

Busted! Now What?

Effects of Cartel Enforcement on M&A

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Abstract

In a cross-country study, we investigate how the increasing costs of collusion shape firm boundaries and affect firm acquisitions 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 convictions, 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 acquisitions, especially horizontal ones. These acquisitions have negative effects for the downstream firms suggesting that mergers substitute cartel activity.

JEL Classification: D22, D43, G34, G38

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The phenomenon of decreasing product market competition around the world has grabbed the attention of academics, policy makers, and media alike.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 fact 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 this paper, we hypothesize that the existence of collusive behavior reduces the need to merge as similar benefits in terms of pricing power can be achieved by illegal price-fixing cartels.

Our empirical findings suggest that when firms face new regulatory constraints in cartel formation, they redraw their boundaries by acquiring other firms and these acquisitions have a more negative effect on customer stock price reactions. While antitrust laws lead to fewer cartels, firms minimize the additional costs introduced by these laws by redrawing their boundaries. 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. The Economist (2016) notes that corporate profits have been rising over time, as does the Council of Economic Advisers (2016). This topic has even featured in the debates of the last US Presidential Election with Hillary Clinton announcing “A new commitment to promote competition, address excessive concentration and the abuse of economic power, and strengthen antitrust laws and enforcement,” and Donald Trump promising to make deregulation one of the focal points of his presidency (Fiscal Times, 2016, “Antitrust Election of 2016”).

We focus on international firms from 1990-2012. Cartel activity is widespread and recent prominent cases span different countries and industries, including LIBOR rates, shipping services, auto parts, LCD panels, municipal bonds, e-books, and chemicals, with a peak fine of \$3.2bn imposed on European truck makers in 2016 by the European Commission.² We study the international setting as it provides an identification strategy that can capture strengthening antitrust enforcement against cartel activities: 63 countries and territories passed so-called leniency legislation and they did so in a staggered manner over a 20-year time frame.

Leniency laws have been among the most important developments for 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 laws 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 strategies. We compare the change in the strategies of firms that are affected by the law to the contemporaneous change in the strategies of the control group of firms that are headquartered in countries that have yet to pass such a law.

We test the hypothesis that firms recreate the benefits of a cartel by acquiring other firms and we do this by focusing on a sample of 48,948 firms over a period of 23 years. 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.

² Since collusion is outlawed in most countries, it is difficult to establish what fraction of the total sales of goods and services is affected by collusive arrangements. However, we can get a sense of the prevalence of cartels from those countries (e.g., Austria, Germany, Switzerland, the Netherlands, the Nordic countries, and Australia) that had cartel registries at a time when cartels were not illegal there. For instance, Hyttinen, Steen, and Toivanen (2016) report that in 105 of 193 Finnish manufacturing industries at least one cartel of national scope was registered over 1950-90. Their estimates, based on a Hidden Markov Model, suggest that by the end of 1990 almost all industries were cartelized. Moreover, Fölster and Peltzman (2010) find that over 1000 cartel agreements were registered in Sweden in 1990 and they affected about 15% of the total sales of goods and services.

We find that restrictions on the ability to create a cartel increase the incentives to engage in M&As. The passage of a leniency law raises the annual total dollar value of acquisitions 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. For every treated country, we find 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 match on GDP per capita as we find that this is the most consistent predictor of the timing of the law passage. For firms in the treated country we then find corresponding firms in the control country that are operating in the same industry and are closest in profitability. We assume that these matched firm pairs face similar challenges in their product markets and would have operated similarly were it not for the leniency law. Controlling for matched-pair*year and firm fixed effects alleviates concerns about residual spurious correlation. The results indicate that passage of the law has a higher effect on the M&A 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. Thus, we construct a weighted measure of leniency law passage, by weighting each leniency law passage by the export share of the firm's industry. For a

subset of firms, we also collect their subsidiary locations and implement weighted leniency law passages based on these actual foreign locations. The analysis based on these alternative and more exogenous measures of a firm's exposure to leniency laws delivers consistent results.

Also, our results hold after controlling for multiple firm- and industry-level characteristics, firm and year fixed effects as well as different measures of competition within an industry, such as the Herfindahl-Hirschman index of product market competition and import penetration, as well as other contemporaneous legal changes, including changes in corporate governance laws, financial development, and other antitrust reforms that limit cartel activity.

Moreover, our results hold both when we restrict the sample to non-US firms, and when we focus on the European Commission's strengthening of its antitrust enforcement instead of the implementation of individual laws in EU countries. Additionally, the results are strongest in the cases where firms are more likely to be parts of cartels or are more likely to be convicted for cartel activity as predicted by a model that exploits information on firm characteristics as well as country and industry fixed effects *before* the leniency law passage.

In the second part of the paper, we investigate the economic effects of such acquisitions for the buyers. We start by demonstrating that firms that pursue M&As 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 M&As. This provides preliminary evidence of a direct beneficial impact of these M&As on a firm's cash flows.

Then, we examine the rationale behind these acquisitions. Acquisitions can increase a firm's scale to achieve cost or revenue synergies. This would require firms to primarily engage in horizontal expansions. Alternatively, if the passage of leniency law affects the probability of distress of the affected firms and impairs access to external capital markets, then the firms would likely react by engaging in diversifying

expansions to form stronger internal capital markets (Stein, 1997). Thus, we examine the type of M&As that firms engage in after the passage of leniency laws. We find no evidence of diversifying acquisitions, either geographically or sector-wise. Instead, we find that leniency laws induce firms to engage in horizontal mergers. This result suggests that when product prices drop following the passage of leniency laws, firms move from collusion to mergers, either to realize revenue synergies (effectively, reconstructing cartels), or to achieve cost synergies (by enlarging the scale and rationalizing operations after the drop in profit margins).

Finally, we distinguish between cost synergies and revenue synergies by studying how downstream firms react to the cartel-busting-related M&As of suppliers. We examine the M&A 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 M&A announcements of supplier firms that follow the passage of leniency law. This suggests that potential customers lose from the acquisitions initiated in the wake of the passage of leniency laws. In other words, upstream firms react to the passage of leniency laws by engaging in M&As that allow them to re-create the market power lost with the breaking up of a cartel.

We contribute to several strands of literature. First, we contribute to the literature on the way firms grow by providing evidence as to how an expansion path is affected by stronger constraints to collusion. In this regard, since collusion usually occurs within an industry, we relate to the literature on merger waves that finds such waves are often triggered by industry shocks (e.g., Mitchell and Mullherin (1996); Andrade, Mitchell, and Stafford (2001); Harford (2005); Ahern and Harford (2014)). We complement

Harford (2005) who finds that among other industry shocks, merger waves are fostered by industry *deregulation*. Our results indicate that the reverse is also true. The introduction of certain regulations could also lead to an increase in merger activity. In addition, by showing how a shock to upstream firms propagates to the stock prices of downstream firms, our results also contribute to the debate on the efficiency of M&As (Maksimovic and Phillips, 2001; Fee and Thomas, 2004; Shahrur, 2005; Bhattacharyya and Nain, 2011).

Second, we contribute to the empirical literature on the boundaries of a firm (e.g. Robinson, 2008). Since Coase (1937), the theoretical literature has been discussing what defines the boundaries of the firm and how they evolve over time. Yet, this issue has not received adequate attention in the empirical analysis, possibly due to the inability to observe exogenous sources of variations in a firm's boundaries. We use the case of antitrust actions against collusive behavior to investigate how convicted firms adjust their boundaries and what effect this has on their corporate policies including M&A activity.

Third, we contribute to the literature regarding the effects of leniency laws. While theoretically the effects are ambiguous (Spagnolo, 2000; Ellis and Wilson, 2003; Motta and Polo, 2003; Harrington, 2008; Chen and Rey, 2013), recent empirical literature has mainly determined that leniency laws 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 laws. We contribute by establishing a link between the passage of leniency laws and firm profitability, which is a less biased proxy 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 laws 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).

Fourth, our paper contributes to the literature on finance and industrial organization (e.g., Phillips, 1995; Kovenock and Phillips, 1997; Campello, 2006; Hou and Robinson, 2006; Lyandres, 2006; Haushalter, Klasa, Maxwell, 2007; Frésard, 2010; Giroud and Mueller, 2010; Hoberg and Phillips, 2010; Valta, 2012). In particular, we relate to the recently growing literature on the interaction between finance and product price-fixing collusion (Campello, Ferres, and Ormazabal, 2016; Ferres, Ormazabal, Povel, and Sertsios, 2016). Our paper examines how stronger antitrust enforcement affects firm M&A activity.

II. Data and Main Variables

We consider all of the firms in the Compustat Global and North America datasets over the period of 1990-2012. Our initial sample covers 543,737 firm-years.

We collect information on the passage of leniency laws in 63 large countries and territories from the Cartel Regulation 2013 published by Getting the Deal Through. We manually check this information and complement it with press releases and news articles. We report the years when leniency laws were passed in Appendix 1 together with the number of observations from each country in our main dataset.

The data on international expansion choices come from the Securities Data Corporation (SDC) Platinum Database, from which we extract all M&As in the world from 1990-2012. In our main analysis, we measure the annual acquisition activity of a firm by the total dollar value of acquisitions over the year, scaled by lagged assets. The stock returns for international firms come from Datastream from 1998-2010. The accounting information comes from the Compustat Global and North America datasets.

