical findings suggest that when firms face new regulatory constraints in cartel formation, they acquire other firms and these mergers have a negative effect on customer stock price reactions. While antitrust laws lead to fewer cartels, firms minimize the additional costs introduced by these laws by increasing merger activity. This mitigates the effect of cartel-busting legislation on competition. Given that breaking up cartels is justified in terms of encouraging competition and protecting consumer welfare, our findings suggest that the effects of such policy are reduced if the firms redraw their boundaries in response to regulatory actions.

¹ Grullon, Larkin, and Michaely (2016) find that 75% of US industries have become more concentrated over the last two decades. 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, among other suggestions, additional resources for cartel enforcement and tighter merger control.

Leniency programs have been among the most important developments in cartel detection and deterrence (Chen and Rey, 2013). By reducing the 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 firm markups of firms, proxied by gross profit margins, and their merger activity. In particular, we compare the change in markups and acquisition strategies of firms that are affected by the law to the contemporaneous change in markups and the strategies of the control group of firms that are headquartered in countries that have yet to pass such a law.

We focus on a sample of 54,189 firms over a 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 (defined in terms of industry's export share to or firm's own operations in that country) to the passage of laws in foreign countries.

Using all three identification strategies, we show that indeed the leniency laws have affected the markups of firms leading to a 14.8% drop with respect to the average sample gross margin before the leniency law passage. This suggests that by and large leniency programs have been effective tools in dissolving existing collusive arrangements or preventing the formation of the new ones. We further find that such restrictions on the ability to create a cartel increase the incentives to engage in mergers. The passage of a leniency law raises the annual total dollar value of mergers from 0.6% of the lagged total assets to 1.3%. 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 from the countries that have not yet passed the law by the

time 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 profitability. Since these firms are in the same industry, similar in size, and located in countries of similar states of economic development, in the absence of leniency laws they would have experienced similar changes in profitability and followed similar corporate policies. Controlling for matched-pair*year and firm fixed effects, the results indicate that passage of the law has a higher effect on the merger activity of firms headquartered in the countries that pass the law, when compared to their counterparts in countries that do not pass the law.

Our results are also robust to an alternative 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 leniency law passage in countries to which the firm's industry is exporting. Since 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 a potential issue that our firm-level observations are not equally spread across countries. The analysis based on these alternative and more exogenous measures of a firm's exposure to leniency laws delivers consistent results.

Further, we investigate the economic effects of such mergers. 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. This provides preliminary evidence of a direct beneficial impact of these mergers on a firm's cash flows.

Finally, we distinguish between efficiency and market power reasons for merging 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 stock price reactions following these deals for firms that are more likely to those that are less likely to be the downstream firms of these merging suppliers. This analysis has the benefit of allowing 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 merger announcements of supplier firms that follow the passage of leniency law. This suggests that potential customers lose from the mergers initiated in the wake of the passage of leniency laws. Thus at least to a certain extent these mergers act as a substitute to now harder to form explicit cartels, mitigating the effectiveness of the leniency programs.

I. Theoretical Background and Contribution to Literature

Leniency programs allow the courts and/or regulators to grant full or partial immunity to companies that have participated in illegal cartels, but cooperate in providing information about the cartel. The US was the first country to adopt such program in 1973. However, this program remained largely ineffective until 1993 when it was strengthened by making the case for 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, would be granted amnesty. The revised nature of the law proved to be 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 paper contributes to the literature regarding the effects of leniency programs (see Spagnolo and Marvão (2017) for an extensive recent summary of the literature). Theoretical literature, which started with the studies how collusion in a hierarchy can be prevented by leniency (e.g., Koffman and Lawarrère, 1996), has largely distinguished two countervailing forces. On the one hand, leniency laws destabilize cartels as they reduce a firm's costs of defection and potentially increase the costs of the rivals if the firm is able to provide evidence against the rivals to the antitrust authority which would impose fines on them (Motta and Polo, 2003; Ellis and Wilson, 2003; Aubert, Kovacic, and Rey, 2006; Harrington, 2008). These extra costs that the rivals are bearing would make the firm more competitive in the product markets. If all firms anticipate this, the cartels become less stable. On the other hand, *ex ante*, the costs of collusion could decrease if the firms expect to be the first ones to apply for leniency and thus they would pay lower than before fines and also impose extra costs on the rivals (e.g., the negative effects have been noticed 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 it depends on the size of fines and the other particulars of the leniency program (e.g., whether the leniency is only awarded to the first applicant).

Recent empirical literature has mainly determined 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, while Borrell, Jiménez, and García (2014) show an improved managerial perception of the competition in the countries that have passed leniency programs. We contribute by establishing a link between the passage of leniency programs and firm markups that capture the decrease in collusion activity in a more comprehensive way than the number of leniency applications and convictions. In addition, we are the first, to the best of our knowledge, to demonstrate how firms react

to leniency programs by changing their strategies. In doing this, 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 have been 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 that restricted cartels as well as Symeonidis (2002) who documents that cartel prohibition induced corporate restructuring in the UK. A few recent papers have systematically looked at the merger activity following cartel detections. For instance, Hüsichelrath and Smuda (2013) and Davies, Ormosi, and Graffenberger (2015) study EC cases and find that merger activity rises after cartel convictions, respectively, at the firm level and at the general worldwide industry level. Marx and Zhou (2015) also study EC cartel cases and find that the merger rates increase following the passage of the EC leniency law in 1996.

As compared to these studies, in our paper, we focus on a larger international sample and this allows us to provide the analysis for a more comprehensive set of firms. In addition, many cartel cases are detected during the merger negotiations by the due diligence teams of the merging parties, suggesting that cartel detection might be endogenous to the merger activity. This means that, for instance, an observed leniency application and the follow-up merger might not necessarily have unambiguous directional effects. Moreover, while detected cases could provide important insights, they could also be endogenous to unobservable factors affecting both cartel and merger prevalence. Such factors could be industry's expected profits, competitor or employee whistleblowing, and lobbying by consumer pressure groups. Thus, our identification strategy which relies on using 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 allows controlling for global or regional industry shocks occurring contemporaneous with the leniency program passage that are difficult to control for when a single jurisdiction is studied, e.g. in Miller (2009) or Marx and Zhou (2015).

Finally, by showing how a shock to upstream firms propagates to the stock prices of downstream firms, our results also contribute to the retrospective studies on whether mergers result in higher market power or the efficiency improvements (e.g., Ashenfelter and Hosken (2010); Ashenfelter, Hosken, and Weinberg (2013, 2014); or Kwoka (2014) for the 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.²

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 the Cartel Regulation 2013 published by Getting the Deal Through.³ We manually check this information and complement it with press releases and news articles. We report the years when leniency programs were passed in Table 1.

² We abstract from whether such effect comes from the unilateral anticompetitive effects from higher market power or from the coordinated effects (i.e., these transactions could further increase collusion by reducing the number of existing players, see e.g., Miller and Weinberg (2017)), as per the distinction in DoJ's and FTC's 1992 Horizontal Merger Guidelines.

³ In some cases, most notably the US, ineffective leniency laws were *strengthened* rather than *introduced* during this period. For the sake of brevity, throughout the paper 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 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, as opposed to some other cartel policy changes, leniency programs have never been reversed and could have been considered near-permanent, reducing 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.

We carefully read online discussions and press announcements to make sure that the passage of these programs is not confounded with one particular trend in terms of 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 the leniency programs after prominent collusion cases.⁴ Some other countries have instead passed leniency programs after significant pressures from the US, the EU, or supranational organizations (Lipsky, 2009).⁵ Moreover, the EU fosters the adoption of leniency programs by its member states and often seeks similar provisions in its bilateral associations and trade agreements, while 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 leniency program passage also varied. For instance, Taiwan passed the law in response to general concerns about rising consumer prices, while Korea passed it after the financial crisis.

