

Busted! Now What?

Effects of Cartel Enforcement on Firm Value and Corporate Policies

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Abstract

In a cross-country study we look at the staggered passage of national leniency laws over 1990-2012. We show that these laws lead to more cartel convictions, and generally increase the costs of collusion by reducing the average gross margins of the affected firms. We further examine how changing costs of collusion shape firm boundaries and show that firms reorganize their activities by engaging in more horizontal acquisitions, both in the roles as the acquirer and the target. These acquisitions tend to be associated with higher announcement returns. We find little evidence of the increase in strategic alliances or greenfield investments.

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Introduction

Recent antitrust cases such as the LIBOR scandal seem to suggest that product market collusion has not become a thing of the past and has spread out from smoke-filled rooms to online chat rooms. In fact, countries around the world have been exerting significant antitrust efforts to combat it. For instance, the Economist (2014) writes that “cartel enforcement is a hot topic in boardrooms. Fines and jail terms have shot up in recent years, greatly raising the costs of collusion. Big firms such as GE and Bosch have assembled teams of in-house lawyers that focus solely on the issue.”

This renewed interest in the topic does however raise the question of the effectiveness of cartel enforcement strategies and their unintended consequences. One may expect that an increasing cost of collusion leads firms not only to expand their legal teams but also to change their strategies. And, indeed, anecdotal evidence from the passage of the Sherman Act in the United States suggests that cartel prohibition led to rearrangements of the firms’ organizational structures and even to merger waves (e.g., Bittlingmayer, 1985). This unintended effect is worrisome: if mergers can provide a solution out of the cartel prohibition, the very *raison d’être* of cartel prohibition can be called into question.

We look at whether this anecdotal evidence is confirmed in the data and if so, which firms react to cartel prohibition by changing their strategies. We focus on global price-fixing cartels over the period 1990-2012. As an exogenous variation of costs of collusion, we exploit a staggered passage of leniency legislation around the world. Leniency programs have been one of the most important developments for cartel detection and deterrence (Chen and Rey, 2013). By allowing reduced fines or even providing immunity for the cartel members that collaborate in the conviction cases, leniency laws are expected to increase the costs of forming cartels and the benefits of breaking them up. A staggered nature of the passage of these laws allows us to identify their causal effect as we are able to compare the change in corporate strategies of firms that were affected by the law to the contemporaneous change in strategies of the control group of firms that were headquartered in the countries that have not yet passed such a law.

Our first contribution is to show that leniency laws have indeed reduced collusive practices. We exploit a difference-in-difference setting based on the staggered passage of leniency laws in 63 countries. As countries passed leniency laws at different points in time between 1993 and 2011, we are able to identify their causal effect on the firm operating performance. After the passage of leniency laws, the gross margin of the affected firms decreases by 5.1 percentage points (relative to the control group), a 14.8% drop compared to the average gross margin before the passage of leniency laws of 34.5%. Moreover, the negative effect on profitability is larger in the industries where collusion is expected to be less stable. This provides the first (to the best of our knowledge) evidence that the passage of leniency laws has adverse effects on the firm performance.

Our second contribution is to document the corporate reaction to the cartel busting – i.e., which actions the firms take after the collusion costs increase. We argue that higher collusion costs should change the incentives to redefine the boundaries of the firm and induce new corporate policies. We exploit the same differences-in-differences strategy of leniency law passage around the world and show that the restrictions on the ability to create a cartel increase the incentives to engage in M&As. The passage of the leniency law raises the probability that a firm becomes an acquirer by 4% and that it becomes a target by 2%. Even more importantly, restrictions on the ability to run a cartel make it more likely that the firm will be acquired by another firm that operates in the same industry and country. We find no such effect for diversifying acquisitions, either geographically or sector-wise. Also, in contrast to the findings on M&As, we find no evidence that firms react to the passage of leniency laws by resorting to strategic alliances, or pursuing greenfield investments by increasing capital expenditures.

How does the market react to these cartel-busting related M&As? We find that the effect is positive. In particular, we rely on the complete hand-collected information on the sanctions imposed on the 7,496 members of 746 large international cartels and find a positive relationship between either the target or the acquirer being convicted in a cartel case over the prior 5 years and the cumulative abnormal return, estimated over a 3 day [0,2] window around the M&A

announcement. The magnitude of the effect corresponds to 0.5% price increase in the case the acquirer has been convicted and to a 1.5% increase in the case the target has been convicted.

One interpretation of our results is that the passage of the laws that make collusion harder reduces product prices and markups in the industry and induces firms to increase output to cover their fixed costs. One way to quickly realize economies of scale is to merge with industry peers. At the same time, affected firms may engage in mergers in order to recreate the market power they used to have. This would suggest exploiting revenue synergies. To distinguish these two alternatives, we perform three sets of tests. First, we look at the type of M&A activity that follows after the passage of the leniency laws. Second, we explore the heterogeneity of the responses and study the subsamples in which we expect the need to exploit synergies to be more important – i.e. firms in the industries that operate at increasing returns to scale. Finally, we look at the customer reaction to M&A deals. All the tests agree to show that the affected firms engage in M&As to gain cost synergies to cover their fixed costs. Such findings have important normative and policy implications. Indeed, they suggest that the cartel-busting related M&As do in fact benefit both shareholders and customers alike.

We contribute to several strands of literature. First, we contribute to the literature on the way firms expand and grow. We provide evidence on how such an expansion path is related to anti-trust regulations across countries. This also allows us to contribute to the determinants of alliances and M&As on a world scale (Rossi and Volpin, 2004). In this regard, we also relate to the literature on merger waves, suggesting that some of the waves can be fostered by stronger constraints to collusion. This is in line with the findings of Mitchell and Mullherin (1996) and Harford (2005) that merger waves are triggered by industry shocks. We complement Harford (2005), who finds that deregulation fostered merger waves. In our case, we find that the reverse is also true – i.e., that the introduction of certain regulations might also lead to an increase in merger activity. We also contribute by finding that M&As triggered by cartel-busting decisions have positive effects also for the acquirer. That is, we find that this is a relatively rare case of M&A activity in which the market appreciates the decision of the buyer to engage in an acquisition. Finally, our results showing that cartel-induced mergers are driven by efficiency

gains (or the reconstruction of collusion) contribute to the recent debate in the merger wave literature (Fee and Thomas, 2004; Shahrur, 2005; Bhattacharyya and Nain, 2011; Maksimovic and Phillips, 2001).

Second, we contribute to the empirical literature on the boundaries of the firm. Ever since Coase (1937), the theoretical literature has discussed what defines the boundaries of the firm and how they evolve over time. This issue has not received adequate attention in the empirical analysis, possibly because of the inability to observe exogenous sources of variations in the firm's boundaries. We take 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. Robinson (2008) has shown in which cases strategic alliances are a preferred way of creating integrated firms. We show that exogenous shocks to the formation of informal and secret "alliances" between firms – the cartels – lead to more mergers.

Finally, and more specifically, we contribute to the literature on the effects of leniency laws. Recent empirical literature has mainly shown that leniency laws have a positive effect on competition. For example, Miller (2009) shows that US leniency program increased cartel deterrence and enhanced detection, while Borrell, Jiménez and García (2013) document that cross-country leniency law passages have improved the managerial perception of the competition in the affected countries. We contribute by establishing the link between the passage of leniency laws and firm profitability. In addition, we are the first, to the best of our knowledge, to show how firms react to leniency laws. In doing this, we thus contribute by providing empirical evidence to the theory literature on the corporate effects of collusion (e.g. Maksimovic, 1988; Spagnolo, 2001).

The remainder of the paper is organized as follows. Section II describes our data. Section III describes leniency laws and the identification strategy that we use. Section IV validates the identification strategy by showing the effect of leniency laws on cartel stability. Section V relates cartel restrictions to investment and the firm expansion choice (M&A and alliances) and how the market evaluates the corporate policies as a reaction to cartel-busting restrictions. Section VI discusses the explanations for these effects. A brief conclusion follows.

II. Data and Main Variables

In our analysis we consider all the firms in Compustat Global and North America datasets over 1990-2012. Our initial sample covers 543,737 firm-years.

We collect information on the passage of leniency laws in 63 large countries from the Cartel Regulation 2013, published by Getting the Deal Through. We manually double check this information and complement it using press releases and news articles. We report the years when leniency laws were passed in Appendix 1.

The data on international expansion choices come from the Securities Data Corporation (SDC) Database, from which we extract all the alliances and M&As in the world for the period between 1990 and 2012.

We then relate these data to the accounting information from Compustat Global and North America, as well as to the stock returns from Datastream for the period of 1998-2010.

We consider all the alliances and M&A transactions. We define alliances as all the partnership agreements and joint ventures in which two or more entities combine the resources to form a new, mutually advantageous business arrangement to achieve predetermined objectives. This includes joint ventures, strategic alliances, research and development agreements, sales and marketing agreements, manufacturing agreements, supply agreements, and licensing and distribution agreements. Following Rossi and Volpin (2004) and Bris and Cabolis (2008), we only look at the M&A transactions for the majority of the shares of the target firm – i.e., the ownership percentage after the deal is above 50%, and where the deal is completed by the end of our sample period. We exclude from the sample repurchases, minority stake purchases, and privatizations.

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 the major private international cartels discovered, disclosed and sanctioned by regulators around the world since January 1986. The dataset omits the cartels for which no sanctions were imposed within five years of the authorities' discovery. It contains 746

cartels involving 7,496 firms (some firms are recidivists and are thus members of multiple cartels).

The data has been collected by reading filings, documents, reports, and press releases from the antitrust authorities in different countries, as well as newspaper and magazine articles retrieved through search engines like Factiva or Lexis-Nexis. The dataset reports the involved firms, their executives (if they are personally prosecuted), the country of incorporation, the markets and continents in which collusion took place, the duration of the collusive agreement, and, if known, the fines imposed, the leniency granted by the regulators, and the estimated overcharges to the consumers. We manually name-match the firms to the Compustat Global and North America datasets 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 in Table 1. The median (mean) cartel involves five (ten) companies and lasts five (seven) years before it gets discovered by the regulator. These cartels have a global nature and are particularly large. The estimated median (mean) dollar value of affected commerce is \$2bn (\$26.7bn).