Export data used to construct measures based on the export shares comes from the CEPII TradeProd database that contains bilateral trade flows for more than 200 countries at the ISIC 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. 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 summary statistics in Table 1, Panel A.

Finally, our source of data on convicted cartels is the Private International Cartel dataset on cartel sanctions created by John Connor and described in detail in Connor (2014). This hand-collected dataset covers all of the major private international cartels discovered, disclosed and sanctioned by regulators around the world since January 1986. It 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. Wherever in doubt, we exclude the firm or the involved cartel from the analysis.

We report some descriptive statistics on these cartels in Table 1, Panel B. The median (mean) cartel involves five (ten) companies and lasts five (seven) years before it is discovered by a regulator. The estimated median (mean) dollar value of the market size that is affected by a cartel is \$2bn (\$26.7bn).

III. Identification Strategy

One approach to study the effect of price-fixing on corporate behavior is to focus on the convicted cases. While convicted cases could provide insights, they could also be endogenous to unobservable factors, such as the industry's expected profits, competitor or employee whistleblowing, and lobbying by consumer pressure groups. Thus, our identification strategy relies on the staggered passage of leniency laws around the world. In this section, we briefly describe the leniency laws, focusing on what drove their implementation and then we present our main econometric specification.

A. Leniency Laws

Leniency laws 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 leniency program in 1973, but it remained largely ineffective until 1993. In 1993, the leniency law was revised and strengthened by making the case for amnesty clearer and broader. The revised law stipulates that if a cartel is not being investigated (or if antitrust authorities such as the Department of Justice and the Federal Trade Commission do not have sufficient evidence), the first self-reporting cartel member, including its managers, employees, and directors, would be granted automatic 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).

Although the laws are not passed in a vacuum and are arguably influenced by economic and political conditions in the respective countries, our reading of the online discussions and press announcements suggests that countries do not seem to have followed a particular trend for such law passage. Some countries passed the law after prominent collusion cases. For instance, 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. Taiwan passed the law in response to general concerns about rising consumer prices, while Korea passed it after the financial crisis.

Some other countries have instead passed leniency laws after significant pressures from the US, the EU or supranational organizations (Lipsky, 2009). For instance, 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.³ Moreover, the EU fosters the adoption of leniency laws by its member states and often seeks similar provisions in its bilateral associations and trade agreements. The IMF and the World Bank request the overhaul of antitrust laws as a condition for loans and other funding (Bradford, 2012).

In some cases, the passage of leniency laws was contentious. For instance, the leniency law met significant opposition in the Swiss Parliament as the law relies on denunciations that run contrary to Swiss legal traditions. The Japanese Business Federation (Nippon Keidanren), the most influential industrial organization in Japan, fervently argued against such a law in Japan claiming that cooperating with authorities and informing on fellow participants in exchange for lower sanctions is an affront to Japanese culture, and should only be considered as part of a wider review of the criminal law system.

Our readings of media articles covering the reasons why these laws were passed do not identify a common trend. In the Internet Appendix, Table IA1, we report the estimation in which we attempt to predict the timing of leniency law 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 economic conditions are unlikely to have been the main driver of leniency law passage. Our further findings thus contrast the findings on how industry deregulation fostered M&A (Harford, 2005). This literature suggests that industry deregulations are often preceded by

³ One may argue that free trade agreements might have a similar effect on market structure as cartel busting. Mindful of Singapore's case, we carefully control for a country's levels of trade and find that this does not affect our results. Moreover, we are not aware of any other case apart from Singapore where leniency law was passed as an outcome of a trade deal.

negative industry performance and M&As following deregulations are a form of exit from ailing industries (Ovtchinnikov, 2013). In our case, if anything, the introduction of regulations was correlated with greater profitability, and M&As serve as a way of preserving this high profitability.

We hereby describe a typical leniency law, by using the case of the European Commission (EC), as described by the Competition Directorate (2013). In order to obtain total immunity under the leniency policy, a firm that participated in a cartel must be the first to inform the EC of an undetected cartel by providing sufficient information to allow the EC to launch an investigation. If the EC is already in possession of enough information to launch an investigation, the firm must provide evidence without which the EC would be unable to prove the existence of the cartel. The firm must cooperate fully with the EC throughout its investigation, provide all evidence in its possession, and cease participation in the cartel immediately. Also, it must not disclose the existence and the content of the investigation to any other firm. The firm may not be granted immunity if it has coerced other firms to participate in the cartel.

Firms that do not qualify for total immunity may benefit from a reduction of fines if they provide evidence that adds “significant value” to that already in the EC’s possession (i.e., evidence that reinforces the EC’s ability to prove the existence of the cartel). The first such firm is granted a 30%-50% reduction, the second a 20%-30% reduction, and subsequent firms an up to 20% reduction in fines.

The impact of leniency laws on the cost of collusion depends upon the firm’s incentive to apply for leniency. For example, new managers could learn about past involvement in the cartel and consider that continuing involvement could harm their reputation or become a potential liability for the firm. Moreover, the incentives to apply for leniency in one sector may be related to cartel busting in other sectors. Indeed, firms compete in multiple product markets and could be involved in multiple cartels. The investigation of a cartel in one product market increases the probability of cartel detection in other product markets. For example, in 1999, the US introduced the Amnesty Plus and Penalty Plus programs. Even if a firm cannot

apply for leniency in the cartel case in which it is already being investigated, under the Amnesty Plus program, it can have its fine reduced in the ongoing investigation by disclosing information about its collusion in other product markets (for which it would receive full amnesty). However, if the firm fails to come clean and the second cartel is discovered, the Penalty Plus program increases the severity of the penalties in both investigations.⁴

B. Econometric Specification

In our estimation, we rely on the staggered nature of the passage of these laws to identify their causal effect. 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 M&A activity of firms that were affected by the law to the contemporaneous change in M&A activity of the control group of firms that were headquartered in the countries that had not yet passed such a law.⁵ Using firm-level data, we estimate our baseline estimation:

$$Acquisitions_{ikt} = \alpha + \beta Leniency Law_{kt} + X_{ikt} + u_i + v_t + \varepsilon, \quad (1)$$

where i indexes firms headquartered in country k , k indexes countries, and t indexes years. $Leniency Law_{kt}$ is our treatment of the passage of a leniency law, X_{ikt} represents a vector of

⁴ 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 the passage of laws is more comparable across countries. For a subset of countries, we collect data on other legislative changes of cartel enforcement and control for them in our specifications.

⁵ 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 IV.C in which we explicitly take into account the geographic nature of firms' international operations.

control variables, while u_i and v_t are firm and 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 M&A policy has changed for German firms as compared to firms in other countries (e.g., Italian firms) which, after controlling for firm fixed effects, are otherwise identical but were exposed to a leniency law on a different date (e.g., 2007 in the case of Italy).⁶

C. Validation of Identification Strategy

We now validate our identification strategy. Theoretically, the effect of leniency laws on the costs of collusion is uncertain. 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 (Ellis and Wilson, 2003; 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 (Spagnolo, 2000; Motta and Polo, 2003; Chen and Rey, 2013). This would stabilize existing cartels or even induce the formation of new ones. Also, stronger antitrust enforcement can increase cartel stability by making meetings, and the future renegotiation of prices, more difficult (McCutcheon, 1997).

⁶ Importantly, our policy changes 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)

Miller (2009) has already 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 – i.e., not only passed but also enforced. We first study the (observed) effect of the staggered passage of leniency laws on cartel convictions. Then, we directly investigate the (observed and unobserved) effect of the passage of leniency laws on firm profitability.

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. Since the total number of convictions in a country is a count variable that takes a value of zero with high frequency (82% of the observations are zero), we hypothesize that the expected number of convictions 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. As a robustness check, we also provide simple OLS estimates with country- and time-fixed effects.

We report the results in Table 2. In Column (1) of Panel A, we find that the passage of a leniency law more than doubles the number of convicted cartels, increasing them by 154%.⁷ This result is robust to controlling for the time-varying macroeconomic conditions in the country (Column 2).

One potentially contaminating effect is that many European countries are governed by both EU and national antitrust laws (effective as long as they do not contradict EU laws). Thus, as a robustness check,

⁷ The economic effect from the Poisson model is estimated to be $154\% = \exp(0.935) - 1$.

in Column (3), we report the results of the analysis in which we consider all EU countries as if they were one single country and consider the strengthening of EU leniency law in 2002 rather than the passage of leniency laws in individual countries. The results are similar. Column (4) reports a fixed effect OLS regression, while in Column (5), the dependent variable is the number of convicted firms. We find that the number of convicted cartel members is higher by a factor of eight.

Next, we perform firm-level analysis and focus on firm profitability. We consider all of the Compustat Global and North America firms within our sample period and employ a differences-in-differences methodology to investigate the effect of the passage of leniency laws on the gross margins of the affected firms. We estimate equation (1), using gross margins as the outcome variable. 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 convictions. Indeed, only about 10%-30% of all cartel conspiracies are discovered (Connor, 2014), and it is likely that leniency laws could affect not only convictions 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 stability (Spagnolo, 2000; Motta and Polo, 2003; Chen and Rey, 2013). More cartel convictions could simply reflect the same conviction rate but a larger number of cartels in the economy. Thus, the gross margins would capture these unobservable effects of the undetected cartels.