In Table 2, 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, suggesting 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 growth, nor the unemployment rate are related to the passage of leniency laws, suggesting that worsening (or improving) economic conditions are unlikely to have been the main driver of leniency law passage.

⁴ US strengthened its leniency program after the vitamin cartel, Hungary did so after facing significant criticism concerning its competition investigation against mobile telephone operators, while Switzerland strengthened its competition law in 2003, and this included passing leniency provisions, 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 that reported its antitrust authority needed better investigative tools, including the ability to give leniency to whistleblowers revealing secret cartel conduct. Similarly, the US bargained for the strengthening of Singapore's antitrust laws in its negotiations for a bilateral free trade agreement. One may argue that free trade agreements might have a similar effect on market structure as cartel busting. We control for a country's levels of trade and find that this does not affect our results.

B. Data on Markups and Mergers

Our main response variables in this paper are markups, captured by accounting gross profit margins, and firm merger activity. The data on the gross profitability margins and other accounting information comes from the Compustat Global and North America datasets. We consider all of the publicly listed firms in the Compustat Global and North America datasets over the period of 1990-2012.⁶

The data on mergers comes 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. We report the number of firms from each country in our main dataset and number of firm-year observations that they contribute to our sample in Table 1 and the summary statistics in Table 3, Panel A.

We start by plotting 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 markups, measured as gross profitability margins, for firms that were affected by leniency laws as well as for a sample of control firms. As a control sample, we consider the firms that did not face the introduction of leniency laws over the same five-year period as the treated firms. The plot depicted in Figure 1 illustrates that while the pre-trends for the treatment and the control groups are similar, there is a drop in the gross margin of the treated firms.

⁶ While Compustat Global sample only includes publicly listed firms and so we do not observe the product portfolio of the firms, it has some advantages. First, it allows us to perform a wide-scale study spanning different industries and countries, providing insights about the global effects of leniency program passages. Second, our identification approach 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 condition in a single-country study. Third, public firm data allows us to look at the stock price reaction and to evaluate how the valuations of consumer firms have changed after the merger transactions that followed leniency program passage.

We do the same for our measure of merger activity, i.e. total dollar value of assets, scaled by lagged assets. The plot depicted in Figure 2 illustrates that while the pre-existing trends of the treatment and the control groups are similar, there is a temporary increase in the merger activity by the treated firms.

We provide the formal tests on the comparison between the affected firms and the control firms in Table 3, Panel B. We follow the same procedure as in Figures 1-2 and we compare the average gross profitability margins as well as average merger activity for firms that were affected by leniency laws as well as for a sample of control firms one year before the leniency program passage for the treated firms. We estimate averages for each country and industry. As a control sample, we consider the firms that did not face the introduction of leniency laws over the same five-year period as the treated firms. We do not find statistically significant differences.

C. Other Data

While in this paper we focus on the effect of leniency programs on the firm profitability and merger activity, we also provide evidence on the cartel detections. The data on international detected cartels comes 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 international⁷ cartels discovered, disclosed, and sanctioned by regulators around the world since January 1986. We use year 2012 edition which contains 746 cartels involving 7,496 firms (some firms are recidivists and, as such, are members of multiple cartels). We manually name-match the firms to Compustat and assign the affected industries their closest relevant SIC code. We report some descriptive statistics on these cartels in Table 3, Panel C. The median (mean) cartel involves five (ten) companies and

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

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).

Table 1 reports the number of the detected (convicted or suspected) cartels in each country in our sample, as well as the number of detected cartel cases. With a few exceptions, the larger economies have more firms and more firm-year observations in our sample and also more detected cartels. The correlation between the number of firms in the Compustat Global and North America dataset and the number of detected cartels is 86% suggesting that larger economies prosecute more collusive activities (likely, proportionally to the sizes of their economies). This correlation also provides a case for using firm-level specifications in our analysis that add more weight to larger economies which also have more cartels.

In addition, we follow the identification strategy where we look at the effects of leniency law passage in foreign countries. We proxy firm's exposure to each foreign country leniency law by the trade activity in the firm's industry between its home country and this foreign country. In order to capture the trade activity, we use export data from the CEPII TradeProd database that contains bilateral trade flows for more than 200 countries at the ISIC three-digit industry level from 1980-2006. We match them to the three-digit SIC codes. If multiple three-digit ISIC codes match to the three-digit SIC codes, we take the average of the respective values within the three-digit SIC. Since CEPII TradeProd database is only available at three-digit industry level, for consistency reasons we adopt this level of granularity throughout the whole paper. For the 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 subsidiary data in the Lexis Nexis Corporate Affiliations database that we manually name-match to Compustat. We report the summary statistics for these variables in Table 3, Panel D.

III. Leniency Law Effects on Cartel Detections and Markups

Miller (2009) has shown 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.

We first study the (observed) effect of the staggered passage of leniency laws on cartel detections at a 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 markups, 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 in the same industry but located in non-lenieny 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 Connor (2014) dataset. Since 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). In particular, 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 Poisson (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:

$$Detections_{kt} = \alpha + \beta Leniency Law_{kt} + X_{kt} + u_k + v_t + \varepsilon, \quad (1)$$

where k indexes countries, and t indexes years. $Detections_{kt}$ is the number of convicted and suspected cartels in the country k 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, while u_k and v_t are country and year fixed effects, respectively. We cluster standard errors at the country level.

We report the results in Table 4. In Column (1) of Panel A, 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 time-varying macroeconomic conditions in the country (Column 2). Column (3) reports a fixed effect OLS regression, while in Column (4), the dependent variable 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.

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

Next, we perform firm-level analysis and focus on firm markups, proxied by the 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 would not be detected by just looking at actual cartel detections. Indeed, only about 10%-30% of all cartel conspiracies are discovered (Connor, 2014), and it is likely that leniency laws could affect not only discovered but also the stability of the undiscovered cartels. In fact, it could be argued that the passage of leniency laws could lead to more cartels in general and/or greater cartel

⁸ The economic effect from the Poisson model is estimated to be $154\% = \exp(0.935) - 1$.

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 margins would capture these unobservable effects of the undetected cartels.⁹

In our estimation, we rely on the staggered nature of the passage of leniency programs to identify their causal effect on firm markups. We use the standard approach used in the literature that relies on a staggered passage of laws in different geographic regions like the business combination laws across the US (Bertrand and Mullainathan, 2003). This allows us to compare the change in markups of firms that were affected by the law to the contemporaneous change in markups of the control group of firms that were headquartered in the 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 estimate our baseline estimation:

$$Markup_{ikt} = \alpha + \beta Leniency Law_{kt} + X_{ikt} + u_i + v_{jt} + \varepsilon, \quad (2)$$

where i indexes firms headquartered in country k , k indexes countries, j indexes three-digit SIC industries, and t indexes years. $Markup_{ikt}$ is firm markups, proxied by the gross profitability margins. $Leniency Law_{kt}$ corresponds to the passage of a leniency law, X_{ikt} represents a vector of control variables, while u_i and v_{jt} are firm and industry-year fixed effects, respectively. Given that 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 different firms (and also implicitly for non-time varying differences between countries in which they are headquartered). For example, let us consider Germany passing a leniency law in 2000. We compare how

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

¹⁰ Although we consider the firm's headquarter country as the country where the firm is conducting most of its business activities, we realize that most firms have international operations and we address this potential concern by adopting an alternative identification strategy in Section III.C in which we explicitly take into account the geographic nature of firms' international operations.

average gross margins has changed for an average German firm as compared to average firms in other countries (e.g., Italian firms) which, after controlling for firm fixed effects, are assumed to be otherwise identical but were exposed to a leniency law on a different date (e.g., 2007 in the case of Italy). Also, firm-level analysis allows us control for any compositional effects, i.e. firm entry and exit. With this analysis we are capturing the effects on the firms that existing both before and after the leniency law passage.