One typical example of a cartel is the arrangement by which Argos and Littlewoods, two UK retailers, fixed prices for some of the children's toys with the help of their manufacturer, Hasbro. The cartel was set-up in 1999 and lasted for 27 months. Britain's leniency laws made Hasbro come forward in 2002 and provide incriminating evidence in return for having a potential \$9.8m fine waived. The retailers were eventually fined \$27.5m and \$8.5m, respectively, which was a UK record, according to *The Economist* (2003).

In Appendix 2, we report preliminary evidence that breaking a cartel has significant effects on the profitability and value of the firm. We find that cartel conviction reduces gross margin by 3.6 percentage points. Moreover, the stock price of the convicted firm drops by 0.7% over a three-day period after the conviction and on average by 2.8% in the first month after the probe has been initiated by the first antitrust authority. The latter effect is even larger if we consider the resolution of the case – i.e. when the first penalty has been awarded. We find no reversal over the longer period.

III. Identification Strategy

One way to study the effect of price-fixing on the firm strategies would be to look at the convicted cases. Although they might be telling, they could also be endogenous to a number of unobservable factors such as the expected industry's profits, competitor or employee whistleblowing and lobbying by consumer pressure groups. Our identification strategy thus relies on the staggered passage of leniency laws around the world. In this section, we briefly describe the leniency laws as well as an alternative identification strategy that we use for the robustness of our analysis.

A. Leniency Laws

Leniency laws allow the courts and/or regulators to grant full or partial immunity to the companies that have participated in illegal cartels but cooperate in providing information about the cartel. The United States was the first country to pass leniency laws in 1973, but they remained largely ineffective until 1993. In 1993, the leniency laws were revised and strengthened by making the case for amnesty clearer and broader. The revised law posits that, if no investigation of cartel is underway (or if Department of Justice does not have sufficient evidence), the first self-reporting cartel member, including its managers, employees and directors, gets automatic amnesty. The revised nature of the law proved to be successful in destabilizing the existing cartels and in deterring new cartel formation and has thus inspired a number of other countries to pass similar laws (Hammond, 2005).

The passage of the leniency laws¹ over the past twenty years has been highly influenced by the continuing attention given to the issue by the United States and the European Union (Lipsky, 2009). For instance, on one occasion, the United States has pressed Singapore for strengthening of Singapore's competition law regime. The European Union has fostered the adoption of leniency laws in its member States and it also often seeks similar provisions in its various bilateral association and trade agreements. IMF and the World Bank impose the overhaul of

¹ In some cases, most notably US, ineffective leniency laws got *strengthened* rather than *introduced* during this period. For the sake of brevity, throughout the paper we use these words interchangeably.

antitrust laws as the condition for loans and other funding (Bradford, 2012). Such external pressure makes the passage of leniency laws rather exogenous to political and economic conditions in the passing country.²

We hereby describe a typical leniency law, using the one adopted by the European Commission (EC), as described by Competition Directorate (2013). In order to obtain total immunity under the leniency policy a firm which participated in a cartel must be the first one to inform the EC of an undetected cartel by providing sufficient information to allow the EC to launch the investigation. If the EC is already in possession of enough information to launch the investigation, the firm must provide evidence without which the EC would be unable to prove the existence of the cartel. In all cases, the firm must fully cooperate with the EC throughout its procedure, provide it with all the evidence in its possession and stop its participation in the cartel immediately. Also, it cannot disclose the existence and the content of the investigation to any other firm. The firm may not benefit from 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 represents "significant added value" to that already in the EC's possession – i.e. it reinforces the EC's ability to prove the existence of the cartel. The first firm to meet these conditions is granted a 30-50% reduction, the second is granted a 20-30% reduction, while the subsequent firms are granted an up to 20% reduction.

The impact of the leniency laws on the cost of collusion depends on the firm incentives to apply for leniency. For example, information about potential or ongoing antitrust investigation might leak from the antitrust authority, providing a strong incentive to apply for leniency. Also, a new management might learn about past involvement in the cartel and consider that as reputationally damaging for them or as a potential liability for the firm. Moreover, the incentives

² One may argue that free trade agreements might have a similar effect on market structure as cartel busting. Aware of Singapore's case, we carefully control for country's levels of trade and 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 the trade deal. Finally, most trade agreements are regional. Controlling for region*year fixed effects also does not affect our results.

to apply for leniency in one sector may be related to cartel busting in others. Indeed, firms compete on multiple product markets and might have formed multiple cartel activities. The investigation of cartel in one product market increases the probability of the cartel detection in the other. For example, in 1999, United States introduced Amnesty Plus and Penalty Plus programs. Even if the firm cannot apply for leniency in the cartel case in which it is already being investigated, under the Amnesty Plus program it can reduce the fine in the ongoing investigation by disclosing the information about its collusion in other product markets (for which it would receive full amnesty). However, if it does not do that and the second cartel is discovered, the Penalty Plus program enhances the severity of the penalties in both investigations.

Theoretically, the effect of the leniency laws on the costs of collusion is uncertain. On the one hand, leniency laws destabilize the cartels as they reduce the firm's costs of defection and potentially increase the costs of the rivals if the firm is able to impose fines on them (Ellis and Wilson, 2001; Harrington, 2008). On the other hand, *ex ante* the costs of collusion might decrease if the firms take into account the reduced fines (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 thus renegotiation of prices – more difficult (McCutcheon, 1997). Finally, Marx and Mezetti (2014) argue that cartels optimally respond to the introduction of a leniency program by reducing the amount of information that each individual cartel member has access to, so that it does not constitute sufficient evidence to support leniency application. This would mitigate the effects of the leniency program.

B. Estimation

In our estimation, we rely on a staggered nature of the passage of these laws that allows us to identify their causal effect. In particular, we are able to compare the change in corporate strategies of firms that were affected by the law to the contemporaneous change in strategies of the control group of firms that were headquartered in the countries that have not yet passed such

a law. We use the standard approach used in the literature that relies on a staggered passage of laws in US states – e.g., business combination laws as in Bertrand and Mullainathan (2003).

Using firm-level data, we estimate our baseline estimation:

$$Outcome_{ist} = \alpha + \beta Leniency Law_{kt} + X_{ikt} + u_i + v_t + \varepsilon$$

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 the leniency law, X_{ikt} represents a vector of control variables, while u_i and v_t are firm and year fixed effects, respectively.

The use of firm fixed effects allows us to fully control for non-time varying differences between different firms (and thus countries in which they are located). For example, let us consider Germany passing the leniency law in 2000. We compare how firm corporate strategies have changed for Germany 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 the leniency law on a different date – e.g., 2007 in the case of Italy.

C. Additional Identification

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 the firm’s country. So, in addition to exploiting the passage of leniency laws in the firm’s headquarter country, we also rely on an additional identification strategy which is based on the passage of laws in those countries whose firms have a high market share in the examined firm’s industry. If the country of firm’s largest competitors passes the leniency law, the firm might find it more difficult to form and sustain international cartels. These industry peers would find it easier to apply for leniency and thus the passage of other country’s leniency laws also increases the costs of collusion.

This additional variable that we call “global leniency law passage” is defined as the weighted average of the passage of laws in all the other countries, excluding the country of the firm’s headquarter:

$$Global\ leniency\ law_{t\hat{k}j} = \sum_{k \neq \hat{k}} w_{kj} L_{kt},$$

where k indexes countries, (\hat{k} is the country of firm i), j indexes three-digit SIC industries and t indexes time. L_{kt} takes the value of 1 if the leniency law is passed in country k by year t . The weights w_{kjt} are equal to the share of each country's output in the total global output of firm i 's three-digit SIC industry j , as reported in the Compustat in year 1990. The variable ranges from 0 when leniency laws are not passed in any countries with any market share in the firm's industry to 1 when all foreign countries with any share in firm's industry have passed the leniency law.

For instance, German car manufacturers produce 24.7% of the global output of car manufacturing firms. So, the passage of a German leniency law in 2000 should have increased the value of *Global leniency law* for all non-German car manufacturers by 24.7%. This variable is more exogenous to the political and economic conditions in the firm's country.³ The results that use this variable are equally robust in terms of both statistical and economic significance.

IV. Effect of Leniency laws on Cartel Stability

We now validate our identification strategy. We start by studying the effect of the passage of leniency laws on cartel conviction. Then, we look at how leniency laws affected firm profitability.

A. Leniency Laws and Cartel Convictions

We now investigate whether the passage of the leniency laws predicts the conviction of cartels in that country. We define the cartel market to be either the geographic market in which the cartel is operated (e.g., Italy, global), or the jurisdiction that investigated or sanctioned the cartel (e.g., the European Commission), whichever of the two is a smaller region.

We estimate a country-level panel specification defined for the years 1990-2012. Since the total number of convictions in the country is a count variable that takes the value of zero with high frequency (82% of the observations are zero), we hypothesize that the expected number of convictions is an exponential function of the leniency law treatment and estimate a Poisson

³ The correlation between the two treatment variables is -0.08.

model (e.g., Hausman, 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 results in Table 2. In Column (1) of Panel A, we document that the passage of a leniency law more than doubles the number of convicted cartels, by increasing them by 154%. This result is robust to controlling for the time-varying macroeconomic conditions in the country (Column 2).⁴

One potentially contaminating effect is that many European countries are both governed by EU and national antitrust laws. Therefore, as a robustness check, in Column (3), we report the results of the analysis in which we consider all the EU countries as if they were one country and consider the passage of EU leniency laws rather than the passage of the individual country leniency laws. The results are similar.

Column (4) reports a fixed effect OLS regression, while Column (5) reports a specification that also controls for regional economic effects and other trends⁵ by adding region*year fixed effects. Also these specifications deliver consistent results.

In Panel B, we further investigate the robustness of our results. We first look at the presence of any pre-existing trends. Column (1) reports the results of a placebo analysis in which, for all the countries, we anticipate the passage of the law by three years. We find that such placebo treatment does not have an influence on the cartel conviction.

⁴ We focus on GDP per capita as this appears to be the most consistent predictor on how fast the country adopts leniency law if we fit a Cox proportional hazards model with a number of macroeconomic variables, political variables such as the political orientation of the governing party, and region fixed-effects.

⁵ We allocate countries into seven geographic regions: North America, Latin America, Western Europe, Central and Eastern Europe, Asia, Africa and Oceania.