We report our baseline differences-in-differences estimations in Columns (1)-(2) of Table 3. We control for firm fixed effects and time fixed effects in Column (1) and additionally for firm and country characteristics in Column (2). In the latter estimation, we find that the passage of leniency laws lowers gross margins by 5.1 percentage points. This represents a 14.8% 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.⁸

Our results are robust, both in terms of statistical significance and economic magnitudes. In Column (3), we additionally control for industry*years fixed effects as per Gormley and Matsa (2014). We consider industries at a two-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 Column (4), instead of clustering standard errors at the country level, we cluster them at the country*industry (three-digit SIC) level. In Column (5), we remove the financial institutions (with SIC codes 6000-6999) from the sample. In all these alternative regressions, we find statistically similar results. We provide additional robustness checks in the Internet Appendix, Table IA2.⁹

Overall, these results validate the use of leniency laws as an identification strategy and show that leniency laws have led to more convictions as well as reduced the profitability of the affected firms. Moreover, these results provide, to the best of our knowledge, the first empirical evidence regarding the effectiveness of the global passage of leniency laws.

⁸ 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.

⁹ Arguably, the profitability of some firms (e.g., those that are customers of colluding firms and do not collude themselves) can be improved by leniency laws. Due to identification constraints, we are unable to show the effect of leniency law on customers, as we are unable to distinguish between “customer” and “supplier” firms, given that our identification is at the country level.

IV. Boundaries of the Firm

We now investigate how firms adapt their corporate policies 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 firm's boundaries. In particular, we investigate the effects of the passage of leniency laws on the main growth-related corporate policy: M&A activities. We follow three estimation strategies. First, we estimate a differences-in-differences 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-leniency law passing-countries. Finally, we apply an identification strategy in which we estimate the effect of leniency laws passed in other countries that are a firm's potential export markets or where the firm has international operations.

A. Difference-in-Difference Estimates

First, we examine how the firm's merger activity is affected. We analyze this using the SDC Platinum database that we merge with the Compustat Global and North America datasets for the 1990-2012 period. 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 total dollar value of assets, scaled by lagged assets, 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-existing trends of the treatment and the control groups are similar, there is a temporary increase in the M&A activity by the treated firms.

We then apply a differences-in-differences methodology, as specified in Section III.B and motivated by our analysis of profitability, to a linear probability model and estimate the effect of the passage of leniency laws on a firm's decision to engage in M&As. Our dependent variable is the total dollar value of acquisitions over the year, scaled by lagged assets.

We report the results in Table 4. Column (1) of Panel A indicates that the passage of leniency laws increases the total dollar value of acquisitions by 0.7% where the mean prior to leniency laws is 0.6%. This result is robust to different specifications and robustness tests. We first consider the set of robustness checks of Section III.C. The results are robust to controlling for firm and country characteristics (Column (2)) and industry*year fixed effects (Column (3))¹⁰, alternative clustering (Column (4)) and removal of the financial firms from the sample (Column (5)).

We continue with more robustness checks in Table 4, Panel B. In particular, we control for potentially confounding effects. In Column (1), we control for product market competition – i.e., the Herfindahl-Hirschman index (HHI) based on the sales of publicly listed firms in the sample. We construct the HHI in different ways, based on two-digit, three-digit or four-digit SIC codes. Moreover, we construct the industry's HHI for all countries, or estimate it separately for each country. Controlling for these alternative proxies delivers consistent results. In the interest of brevity, we only report estimations in which we control for the HHI based on three-digit SIC industries estimated separately for each country.

As a further robustness check, we also control for other legislative changes. In Column (2), 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

¹⁰ In the Internet Appendix Table IA3, we also provide an estimation in which we replicate this table but include industry*geographic region*year fixed effects (i.e., effectively performing within-region and within-industry estimation). Our results are consistent.

that the first competition law also positively affects acquisitions, but its impact does not render leniency laws insignificant. Similarly, Column (3) restricts the sample to countries that already had general competition law by 1990.

In Column (4), 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 M&A 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 recall that the main reason 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 penalties¹¹) or could have unclear effects on cartel conduct (e.g., changes in violation provisions).

Even if there were correlated changes in anti-cartel provisions, our identification based on leniency laws would then be a proxy for a general strengthening of the anti-cartel provisions and 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

¹¹ 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).

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 (5), 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 M&A activity.

Finally, in Column (6), we control for the possible effects of strategic relocations by restricting the sample to those firms that had not changed their headquarter countries. Our results are robust to all of these changes in our specification.

We further examine the robustness of estimating standard errors. In Table 4, Panel A, Column (4), we provide the results with alternative clustering by country*industry. In untabulated results, we also find that the results are robust to clustering by firm as well as two-way clustering by country and industry, country and year, and industry and year. Moreover, in the Internet Appendix, Figure IA1, we also provide additional treatment of the standard errors by randomizing the passages of leniency law. As noted by Bertrand, Duflo, and Mullainathan (2004), within our context, clustering standard errors may not properly account for the correct structure of the error covariance matrix. Thus, we assign a random year to 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 and compare the coefficients obtained from identification using our actual leniency laws with those from identification using these pseudo leniency laws. We find that the coefficients from the simulated samples are smaller in 4,880 of the 5,000 cases (97.8%).

One potential issue with our results is related to the fact that our identification of M&A 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 M&A cases and the match between the two data

sources, especially for international firms, may not be perfect. To address this issue, in Table 5, 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 acquisitions.

We report the results in Table 5. In Panel A, 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)), industry*year fixed effects (Column (3)), alternative clustering (Column (4)), and removal of the financial firms from the sample (Column (5)). In Panel B, we perform additional robustness tests in which we control for the degree of competition in the firm's industry (Column (1)), other legislative changes, such as introduction of the first competition law in the country (Columns (2)-(3)), the country's financial development (Column (4)), and the passage of other antitrust legislation against cartels (Column (5)). Our results are also unaffected by the possible strategic relocation effects (Column (6)).

B. Targeted Treatment

If our hypotheses are 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 three sets of tests of heterogeneity of the effect and report them in Table 6.

First, we estimate the likelihood that a firm will be convicted 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 determinant of the potential for cartelization. We fit the

prediction model using only pre-leniency observations and predict the probability that a firm will be convicted 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 conviction. We find that firms that are more likely to be convicted conduct more M&As.

Second, we sort firms according to their ROA with respect to their country and three-digit SIC industry in a particular year, and create a dummy if the firm's ROA is higher than that of the median peer ROA (by country, industry, and year). It is likely that those firms more likely to be engaged in cartel activity are the most profitable firms within the industry, and that they are also more likely to attract the attention of antitrust authorities. We then interact leniency law passages with this dummy variable and report the results in Column (2). We find that our results on M&A are stronger for more profitable firms in the industry.

Finally, in Column (3), we go further and narrow our focus to the most profitable firms defined as the top 10% in terms of profitability in each three-digit SIC industry, country, and year. In line with our expectations, the effect of the passage of leniency laws is indeed stronger for the most profitable firms.

C. Matched Sample

Our next analysis is based on a matched sample technique. The goal is to assess the impact of the passage of leniency laws 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 law. 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 M&A activity 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 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 7. We initially report the results for gross margin and then continue with the results on the total dollar value of the acquisitions, scaled by lagged assets. In Columns (1) and (2), we find that the passage of a law has a higher negative effect on profitability and a higher positive effect on the M&A activity of those firms headquartered in the countries that pass a leniency law than on the M&A 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 M&A by 0.9 percentage points.

In Columns (3) and (4), we focus on those cases where a firm has a non-zero estimated probability of being convicted 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. 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. Identification Based on Foreign Laws

A possible critique of an identification based on the passage of leniency laws is that it may not be fully exogenous to the political and economic conditions of a firm's country. To address this issue, we consider two additional identification strategies. The first strategy relies on a definition of treatment based on a firm's exposure to the passage of leniency laws 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 law 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 laws 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.¹² 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.

Moreover, in the specifications above, we define our treatment of leniency law passage on the basis of a firm's headquarter country, where, presumably, an average firm makes most of its sales. However, firms also sell to other countries and are exposed to their antitrust codes. 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 laws 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 law 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

¹² 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).

industry has passed a law to one when all the foreign countries with exports from the firm’s industry have passed leniency laws.

Our first alternative specification is then as follows:

$$Acquisitions_{ikt} = \alpha + \beta(\text{Export Market Leniency Law})_{jkt} + \delta X_{ikt} + u_i + v_t + \varepsilon. \quad (2)$$

Unlike in Equation (1), in Equation (2), a firm is considered as “treated” if at least one country to which its industry is exporting has passed a leniency law. The intensity of treatment changes as more of the countries to which this industry exports adopt leniency law. Given that the identification is at the country-industry level, we cluster standard errors at the country*industry (three-digit SIC) level.

The second alternative identification strategy relies even more directly on the international nature of firm operations. For a subset of firms, we collect data on their actual international operations. 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 operations around the world in terms of sales as recorded in the Lexis-Nexis Corporate Affiliations data.

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

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

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:

$$Acquisitions_{ikt} = \alpha + \beta(\text{Subsidiary Based Leniency Law})_{ikt} + \delta X_{ikt} + u_i + v_t + \varepsilon \quad (3).$$

We report the findings in Table 8. We start with the effect on profitability as the motivation for these two identification strategies (Panel A). We continue with the effect on M&A as measured by total deal volume, scaled by lagged assets (Panel B), and total change in fixed assets as per Compustat (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 and a significant positive effect on acquisitions. These results are robust after controlling for firm and country characteristics (Column (2)) or two-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 two-digit SIC industry. In particular, within a two-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 law 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 law in that particular year.