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

We further control for geography specific time trends, given that the leniency program passage can be correlated with the general economic cycles. Since our identification comes from the country*year variation, we are not able to include country-specific time trends on an annual basis as that would be collinear with our leniency program variable. Instead, we construct time buckets at the three-year

¹¹ We also perform the same estimation separately for every SIC3 industry. One concern could be that the negative effect is 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.

intervals, starting with year 1990 (i.e. the first bucket spans years 1990-1992, the second spans years 1993-1995, etc.). In this manner, 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 drops twice to 7.2% compared to the pre-leniency law average but remains statistically significant.

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. After simultaneously controlling for region, country and industry trends, we find that the economic effect is 3.8% drop in markups, compared to pre-leniency law passage.¹²

In Column (6), we report the results where we add treatment-time fixed effects that are common for both treated and control firms in our analysis. In particular, we implement matched difference-in-differences estimator as in Gormley and Matsa (2011, 2016).¹³ For each year when leniency program has been adopted in at least one country, we create the sample of treated firms if they are headquartered in the country that adopted the leniency program in that year. We then create the control sample from the firms that are not headquartered in these countries. For each of such cohort samples, we analyze firm-year observations in the five years before and the five years after the law is passed. The control sample of firms is restricted to the countries that do not pass the law during this time period. For instance, the year

¹² We provide additional robustness tests in Section VII where we control for contemporaneous economic policy changes. In the Internet Appendix, we also provide additional robustness to the estimation of standard errors, i.e. by clustering them by industry*country; double-clustering by country and year; double-clustering by industry and year; or double-clustering by country and industry. Our results are also robust if we exclude observations with Cook's D statistic greater than $4/N$, thus it is unlikely that our estimates are driven by outliers.

¹³ We refer to Gormley and Matsa (2016) for the discussion on the similarities between differences-in-differences methodology in our equation (2) and matching differences-and-differences estimator in our equation (3). We are unable to directly control for treatment-time fixed effects in our baseline estimation (2) as in our setup some control firms get eventually treated over time.

1997 cohort includes Korean firms as treated sample and firms from the countries that do not pass the law between years 1992-2002 as the control sample. We pool these samples that are created for different years and estimate the average effect using the regression:

$$Markup_{ickt} = \alpha + \beta Leniency Law_{kt} + X_{ickt} + u_{ic} + v_{tjc} + \varepsilon, \quad (3)$$

where as before i indexes firms headquartered in country k , k indexes countries, j indexes three-digit SIC industries, c indexes cohorts, and t indexes years. Instead of firm and industry-year fixed effects as before, we now include u_{ic} and v_{tjc} which correspond to firm-cohort and industry-year-cohort fixed effects. Effectively, industry-year-cohort fixed effects can then be interpreted as treatment-time fixed effects, estimated separately by each industry, since within each cohort, where treatment year is fixed, calendar years are equivalent to years relative to the treatment year. We report the results using this estimation in Column (6).

Overall, these results show that leniency laws have reduced profitability of the affected firms, suggesting the effectiveness of the global passage of leniency laws.

C. Effects on Markups: 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 pass the laws earlier – might be different from the control group of countries – those that pass the laws later. For instance, more developed countries might pass the laws earlier, as we see from the Table 2. In addition, the industrial composition of the two sets of countries might differ. We address these concerns based on a matched sample technique. The goal is to assess the impact of the passage of leniency programs on firms that are otherwise similar but differ in terms of when they face the passage of the law.

More specifically, we match firms to their peers headquartered in countries that had not passed 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 gross margins three years prior to leniency law passage. We proceed as follows. 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 digit industry. In the case of multiple matches, we focus on firms that have the closest gross margin three years prior to the passage of the law. We discard any match where gross margins differ 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 since these firms are in the same industry, similar in size, and located in countries of 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., some technological shocks that could

have similarly affected these firms). It could be argued that industrial changes could have changed incentives to collude, while heightened collusion have led to the enactments 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. As such, their reaction functions to any industrial shocks would be similar.

We provide the results in Table 5, Panel B. In Column (1), we find that the passage of a law has a higher negative effect on markups for those firms headquartered in the countries that pass a leniency law than on the markups of their counterparts in countries that do not pass this law. In Column (2), we focus on those cases where a firm has 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), country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. We find consistent results when looking at these cases in which cartel incidence is more likely. These results confirm the previous ones and suggest that even when we examine similar firms, we find consistent results.

D. Effects on Markups: 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 tend to be international (Ghosal and Sokol, 2018). The firm that colludes with other firms in the 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 since

antitrust authorities coordinate their actions in prosecuting similar cartels.¹⁴ Even if international antitrust authorities do not share information as per leniency applicant's request, the information on the actual decision by 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 leniency program passage in foreign countries.

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

¹⁴ DoJ's Deputy Assistant Attorney General Brent Snyder was quoted saying that 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" and "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).

¹⁵ It is unlikely that powerful firms in the US and other western countries were successful in 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 (e.g., US-Singapore FTA).

Our Export Market Leniency Laws measure considers this explicitly by assuming that the export share to a specific country of the industry in which the firm operates is a good proxy for its exposure to that country. 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} has passed a leniency program by year t , and zero otherwise. To avoid any endogeneity of the industry structure, we remove the time variation and 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 has passed a law to one when all the foreign countries with exports from the firm's industry have passed leniency programs.

Our first alternative specification is then as follows:

$$\text{Markup}_{ijkt} = \alpha + \beta(\text{Export Market Leniency Law})_{jkt} + \delta X_{ikt} + u_i + v_{jt} + \varepsilon. \quad (4)$$

Unlike in equations (1)-(3), in equation (4), 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 this industry exports adopt leniency program.

The second way to measure firm's exposure relies even more directly on the international nature of firm operations. For a subset of firms, we collect data on their actual international subsidiaries. This allows us to test whether the passage of laws in other countries in which they operate also has a significant effect. More 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 would have been more affected than firm B.

We call this exposure variable “Subsidiary Based Leniency Laws”. 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:

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

where \hat{k} denotes any country other than country k , i denotes a firm, and t denotes 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} has passed a leniency law by year t , and zero otherwise. The variable ranges from zero when leniency laws are not passed in none of the foreign countries in which the firm has subsidiaries to one when all of the foreign countries in which the firm has subsidiaries have passed leniency laws.

Our second alternative specification is then as follows:

$$\text{Markup}_{ikjt} = \alpha + \beta(\text{Subsidiary Based Leniency Law})_{ikt} + \delta X_{ikt} + u_i + v_{jt} + \varepsilon. \quad (5)$$

We report the findings in Table 5, Panel C. The first three columns present the results in which for our identification we rely on the passages of laws in the countries where the firm's industry is exporting (Export Market Leniency Law). In Column (1), we find that the passage of laws in these other countries

has a significant negative effect on profitability. These results are robust after 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. In particular, within a three-digit SIC industry, 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 particular year.

In the next three columns of the panels in Table 5, Panel C, we present the results in which we rely on the passage of laws in the other countries where the firm has subsidiaries for our identification.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 controlling for firm and country characteristics (Column (5)) as well as for three-digit SIC industry*year fixed effects (Column (6)).

E. Effect on Markups: 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 law is likely to primarily affect those firms that are engaged in collusion, or have the potential to form cartels in the future. We conduct a set of tests of heterogeneity of the effect and report them in Table 5, Panel D.

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

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), 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. We report the results in Column (1) 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 the cartel case experience a larger drop in markups.

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 3 years, while in Column (3) we interact leniency law passage with the dummy if the firm was detected in the past 5 years. We find that the markups drop more for the firms that were recently detected in the cartel case.