Similarly, in Column (2) we add time dummies that span three years before the passage of leniency law, as well as six to three years before the passage of leniency law. If these variables were significant, this would suggest the presence of pre-existing trends and this would violate the parallel trend hypothesis. Reassuringly, we do not find that this is the case.

In Column (3), we perform a similar exercise by looking at how quickly the effect dissipates over time. We create dummies spanning different windows: the first three years after the passage of the leniency law, the period between year 3 and year 6 after the passage of the law and the period between year 6 and year 9 after the passage of the law. We find that the effect of the passage of the law is concentrated in the first six years after the passage of leniency law, when presumably the conviction of the cartels that existed before the passage of the laws takes place. Later on, the cartel activity should decrease because of deterrence effect, and thus fewer cartels are convicted, compared to the initial rise (in line with Miller (2009)).

In Column (4), we also control for the introduction of competition laws that outlaw price fixing and other cartel arrangements. Indeed, some countries (e.g. Denmark) introduced competition laws over our study period. Although, as expected, the introduction of competition laws is also an important determinant for cartel conviction, still leniency laws retain their own distinct impact on cartel stability. In Column (5), we limit the sample to the countries that had competition law in place before the start of our sample period in 1990 and again the results are consistent.

Next, we investigate the changes in the type of convicted cartels. In particular, we relate the size of the convicted cartels to the passage of the leniency laws. We report the results in Panel C. We define the size of the cartel in terms of the total sales of the “cartelized market” (also known in the literature as the dollar value of the cartel-affected commerce). In Columns (1)-(3), we show that leniency laws lead to the prosecution of the larger cartels. In particular, after the passage of leniency laws the dollar value of convicted cartel-affected commerce is higher by 134%. It is interesting to note that, while considering EU leniency law leads to a smaller economic effect than individual country leniency laws in terms of the number of convicted cartels (Panel A), the economic effect in terms of the convicted cartel-affected commerce is

much larger. In this case, Column (2) of Panel B shows that leniency laws lead to 190% more damaging cartels being prosecuted.

Also, in Columns (4)-(5), we show that the mean pre-conviction market share of the convicted cartels is larger by 7.3 percentage points after the passage of the leniency laws if compared to the pre-passage period and the number of convicted members of cartel is higher by a factor of eight.

Overall, these results not only validate our difference-of-difference strategy, but they also provide, to the best of our knowledge, the first empirical evidence of the effect of the global passage of leniency laws on the cartel conviction.

B. Profitability

Next, we employ a difference-in-difference methodology to investigate the effect of the passage of the leniency laws on the gross margins of the affected firms. Given that only about 10% to 30% of all cartel conspiracies are discovered (Connor, 2014), and it is likely that leniency laws might have affected the stability of the undiscovered cartels, we focus on the general effect on the firm profitability.

We consider all the Compustat Global and North America firms within our sample period, and estimate the effect of the passage of leniency laws. If cartel stability increased following leniency laws and colluding firms raise margins more when detection probability is higher, we should expect a positive effect on the firm profitability. If, on the contrary, cartel stability decreases, we should expect a drop in firm profitability.

These tests based on the passage of leniency laws are more powerful in detecting the effects of the passage of the leniency laws on cartel stability than those based on the cartel convictions. The results on profitability also capture the effects of unobservable breakups of existing cartels and lower probability of their formation. In addition, these tests also capture the effects of leniency laws on the tacit collusion among affected firms if there was any substitution from explicit to tacit collusion after the passage of leniency laws.

We first plot the effect of leniency laws for the period ranging from the 2 years before to the 3 years after the passage of the law. In particular, we show the average gross margin for the firms that were affected by the leniency laws as well as for a sample of otherwise similar control firms. As a control sample, we consider the firms that were not affected by leniency law over the same period as the treated firm – i.e. control firms did not face the introduction of leniency laws over the period of 2 years before to 3 years after the introduction of leniency law for the treated firm. The plot depicted in Figure 1 shows clear trends in diverging profitability after the passage of the leniency laws.

Next, we move to regressions specifications and report our baseline difference-in-difference estimations in Columns (1)-(2) of Table 3, Panel A. 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 the leniency laws lowers gross margins by 5.1 percentage points. This represents a 14.8% drop with respect to the average sample gross margin before the passage of leniency laws of 34.5%. These results provide evidence that leniency laws have a sizable negative effect on profitability.

Our results are robust, both in terms of statistical significance and economic magnitudes. Column (3) includes an additional global leniency law variable that measures the passage of the laws in the countries whose firms have a high market share in the firm's industry. As firm's competitors get more accustomed with the leniency laws, the firm risks that the rivals will apply for leniency if it enters into the cartel with them. Thus, the higher is the market share of rivals with the existing leniency laws in their headquarter countries, the lower is the chance for the firm to form a stable international cartel. We find that the effect of this variable is comparable to that of the passage of the leniency laws in the firm's country. That is, if the leniency laws were passed in the same year in all the countries whose firms have a high market share in the firm's industry, the firm's gross margin would drop by additional 6 percentage points.

In Column (4), we look at the speed of the impact following the passage of the law. We therefore use dummies spanning the first three years after the passage of the leniency law as well as the years 3 to 6 after the passage and the years 6 to 9 after the passage, as well as all following

years. We find that the effect is mainly present in the first three years after the passage of leniency law, when presumably most of the cartels break or are convicted. In the following periods, after the cartel activity has already decreased, fewer cartels break or are convicted.

In Column (5), we replace our measure of profitability with the returns on assets (ROA). We find statistically similar results. However, in economic terms, we find even larger effects, compared to the mean level of profitability measure.

We continue with the robustness checks in Table 3, Panel B. First, in Row (1) we 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 removes 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 events affecting the industry (e.g., a general change in industry's international trade levels).

Since US firms constitute a significant fraction of the sample, in Row (2), we restrict the sample to non-US firms. We see that although the impact is lower in non-US countries than in the US (possibly due to different degrees of enforcement), it is equally statistically strong. In Row (3), instead of clustering standard errors at the country level, we cluster them at the country*industry level. In Row (4), we restrict the sample to the firms that do not change their headquarter countries to take away any strategic relocation effects, and cluster standard errors at the country level. Row (5) removes financial institutions (with SIC codes 6000-6999) from the sample. Our results are robust to all these changes in our specification.

In Rows (6)-(9), we additionally control for the other legislative changes. First, in Row (6) we control for the introduction of first competition law in the country. In doing this, we want to control for the potential spurious effect due to newly introduced restrictions on other anti-competitive actions. We find that other competition laws also negatively affect firm profitability but their effect does not render leniency laws insignificant. Similarly, Row (7) restricts the sample to countries that had competition law passed on or before 1990.

In Row (8) we control for changes in corporate governance laws, obtained from Kim and Lu (2013). Since corporate governance law changes are available for years 1991-2007, we limit our sample to this period. We find equally strong effect for leniency laws and no effect of corporate governance changes. In Row (9) we control for both passage and enforcement of insider trading laws (Bhattacharya and Daouk, 2002). We limit the sample to pre-1998 period. Finally, we control for financial development of the country. We use Chinn-Ito index to capture a country's degree of capital account openness (Chinn and Ito, 2006), country's stock market capitalization as a percentage of GDP as well as private credit as a percentage of GDP (both measures from the 2013 update of the database of Beck et al., 2000). Controlling for these changes in financial market development does not affect our estimates.

Next, in Table 3, Panel C, we investigate in which industries the impact is larger. We expect a more negative effect on profitability to occur in the industries in which sustaining collusion is more difficult, as only very profitable cartels would be present in such cases.

First, we test the conjectures of Hay and Kelley (1974) and Whinston (2006) that industries which rely on few major customers should be associated with lower observability of firms' offered prices, as well as higher lumpiness in demand, both of which make sustaining a collusive scheme more difficult. If the conjectures of Hay and Kelley (1974) and Whinston (2006) are correct, we should thus find a more negative effect on profitability in the presence of a few major customers. On the other hand, one could also claim that having a large customer makes it easier to collude vertically, and could increase the likelihood of collusion. It is easier to coordinate with a large vertical partner and ultimately earn higher rents from the final end users of the supply chain. In Columns (1)-(2), we split the sample according to whether the industry is expected to have large customers.⁶ Column (1) reports the results for the subsample of the firms in industries where the average firm's largest customer has a sales fraction of more than the sample median

⁶ We rely on data on the US firms and, following the same approach as Rajan and Zingales (1998), we apply the measures estimated on the basis of US firms to the industries in other countries. In particular, we split the sample using the median fraction of top customer sales out of all the firm's sales in the SIC four-digit industry, estimated across all US firms in year 1990, at the beginning of our sample.

(19%) and Column (2) reports the results for the subsample of the firms in industries in which the largest customer sales fraction is below median. In line with our expectations, we find a much larger drop in profitability in industries that rely on a single major customer. Similarly, in Columns (3)-(4), we split the sample according to whether the median fraction of the sales of the largest four customers is above or below the sample median (29%), and we find consistent results.

In Columns (5)-(6), we split the sample according to the predicted probability of getting convicted in the cartel case. Presumably, firms differ in their involvement in cartels based on industries they are in as well as other characteristics. We thus want to see whether the firms, which were more likely to get convicted before the passage of leniency laws, experienced a larger drop in profitability when the laws got passed. Our prediction model is based on time-varying firm characteristics (asset size, leverage and gross margin), country characteristics (GDP and unemployment), as well country fixed effects and three-digit SIC fixed effects. We fit the prediction model only using pre-lenieny observations and predict the probability that the firm will be convicted in the cartel case in the year after the passage of leniency law. We split the sample according to the size of this probability and find that our results are stronger in the subsample where the probability of getting convicted is higher. The result is consistent with our prediction.

C. Randomization and Treatment of Errors

We now deal with the treatment of the standard errors. As pointed out by Bertrand, Duflo and Mullainathan (2004), within our context, clustering the standard errors may not properly account for the correct structure of the error covariance matrix. Therefore, 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 the leniency law in each country. We repeat this procedure 5,000 times to get 5,000 randomized leniency law samples. In each of these randomized leniency law samples, we run our baseline regressions as in Table 2, Panel A, Column (1) and Table 3, Panel A, Column (1), and save the relevant coefficients. Finally, we

compare the coefficient from our actual leniency laws with those obtained from the identification using these pseudo leniency laws.