In the next three columns of the panels in Table 8, 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.¹³ We perform tests similar to those of the Export Market Leniency Law variable. In Column (4), we find that the passage of leniency laws in the countries where the firm has subsidiaries significantly reduces profitability and increases acquisitions 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 two-digit SIC industry*year fixed effects (Column (6)).

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 M&A activities.¹⁴

V. Economic Effects and Drivers of Leniency Law-Induced M&As

In this section, we first show a link between M&As and profits. That is, we show that it is the same set of firms that experience a drop in profitability that also pursue an M&A and this M&A has a positive effect on future profitability. Then, we attempt to distinguish between the reasons for the M&A by investigating whether the firms pursue horizontal acquisitions to gain synergies or diversifying acquisitions to reduce the costs of distress. Finally, we explore the type of synergies firms are generating by focusing on the customer reaction to M&A deals that follow leniency law passages and distinguishing between revenue and cost synergies.

A. The Effects on the Acquirers

¹³ Given the limited subsidiary data, our sample is significantly reduced.

¹⁴ Since we have data on the convicted 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 the conviction. It could be that firms expected that the antitrust authorities’ concerns about these mergers would just be too strong.

Our findings thus far have indicated that leniency laws lead to higher convictions (Table 2), lower profitability (Table 3), and induce firms to pursue more M&As (Tables 4-8). However, we have not determined whether firms that experienced a drop in profitability and pursued M&As enjoy better performance in the future than a firm that experienced a drop in profitability, but did not pursue M&As. It could be that the set of firms that are affected by a drop in profitability due to leniency laws that pursue M&As are different from those firms that do not pursue M&As. Demonstrating that these effects occur simultaneously is challenging. Thus, we estimate the effects industry by industry and determine whether industries where profitability dropped also exhibit an increase in M&A activity. In particular, for each three-digit SIC industry, we separately estimate our baseline difference-in-difference specifications, reported in Table 3, Column (2), as well as in Table 4, Panel A, Column (2). We obtain estimates of the leniency law effect for 282 industries for both gross margins and M&As. We find that for 137 of these industries, the effect on gross margins was negative and the effect on M&A activity was positive, suggesting that in almost half of the industries these effects occur simultaneously.¹⁵

We further examine whether firms that are affected by leniency laws and subsequently pursue M&As 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 M&A 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 2. While this figure does not establish causality, it displays a clear correlation that firms that pursue

¹⁵ In the other cases, profitability decreased and M&A activity decreased in 61 industries, profitability increased and M&A activity increased in 56 industries, and profitability increased and M&A activity dropped in 28 industries.

acquisitions, following leniency laws, experience better outcomes in product markets than those that do not pursue acquisitions.

Moreover, we recognize that firms that pursue acquisitions could be different in nature than those that do not thereby creating a self-selection bias (Li and Prabhala, 2010). In Figure 3, we limit the control group of firms to those that announce M&As in the first two years within leniency law passage, but the M&As have failed for some reason (similarly to Savor and Lu (2009) and Seru (2014)). Both the treatment and control firms attempt to engage in acquisitions, but only the treatment firms were successful. We find that the profitability of firms with successful M&As suffered less than the profitability of firms with failed M&As following the implementation of leniency laws.

B. The Drivers of Leniency Law-Induced M&As: Diversification vs. Synergies

We now examine the type of M&As that the affected firms conduct following the passage of leniency laws. One possibility is that the passage of leniency laws leads to the reorganization of a firm's activities to exploit revenue or cost synergies. If these synergies drive the M&A activity, then we would expect firms to primarily engage in horizontal acquisitions. That is, acquisitions of competitors operating in the same industry and market as acquisitions of firms with similar operations are more likely to generate revenue or cost synergies.

Another possibility is that the passage of leniency laws affects the probability of distress of the affected firms. When a leniency law is passed, the ensuing drop in profitability triggers a corresponding increase in the probability of distress (Bizjak and Coles, 1997). This could reduce access to external capital markets and motivate the firm to form stronger internal capital markets through M&As (Stein, 1997). If this is the case, we would expect the firms to make diversifying acquisitions that increase the usefulness of internal capital markets (i.e., acquisitions in different industries and markets).

We test these two possibilities by focusing on whether the acquisitions are performed within the same country and industry. The results, reported in Table 9, indicate that following leniency laws firms increase their acquisitions of competitors from the same industry and country. In particular, in Column (1), we focus on acquisitions in the same three-digit SIC industry as the firm, regardless of the target country. We find that leniency laws increase these acquisitions.

In Column (2), we further split the total dollar value of acquisitions over the year, scaled by lagged assets in the same three-digit SIC industry, into acquisitions of targets headquartered in the acquirer's country. We find that the passage of leniency laws primarily increases the within-country acquisitions of competitors. In Column (3), we focus on foreign acquisitions (i.e., whether firms increase acquisitions in the same three-digit SIC industry, but a different country). We do not find any evidence that firms expand across borders after the passage of a leniency law. Finally, in Column (4), we focus on diversifying acquisitions (i.e., when the target is in a different industry from the acquirer). We do not find that the passage of leniency laws affects these diversifying acquisitions.

Overall, these findings imply that firms merge to exploit synergies rather than to reduce their higher costs of distress by diversifying their acquisitions.

C. The Drivers of Leniency Law-Induced M&As: Which Types of Synergies?

The previous section suggests that firms engaging in acquisitions following increased collusion costs were more likely to exploit synergies than diversify. 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. M&As are thus one way of improving efficiency and realizing the required economies of scale (i.e., cost synergies). Alternatively, mergers could be pursued to recreate market power, thus exploiting

revenue synergies. 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 these types of synergies. We focus on the stock price reaction of the customers of firms involved in M&As following a recent passage of leniency laws. If these M&A transactions were led by cost synergies, 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 M&A transactions were led by revenue synergies (i.e., desire to preserve market power), then the stock prices of customers would respond more negatively.¹⁶

We identify the supplier-customer relationships from the OECD Input-Output tables of year 2002.¹⁷ 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 Section V.B indicated that leniency laws predominantly affected horizontal mergers and they are also most likely to have a negative effect on consumer welfare, in this 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

¹⁶ 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."

¹⁷ 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.

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).

As we are specifically interested in cartel cases, we dig further and examine whether this differential reaction between customers and non-customers differs in the case where suppliers are 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.

Alternatively, we could have simply identified customer firms and determined whether the stock market reaction for these firms differed after leniency laws were passed. However, this is problematic in our case for at least two reasons. The first is related to data availability for international firms. Input-Output tables do not let us identify actual customers but based on the overall trend statistics, give a probability that a certain firm is a customer firm. 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. The second reason is 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.

We report the results in Table 10. 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 M&A activity following a rise in the costs of collusion is harmful to downstream firms, hinting that these M&A transactions could be driven by revenue synergies.

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 (4), 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 M&A activity in the upstream industry has a differential effect on customer stock price reaction.

It is worth noting that while these M&As 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 M&A activity that harms consumers.

VI. Discussion and Conclusion

In this paper we show that by reducing corporate performance leniency laws induce firms to redefine their boundaries and to recreate the benefits that cartel collusion used to bring. Following the passage of leniency laws, which increase the costs of collusion stability, firms reorganize their activities by pursuing more M&A transactions, especially those in the same industry. These M&As generate a more negative stock price reaction for customers around their announcements, as compared to other M&A transactions.

These results offer a first understanding as to how the increased costs of collusion and, in particular, how leniency laws affect firm behavior. With these findings, we also 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 M&A 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. Stigler (1950) suggests that one reason why mergers are preferred to cartels is the illegality of the latter. In line with this, we find that when antitrust authorities have better tools to prosecute cartels, firms switch to mergers, thus mitigating the effectiveness of the antitrust policies.

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Table 1. 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, industry/country or cartel level.

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 + M&A	Compustat	397,697	0.13	0.05	0.32
Dollar value of acquisitions over lagged assets	SDC Platinum	543,737	0.014	0.00	0.079

Panel B. Convicted 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 convicted cartel participants	292	0.87	0.93	0.16
Cartel length (year)	616	7.39	5	9.17

Table 2. Convicted 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 convicted 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. In Column (3), we treat the EU as one country, and consider the passage of leniency law legislation at the EU level (we keep one observation for the EU). Column (4) reports the OLS estimates. In Column (5), the dependent variable is the number of convicted firms. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	0.935*** (2.987)	0.869*** (3.383)	0.716* (1.686)	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)			
Constant				0.063 (0.528)	
Country f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y
R-squared				0.475	
N	1,449	837	1,049	1,449	1,449

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

Table 3. Profitability

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. In Column (3), we control for industry (two-digit SIC)*year fixed effects. In Column (4), we cluster standard errors at the country*industry (three-digit SIC) level. In Column (5), we remove financial institutions (SIC codes 6000-6999) from the sample. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	-0.062** (-2.213)	-0.051** (-2.093)	-0.054** (-2.368)	-0.051*** (-4.902)	-0.056** (-2.140)
Assets		0.080*** (6.811)	0.081*** (6.340)	0.080*** (7.159)	0.078*** (6.528)
Leverage		0.000 (-0.589)	0.000 (-0.454)	0.000 (-0.471)	0.000 (-0.813)
GDP per capita		-0.120*** (-3.501)	-0.126*** (-4.285)	-0.120*** (-7.714)	-0.121*** (-3.775)
Unemployment		0.007 (1.565)	0.007 (1.652)	0.007*** (3.233)	0.009* (1.873)
Imports as a % of GDP		-0.004** (-2.213)	-0.004** (-2.141)	-0.004*** (-5.670)	-0.004** (-2.116)
Constant	0.251*** (9.998)	1.098*** (3.286)	1.134*** (3.876)	1.098*** (8.992)	1.097*** (3.577)
Firm f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	Y	Y
Industry*year f.e.	N	N	Y	Y	Y
R-squared	0.523	0.531	0.532	0.531	0.535
N	473,369	404,107	404,107	404,107	366,951