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 a particular year. For each industry and country, we estimate HHI based on the sales of firms in that industry and country as reported in Compustat Global and North America datasets. The firms in the concentrated industries with fewer symmetric players might find it easier to collude (e.g., see discussion in Motta (2004) on this being the most important cross-sectional factor empirically predicting collusion as well the experimental evidence in Huck, Normann, and Oeschssler (2004)). We then interact leniency law passages with HHI. We find that our results on markups are stronger for more concentrated industries.

Next, we recognize that the measures of Herfindahl-Hirschman index 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 using international data. However, the industries are likely to be more oligopolistic if they exhibit increasing returns to scale of the industry. We thus classify the firms on the basis of whether the industry in which they operate was experiencing increasing or decreasing returns to scale in year 1996. We measure the latter 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 firms' property, plants and equipment. We then add the coefficients for the proxies for labor and capital, and define those 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 (5), we show that the results are strongest in the industries that were experiencing increasing returns to scale.¹⁷

Finally, we look at recent industry growth. A high growth industry will in general be 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 if compared with the gain from cheating today. Second, if the recent high growth is associated with a (temporary) upturn in a cyclical industry – in this case, also, the gain from deviation today would outweigh the loss from punishment in the future and collusion could be more difficult to sustain (Rotemberg and Saloner, 1986). We use industry median of sales growth in firm's country as the proxy for the maturity of an industry. As

¹⁷ Our results are consistent if we instead adopt semi-parametric methodology of Olley and Pakes (1996). In addition, Olley and Pakes (1996) approach controls for the selection problem that 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 the observable firm level productivity using its investment decisions – i.e., the changes in the property, plants and equipment and intangible assets.

shown in Column (6), we find that the effect on markups is higher when interacted with this cross-sectional characteristic.

IV. Leniency Law Effects 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 the main growth-related corporate policy: merger activities. We analyze this using the SDC Platinum database that we merge with the Compustat Global and North America datasets for the 1990-2012 period.¹⁸

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:

$$Mergers_{ijkt} = \alpha + \beta Leniency Law_{kt} + X_{ikt} + u_i + v_{jt} + \varepsilon, \quad (6)$$

where i indexes firms headquartered in country k , k indexes countries, and t indexes years. $Mergers_{ikt}$ is measured by the total dollar value of mergers and acquisitions over the year, scaled by lagged assets.¹⁹ $Leniency Law_{kt}$ is our treatment of the passage of a leniency law, X_{ikt} represents a vector of control variables, while u_i and v_{jt} are firm and industry-year fixed effects, respectively. We cluster standard errors at the country level.

¹⁸ In this Section we follow the specifications described in Section III and thus for the sake of brevity we refer the reader to Section III for the details in the methodological choices.

¹⁹ In 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 only capture horizontal mergers, i.e. those mergers that involve the firms in the same industry and the same country.

We report the results in Table 6. Column (1) of Panel A 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%. 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 the most saturated model with industry*year fixed effects, country*three-year interval fixed effects, and region*year fixed effects (Column (5)). In Column (6), we control for treatment-time fixed effects using matched difference-in-differences estimator as in Gormley and Matsa (2011, 2016), described in Section III.B.

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 6, Panel B, 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 those firms headquartered in the countries that pass a leniency law than on the merger activity of their counterparts in countries that do not pass this law. In particular, we find that the passage of a leniency law increases the dollar value of merger by 0.9 percentage points.

In Column (2), we focus on those cases where a firm has 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), country characteristics (GDP and unemployment), as well as country fixed effects and three-digit SIC fixed effects. The results within this sample in which cartel incidence is more likely are consistent

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 firm’s merger activity:

$$Mergers_{ijkt} = \alpha + \beta(\text{Export Market Leniency Law})_{jkt} + \delta X_{ikt} + u_i + v_{jt} + \varepsilon. \quad (7)$$

In Table 6, Panel C, we report that foreign leniency program passages have led to higher merger activity (Columns (1)-(3)). These results also hold if we estimate an alternative specification based on where firm subsidiaries are located as we report in Columns (4)-(6):

$$Mergers_{ijkt} = \alpha + \beta(\text{Subsidiary Based Leniency Law})_{ikt} + \delta X_{ikt} + u_i + v_{jt} + \varepsilon \quad (8).$$

Jointly taken, the findings in this Section outline firm preferences consistent with a “pecking order” in its choice of the organizational form. Firms prefer to collude, but when collusion becomes more difficult, they pursue merger activities.²⁰

V. Economic Effects of Leniency Law-Induced Mergers

In this section, we show a link between mergers and profits. That is, we show that it is the same set of firms that experience a drop in profitability that also pursue a merger transaction and this merger has a positive effect on future profitability.

Our findings thus far have indicated that leniency laws lead to higher number of cartel detections (Table 4), lower markups (Table 5), and induce firms to pursue more mergers (Table 6). However, we have not determined whether firms that experienced a drop in profitability and pursued mergers enjoy better performance in the future than a firm that experienced a drop in profitability, but did not pursue mergers. It could be that the set of firms that are affected by a drop in profitability due to leniency laws that pursue mergers are different from those firms that do not pursue mergers. Demonstrating that these

²⁰ Since 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 the cartel case. It could be that firms expected that the antitrust authorities’ concerns about these mergers would just be too strong.

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. In particular, for each three-digit SIC industry, we separately estimate our baseline difference-in-difference specifications, reported in Table 5, Panel A, Column (2), as well as in Table 6, Panel A, Column (2). We obtain estimates of the leniency law effect for 282 industries for both gross margins and mergers. We find that for 137 of these industries, the effect on gross margins was negative and the effect on merger activity was positive, suggesting that in almost half of the industries these effects occur simultaneously.²¹

We further examine whether firms that are affected by leniency laws and subsequently pursue mergers are in fact able to improve their profitability, thus negating the negative effect of the leniency laws. We illustrate the effect graphically by identifying firms that pursue merger activities within the first two years following leniency law passage. For each of these firms, we then find one control firm that is in the same country and three-digit SIC industry, and closest in terms of profitability two years before the leniency law passage. We assume that these firms are exposed to leniency laws in similar ways, but pursue different strategies: some acquire other firms, while others do not. We present the differences in Figure 3. While this figure does not establish causality, it displays a clear correlation that firms that pursue mergers, following leniency laws, experience better outcomes in product markets than those that do not pursue mergers.

Moreover, we recognize that firms that pursue mergers could be different in nature than those that do not thereby creating a self-selection bias (Li and Prabhala (2010); Kwoka (2014)). In Figure 4, we limit the control group of firms to those that announce mergers in the first two years within leniency law

²¹ In the other cases, profitability decreased and merger activity decreased in 61 industries, profitability increased and merger activity increased in 56 industries, and profitability increased and merger activity dropped in 28 industries.

passage, but the mergers have failed for some reason (similarly to Savor and Lu (2009) and Seru (2014)). Both the treatment and control firms attempt to engage in mergers, but only the treatment firms were successful. 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.

VI. Drivers of Leniency Law-Induced M&As

Finally, we explore the drivers of leniency-law induced mergers by focusing on the customer reaction to merger deals that follow leniency law passages. For instance, the laws that make collusion more difficult reduce prices and markups in the industry requiring firms to 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 on the market power. In the first case, customers are indifferent or may even benefit if some of the cost savings are passed through. In the second case, customers are made 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 led 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 are passed onto the customers). If, on the contrary, these merger transactions were led by the desire to preserve market power, then the stock prices of customers would respond more negatively.²²

²² As suggested by Eckbo (1983), “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.” We only focus on the reaction to customers as the predictions on suppliers are less straightforward: some of the efficiency improvements might involve optimizing supply chains with potentially negative effects on some suppliers. Such mergers might also have effects on rivals (e.g., Davies, Ormosi, and Graffenberger, 2015), however, a positive effect on the rival stock prices might not only indicate the concentration effect but could also capture the increase in the expected probability of them being taken over (Eckbo, 1983).