We report the distribution of the coefficients in Figure 2. The upper figure shows the coefficients on conviction as in Table 2, Panel A, Column (1) while the lower figure shows the coefficients on profitability as in Table 3, Panel A, Column (1). The figures demonstrate that our actual coefficients differ from the randomized placebo samples in 4,975 cases out of 5,000 (99.5%) in the case of the effect on conviction and 4,819 cases out of 5,000 (96.4%) in the case of the effect on profitability. The non-parametric bootstrap flavor of this analysis also further guarantees that a potential incorrect specification of the error covariance matrix does not affect our results.

Overall, these results validate the use of leniency laws as the identification strategy. We have shown that leniency laws have led to more convictions as well as reduced the profitability of the affected firms.⁷

V. Firm Strategies

We now investigate how the firms adapt their corporate policies. We posit that the increased costs of maintaining or starting new links with the firm's competitors will induce a change in the firm's boundaries. As an identification strategy, we again rely on a difference-in-difference estimation, and look at the effects of the passage of the leniency laws on the main growth-related corporate policies of the firm: investment, M&A activities and the formation of strategic alliances.

A. Investment, M&A Activity and Strategic Alliances

We first look at how the firm's investment is affected. We measure investment as the firm's change in the property, plant and equipment and goodwill, adjusted for depreciation and

⁷ Note that some of the reduction in profitability might have come from the higher entrance of new firms into the market once the industries became less monopolized. Although the creation of new firms after the passage of leniency laws is beyond the research focus of this study, it remains an interesting unexplored question.

amortization and scaled by one-year lagged asset size. Our measure thus includes both capital expenditures as well as acquisitions.

We estimate a specification similar to the previous one, applying the same identification approaches to all the Compustat Global and North America firms over the 1990-2012 period. We report the results in Table 4. In Column (1), we find that the investment of the affected firms increases by 2.8%. The result is robust to controlling for firm and country characteristics (Column (2)) as well as to restricting the sample to non-US firms (Column (3)). In Column (4), we document that the passage of leniency laws in all countries whose firms have a high market share in the firm's industry, captured by the global leniency law variable, contributes an additional 2.4% increase of investment. We find no effect if the dependent variable is just the capital expenditures of the firm, scaled by one-year lagged asset size (Columns (5)-(6)). This suggests that most of the effect from the passage of leniency laws on the investment should be coming from the M&A activity.

As a second step, we therefore further investigate the M&A activity of the affected firms. We use the data from SDC Platinum database and merge with it with the Compustat Global and North America datasets for the 1990-2012 period. In Table 5, we look at the firm decision to engage in M&A either as an acquirer or as a target. Our baseline specification uses log dollar value of acquisitions as the dependent variable, and, as before, applies difference-in-difference methodology to estimate the effect of the passage of leniency laws.

Column (1) shows that the passage of the leniency laws and thus higher collusion costs increase the total deal size of firm's acquisitions by 5.3%. This result is robust to different specifications. In Column (2), we add firm and country level controls, while in Column (3), we limit the sample to non-US firms.

We further control for industry effects. First, in Column (4) we exclude the mergers of financial institutions to remove the effect of the global banking merger wave in 1990s. Second, in Column (5) we control for two-digit SIC industry*year fixed effects that should take care of any other unobservable time-varying drivers of global industry consolidation. Even if there were global trends in the consolidation of certain industries, we find that the timing of passage of

leniency laws across countries induced cross-sectional variation in industry consolidation across countries.

In Column (6), we focus on the passage of the laws in countries whose firms have a high market share in the firm's industry. As we mentioned before, the fact that those laws affects firm's principal competitors, they should have a direct impact on firm's ability to get involved in collusion even if its own country does not pass any leniency law. And indeed, we find that the passage of leniency laws in the countries whose firms have a high market share in the firm's industry also contributes to larger M&A transactions.

The results are also robust if we simply look at the probability of acquiring another firm. In Column (7), we show that the passage of the leniency laws raises the probability of the firm initiating an acquisition by 4%. This result suggests that most of the effects come from more acquisitions rather than their larger size. Similarly, if we focus on the probability of becoming a target, we find (Column (8)) a lower, albeit still statistically significant, effect of 2%.

Finally, we look at strategic alliances. The rise in covert collusion costs may induce the colluding firms to make their ties more explicit by forming publicly visible strategic alliances. We therefore test this possibility focusing strategic alliances. We perform the estimation using the panel at an industry*country level. In particular, we focus on the three-digit SIC industries as reported in Compustat Global and North America, and check the number of firms involved in alliances in each of them by country and year. We then relate them to the passage of the leniency laws. All the regressions include industry*country and time fixed effects.

The results, reported in Table 6, display no effect on either the number of strategic alliances formed in the country/industry (Columns (1)-(3) or the number of firms involved in strategic alliances in the country/industry (Column (4)). This result is the same whether we control for different country specific variables, or if we employ our additional identification strategy of laws passed in countries whose firms have a big market share in the firm's industry.

Finally, given that this analysis uses a more coarse methodology than the one we used for the M&A analysis, we reconfirm that our M&A results hold in this case too. In Columns (5)-(6), we show that the number of M&A transactions at the industry*country level rises after the passage

of leniency law. It is worth mentioning that, here, unlike Table 4, we also capture the M&A transactions between private firms. This suggests that that leniency laws have wider economic effects beyond publicly listed firms.

B. Market Reaction to M&A Announcements

We now investigate how these M&A transactions induced by cartel busting are perceived by the stock market. We use a standard approach in corporate finance by comparing the reaction to the deals that followed cartel convictions to the reaction to a diverse set of other transactions. Even if we know that on average, the number of M&A transactions has increased following leniency laws, we cannot know for sure which particular M&A deals were driven by leniency laws. In our estimation of the abnormal returns, we thus slightly depart from our earlier methodology and look at the cases that followed the actual convictions in cartel investigations.

We employ an event study methodology, in which we compute the cumulative abnormal returns by using three alternative factor-pricing models: Model 1 estimates the expected returns using a two-factor model (domestic market factor and global market factor); Model 2 estimates the expected returns using a three-factor model (domestic market, SMB, HML factors); Model 3 estimates the expected returns using a six-factor model (domestic market, SMB, HML factors and global market, SMB, HML factors).

We use two alternative estimation periods – 180 days, or 60 days – and four alternative windows around the event – i.e., 3 day [0,2], 5 day [0,4], 30 day [0,29] and 3 month [0,89]. We winsorize the abnormal returns at the 1% level. We investigate whether the effect on the acquirer's stock differs from other (non-cartel busting-induced) M&A transactions. We therefore condition on whether either the acquirer or the target has been convicted in the cartel case recently (over last three, five years, or since the start of our sample).

The results, reported in Table 7, display that the announcement returns are higher when either the acquirer or the target has been convicted than in the M&As in which none of the parties has been investigated. Column (1) reports the baseline specification, where the expected returns are estimated using domestic market and global market returns over 180 days estimation period. We

find a positive relationship between the dummy for the case the target or the acquirer had been convicted in the cartel case over the prior five years and the abnormal return. The effect corresponds to 0.5% abnormal return in the case the acquirer has been convicted and to 1.5% abnormal return in the case the target has been convicted.

The results are robust across different models and specifications. More specifically, Column (2) adds acquirer country fixed effects and acquirer three-digit SIC industry fixed effects. Column (3) clusters the standard errors at the acquirer country level. In Column (4), the expected returns are estimated using a model with three domestic factors, while in Column (5) they are estimated using a model with three domestic factors and three global factors. Column (6) reports the results in which the main variables of interest are dummies representing whether the target and acquirer were convicted in the cartel case over the prior three years while in Column (7) they represent whether they were convicted since the start of the sample period.

Jointly taken, these findings outline firm preferences consistent with a pecking order structure in organizational form. Firms first prefer to collude, and when such opportunities become more difficult, they pursue M&A activities. The market is aware of this pecking order and fully supports managerial decisions aligned with it. A busted cartel shocks the firms out of their optimum and thus the firms react by choosing a second best option: the acquisitions. This can be interpreted as the M&A helping the firm to achieve economies of scale or scope that were previously guaranteed by the cartel. Alternatively, a target that was previously convicted will generate positive announcement returns when it is acquired because this will fix the reputation problem that the conviction had caused. For example, the acquirer can retool the target assets to sell product under a non-tarnished brand, and thus restore firm reputation. Indeed, if one looks closely at Table 7, the table shows that announcement returns are larger when the target was previously convicted than when the acquirer was convicted.

To better investigate this issue, we now focus on the M&A drivers in the presence of cartel busting. This is the topic of the next section. **VI. M&A Drivers**

The findings that firms engage in more acquisitions following increased collusion costs can be explained in terms of a bigger need to exploit synergies. For instance, the laws that make collusion harder reduce prices and markups in the industry requiring the firms to increase their output to cover their fixed costs. Mergers and acquisitions are thus one way of improving efficiency and realizing the required economies of scale – i.e., cost synergies. On the other hand, mergers could be pursued to recreate the market power, and thus exploit revenue synergies. In the first case, customers may benefit, in the second case, the adjustment is at the expense of the customers. To distinguish these two alternatives, we perform three sets of tests. First, we look at the type of M&A activity that follows after the passage of the leniency laws. Second, we explore the heterogeneity of the responses and study the subsamples in which we expect the need to exploit synergies to be more important – i.e. firms in the industries that operate at increasing returns to scale. Finally, we look at the customer reaction to M&A deals.

A. Horizontal M&As

Both cost and revenue hypotheses predict that firms pursue a horizontal integration by merging (getting acquired or acquiring) with competitors rather than following diversifying acquisitions. We test this explicitly by focusing on horizontal acquisitions - i.e., acquisitions of competitors that come from the same industry. The results, reported in Table 8, display evidence that following leniency laws firms increase their acquisitions of competitors that come from the same industry and country. In particular, in Columns (1)-(2), we use as dependent variable the log dollar value of acquisitions in the same three-digit SIC industry as the firm. We find that both leniency laws as well as leniency laws in countries with strong market position in the firm's industry increase such acquisitions.

In Columns (3)-(6), we further split the log dollar value of acquisitions in the same three-digit SIC industry into the acquisitions of firms headquartered in the firm's country (Columns 3-4) and headquartered outside of firm's country (Columns 5-6). We find that although leniency law in the country increases within-country acquisitions of competitors, leniency laws passed in other countries also affect the international acquisitions of competitors.