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

Table 4. M&A activity

Panel A. Differences-in-differences estimation

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 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. In Column (3), we control for industry (two-digit SIC)*year fixed effects. In Column (4), we cluster standard errors at the country*industry (three-digit SIC) level. In Column (5), we remove financial institutions (SIC codes 6000-6999) from the sample. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	0.007*** (3.999)	0.006*** (3.505)	0.006*** (9.167)	0.006*** (3.584)	0.006*** (3.489)
Assets		0.009*** (5.078)	0.009*** (9.514)	0.009*** (5.600)	0.010*** (5.473)
Leverage		0.000 (0.959)	0.000 (1.180)	0.000 (1.107)	0.000 (1.108)
GDP per capita		-0.002 (-1.385)	-0.002** (-2.046)	-0.002* (-1.828)	-0.002 (-1.595)
Unemployment		-0.001 (-0.572)	-0.001*** (-0.832)	-0.001 (-0.705)	-0.001* (-0.989)
Imports as a % of GDP		0.000 (-0.044)	0.000 (-0.135)	0.000 (-0.033)	0.000 (-0.119)
Constant	-0.005 (-1.400)	-0.021 (-1.095)	-0.021*** (-3.120)	-0.014 (0.842)	-0.019 (-1.021)
Firm f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	Y	Y
Industry*year f.e.	N	N	Y	N	N
R-squared	0.109	0.117	0.117	0.120	0.116
N	543,736	434,860	434,860	434,860	396,550

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

Panel B. Additional robustness checks

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 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 HHI in the firm's industry (three-digit SIC) in its country. In Column (2), we control for the introduction of competition law. In Column (3), we restrict the sample to countries that passed the competition law in or prior to 1990. In Column (4), we control for the country's financial market development. In Column (5), we control for other ways in which anti-cartel legislation was strengthened. In Column (6), we restrict the sample to firms that do not change their headquarter countries. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.006*** (3.530)	0.006*** (3.486)	0.008*** (3.043)	0.005*** (2.788)	0.007*** (3.630)	0.007*** (3.518)
Assets	0.009*** (5.712)	0.009*** (5.715)	0.010*** (5.771)	0.009*** (5.511)	0.008*** (4.715)	0.009*** (5.699)
Leverage	0.000 (0.964)	0.000 (0.966)	0.000 (1.018)	0.000 (1.389)	0.000* (1.988)	0.000 (1.074)
GDP per capita	-0.002 (-1.460)	-0.002 (-1.482)	-0.002 (-0.855)	-0.002 (-0.724)	-0.006 (-1.224)	-0.001 (-1.082)
Unemployment	-0.001 (-1.517)	-0.001 (-1.573)	-0.001 (-0.967)	-0.001 (-1.051)	-0.001 (-1.395)	-0.001 (-1.37)
Imports as a % of GDP	0.000 (-0.058)	0.000 (-0.294)	0.000 (0.265)	-0.000* (-1.766)	0.000 (1.481)	0.000 (-0.404)
HHI	-0.002 (-1.252)					
Competition law		0.004** (2.249)				
Chinn-Ito index				-0.009 (-0.967)		
Stock market capitalization to GDP				0.000*** (3.503)		
Private credit to GDP				-0.000* (-1.992)		
Increase in penalties					-0.005*** (-4.282)	
Change in investigative powers					-0.003 (1.493)	
Change in cartel definitions					-0.011** (-1.932)	
Other cartel laws					0.017** (2.583)	
Constant	-0.019 (-1.003)	-0.023 (-1.158)	-0.018 (-0.509)	-0.004 (-0.122)	0.027 (0.398)	-0.023 (-1.097)
Firm f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
R-squared	0.117	0.117	0.18	0.118	0.148	0.118
N	434,860	434,860	355,378	423,497	295,842	428,149

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

Table 5. M&A and capital expenditures

Panel A. Differences-in-differences estimation

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the change in the value of tangible and intangible assets, adjusted for depreciation, scaled by last year's assets. 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. In Column (3), we control for industry (two-digit SIC)*year fixed effects. In Column (4), we cluster standard errors at the country*industry (three-digit SIC) level. In Column (5), we remove financial institutions (SIC codes 6000-6999) from the sample. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	0.028** (2.603)	0.031*** (2.808)	0.024** (2.629)	0.032*** (2.829)	0.031*** (6.158)
Assets		0.078*** (11.197)	0.083*** (10.564)	0.081*** (12.169)	0.078*** (25.859)
Leverage		0.001 (1.115)	0.001 (1.242)	0.001 (1.181)	0.001** (2.366)
GDP per capita		-0.030 (-0.964)	-0.037 (-1.357)	-0.032 (-1.004)	-0.030*** (-4.602)
Unemployment		-0.002 (-0.990)	-0.003 (-1.350)	-0.003 (-1.549)	-0.002* (-1.915)
Imports as a % of GDP		0.000 (-0.490)	0.000 (-0.573)	0.000 (-0.501)	0.000 (-1.584)
Constant	0.040** (2.619)	-0.056 (-0.162)	0.171 (0.630)	-0.028 (-0.079)	-0.056 (-0.797)
Firm f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	N	Y	Y
Industry*year f.e.	N	N	Y	N	N
R-squared	0.166	0.193	0.209	0.191	0.193
N	397,697	340,207	340,207	314,372	340,207

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

Panel B. Additional robustness checks

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, where the dependent variable is the change in the value of tangible and intangible assets, adjusted for depreciation, scaled by last year's assets. 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 HHI in the firm's industry (three-digit SIC) in its country. In Column (2), we control for the introduction of competition law. In Column (3), we restrict the sample to countries that passed competition law in or prior to 1990. In Column (4), we control for the country's financial market development. In Column (5), we control for other ways in which anti-cartel legislation is strengthened. In Column (6), we restrict the sample to firms that do not change their headquarter countries. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.031*** (2.808)	0.030*** (2.733)	0.036*** (3.104)	0.019** (2.276)	0.036*** (3.775)	0.032*** (2.985)
Assets	0.078*** (11.176)	0.078*** (11.160)	0.078*** (10.209)	0.081*** (9.748)	0.079*** (9.930)	0.078*** (10.745)
Leverage	0.001 (1.111)	0.001 (1.128)	0.001 (1.231)	0.001 (1.595)	0.001 (1.264)	0.001 (1.169)
GDP per capita	-0.029 (-0.951)	-0.032 (-1.043)	-0.051 (-1.289)	-0.031 (-1.181)	-0.021 (-0.580)	-0.03 (-0.875)
Unemployment	-0.002 (-0.989)	-0.003 (-1.151)	-0.004 (-1.211)	-0.004 (-1.670)	-0.005 (-1.609)	-0.002 (-1.021)
Imports as a % of GDP	0.000 (-0.481)	-0.001 (-0.744)	0.001 (0.867)	-0.001* (-1.810)	0.001 (0.429)	0.000 (-0.514)
HHI	0.009 (0.862)					
Competition law		0.043* (1.981)				
Chinn-Ito index				0.035 (0.595)		
Stock market capitalization to GDP				0.000** (2.076)		
Private credit to GDP				-0.001*** (-2.766)		
Increase in penalties					-0.022* (-1.840)	
Change in investigative powers					0.028* (1.745)	
Change in cartel definitions					-0.052* (-1.944)	
Other cartel laws					-0.011 (-0.345)	
Constant	-0.062 (-0.182)	-0.066 (-0.190)	0.332 (0.815)	0.074 (0.291)	-0.095 (-0.209)	-0.052 (-0.137)
Firm f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
R-squared	0.193	0.193	0.203	0.200	0.200	0.195
N	340,207	340,207	276,130	330,313	289,932	334,488

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

Table 6. 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 total dollar value of 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.