We identify the supplier-customer relationships from the OECD Input-Output tables of year 2002.²³ We then examine the cumulative abnormal returns, defined as before, over a three-day [0,2] window to determine whether they differ based on whether a leniency law was passed in the country in the last five years. Given that horizontal mergers are those that are also most likely to have a negative effect on consumer welfare and in order to make the analysis more tractable, in this set of analysis, we only consider horizontal mergers (i.e., those cases where both the acquirer and the target are in the same three-digit SIC industry and country).

We expect, *ceteris paribus*, the abnormal returns to be more negative for customers who would be more negatively affected if their suppliers merge as compared to non-customers. We measure this differential effect by the fraction of inputs that the firm's OECD industry category gets from 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, with our method, we are able to 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.

As we are specifically interested in changing costs of collusion, we dig further and examine whether this differential reaction between customers and non-customers differs in the case where suppliers are

²³ 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.

exposed to a 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 but also 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 strong customer links with those 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 type of deals could have changed after leniency law passage and the changed composition of the deals could explain the differences in customer stock reaction. Our methodology with deal fixed effects allows us control for these possible changes in observed and unobserved deal characteristics, assuming that overall stock price reaction function to deal announcements has not changed after the passage of leniency laws and remained linear in customer industries' share of supplies.

We report the results in Table 7. We winsorize the abnormal returns at the 1% level, and cluster the standard errors at the customer's three-digit SIC level. In the baseline specifications in Column (1), we find that while the degree of industrial connectedness (i.e., industry links) between the supplier and the customer industries affects customer abnormal returns negatively, the effect is even more negative if the merging firms were recently exposed to a leniency law passage. 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 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 instead. In Column (6), we examine an even more recent passage of leniency law, i.e. if the law has been passed within the past three years. In all of these different specifications the main result holds. The only case in which 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 suggesting that only the *recent* passage of leniency laws that sparks merger activity in the upstream industry has a differential effect on customer stock price reaction.

It is worth noting that while these mergers seem to be harmful for consumers, it does not necessarily mean that firms merge for the purpose of replacing cartels. It could well be that as collusion costs rise, firms merge to reduce the asymmetry in size between the remaining market participants, which could help to facilitate collusion in the future (Bos and Harrington, 2010). Merger activity could not substitute but could in fact complement cartel activity in the future. Either way, this is consistent with our thesis that antitrust legislation against cartels fosters merger activity that harms consumers.

VII. Robustness to Confounding Policies and Economic Trends

One common concern in such type of studies that rely on evaluating policy response using the data from different countries is that the passage of the laws that are being evaluated is correlated with other type of legislation or broader economic conditions in general. While it is hardly possible to control for *all* other laws, in this section we aim to provide robustness of our finding by controlling for the most likely confounding legislation. We perform the same specifications for markup analysis as in Table 5, Panel A,

Column (3), and merger analysis as in Table 6, Panel A, Column (3), and report the results in Table 8, Panels A and B, respectively. The effect of leniency program 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 over our study period. As expected, we find that the first competition law also positively affects merger activity, but its impact does not render leniency laws insignificant.

In Column (2), we control for the degree of financial development of the country. Indeed, it could be that at the same time as the country passes the leniency law, it also adopts policies at the same time 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 the merger wave 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 as well as the 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 recognize that other changes in antitrust law occurred during our period of study. In particular, there has been a significant increase in penalties, changes in what constitutes cartel conduct as well as increase in investigative powers of antitrust authorities. We remind that we focus on leniency laws is that the passage of a leniency law is a clearly identifiable and measurable event, while the other provisions could occur multiple times in each country (e.g., change in penalties²⁴) or could have unclear effects on cartel conduct (e.g., changes in violation provisions). Even if there were correlated changes in anti-cartel

²⁴ The size of the measurable changes such as changes in penalties could have been anticipated and a binary treatment could overshoot or undershoot the impact, depending upon the market's expectations (Hennessy and Strebulaev, 2015).

provisions, our identification based on leniency laws would then be a proxy for a general strengthening of the anti-cartel provisions and 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 are able to 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, giving confidence that the passage of leniency laws has been a crucial measure in fighting cartels. Interestingly, an increase in penalties reduces merger activity.

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

Finally, in Column (5), we control for the changes of takeover legislation that might have coincided with the anti-cartel enforcement legislation if the countries have been changing different policies at the same time. We draw the list of merger legislation changes from Lel and Miller (2015) and we control it in our estimations. Neither the leniency law effect on markups, nor more importantly on merger activity is affected by this confounding legislation.

VIII. Discussion and Conclusion

Stigler (1950) suggested that one reason why mergers are preferred to cartels is the illegality of the latter. In line with this, in this paper we provide evidence that when antitrust authorities have better tools to

prosecute cartels, firms switch to mergers, thus mitigating the effectiveness of the antitrust policies. 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 affecting firms. We show that leniency programs were effective, reducing average markups, of firms headquartered in the leniency program passing countries or trading with them. Moreover, following the passage of leniency programs, which increase the costs of collusion stability, firms reorganize their activities by pursuing more merger transactions. These mergers generate a more negative stock price reaction for customers around their announcements, as compared to other merger transactions. With these findings, we demonstrate that sometimes firms prefer weaker integration in the form of cartels to stronger integration through merging, and resort to the latter only when collusion costs increase.

Our results have important policy implications. While, in general, recent antitrust legislation, such as leniency laws, has been successful in combating product market collusion, 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 the potential loss to consumer welfare arising from the cartels when compared to their welfare arising from the oligopolistic competition between the same numbers of formerly colluding firms. However, the correct counterfactual must take into account how firms would reorganize themselves once collusion costs increase.

Our results raise an interesting question: if a bigger scale is optimal, why had the cartel participants not merged even before the leniency laws were passed? Due to the risk of deviation of the colluding firms, a cartel could not fully replicate the merged firm, while the latter might be able to achieve what a cartel does. While addressing this issue is outside of the scope of this current paper, we offer three alternative justifications.

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 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 for 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 the principal-agent relationships, arising from CEOs who 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 could resist high prices if they do not discern the policing force of the 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 face reduced buyer resistance, especially in procurement contracts. Then, the need for the appearance of competition in 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 would prefer collusion.

All of these alternative explanations are in line with our general empirical findings that when collusion becomes less feasible, firms could move to the next best alternative, i.e. the mergers. Further research could analyze the reasons why mergers do not precede the stronger antitrust enforcement.

Moreover, while we control for contemporaneous law passage and changing economic conditions, any new legislation is not passed in vacuum and thus further research could analyze the political economy of increasing cartel enforcement which could allow understanding any possible pre-existing trends that might affect such analysis. In addition, Compustat sample only includes publicly listed firms and does not allow us observe the product portfolio of the firms. Better data could clarify the channels how leniency programs reshape the industrial structure of the economies. Finally, our analysis is limited to the partial equilibrium adjustments. It would be interesting to see how leniency laws have affected entry and exit dynamics into the affected industries, capital and labor mix and wage dispersion, especially for those firms that conducted follow-up mergers, and whether they lead to general welfare improvements.