Finally, in Columns (7)-(8), we focus on diversifying acquisitions – i.e., the ones coming from a different industry – by using the log dollar value of acquisitions in a different three-digit SIC industry from the firm as our dependent variable. We do not find that the passage of leniency law in firm’s country affects these acquisitions. Overall these finding are in line with the synergies hypothesis.

The second set of tests investigates whether the cartel busting-related M&A activity is directed towards other firms that used to be part of the busted cartel. In other words, we ask whether the affected firms merge with the former cartel members. After manually inspecting the names indicted in the cartels, we find only a handful of cases where former cartel members – private or publicly listed – merged after the conviction, possibly due to antitrust concerns. These results suggest that firms react to the cartel sanction by acquiring size and market power that helps them to replace the cartel, without necessarily doing it by aggregating former cartel members.

Overall these finding are in line with the synergies hypothesis.

B. Increasing Returns to Scale

We now try to distinguish between the explanation based on revenue synergies and cost synergies. We provide several tests. Our first set of tests relies on splitting the sample according to alternative proxies for the presence of increasing returns to scale in the industry. If M&As are driven by cost synergies, we should see more transactions in the industries that are currently experiencing increasing returns to scale. We therefore replicate our baseline specification of Table 5 where we estimate the effect of the leniency laws on firm’s log dollar value of acquisitions, conditioning for the presence of returns to scale in the industry. Table 9 reports the results.

We consider five different ways of proxying for the degree of increasing returns to scale of the industry. The first classifies the firms on the basis of whether the industry in which they operate was experiencing increasing or decreasing returns to scale in year 1996. We measure the latter by estimating a two factor Cobb-Douglas production function for each two-digit SIC

industry in year 1996, using all the Compustat Global and North America firms. We proxy for the firm's output by its sales, for the firm's labor by the number of its employees and for the firm's capital by the firms' property, plants and equipment. We then add the coefficients for the proxies for labor and capital, and define those industries in which the sum of coefficients is higher than one as having increasing returns to scale, and those in which the sum of the coefficients is lower than one as having decreasing returns to scale. In Columns (1)-(2), we show that the results are strongest in the industries that were experiencing increasing returns to scale.

The second approach is based on the semi-parametric methodology of Olley and Pakes (1996). This method controls for selection and simultaneity biases by allowing for firm-specific productivity differences and endogenizing the firm's liquidation decision.⁸ In particular, Olley and Pakes (1996) method assumes that productivity is observed by the firm before it makes decisions on its variable input (labor). This gives rise to the simultaneity problem as both labor and output are affected by the level of productivity that is unobservable for econometrician. For instance, following a positive productivity shock, a firm will increase its use of labor and thus OLS estimation of production functions will yield biased parameter estimates. This simultaneity problem is addressed by using investment to proxy for an unobserved time-varying productivity shock.⁹

As before, we proxy for firm's output by its sales, for labor by the number of employees and for capital by the property, plants and equipment. Then, in line with Olley and Pakes (1996), we proxy for the observable firm level productivity using its investment decisions – i.e., the changes in the property, plants and equipment and intangible assets.

We estimate the equations for each global two-digit SIC industry and add the coefficients for labor and capital proxies. An industry has increasing returns to scale if the sum of the

⁸ An alternative widely accepted method to specify firm production function is developed by Levinsohn and Petrin (2003) who use materials used in production in their specifications to proxy for productivity. Since data on materials is not readily available to us, we follow Olley and Pakes (1996) to estimate firm level productivities.

⁹ In addition, Olley and Pakes (1996) approach controls for the selection problem that recognizes the relationship between the productivity shocks and the firm's exit from the market. For instance, after a low productivity shock a firm with a larger capital stock is more likely to stay in the market than a firm with a smaller capital stock, and so the coefficient on the capital variable tends to be biased downward. Such selection problems are addressed by using survival probabilities.

coefficients is higher than one. We use the full sample and estimate the parameters of the productivity equation. We find few industries with increasing returns to scale. We therefore split the sample according to the median value of 0.716. In Columns (3)-(4), we show that the results are strongest where this value is higher – i.e. in the industries with more increasing returns to scale.

The third approach relies on the growth literature and as estimates of the industry's returns to scale uses the estimates of the sensitivity of the industry's growth of value added to changes in industry level capital and labor input growth (Hall, 1988; Caballero and Lyons, 1989). In particular, we draw estimates from Burnside (1996) and sort the two-digit SIC manufacturing industries according to whether industry's returns to scale are increasing (Column (5)) or decreasing (Column (6)) based on whether value to returns to scale parameter is greater than one.¹⁰ We find that after the passage of leniency laws M&A activity increased in industries with increasing returns to scale.

The fourth approach is based on sorting industries according to the engineering estimates of minimum efficient scale of production in each industry. This approach constructs hypothetical production cost functions at different output levels. Sutton (1991) shows that it has high correlation with the proxies of minimum efficient scale of the plant such as medium size of the plant in the industry. We use the qualitative ranking in Pratten (1988) to split the industries in terms of engineering estimates of minimum efficient scale of production. The results are consistent with the previous ones. In particular, as reported in Columns (7)-(8), we find stronger M&A effect in industries with more increasing economies of scale.

Finally, the fifth approach relies on the intuition that the smallest firms are the most likely to face higher need to cover fixed costs, so most of the benefit from merging should be reaped by the smallest firms. We therefore split the sample on the basis of the firm size within its country

¹⁰ In particular, according to Burnside's (1996) estimates, two-digit SIC industries 24-27 and 32-38 have increasing returns to scale while others have decreasing returns to scale. We report the results based on the model that only considers internal returns to scale (as in Table 1 in Burnside, 1996). The results based on the overall returns to scale that include external factors (as in Table 2 in Burnside, 1996) are very similar and we do not report them due to brevity. Our results are also consistent if we use estimates from Caballero and Lyons (1989).

of domicile (i.e. we perform the splits at the country level) in year 1996, before the beginning of most of the leniency law passage. In Columns (9)-(11), we document that the results on the acquisition activity are only present among the smallest firms in the sample. For these firms the passage of the leniency law and thus higher collusion costs increase the probability that the firm is an acquirer by 3.8%. In fact, the acquisition activity does not increase for the medium-sized and the largest firms.

In summary, all the different econometric approaches agree in pointing to the fact that M&A activity primarily increased in the industries associated with higher returns to scale.¹¹

C. Customer Reaction

Finally, an alternative way of distinguishing between revenue and cost synergies is to focus on the stock price reaction of the customers of the firms involved in M&As following cartel convictions. If these M&A transactions were led by cost synergies, the stock prices of the customer firms should not respond (or respond positively if the costs are passed through to customers). If, on the contrary these M&A transactions were led by revenue synergies – i.e. willingness to preserve market power – the customer firm’s stock price should respond negatively.¹²

We identify the supplier-customer relationships from the OECD Input-Output tables of year 2002. We then look at the cumulative abnormal returns, defined as before, over 3 day [0,2] window and try to see whether they differ based on whether this supplier which engaged in the M&A was also convicted in a cartel case over the last 5 years. We only consider horizontal mergers – i.e. those cases where both acquirer and target were in the same three-digit SIC industry. We winsorize abnormal returns at 1% level, and cluster standard errors at the customer’s three-digit SIC level. In this analysis we exploit the cross-sectional power coming

¹¹ One may argue that this effect comes from the fact that industries with decreasing returns to scale simply experience less bursting of cartels. However, if we perform the regressions as in Table 2 at the country*industry level we find that the effect of leniency law is present among industries with both higher and lower returns to scale. These results are available at request.

¹² 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.”

from the supplier-customer relationships for each deal. We therefore add deal fixed effects to control for any unobserved differences that different M&A transactions could have had on a wider economy.

That is, our estimation compares how *for the same deal* the market price reaction differed across different firms that were not involved in the deal. In particular, we investigate how market price reaction differed for firms that had strong customer links with the firms involved in the deal, as compared to the firms that had weaker customer links with the firms involved in the deal. We thus estimate a differential effect of customer reaction to non-customer reaction after the same M&A transaction. On average, we expect such reaction to be negative as the customers should be more negatively affected if their suppliers merge, compared to non-customers. We measure this differential effect by the fraction of inputs that firm's OECD industry category gets from merging firms' OECD industry category, out of all domestic value of inputs. A higher fraction of inputs corresponds to a stronger intensity of relationship, i.e. higher likelihood that the firm is in a customer industry of the merging firms. So, we estimate how market values of firms with high value of intensity of relationship (i.e. customers) reacted differently from market values of firms with low value of intensity of relationship (i.e. non-customers).

As we are specifically interested in the cartel cases, we go further and see whether such differential reaction between customers and non-customers differs in the case when one of the merging suppliers was convicted in a cartel investigation. Our primary variable of interest is thus the interaction between the degree of industry links between customer and supplier industries, and the past conviction of one of the suppliers.

We report results in Table 10. In the baseline specifications in Column (1), we find that although the degree of industry links between supplier and customer industries affects customer abnormal returns negatively, past conviction reverses the effect. This suggests that cartel-induced mergers have more efficiency gains that are also passed through to the customers. Such evidence is more suggestive of the cost synergies being in place, which is line with the earlier findings by Fee and Thomas (2004) and Shahrur (2005) on the full sample of M&A transactions of US firms.

However, in contrast to these findings, we find that non-cartel related M&A transactions result in lower abnormal returns for customers.

Our results are robust. In Column (2) we cluster the standard errors at the deal level. Column (3) focuses on non-US transactions. Column (4) reports the results in which, instead of conditioning on target or acquirer convictions over the prior five years, we condition on the convictions over the prior three years. In Column (5), we look at the cartel conviction since the start of the sample period. Finally, in Column (6) we report the estimates of the pooled regression without deal fixed effects but with year fixed effects. In this case, we can estimate the level effect of the conviction without interacting it with the intensity of relationship between customer and supplier industries. We do not find it significantly different from zero, again supporting efficiency explanation of the M&A transactions. Our results are robust across all these alternative econometric specifications and robustness checks.