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 convicted in a cartel case in the year after the passage of the leniency law. In Column (2), our main variable of interest is the interaction between the passage of leniency laws and a dummy variable if the firm's profitability is higher than the median profitability in its country and industry in a specific year. In Column (3), our main variable of interest is the interaction between the passage of leniency laws and a dummy variable if the firm's ROA is higher than the 90th percentile in terms of ROA in its country and industry in a specific year. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Leniency law	0.005*** (3.264)	0.003* (1.969)	0.003* (1.772)
Leniency law x Conviction probability	0.030* (1.797)		
Leniency law x High ROA		0.004** (2.591)	
Leniency law x Top 10% ROA			0.010*** (3.427)
High ROA		0.006** (2.286)	
Top 10% ROA			0.008*** (3.182)
Assets	0.007*** (4.546)	0.007*** (4.442)	0.007*** (4.388)
Leverage	-0.002 (-0.769)	-0.006*** (-2.702)	-0.006*** (-2.684)
GDP per capita	0.000 (-0.097)	0.000 (-0.096)	-0.001 (-0.203)
Unemployment	0.000 (-0.777)	0.000 (-0.666)	0.000 (-0.724)
Imports as a % of GDP	0.000 (0.419)	0.000 (0.539)	0.000 (0.512)
Constant	-0.024 (-0.569)	-0.026 (-0.602)	-0.021 (-0.418)
Firm f.e.	Y	Y	Y
Year f.e.	Y	Y	Y
R-squared	0.146	0.146	0.148
N	379,111	383,351	383,351

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

Table 7. Matched firm analysis

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 in Columns (1) and (3), the dependent variable is the gross margin, while in Columns (2) and (4), the dependent variable is the total dollar value of acquisitions, scaled by lagged assets. Columns (1) and (2) report the results for the full sample, while Columns (3) and (4) present the results for those cases where the predicted probability of conviction 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-lenieny observations and predict the probability that the firm is convicted 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 *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(3)
Leniency law	-0.140*** (-6.462)	0.009** (2.657)	-0.126*** (-3.22)	0.011** (2.31)
Firm f.e.		Y	Y	Y
Matched pair*year f.e.		Y	Y	Y
R-squared	0.612	0.120	0.620	0.134
N	62,597	62,597	43,674	43,674

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

Table 8. Identification based on foreign laws

Panel A. Profitability

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. In Columns (3) and (6), we control for industry (two-digit SIC)*year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	-0.106*** (-2.898)	-0.092*** (-3.622)	-0.080*** (-4.966)			
Subsidiary-based leniency laws				-0.075*** (-2.929)	-0.051*** (-2.686)	-0.065*** (-2.876)
Assets		0.087*** (3.277)	0.085*** (3.320)		0.034** (2.188)	0.038** (2.123)
Leverage		0.000 (-0.350)	0.000 (-0.328)		0.000 (-0.213)	0.000 (-0.251)
GDP per capita		-0.137*** (-4.150)	-0.137*** (-4.138)		-0.166*** (-5.610)	-0.169*** (-6.056)
Unemployment		0.007** (2.200)	0.006*** (2.610)		0.005 (0.972)	0.008*** (2.643)
Imports as a % of GDP		-0.004*** (-3.654)	-0.004*** (-4.202)		-0.009*** (-5.746)	-0.009*** (-6.679)
Constant	0.218*** (11.880)	1.206*** (5.624)	1.233*** (5.349)	0.312*** (11.162)	2.052*** (7.517)	1.915*** (7.528)
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	0.561	0.567	0.568	0.697	0.711	0.713
N	202,564	164,389	164,389	101,169	82,955	82,955

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

Panel B. M&A activity

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 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. In Columns (3) and (6), we control for industry (two-digit SIC)*year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	0.008*** (6.692)	0.007*** (5.415)	0.007*** (5.533)			
Subsidiary-based leniency laws				0.007** (2.511)	0.007** (2.343)	0.007** (2.329)
Assets		0.009*** (12.335)	0.008*** (12.812)		0.009*** (6.025)	0.009*** (5.824)
Leverage		0.000 (1.317)	0.000 (1.403)		0.000 (1.495)	0.001* (1.675)
GDP per capita		-0.001 (-1.078)	-0.001 (-0.913)		-0.002 (-0.348)	-0.002 (-0.424)
Unemployment		-0.001*** (-5.046)	-0.001*** (-5.453)		0.000 (0.474)	0.000 (0.398)
Imports as a % of GDP		0.000** (1.976)	0.000** (2.048)		0.000 (0.910)	0.000 (0.856)
Constant	-0.007*** (-6.615)	-0.029*** (-3.322)	-0.026*** (-2.694)	0.006 (0.969)	-0.033 (-0.604)	-0.004 (0.092)
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	0.085	0.088	0.088	0.109	0.126	0.13
N	216,649	172,457	172,457	105,327	85,517	85,517

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

Panel C. M&A and capital expenditures

We consider all Compustat Global and North America firms over 1990-2012. This table reports OLS regressions, where the dependent variable is the change in the value of tangible and intangible assets, adjusted for depreciation, scaled by last year's assets. 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. In Columns (3) and (6), we control for industry (two-digit SIC)*year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Export market leniency laws	0.029*** (5.154)	0.041*** (8.567)	0.040*** (8.968)			
Subsidiary-based leniency laws				0.021*** (2.941)	0.037*** (4.660)	0.031*** (4.190)
Assets		0.075*** (19.905)	0.075*** (20.544)		0.089*** (14.479)	0.096*** (14.925)
Leverage		0.001* (1.809)	0.001** (2.032)		0.001 (0.603)	0.001 (0.884)
GDP per capita		-0.023*** (-2.973)	-0.025*** (-3.383)		-0.013 (-0.799)	-0.019 (-1.129)
Unemployment		-0.002** (-2.037)	-0.002** (-2.072)		0.003 (1.326)	0.002 (1.167)
Imports as a % of GDP		0.000 (-0.175)	0.000 (-0.352)		0.001 (0.889)	0.001 (1.247)
Constant	0.033** (2.163)	0.041 (0.556)	0.043 (0.593)	0.056*** (3.377)	-0.441** (-2.520)	-0.237 (-1.350)
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	0.098	0.127	0.129	0.249	0.264	0.281
N	166,810	136,143	136,143	93,505	76,269	76,269

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

Table 9. Horizontal and diversifying M&A

We consider all Compustat Global and North America firms over 1990-2012. This table reports regressions where the dependent variables are measures of a firm's M&A activity in a particular year. 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), the dependent variable is the total dollar value of acquisitions in the same industry (three-digit SIC) over the year, scaled by lagged assets and winsorized at 1%. In Column (2), the dependent variable 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%. In Column (3), the dependent variable is the total dollar value of acquisitions in the same industry (three-digit SIC) but located in a different country over the year scaled by lagged assets and winsorized at 1%. In Column (4), the dependent variable is the total dollar value of acquisitions in a different industry (three-digit SIC) over the year scaled by lagged assets and winsorized at 1%. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Leniency law	0.001** (2.585)	0.001*** (2.900)	0.000 (0.620)	0.366 (0.978)
Assets	0.002*** (9.112)	0.001*** (6.630)	0.000*** (3.473)	2.727*** (7.790)
Leverage	-0.000* (-1.704)	-0.000*** (-3.433)	-0.000*** (-3.174)	0.024 (1.103)
GDP per capita	-0.001** (-2.278)	-0.000* (-1.845)	0.000 (-1.461)	-1.116 (-1.388)
Unemployment	-0.000*** (-2.844)	-0.000*** (-3.074)	0.000 (-1.400)	-0.381** (-2.202)
Imports as a % of GDP	0.000 (-0.462)	0.001 (-0.950)	0.000 (0.856)	-0.012 (-0.647)
Constant	-0.001 (0.378)	0.000 (0.095)	0.000 (0.458)	-1.464 (-0.180)
Firm f.e.	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y
R-squared	0.093	0.090	0.089	0.170
N	434,616	434,860	432,425	434,860

Table 10. Customer reaction to M&A 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 M&A announcement estimated over a three-day [0,2] window, and winsorized at 1%, where the expected returns are estimated using domestic country and global market returns over a 180-day estimation period. Data for non-US firms for years 1998-2010 are used in this analysis. Only transactions where the acquirer and the target are in the same 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. In Columns (1)-(5), 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. *, **, 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)		
Investment		0.001 (0.859)	0.001* (1.665)	-0.001 (-1.228)	-0.001 (-1.100)	0.001 (0.860)	0.001 (0.831)
Profitability		0.001*** (5.035)	0.001*** (8.683)	0.001*** (9.214)	0.001*** (9.086)	0.001*** (5.127)	0.001*** (5.105)
Leverage		0.000 (0.549)	0.000 (1.232)	0.000* (1.835)	0.001* (1.919)	0.000 (0.540)	0.000 (0.508)
Assets		0.000 (-0.433)	0.000 (-0.807)	0.000 (0.572)	0.000 (0.663)	0.000 (-0.448)	0.000 (-0.476)
Constant	-0.002*** (-9.239)	-0.002* (-1.947)	-0.002*** (-3.662)	-0.005* (-1.819)	-0.006** (-1.999)	-0.002* (-1.956)	-0.002** (-2.131)
Deal f.e.		Y	Y	Y	N	N	Y
Year f.e.		N	N	N	Y	Y	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

Figure 1. M&A trends around leniency laws

We plot the total dollar value of acquisitions 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).