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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 year of leniency program adoption, 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. Predicting leniency laws

This table reports the coefficients from the Cox proportional hazards model, estimated at the country level over the 1990-2012 period. The hazard is the passage of leniency laws. Column (1) uses macro-economic variables and region dummies. Column (2) also includes the political orientation of the government. Column (3) includes two measures of financial development. We report t-statistics in the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Log GDP	0.647*** (2.657)	0.794*** (4.079)	0.734*** (2.704)
GDP growth	1.174 (0.897)	1.528 (0.784)	0.871 (0.514)
Unemployment rate	0.038 (0.801)	0.025 (0.447)	-0.007 (-0.144)
Exports as % of GDP	-0.004 (-0.927)	-0.002 (-0.458)	-0.007 (-1.348)
Latin America	-0.978 (-1.035)	-0.837 (-0.755)	-2.291* (-1.742)
Western Europe	-0.612 (-0.618)	-0.743 (-0.648)	-1.904 (-1.586)
Central and Eastern Europe	-0.199 (-0.238)	-0.456 (-0.523)	-1.718 (-1.485)
North America	2.338* (1.890)	1.778 (1.261)	0.954 (0.704)
Asia	-0.696 (-0.767)	0.175 (0.166)	-1.836* (-1.787)
Oceania	0.134 (0.123)	-0.037 (-0.026)	-1.302 (-1.025)
Right-wing government party		0.550 (0.801)	
Right-wing chief executive		-1.609*** (-2.892)	
Private credit as % of GDP			-0.007 (-1.630)
Chinn-Ito index			0.102 (0.553)
N	823	620	728

Table 3. 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 (Panel A and D) or cartel (Panel C) level. Panel B compares the average gross margin and the average merger activity of the firms in the countries that passed leniency programs to the contemporaneous gross profitability of the firms in the countries that did not pass leniency programs. 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%. For this table we report t-statistics in the brackets.

Panel A. Firm variables

	Source	N	Mean	Median	St. Dev.
Assets (m)	Compustat	507,354	2,886.19	130.75	37,700.46
Gross margin	Compustat	473,369	0.26	0.33	0.98
Leverage (debt over book equity)	Compustat	501,357	0.83	0.37	2.15
Capital expenditures + mergers	Compustat	397,697	0.13	0.05	0.32
Dollar value of mergers and acquisitions over lagged assets	SDC Platinum	543,737	0.014	0.00	0.079

Panel B. Treated vs control firms

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

Panel C. Detected cartels

	N	Mean	Median	St. Dev.
Number of participants per cartel	746	10.04	5	30.55
Dollar value of cartel affected commerce (m)	526	26,752.24	1967	139,174.60
Market share of detected cartel participants	292	0.87	0.93	0.16
Cartel length (year)	616	7.39	5	9.17

Panel D. Additional constructed variables

	N	Mean	Median	St. Dev.
Export market leniency laws	216,649	0.579	0.790	0.381
Cartel probability	496,018	0.004	0.000	0.021
Intensity of relationship	5,770,510	0.180	0.138	0.100

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 baseline specification. Column (2) controls for the country's macroeconomic conditions. Column (3) reports the OLS estimates. In Column (4), the dependent variable is the number of firms detected in cartel cases. We report t-statistics in the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Leniency law	0.935*** (2.987)	0.869*** (3.383)	0.635*** (3.755)	2.274*** (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			0.475	
N	1,449	837	1,449	1,449

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

Table 5. Markups

Panel A. 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. In Column (6), we provide estimates based on matched difference-in-differences. We report t-statistics in the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	-0.062** (-2.218)	-0.051** (-2.100)	-0.050** (-2.381)	-0.025*** (-2.667)	-0.013* (-1.673)	-0.077*** (-4.516)
Controls	N	Y	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	N
Year f.e.	Y	Y	N	N	N	N
Industry*year f.e.	N	N	Y	N	Y	N
Country*three-year f.e.	N	N	N	Y	Y	N
Region*year f.e.	N	N	N	N	Y	N
Cohort*firm f.e.	N	N	N	N	N	Y
Cohort*industry*year f.e.	N	N	N	N	N	Y
R-squared	0.516	0.525	0.525	0.528	0.528	0.616
N	471,452	401,580	401,461	401,567	401,447	1,815,734

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

Panel B. 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 digit industry and, in the case of multiple matches, we select the one that has the closest gross margin (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, while 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), country characteristics (GDP and unemployment), as well 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.

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 the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Leniency law	-0.140***	-0.126***
	(-6.462)	(-3.22)
Firm f.e.	Y	Y
Matched pair*year f.e.	Y	Y
R-squared	0.612	0.620
N	62,597	43,674

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

Panel C. 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 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 the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	-0.106** (-2.195)	-0.092*** (-3.097)	-0.067*** (-2.697)			
Subsidiary-based leniency laws				-0.075*** (-2.886)	-0.051** (-2.297)	-0.053** (-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

Panel D. 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), country characteristics (GDP and unemployment), as well country fixed effects and the three-digit SIC fixed effects. We fit the prediction model using only pre-lenieny 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, while 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 industry in its country in a particular year. 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 firms' property, plants and equipment. We then add the coefficients for the proxies for labor and capital, and define those 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 industry growth, defined as industry median of sales growth in firm's country.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	-0.042*	-0.050**	-0.050**	0.013	0.137	0.095
	(-1.787)	(-2.365)	(-2.360)	(0.497)	(1.572)	(1.557)
Leniency law x Cartel probability	-0.821***					
	(-7.544)					
Leniency law x Recent cartel detection		-0.036**	-0.042***			
		(-2.523)	(-2.720)			
Leniency law x HHI				-0.139***		
				(-4.246)		
Leniency law x Returns to scale					-0.189**	
					(-2.263)	
Leniency law x Industry growth						-0.135**
						(-2.636)
Recent cartel detection		-0.001	-0.004			
		(-0.128)	(-0.303)			
HHI				0.010		
				(0.612)		
Industry growth						0.094
						(1.631)
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	0.538	0.525	0.525	0.525	0.525	0.53
N	354,271	40,1461	401,461	401,461	400,411	383,819

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

Table 6. Merger activity
Panel A. 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 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. In Column (6), we provide estimates based on matched difference-in-differences. We report t-statistics in the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.007*** (4.005)	0.006*** (3.515)	0.006*** (3.455)	0.004* (1.952)	0.006** (2.530)	0.006*** (3.446)
Controls	N	Y	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	N
Year f.e.	Y	Y	N	N	N	N
Industry*year f.e.	N	N	Y	N	Y	N
Country*three-year f.e.	N	N	N	Y	Y	N
Region*year f.e.	N	N	N	N	Y	N
Cohort*firm f.e.	N	N	N	N	N	Y
Cohort*industry*year f.e.	N	N	N	N	N	Y
R-squared	0.112	0.122	0.124	0.124	0.127	0.128
N	541,940	432,447	432,330	432,433	432,316	1,976,299

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

Panel B. 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 digit industry and, in the case of multiple matches, we select the one that has the closest gross margin (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, while Column (2) present 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), country characteristics (GDP and unemployment), as well 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.

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 the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Leniency law	0.009**	0.011**
	(2.657)	(2.31)
Firm f.e.	Y	Y
Matched pair*year f.e.	Y	Y
R-squared	0.120	0.134
N	62,597	43,674

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

Panel C. 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 the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	0.008*** (4.308)	0.007*** (4.755)	0.007*** (4.263)			
Subsidiary-based leniency laws				0.007*** (3.744)	0.007*** (4.273)	0.006*** (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 7. Customer 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 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 is used in this analysis. Only transactions where the acquirer and the target are in the same SIC three-digit 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 SIC three-digit 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 leniency law. In Columns (1)-(5), the leniency law dummy takes a value of one if 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 leniency law was passed in the past three years. In Column (7), the leniency law dummy takes a value of one if leniency law was passed at any time in the past. We report t-statistics in the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Recent leniency law *							
Intensity of relationship	-0.006*** (-4.409)	-0.008*** (-3.239)	-0.008*** (-3.916)		-0.009*** (-3.393)	-0.007** (-2.498)	-0.005 (-1.188)
Intensity of relationship	0.000 (0.164)	0.000 (-0.309)	0.000 (-0.530)	-0.003** (-2.200)	0.001 (0.409)	-0.002* (-1.950)	0.002 (0.447)
Recent leniency law				0.002*** (4.933)	0.003*** (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 8. 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 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. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	-0.050** (-2.374)	-0.047** (-2.355)	-0.055*** (-4.134)	-0.066*** (-3.131)	-0.043** (-2.257)
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)	

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

* 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. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	0.006*** (3.427)	0.005*** (2.953)	0.007*** (3.872)	0.006** (2.366)	0.006** (2.573)
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)

(table continued from the previous page)

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

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

Figure 1. Markup trends around leniency laws

We plot the gross margins in the same industry (three-digit SIC) over the year scaled by lagged assets and winsorized at 1%, that were affected by leniency law for the period from two years prior to two years after the leniency law. As the control sample we consider firms that did not face the introduction of a leniency law over the same period as the treated firm (i.e., control firms did not have a leniency law introduced over two years before to two years after the introduction of a leniency law for the treated firm).