Overall, these findings suggest that the M&A activity following the rise in the costs of collusion is driven by the cost synergies that to some extent are also passed through the customers of the affected firms.¹³

D. Alternative Motives

Our results could also be explained in alternative ways. For instance, one may argue that these findings could be related to distress. As the leniency law is passed and/or a firm is convicted, the ensuing drop in profitability triggers a corresponding increase in the probability of distress (similar to the arguments in Bizjak and Coles, 1997). This makes it more difficult to access the external capital markets and induces the firm to form stronger internal capital markets (Stein, 1997), and M&As are one way of achieving. However, in this case the firms should follow diversifying acquisitions that increase the usefulness of internal capital markets, contrary to what we find and report in Table 8.

¹³ If the mergers are driven by economies of scale, one may ask why firms do not merge before the increase in the costs of collusion. One explanation could be that managers have private benefits of control, especially if the profitability of the firm is high. Such private benefits of control would be lost if the firm is acquired. Once the profitability drops, the private benefits of control are also lower, and it becomes more attractive to pursue the merger.

Moreover, the industrial organization literature (Stigler, 1964; Eckbo, 1983; Salant, Switzer and Reynolds, 1983; Perry and Porter, 1985; Compte, Jenny and Rey, 2002; Vasconcelos, 2005; Bos and Harrington, 2010) argues that, as collusion costs rise, firms merge to reduce the asymmetry in size between the market participants, so as to facilitate collusion in the future. However, this hypothesis predicts that the effect is strongest for medium sized firms (Bos and Harrington, 2010), while in Table 9 we find that most of the effect comes from the smallest firms in the sample.

E. Merger Timing

An interesting question is why, if a bigger scale was optimal, the cartel participants had not merged before and moved toward the optimal scale even before the leniency laws were passed. Indeed, one may claim that because of possible deviation of some colluding firms, cartel might not fully replicate the merged firm while the latter might seem to be able to achieve most of what cartel does.

One explanation could be that managers have private benefits of control that they do not want to give up. Indeed, cartels allow members to achieve coordination and outcomes not very dissimilar from better economies of scale, still *preserving the independence of the firm* and, say, allowing the executives to run two independent firms. Such benefits for CEOs might be lost if one firm is acquired. Once sustaining the cartel becomes more difficult, it becomes more attractive to pursue the merger. Moreover, such private benefits of control might be higher in more profitable firms (when “quiet life” is more appealing as in Bertrand and Mullainathan (2003)). When profitability drops after the passage of leniency laws, the attractiveness to maintain private benefits of control and resist mergers also recedes.

Kumar et al. (2015) provide an alternative theory of why firms might prefer cartels over mergers. They suggest that customers might resist high prices if they do not observe the policing force of the competition. Thus, a clandestine cartel may be able to take advantage of customer beliefs that the competition might still be in place, and firms may face reduced buyer resistance,

especially in procurement contracts. They show that, even with merger efficiencies, firms might prefer collusion over merger.¹⁴

Finally, mergers may involve transaction costs (e.g. fixed integration costs), or create agency problems (e.g. empire building), and on the margin some firms could thus prefer collusion.

In all these cases, when collusion becomes less feasible, the firm might move to the next best: the merger, which is consistent with our empirical findings. Indeed, Stigler (1950) suggests that part of the reason why mergers are preferred to cartels is illegality of the latter.

Conclusion

We show that the leniency laws have a negative effect on the firm performance. By looking into a wide sample of firms across 63 countries and employing a difference-in-difference strategy using the passage of leniency laws in different countries, we find that increasing costs of collusion stability reduce firm gross margins by 6 percentage points. Leniency laws also lead to more cartel convictions. Moreover, we document that following increased costs of collusion stability, firms reorganize their activities by pursuing more M&A transactions, especially those in the same industry. Compared to other M&A transactions, these M&A have higher abnormal announcement returns. We interpret these findings as a need of firms, whose profitability has been negatively impacted by cartel busting, to increase their output to cover their fixed costs. Firms react to it not by organic growth or alliances but by engaging in non-diversifying horizontal M&As.

These results provide a first understanding on the effect of increased cost of collusion and in particular the passage of the leniency laws on the firm behavior. Our results provide a more precise explanation on how industry effects contribute to the merger waves. We also show that sometimes firms prefer weaker integration in the form of cartels over the integration by merging, and only resort to the latter when collusion costs increase.

¹⁴ This contemporaneous paper also provides anecdotal evidence that Sherman Act was followed by mergers in eight out of ten largest manufacturing industries in 1887-1905.

Our results have several policy implications. First, we establish that recent antitrust legislation such as leniency laws has been successful in combating product market collusion. Moreover, we show that there is a substitution of firms pursuing more horizontal mergers after the costs of price fixing rise and thus due to potential dynamics of competition we argue that competition policy should be more integrated between price-fixing and merger review arms.

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Table 1. Summary statistics

This table reports summary statistics for the main variables used in the subsequent analysis. The summary statistics are reported at a firm, industry/country or cartel level.

Panel A. Firm variables

	N	Mean	Median	St. Dev.
Assets (m)	507,354	2886.19	130.75	37700.46
Gross margin	473,369	0.26	0.33	0.98
ROA	445,760	0.024	0.087	0.4
Leverage (debt over book equity)	501,357	0.83	0.37	2.15
Investment	397,697	0.13	0.048	0.32
Acquisitions (m)	543,737	13.62	0	522.29
Acquiror dummy	543,737	0.04	0	0.2
Target dummy	543,737	0.02	0	0.13

Panel B. Industry variables

	N	Mean	Median	St. Dev.
Dollar value of acquisitions in the country/industry	408,618	87.92	0	2290.05
No. of acquisitions in the country/industry	408,618	1.24	0	16.60
No. of strategic alliances in the country/industry	408,618	0.25	0	5.76
No. of participants in strategic alliances in the country/industry	408,618	0.29	0	6.04

Panel C. Convicted cartels

	N	Mean	Median	St. Dev.
Number of cartel participants per cartel	746	10.04	5	30.55
Dollar value of cartel affected commerce (m)	526	26,752.24	1967	139,174.6
Market share of convicted cartel participants	292	0.87	0.93	0.16
Cartel length (year)	616	7.39	5	9.17

Table 2. Leniency laws

Panel A. Number of convicted cartels

This table reports 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 number of cartels convicted in the country in a particular year.

Our main variable of interest is Leniency law dummy. Column (1) provides baseline specification. Column (2) controls for the country's macroeconomic conditions. In Column (3) we treat EU as one country, and consider the passage of leniency law legislation at EU level (we keep one observation for EU). Column (4) reports OLS estimates. Column (5) also reports OLS estimates but controls for regional trends by adding region*year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	0.935***	0.869***	0.716*	0.635***	0.667***
	2.987	3.383	1.686	3.755	3.528
GDP per capita		1.102**			
		2.069			
Unemployment		-0.025			
		-0.584			
Imports as % of GDP		0.031*			
		1.947			
Constant				0.063	-0.063
				0.528	-0.239
R-squared				0.475	0.542
N	1449	837	1049	1449	1449

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

Panel B. Additional robustness checks

This table reports 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 number of cartels convicted in the country in a particular year.

Our main variable of interest is Leniency law dummy. Column (1) reports estimates for the placebo treatment, where the passage of the leniency laws is anticipated by three years. In Column (2) we show the pre-existing trends by adding the dummies of three-year increments before the passage of the law. In Column (3) we show the response to leniency law, modeled by three-year increments after the passage of the law. In Column (4) we control for the presence of the competition law. In Column (5) we restrict the sample to countries that had competition law passed on or before 1990. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law		0.858**		0.901***	0.659**
		2.027		3.087	2.182
Leniency law (years 1-3)			0.668***		
			2.823		
Leniency law (years 4-6)			0.774**		
			2.514		
Leniency law (years 7-9)			0.437		
			1.284		
Leniency law (after 9 years)			0.385		
			0.936		
Placebo law	0.579				
	1.48				
Years 4-6 before leniency law		-0.151			
		-0.388			
Years 1-3 before leniency law		0.14			
		0.411			
Competition law				1.847**	
				2.399	
Constant		0.992**	1.126**		0.658
		2.054	2.229		0.986
N	1081	985	985	1081	484

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

Panel C. Importance of convicted cartels

This table reports OLS and Poisson quasi maximum likelihood regressions. All regressions include country fixed effects and time fixed effects. Standard errors are clustered at the country level. Dependent variables are various measures of importance of convicted cartels.

Our main variable of interest is Leniency law dummy. In Column (1)-(3) the dependent variable is the log value of cartel affected commerce. Column (1) reports the baseline regression. Column (2) treats EU as one country, and consider the passage of leniency law legislation at EU level (we keep one observation for EU). Column (3) controls for regional trends by adding region*year fixed effects. In Column (4) the dependent variable is the mean market share of the convicted cartels. In Column (5) the dependent variable is the number of convicted firms. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	1.343***	1.902***	1.393***	0.073**	2.274***
	3.863	3.43	3.624	2.513	3.565
Constant	0.205	0.205	-0.16	0.031*	
	1.383	0.512	-0.129	1.862	
R-squared	0.373	0.62	0.397	0.223	
N	1449	1449	1449	1449	1449

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

Table 3. Profitability

Panel A. Differences-in-differences estimation

We consider all Compustat Global and North America firms over 1990-2012. This table reports OLS regressions, where the dependent variable is gross margin in Columns (1)-(6) and return on assets (ROA) in Column (7). All regressions include firm fixed effects, time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is Leniency law dummy. In Column (1) we test its effect without any additional controls. In Column (2) we control for firm and country characteristics. Column (3) includes an additional variable Global leniency law which measures the passage of laws in the countries that are strong in firm's industry. Column (5) shows the response to leniency law, modeled by three-year increments after the passage of the law. Column (6) uses ROA as the dependent variable. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
Leniency law	-0.062**	-0.051**	-0.056**		-0.013***
	-2.213	-2.093	-2.52		-3.106
Global leniency law			-0.049*		
			-1.876		
Leniency law (years 1-3)				-0.038*	
				-1.952	
Leniency law (years 4-6)				-0.039	
				-1.379	
Leniency law (years 7-9)				-0.076	
				-1.799	
Leniency law (post year 9)				-0.004	
				-0.121	
Assets		0.080***	0.080***	0.079***	
		6.811	6.745	6.749	
Leverage		0	0	0	
		-0.589	-0.594	-0.623	
GDP per capita		-0.120***	-0.115***	-0.106**	
		-3.501	-3.406	-2.461	
Unemployment		0.007	0.007	0.006	
		1.565	1.596	1.338	
Imports as % of GDP		-0.004**	-0.003**	-0.003**	
		-2.213	-2.105	-2.118	
Constant	0.251***	1.098***	1.049***	0.970**	0.056***
	9.998	3.286	3.233	2.328	3.217
R-squared	0.523	0.531	0.531	0.531	0.597
N	473369	404107	404107	404107	445760

* 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 OLS regressions, where the dependent variable is gross margin and the Leniency law dummy as main explanatory variable. All regressions include firm fixed effects, time fixed effects as well as additional firm and country controls, as in Table 3, Panel A, Column (2). We only report the coefficients and the associated standard errors for Leniency law variable. Other coefficients, unreported to save space, are available upon request. Unless noted otherwise, standard errors are clustered at the country level.