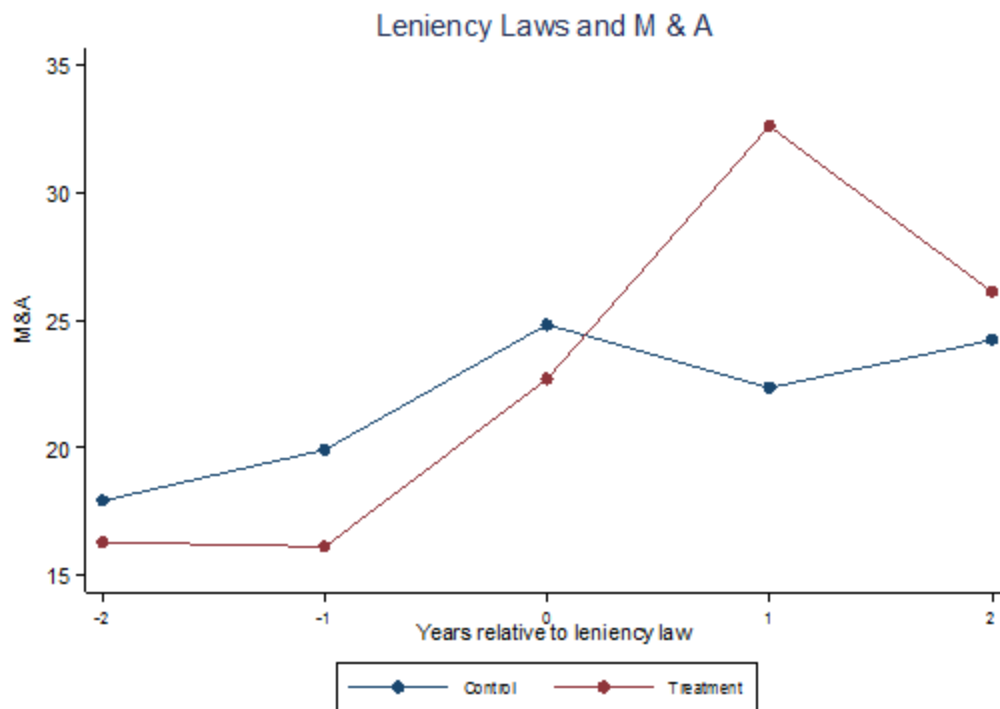


Figure 2. M&A activity and profitability

We plot the average gross margin for firms that were affected by a leniency law and pursued M&A 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 acquisitions in the first two years after the passage of a leniency law.

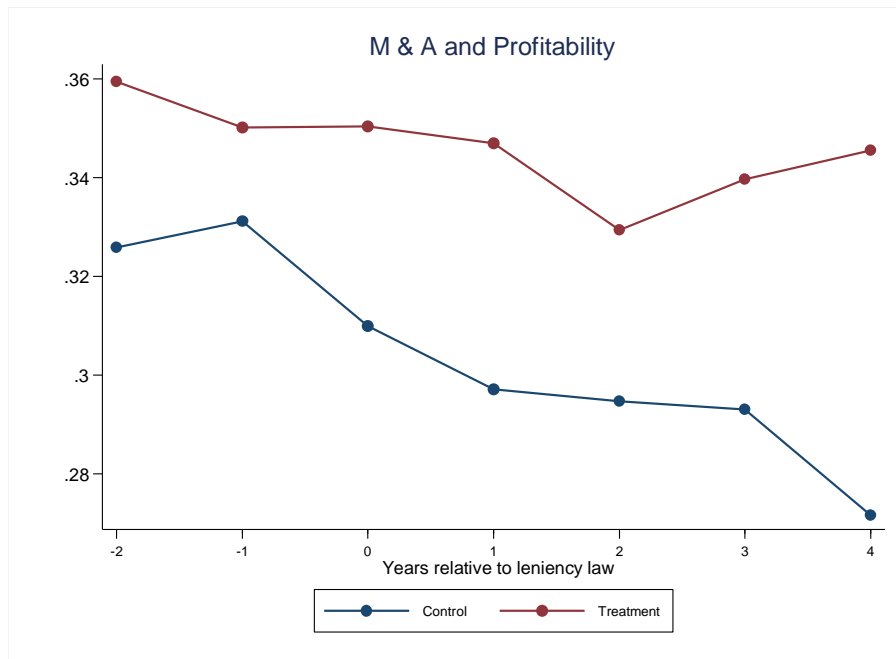
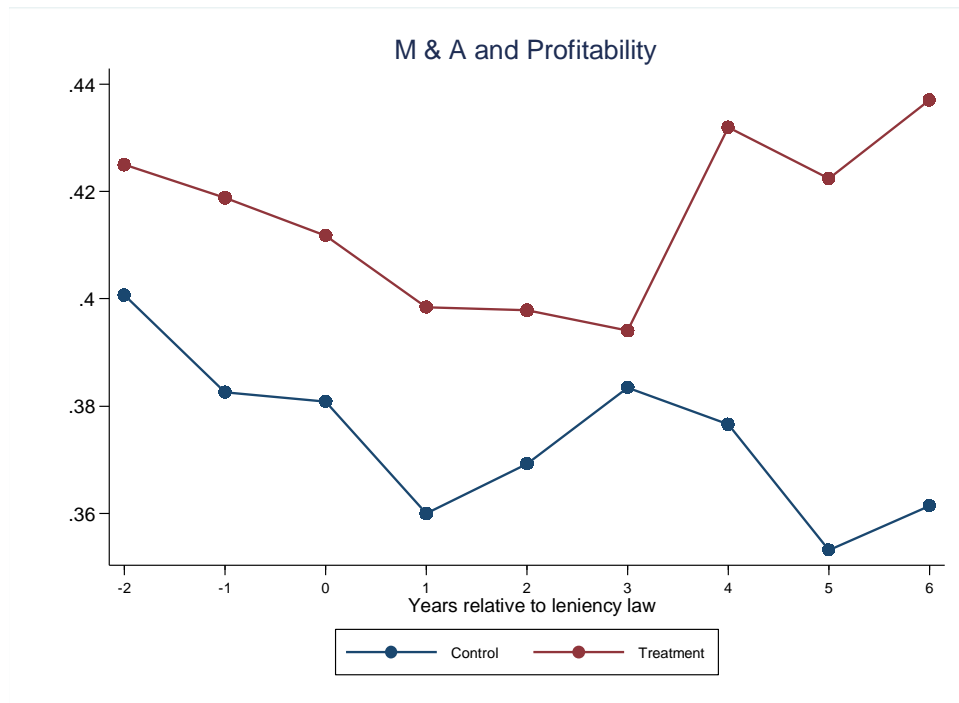


Figure 3. Failed M&A as controls and profitability

We plot the average gross margin for firms that were affected by a leniency law and pursued M&A 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 acquisitions in the first two years after the passage of a leniency law, but the acquisitions have failed.



Appendix 1. Leniency laws

This table reports leniency law passage by country and the observations from that country in our data. 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.

Country	Year	Obs.	Country	Year	Obs.
Argentina	None	925	Lithuania	2008	305
Australia	2003	20,082	Luxembourg	2004	416
Austria	2006	1,472	Malaysia	2010	12,848
Belgium	2004	1,843	Mexico	2006	1,681
Brazil	2000	4,492	Netherlands	2002	2,884
Bulgaria	2003	126	New Zealand	2004	1,623
Canada	2000	28,176	Nigeria	None	500
Chile	2009	2,124	Norway	2005	3,063
China	2008	27,416	Oman	None	604
Colombia	2009	349	Pakistan	2007	2,232
Croatia	2010	255	Peru	2005	931
Cyprus	2011	275	Philippines	2009	2,154
Czech Republic	2001	274	Poland	2004	3,454
Denmark	2007	2,335	Portugal	2006	838
Ecuador	2011	17	Romania	2004	357
Estonia	2002	181	Russia	2007	1,647
Finland	2004	2,060	Singapore	2006	8,066
France	2001	11,219	Slovakia	2001	75
Germany	2000	11,508	Slovenia	2010	253
Greece	2006	2,223	South Africa	2004	3,857
Hong Kong	None	3,654	Spain	2008	2,293
Hungary	2003	306	Sweden	2002	5,694
Iceland	2005	80	Switzerland	2004	3,596
India	2009	24,173	Taiwan	2012	14,395
Indonesia	None	3,775	Thailand	None	5,723
Ireland	2001	1,209	Turkey	2009	1,538
Israel	2005	2,601	Ukraine	2012	38
Italy	2007	3,643	United Kingdom	1998	28,168
Japan	2005	53,694	USA	1993	213,914
Jordan	None	828	Venezuela	None	222
Korea	1997	8,701	Zambia	None	81
Latvia	2004	270			

Internet Appendix

I. Predicting Leniency Laws

In Table IA1, we report the coefficients of the Cox proportional hazard model in which we predict the passage of leniency law in the country. The most consistent predictor is the economic development of the country as proxied by the log GDP. Right-wing affiliation of the chief executive has a negative effect, as compared to center- or left-wing orientation. Finally, the financial development of the country does not have a strong predictive power.

II. Robustness for Profitability

We report robustness checks for the gross margin results in Table IA2. The estimates in these tables follow the same structure as Table 4, Panel B. We report the results of additional robustness tests in which we control for the degree of competition in the firm's industry (Column (1)), other legislative changes, such as the introduction of the first competition law in the country (Columns (2)-(3)), the country's financial development (Column (4)) and the passage of other antitrust legislation (Column (5)). Our main results on profitability hold. Our results are unaffected by the possible strategic relocation effects (Column (6)).

We also plot the effect of leniency laws for the period ranging from the two years before to the three years after the passage of the law. We display the average gross margin for firms that were affected by leniency laws as well as for a sample of control firms. As a control sample, we consider firms that did not face the introduction of the leniency laws over the same period as the treated firm (i.e., firms that did not face the introduction of leniency laws over the period of two years before to three years after the

introduction of leniency law for the treated firm). The plot depicted in Figure IA1 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.

III. Within Industry and Region Estimates

We replicate Table 4 by estimating all of the regressions using industry*region*year fixed effects (i.e., performing estimations within region and industry). We consider two-digit SIC industries and seven geographic regions: North America, Latin America, Western Europe, Central and Eastern Europe, Asia, Africa, and Oceania. We find that our estimates are consistent if we use this estimation.