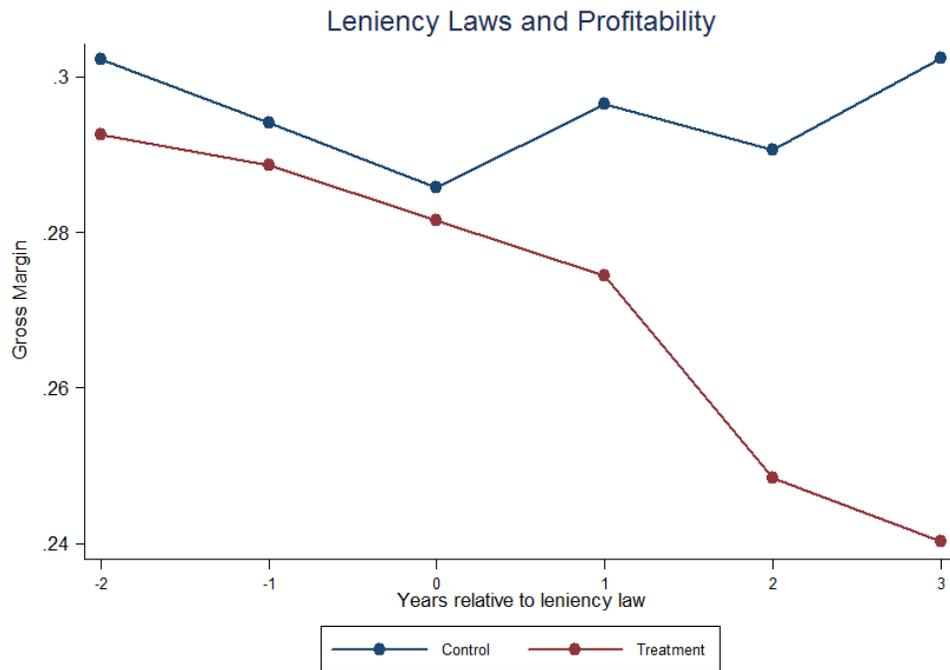


Figure 2. Merger trends around leniency laws

We plot the total dollar value of mergers and acquisitions over the year scaled by lagged assets and winsorized at 1%, that were affected by leniency law for the period from two years prior to two years after the leniency law. As the control sample we consider firms that did not face the introduction of a leniency law over the same period as the treated firm (i.e., control firms did not have a leniency law introduced over two years before to two years after the introduction of a leniency law for the treated firm).

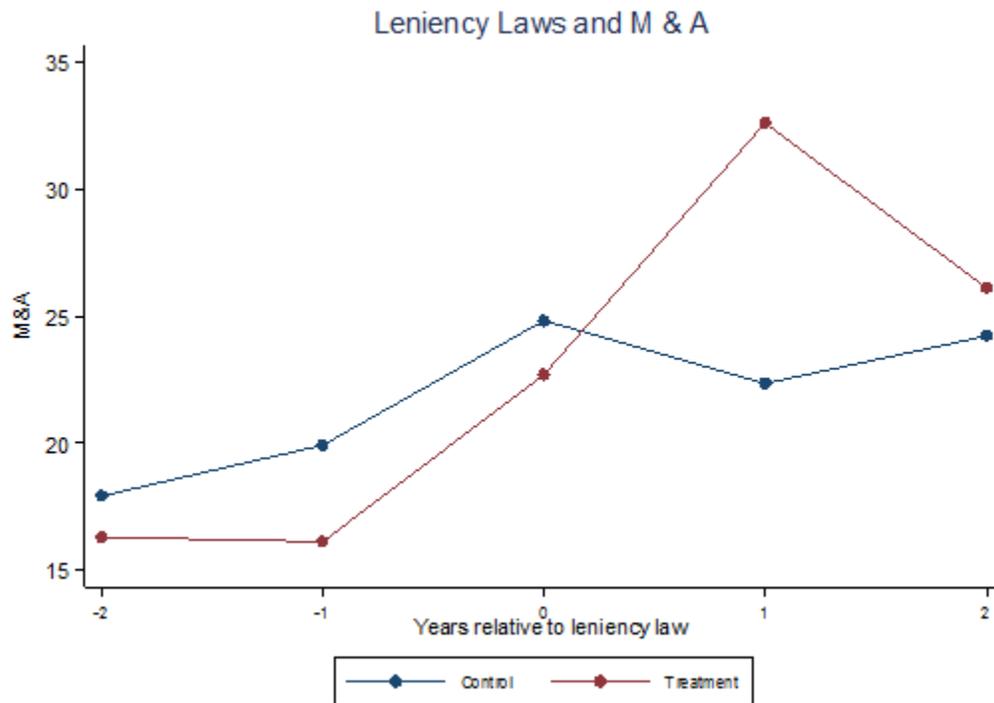


Figure 3. Merger activity and profitability

We plot the average gross margin for firms that were affected by a leniency law and pursued mergers in the first two years after the passage of the leniency law. We plot gross margins for the period from two years prior to four years after the leniency law.

As the control sample we consider firms that are in the same country and industry as the treated firm, but did not pursue mergers in the first two years after the passage of a leniency law.