Row (1) controls for industry*year fixed effects. Row (2) restricts the sample to non-US firms. Row (3) clusters standard errors at country*industry level. Row (4) restricts the sample to the firms that do not change their headquarter countries. Row (5) removes financial institutions (SIC codes 6000-6999) from the sample. Row (6) controls for the introduction of competition law. Row (7) restricts the sample to countries that had competition law passed on or before 1990. Row (8) controls for the change in corporate governance law, limiting the sample between 1998 and 2006. Row (9) controls for the change in insider trading laws, limiting the sample between 1990 and 1998. Row (10) controls for country's financial market development.

	Leniency law	t-statistic	Number of obs.
(1) Industry*year fixed effects	-0.052**	-2.225	404107
(2) Non-US firms	-0.024***	-3.047	237098
(3) Non-Top 5 country firms	-0.044***	-3.420	124041
(3) Cluster at country*industry level	-0.051***	-4.902	404048
(4) Firms that do not change HQ	-0.052**	-2.125	398173
(5) No financial institutions in the sample	-0.056**	-2.140	366951
(6) Control for competition law	-0.050**	-2.089	404107
(7) Countries with competition law before 1990	-0.047*	-2.066	333609
(8) Control for corporate governance law	-0.059**	-2.300	328486
(9) Control for insider trading laws	-0.017*	-1.938	127259
(10) Control for financial market development	-0.047**	-2.157	394104

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

Panel C. Heterogeneous responses

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

Our main variable of interest is Leniency law dummy. Columns (1)-(2) split the sample according to whether industry is expected to have a single large customer. The split variable is defined as the median fraction of top customer sales out of all firm's sales in SIC four-digit industry, estimated across all US firms in year 1990. Column (1) is the subsample of the firms in industries with top customer sales fraction over 19% and Column (2) is the subsample of other firms. Columns (3)-(4) split the sample according to whether industry is expected to have large customers. The split variable is defined as the median fraction of top 4 customer sales out of all firm's sales in SIC four-digit industry, estimated across all US firms in year 1990. Column (3) is the subsample of the firms in industries with top 4 customer sales fraction over 29% and Column (4) is the subsample of other firms. Columns (5)-(6) split the sample according to the predicted probability of getting convicted in the cartel case, based on the prediction model fit to the pre-leniency observations. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	-0.079***	-0.044*	-0.081***	-0.040*	-0.080**	-0.042*
	-3.334	-1.895	-3.295	-1.799	-2.653	-1.88
Global leniency law	-0.066***	-0.049*	-0.050**	-0.060**	-0.063**	-0.060***
	-3.388	-1.677	-2.565	-2.351	-2.251	-2.708
Assets	0.099***	0.055***	0.096***	0.056***	0.087***	0.073***
	6.368	6.075	6.639	4.281	4.74	6.047
Leverage	0.001	0	0	0	-0.001	0
	0.834	-0.763	0.248	-0.154	-0.575	-0.004
GDP per capita	-0.127***	-0.102**	-0.132***	-0.097**	-0.128***	-0.107**
	-4.8	-2.65	-4.817	-2.483	-4.632	-2.438
Unemployment	0.012*	0.005	0.011*	0.005	0.011**	0.005
	1.823	1.318	1.819	1.291	2.1	0.928
Imports as % of GDP	-0.005**	-0.002*	-0.005**	-0.002	-0.004**	-0.003*
	-2.276	-1.709	-2.566	-1.505	-2.212	-1.845
Constant	1.031***	1.088***	1.092***	1.035***	1.109***	1.030**
	3.722	3.003	3.827	2.877	5.291	2.338
R-squared	0.531	0.491	0.531	0.492	0.542	0.515
N	178393	187002	182882	182513	173274	230833

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

Table 4. Investment

We consider all Compustat Global and North America firms over 1990-2012. This table reports regressions where the dependent variables are various measures of investment by a firm in a particular year. All regressions include firm fixed effects, time fixed effects. Standard errors are clustered at the country level.

In Columns (1)-(4) the dependent variable is the change in the value of tangible and intangible assets, adjusted for depreciation, scaled by last year's assets. In Columns (1)-(4) our main variable of interest is 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 restrict the sample to non-US firms. Column (4) includes an additional variable Global leniency law which measures the passage of laws in the countries that are strong in firm's industry.

In Columns (5)-(6) the dependent variable is capital expenditures by the firm, by last year's assets. Our main variable of interest is Leniency law dummy. Column (5) reports the regression with firm and country controls, equivalent to Column (2). Column (6) includes an additional variable Global leniency law which measures the passage of laws in the countries that are strong in firm's industry. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.028**	0.031***	0.022***	0.033***	0.004	0.003
	2.603	2.808	3.568	2.94	0.723	0.68
Global leniency law				0.024*		-0.001
				1.882		-0.224
Assets		0.078***	0.090***	0.078***	0.013***	0.013***
		11.197	27.422	11.145	3.85	3.856
Leverage		0.001	-0.001	0.001	-0.000**	-0.000**
		1.115	-1.263	1.114	-2.11	-2.111
GDP per capita		-0.03	-0.029***	-0.033	-0.002	-0.002
		-0.964	-3.657	-1.062	-0.19	-0.179
Unemployment		-0.002	-0.003***	-0.002	-0.002*	-0.002*
		-0.99	-4.259	-1.001	-1.9	-1.884
Imports as % of GDP		0	-0.001***	0	0	0
		-0.49	-2.827	-0.611	0.34	0.357
Constant	0.040**	-0.056	0.066	-0.039	0.061	0.059
	2.619	-0.162	0.904	-0.113	0.598	0.618
R-squared	0.166	0.193	0.2	0.193	0.409	0.409
N	397697	340207	208938	340207	180778	180778

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

Table 5. M&A activity

We consider all Compustat Global and North America firms over 1990-2012. This table reports regressions where the dependent variables are various measures of M&A activity by a firm in a particular year. All regressions include firm fixed effects, time fixed effects. Standard errors are clustered at the country level.

In Columns (1)-(5) the dependent variable is the log dollar value of the acquisitions by a firm in a particular year. In Columns (1)-(4) our main variable of interest is 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 restrict the sample to non-US firms. In Column (4) we restrict the sample to non-financial institutions. Column (5) controls for industry*year fixed effects. Column (6) includes an additional variable Global leniency law which measures the passage of laws in the countries that are strong in firm's industry. In Column (7) the dependent variable is dummy variable if the firm has completed any M&A transactions as an acquirer in a particular year. In Column (8) the dependent variable is the dummy variable if the firm has been acquired in a particular year. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leniency law	0.053**	0.055***	0.037***	0.050***	0.049***	0.065***	0.040*	0.020*
	2.38	5.542	3.193	5.045	4.949	6.325	1.941	1.915
Global leniency law						0.094***	0.035**	0.011
						4.392	2.185	1.425
Assets		0.059***	0.132***	0.064***	0.059***	0.059***	0.013	0
		7.089	11.333	6.86	7.374	7.146	1.443	-0.087
Leverage		-0.002***	-0.004**	-0.002**	-0.002**	-0.002***	-0.001	0
		-2.611	-2.507	-2.144	-2.261	-2.638	-1.535	0.717
GDP per capita		-0.084***	-0.123***	-0.090***	-0.097***	-0.093***	-0.043	-0.016
		-3.837	-5.224	-4.101	-4.449	-4.224	-1.446	-0.804
Unemployment		0.002	-0.006**	0.001	0	0.002	-0.002	-0.001
		0.745	-2.249	0.299	-0.014	0.67	-0.598	-0.365
Imports as % of GDP		0.002***	-0.001*	0.002***	0.059**	0.002***	0	0.001
		3.247	-1.691	2.879	2.519	2.715	0.615	1.642
Constant	0.071	0.590***	0.719***	0.649***	0.678***	0.679***	0.353	0.142
	1.288	2.928	3.522	3.235	3.329	3.336	1.24	0.759
R-squared	0.24	0.251	0.241	0.25	0.252	0.251	0.191	0.087
N	543736	434791	260809	396481	434791	434791	434860	434860

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

Table 6. Strategic alliances

We consider all country and SIC 3-digit industry pairs over 1990-2012. This table reports regressions, where in Columns (1)-(3) the dependent variable is number of strategic alliances in the country/industry, in Column (4) the dependent variable is the number of participants in strategic alliances in the country/industry, and in Columns (5)-(6) the dependent variable is the number of M&A transactions in the country/industry. All regressions include country*industry fixed effects and time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is Leniency law dummy. In Columns (1) and (5) we test its effect without any controls. Columns (2), (4) and (6) include an additional variable Global leniency law which measures the passage of laws in the countries that are strong in firm's industry. In Column (3) we control for country characteristics. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Leniency law	0.168	0.169	0.175	0.156	0.586*	0.583*
	0.932	0.934	0.966	0.914	1.821	1.813
Global leniency law		-0.104	-0.103	-0.124		0.735***
		-0.779	-0.822	-1.155		5.179
GDP per capita			0.351**			
			2.074			
Unemployment			-0.012			
			-1.058			
Imports as % of GDP			0.003			
			1.339			
Constant	0.166***	0.166***	-2.926*	0.129***	0.496**	0.496***
	3.784	3.741	-1.904	2.673	2.617	2.677
R-squared	0.725	0.725	0.76	0.647	0.822	0.822
N	408618	408618	318378	408618	408618	408618

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

Table 7. M&A announcement effects

This table reports regressions, where the dependent variable is cumulative abnormal returns on the acquirer's stock after M&A announcement, estimated over 3 day [0,2] window, and winsorized at 1%. Data for years 1998-2010 is used in this analysis. All regressions include year fixed effects. In the baseline specifications, the standard errors are clustered at the three-digit SIC industry level.