IV. Randomization of Laws and Standard Errors

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 2, Column (1), Table 3, Column (1), and Table 4, 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 IA2. The first figure presents the coefficients in conviction regressions as in Table 2, Column (1), the second figure provides the coefficients in

profitability regressions as in Table 3, Column (1), and the third figure reports the coefficients in M&A regressions. The figures demonstrate that our actual coefficients are smaller in 4,975 of 5,000 (99.5%) randomized samples in the case of the effect on conviction, 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 M&A. The non-parametric nature of this analysis suggests that the specification of the error covariance matrix does not affect our results.

Table IA1. 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. *, **, 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 IA2. Robustness for 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 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 HHI in the firm's industry (three-digit SIC) in its country. In Column (2), we control for the introduction of competition law. In Column (3), we restrict the sample to countries that passed competition law in or before 1990. In Column (4), we control for a country's financial market development. In Column (5), we control for other ways in which anti-cartel legislation was strengthened. In Column (6), we restrict the sample to firms that do not change their headquarter countries. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	-0.051**	-0.050**	-0.047*	-0.047**	-0.057***	-0.052**
	(-2.092)	(-2.089)	(-2.066)	(-2.157)	(-4.204)	(-2.125)
Assets	0.080***	0.080***	0.088***	0.080***	0.084***	0.078***
	(6.809)	(6.799)	(9.919)	(6.671)	(8.507)	(6.517)
Leverage	0.000	0.000	0.000	0.000	0.000	0.000
	(-0.583)	(-0.595)	(-0.765)	(-0.509)	(-0.311)	(-0.506)
GDP per capita	-0.121***	-0.119***	-0.154***	-0.145***	-0.105*	-0.132***
	(-3.508)	(-3.568)	(-3.531)	(-3.847)	(-1.994)	(-2.955)
Unemployment	0.007	0.007	0.01	0.010*	0.002	0.007
	(1.560)	(1.597)	(1.581)	(1.990)	(0.433)	(1.529)
Imports as a % of GDP	-0.004**	-0.003**	-0.006**	-0.004**	-0.008**	-0.003*
	(-2.220)	(-2.109)	(-2.120)	(-2.414)	(-2.066)	(-1.831)
HHI	-0.011					
	(-0.754)					
Competition law		-0.033				
		(-1.338)				
Chinn-Ito index				0.061		
				(0.560)		
Stock market capitalization to GDP				0.000		
				(1.329)		
Private credit to GDP				0.000		
				(0.558)		
Increase in penalties					0.076**	
					(2.133)	
Change in investigative powers					0.068*	
					(1.994)	
Change in cartel definitions					-0.058	
					(-1.097)	
Other cartel laws					0.025	
					(0.343)	
Constant	1.108***	1.115***	1.402***	1.232***	1.034	1.220***
	(3.298)	(3.375)	(2.819)	(3.237)	(1.686)	(2.881)
Firm f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
R-squared	0.531	0.531	0.531	0.53	0.535	0.534
N	404,107	404,107	333,609	394,104	347,994	398,173

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

Table IA3. M&A activity: within industry and region estimates

Panel A. Differences-in-differences estimation

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, in which the dependent variable is the total dollar value of acquisitions over the year scaled by lagged assets and winsorized at 1%. All of the regressions include firm fixed effects and industry (two-digit SIC)*geographic region*year 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. In Column (3), we cluster standard errors at country*industry (three-digit SIC) level. In Column (4), we remove financial institutions (SIC codes 6000-6999) from the sample. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Leniency law	0.005** (2.114)	0.005** (2.329)	0.005*** (7.151)	0.005** (2.22)
Assets		0.009*** (5.493)	0.009*** (10.086)	0.010*** (5.206)
Leverage		0.000 (1.196)	0.000 (1.535)	0.000 (1.252)
GDP per capita		-0.001 (-0.42)	-0.001 (-0.756)	-0.001 (-0.575)
Unemployment		-0.001*** (-3.194)	-0.001*** (-6.705)	-0.001*** (-3.108)
Imports as a % of GDP		0.000 (-0.568)	0.000 (-1.071)	0.000 (-0.597)
Firm f.e.	Y	Y	Y	Y
Industry*region*year f.e.	Y	Y	Y	Y
R-squared	0.117	0.125	0.125	0.122
N	540,930	431,588	431,588	393,676

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

Panel B. Additional robustness checks

We consider all Compustat Global and North America firms over 1990-2012. This table reports the OLS regressions, in which the dependent variable is total dollar value of acquisitions over the year scaled by lagged assets and winsorized at 1%. All of the regressions include firm fixed effects and industry (two-digit SIC)*geographic region*year 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 HHI in the firm's industry (three-digit SIC) in its country. In Column (2), we control for the introduction of competition law. In Column (3), we restrict the sample to countries that passed competition law in or prior to 1990. In Column (4), we control for the country's financial market development. In Column (5), we control for other ways anti-cartel legislation was strengthened. In Column (6), we restrict the sample to firms that do not change their headquarter countries. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.005** (2.328)	0.005** (2.312)	0.008** (2.56)	0.005** (2.639)	0.007* (2.299)	0.005** (2.353)
Assets	0.009*** (5.712)	0.009*** (5.715)	0.010*** (5.771)	0.009*** (5.511)	0.010*** (5.817)	0.009*** (5.699)
Leverage	0.000 (0.964)	0.000 (0.966)	0.000 (1.018)	0.000 (1.389)	0.000* (1.847)	0.000 (1.074)
GDP per capita	-0.001 (-0.437)	-0.001 (-0.511)	0.003 (1.495)	0.000 (-0.18)	0.009 (1.307)	-0.001 (-0.711)
Unemployment	-0.001*** (-3.197)	-0.001*** (-3.3)	-0.002*** (-4.447)	-0.001*** (-2.875)	-0.001*** (-2.949)	-0.001*** (-3.149)
Imports as a % of GDP	0.000 (-0.581)	0.000 (-0.999)	0.000 (0.585)	0.000 (-1.452)	0.000 (0.764)	0.000 (-0.561)
HHI	0.000 (-0.571)					
Competition law		0.003 (1.092)				
Chinn-Ito index				-0.004 (-1.145)		
Stock market capitalization to GDP				0.000** (2.476)		
Private credit to GDP				0.000 (-1.63)		
Increase in penalties					-0.003 (-1.51)	
Change in investigative powers					-0.001 (-0.523)	
Change in cartel definitions					-0.009** (-2.078)	
Other cartel laws					0.011*** (3.239)	
Firm f.e.	Y	Y	Y	Y	Y	Y
Industry*region*year f.e.	Y	Y	Y	Y	Y	Y
R-squared	0.125	0.125	0.128	0.126	0.126	0.125
N	431,588	431,588	352,868	420,075	368,609	424,844

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

Figure IA1. Profitability trends around leniency laws

We plot the average gross margin for firms that were affected by leniency law for the period from two years before to three 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 three years after the introduction of a leniency law for the treated firm).

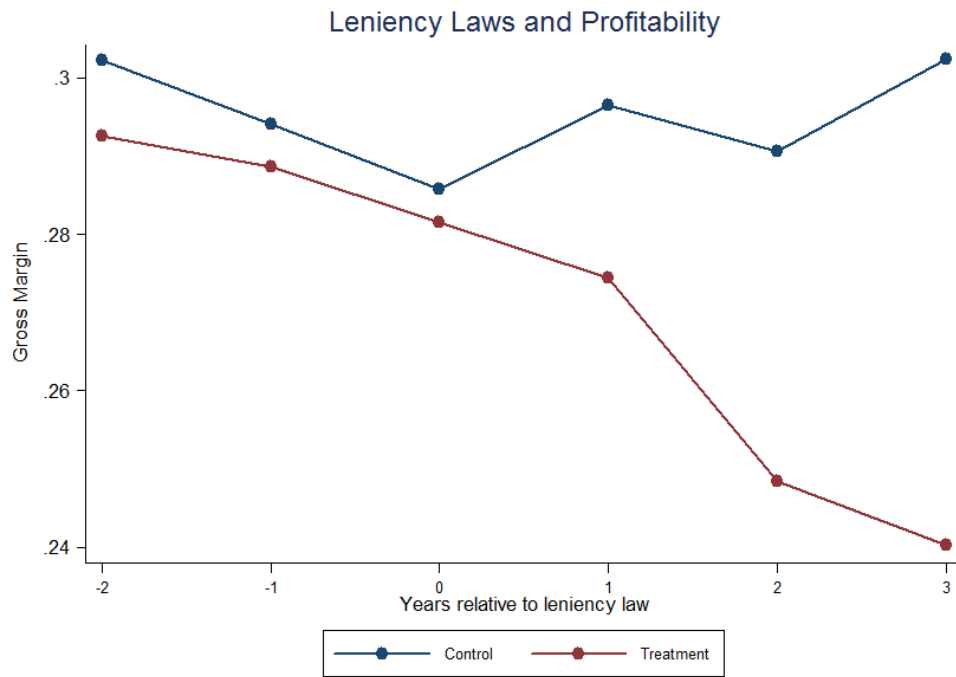


Figure IA2. Simulated distributions of leniency laws

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 illustrates the coefficients on conviction as in Table 2, Column (1). The second figure presents the coefficients on profitability as in Table 3, Column (1). The third figure reports the coefficients on M&A as in Table 4, Panel A, Column (1). Vertical lines indicate the coefficient from the actual regression.