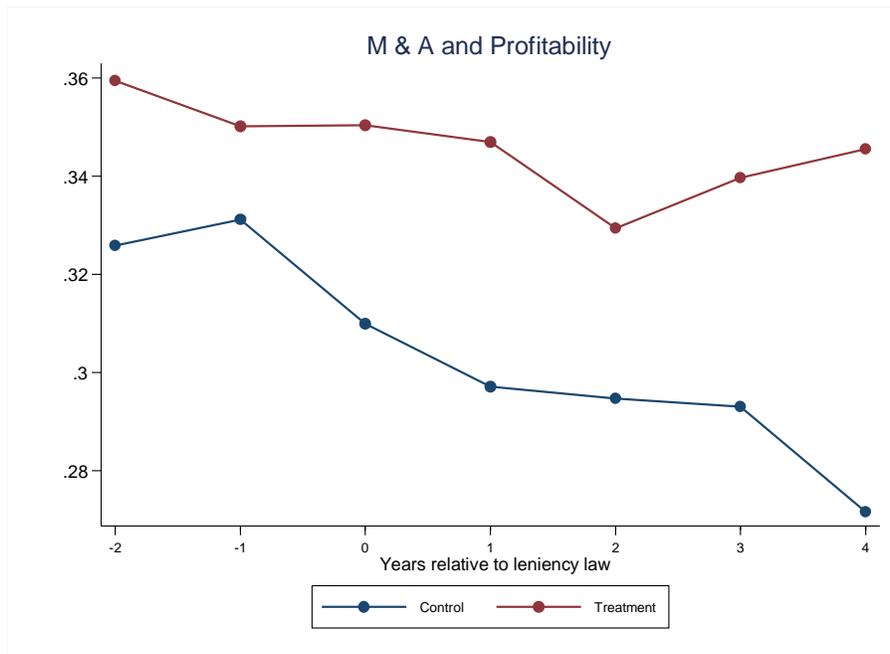
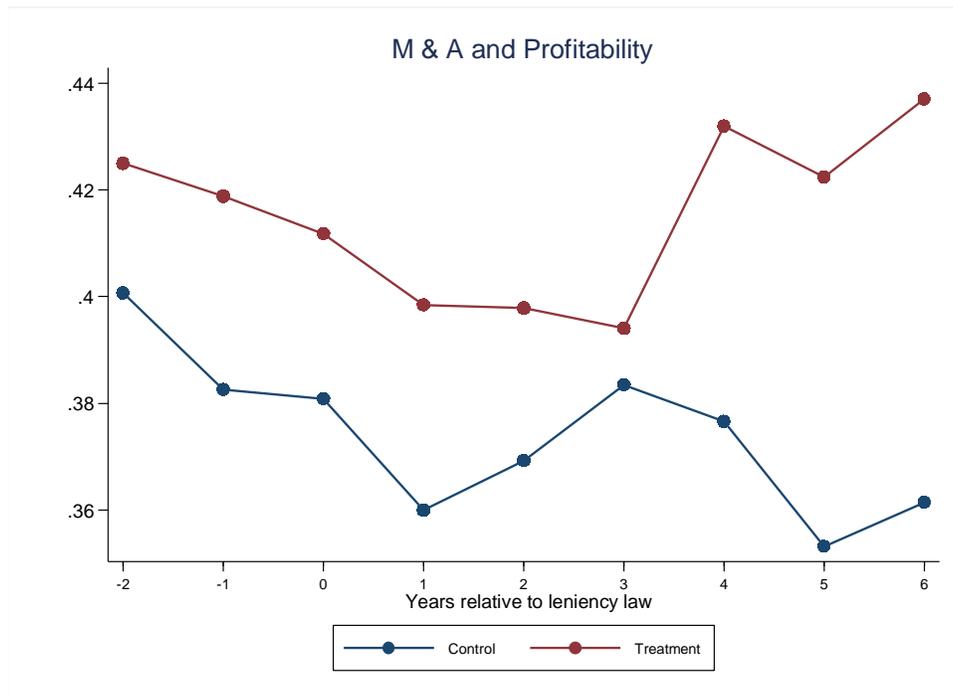


Figure 4. Failed mergers as controls and profitability

We plot the average gross margin for firms that were affected by a leniency law and pursued mergers in the first two years after the passage of the leniency law. We plot gross margins for the period from two years prior to four years after the leniency law.

As the control sample, we consider firms that are in the same country and industry as the treated firm and that also announced mergers in the first two years after the passage of a leniency law, but the mergers have failed.



Internet Appendix

I. Alternative Definitions of Merger Activity

One potential issue with our results is related to the fact that our identification of merger activity relies on the match between the SDC Platinum database and the Compustat Global and North America datasets. However, SDC Platinum may not have captured all merger cases and the match between the two data sources, especially for international firms, may not be perfect. To address this issue, we report the results of a specification in which we only rely on the Compustat Global and North America datasets and focus on investment defined as the firm's change in property, plant and equipment, and goodwill, adjusted for depreciation and amortization and scaled by one-year-lagged asset size. This measure thus includes both capital expenditures and mergers.

We report the results in Table IA1. In Column (1), we find that investment of affected firms increases by 2.8%. The result is robust to controlling for firm and country characteristics (Column (2)) and industry*year fixed effects (Column (3)).

In addition, we deconstruct the total dollar value of acquisitions over the year, scaled by lagged assets, that we use as our main merger activity variable throughout the paper by only taking into account acquisitions where target and acquirer come from the same three-digit SIC industry and are headquartered in the same country. We indeed find that the passage of leniency laws increases the within-country acquisitions of competitors. In Table IA1, Column (4), we provide the specification without the controls; in Column (5) we control for firm and country characteristics, and in Column (6) for industry*year fixed effects.

II. Standard Errors

We explore the sensitivity of our estimates' statistical significance when we cluster our standard errors in a different way. In Table IA2, we report the estimates for gross profitability margin in Columns (1)-(4) and for merger activity in Columns (5)-(8). While in our baseline specifications we cluster them at the country level, in Table IA2 we report that the statistical significance is consistent if we cluster them by industry*country (Columns (1) and (3)), double-cluster by country and year (Columns (2) and (4)), double-cluster by industry and year (Columns (3) and (5)), or double-cluster by country and industry (Columns (4) and (6)). The estimates remain statistically significant at conventional levels.

Moreover, as pointed out by Bertrand, Duflo, and Mullainathan (2004), within our context, it may not be possible to properly account for the correct structure of the error covariance matrix simply by clustering the standard errors. Thus, we provide a test in which we randomize the assignment of leniency law years.

In particular, we assign a random year for the passage of leniency laws in each country. We repeat this procedure 5,000 times to obtain 5,000 randomized leniency law samples. In each of these randomized leniency law samples, we run our baseline regressions as in Table 5, Panel A, Column (1), and Table 6, Panel A, Column (1), and save the relevant coefficients. Finally, we compare the coefficients from identification using our actual leniency laws with those obtained from identification using these pseudo leniency laws.

We report the distribution of the coefficients in Figure IA1. The first figure presents the coefficients in cartel detection regressions as in Table 5, Panel A, Column (1), and the second figure reports the coefficients in merger regressions as in Table 6, Panel A, Column (1). The figures demonstrate that our actual coefficients are larger in 4,819 of 5,000 (96.4%) randomized samples in the case of the effect on

profitability, and are smaller in 4,880 of 5,000 (97.8%) randomized samples in the case of the effect on merger activity. The non-parametric nature of this analysis suggests that the specification of the error covariance matrix does not affect our results.

References

Bertrand, M., E. Duflo, and S. Mullainathan, 2004, How Much Should We Trust Differences-in-Differences Estimates?, *Quarterly Journal of Economics* 119, 249-275.

Table IA1. Robustness to definitions of merger activity

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable in Columns (1)-(3) is the change in the value of tangible and intangible assets, adjusted for depreciation, scaled by last year's assets and winsorized at 1%, and in Columns (4)-(6) is the total dollar value of acquisitions in the same industry (three-digit SIC) and located in the same country 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 Columns (1) and (4), we test the 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 the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.028** (2.615)	0.031*** (2.825)	0.022** (2.473)	0.003*** (3.426)	0.003*** (3.278)	0.003*** (3.321)
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	0.148	0.176	0.193	0.063	0.07	0.068
N	394,246	336,104	335,975	541,869	432,447	432,330

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

Table IA2. Robustness to estimation of standard errors

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable in Columns (1)-(4) is the gross margin, winsorized at the 1% level, and in Columns (5)-(8) 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 industry*time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is the leniency law dummy. In Columns (1) and (5), we cluster standard errors by industry*country. In Columns (2) and (6), we double-cluster standard errors by country and year. In Columns (3) and (7), we double-cluster standard errors by industry and year. In Columns (4) and (8), we double-cluster standard errors by industry and year. We report t-statistics in the brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leniency law	-0.050*** (-7.647)	-0.050** (-2.344)	-0.050*** (-3.846)	-0.050** (-2.451)	0.016*** (8.158)	0.016*** (3.516)	0.016*** (5.074)	0.016*** (3.715)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm f.e.	Y	Y	Y	Y	Y	Y	Y	Y
Industry*year f.e.	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.525	0.525	0.524	0.524	0.084	0.084	0.084	0.084
N	401,461	401,461	401,461	401,461	432,330	432,330	432,330	432,330

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

Figure IA1. Simulated distributions of leniency laws

We plot the distributions of the coefficients of the regressions based on the randomized passage of leniency laws. We randomize the passage of laws in 63 countries 5000 times. The first figure presents the coefficients on profitability as in Table 5, Panel A, Column (1). The second figure reports the coefficients on mergers as in Table 6, Panel A, Column (1). Vertical lines indicate the coefficient from the actual regression.