In Columns (1)-(5) our main variables of interest are dummies whether the target and acquirer were convicted in the cartel case over the last 5 years. Column (1) reports baseline specification, where expected returns are estimated using domestic country and global market returns over 180 days estimation period. Column (2) adds acquirer country fixed effects and acquirer three-digit SIC industry fixed effects. Column (3) clusters standard errors at the acquirer country level. Expected returns in Column (4) are estimated using three domestic factor (domestic market, SMB, HML) model while in Column (5) they are estimated using three domestic factor (domestic market, SMB, HML) and three global factor (global market, SMB, HML) model. Column (6) reports the results where the main variables of interest dummies whether the target and acquirer were convicted in the cartel case over the last 3 years while in Column (7) whether they were convicted since the start of the sample period. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Acq. cartel last 5 years	0.005**	0.004*	0.005*	0.004**	0.005**		
	2.147	1.892	1.802	2.028	2.033		
Target cartel last 5 years	0.015***	0.014***	0.015***	0.014***	0.016***		
	2.748	2.609	3.285	2.63	2.988		
Acq. cartel last 3 years						0.006**	
						2.418	
Target cartel last 3 years						0.014**	
						2.378	
Acq. cartel since start							0.004**
							2.018
Target cartel since start							0.013***
							2.876
Investment	-0.020***	-0.028***	-0.020***	-0.018**	-0.018**	-0.020***	-0.020***
	-2.8	-3.906	-3.056	-2.344	-2.239	-2.791	-2.802
Profitability	0	0	0	0	-0.001	0	0
	-0.387	-0.287	-0.891	-0.172	-0.781	-0.391	-0.386
Leverage	0.004	0.003	0.004	0.004	0.003	0.004	0.004
	1.308	1.279	1.27	1.335	1.008	1.304	1.304
Assets	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***
	-15.041	-17.245	-10.627	-14.505	-13.929	-15.148	-14.97
Constant	0.057***	0.100***	0.057***	0.057***	0.058***	0.057***	0.057***
	11.122	14.034	12.819	11.319	12.024	11.15	11.108
R-squared	0.023	0.027	0.023	0.024	0.016	0.023	0.023
N	86840	86840	86840	85557	85557	86840	86840

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

Table 8. 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 various measures of M&A activity by a firm in a particular year. All regressions include firm fixed effects, time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is Leniency law dummy. In Columns (1)-(2) the dependent variable is the log dollar value of acquisitions in the same three-digit SIC industry as the firm. In Columns (3)-(4) the dependent variable is the log dollar value of acquisitions in the same three-digit SIC industry and located in the same country as the firm. In Columns (5)-(6) the dependent variable is the log dollar value of acquisitions in the same three-digit SIC industry as the firm but located in a different country. In Columns (7)-(8) the dependent variable is the log dollar value of acquisitions in a different three-digit SIC industry from the firm. Columns (2), (4), (6) and (8) include an additional variable Global leniency law which measures the passage of laws in the countries that are strong in firm's industry. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leniency law	0.029**	0.034**	0.024**	0.026**	0.005	0.008	0.027	0.034
	2.117	2.215	2.134	2.174	0.578	0.945	1.165	1.45
Global leniency law		0.045*		0.019		0.031***		0.066**
		1.847		1.051		2.887		2.24
Assets	0.023	0.023	0.011	0.011	0.013	0.013	0.041	0.041
	1.567	1.582	1.518	1.534	1.594	1.606	1.481	1.493
Leverage	-0.001	-0.001	-0.001	-0.001	0	0	-0.001	-0.001
	-1.546	-1.536	-1.48	-1.476	-1.195	-1.199	-1.272	-1.264
GDP per capita	-0.056*	-0.060*	-0.045	-0.047	-0.012	-0.015	-0.042	-0.049
	-1.676	-1.806	-1.26	-1.322	-1.027	-1.306	-0.635	-0.735
Unemployment	-0.001	-0.001	0.001	0.001	-0.002	-0.002	0.001	0.001
	-0.246	-0.277	0.694	0.68	-0.895	-0.915	0.225	0.205
Imports as % of GDP	0.001	0.001	0	0	0.001	0.001	0.001	0.001
	0.874	0.765	0.071	-0.057	1.515	1.389	0.609	0.493
Constant	0.438	0.48	0.386	0.403	0.06	0.088	0.271	0.333
	1.365	1.495	1.141	1.2	0.527	0.803	0.436	0.536
R-squared	0.129	0.129	0.062	0.062	0.121	0.121	0.21	0.21
N	434860	434860	434860	434860	434860	434860	434860	434860

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

Table 9. Heterogeneity of 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 log dollar value of the acquisitions by a firm in a particular year. All regressions include firm fixed effects, time fixed effects. Standard errors are clustered at the country level.

Our main variable of interest is Leniency law dummy. Columns (1)-(2) split the sample according to the economies of scale in firm's industry, as measured in year 1996. Column (1) reports results for firms in industries with increasing economies of scale, defined when the sum of betas in Cobb-Douglas production function is larger than 1, while Column (2) reports results for those with the sum of betas lower than 1. Columns (3)-(4) split the sample according to the economies of scale in firm's industry, as estimated using Olley and Pakes (1996) procedure. Column (3) reports results for firms in industries with the sum of betas in production function larger than 0.716, while Column (4) reports results for those with the sum of betas lower than 0.716. Columns (5)-(6) split the sample according to the estimates of economies of scale as reported in Burnside (1996). Column (5) reports estimates for firms in manufacturing industries with higher economies of scale while Column (6) reports results for those with lower economies of scale. Columns (7)-(8) split the sample according to the engineering estimates of economies of scale with Column (7) reporting results for manufacturing industries with higher economies of scale while Column (8) reporting results for those with lower economies of scale. Columns (9)-(11) split the sample according to firm size within its country in year 1996. Column (9) is the tercile of the smallest firms, Column (10) is tercile of the medium sized firms and Column (11) is the tercile of the largest firms. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Leniency law	0.062*	0.05	0.065**	0.041	0.050*	0.03	0.056*	0.019	0.073***	0.036	0.089
	1.933	1.47	2.062	1.256	1.81	0.756	1.745	0.523	2.895	1.052	1.166
Assets	0.053	0.068	0.054	0.073*	0.059	0.063	0.05	0.090**	0.032	0.049	0.083
	1.529	1.513	1.472	1.699	1.53	1.594	1.433	2.061	1.188	1.208	1.204
Leverage	-0.001	-0.003	-0.002	-0.001	-0.002	-0.002	-0.001	-0.003	0	-0.001	-0.004
	-1.236	-1.381	-1.487	-0.965	-1.204	-1.313	-1.007	-1.252	-0.285	-0.788	-1.274
GDP per capita	-0.074	-0.097	-0.085	-0.093	-0.06	-0.057	-0.051	-0.073	0.06	-0.052	-0.355**
	-0.874	-0.928	-0.8	-1.099	-0.663	-0.628	-0.641	-0.673	1.379	-0.507	-2.051
Unemployment	0.006	-0.002	0.007	-0.004	0.005	-0.003	0.003	0	0.004	0.001	0.012
	0.834	-0.198	0.849	-0.427	0.583	-0.353	0.34	-0.073	0.854	0.088	0.531
Imports as % of GDP	0.003	0.001	0.002	0.002	0.003	0.003	0.004	0.001	0.003	0.003	0.004
	0.962	0.547	0.655	0.699	1.195	1.147	1.374	0.629	1.481	1.374	0.771
Constant	0.476	0.704	0.624	0.63	0.326	0.326	0.267	0.374	-0.709*	0.288	3.020*
	0.602	0.729	0.629	0.798	0.396	0.414	0.374	0.386	-1.765	0.29	1.811
R-squared	0.257	0.245	0.231	0.265	0.24	0.247	0.239	0.251	0.15	0.2	0.311
N	224742	208863	204532	203542	117606	73469	127038	64037	79037	80617	98343

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

Table 10. Customer reaction to M&A announcements

This table reports regressions, where the dependent variable is cumulative abnormal returns on the stock of a firm in customer industry of the acquirer and target industry after M&A announcement, estimated over 3 day [0,2] window, and winsorized at 1%, where the expected returns are estimated using domestic country and global market returns over 180 days estimation period. Data for years 1998-2010 is used in this analysis. Only transactions where acquirer and target are in the same SIC 3-digit industry are considered in the analysis. 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 3-digit industry level of the customer industry.

In Columns (1)-(5) our main variables of interest are the interactions between the intensity of relationship (i.e. the fraction of inputs that customer gets from supplier industries) and the dummies whether the target and acquirer were convicted in the cartel case over the last 5 years. Column (1) reports baseline specification. Column (2) clusters standard errors at the deal level. Column (3) focuses on non-US transactions. Column (4) reports the results based on whether the target and acquirer were convicted in the cartel case over the last 3 years while in Column (5) whether they were convicted since the start of the sample period. In Column (6) we report the estimates of the pooled regression without deal fixed effects but with year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Intensity of relationship	-0.004***	-0.004***	-0.004***	-0.004***	-0.004***	-0.004***
	-4.006	-5.75	-4.006	-4	-3.989	-4.244
Acq. cartel * Intensity of relationship	0.017***	0.017*	0.017***	0.017***	0.018***	
	3.12	1.944	3.12	3.015	3.599	
Target cartel * Intensity of relationship	0.014	0.014	0.014	0.016*	0.004	
	1.421	1.173	1.421	1.734	0.343	
Acq. cartel						0
						0.125
Target cartel						0
						-0.134
Investment	0.001	0.001	0.001	0.001	0.001	-0.002**
	0.474	1.005	0.474	0.475	0.475	-2.331
Profitability	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	5.053	9.722	5.053	5.053	5.054	10.299
Leverage	0	0.000**	0	0	0	0.001***
	0.84	1.99	0.84	0.84	0.841	2.646
Assets	0	0	0	0	0	0.000***
	0.514	1.236	0.514	0.514	0.513	3.045
Constant	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.002
	-2.99	-7.256	-2.99	-2.989	-2.993	-0.63
R-squared	0.061	0.061	0.061	0.061	0.061	0.014
N	3468841	3468841	3468841	3468841	3468841	3468841

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

Figure 1. Profitability trends around leniency laws

We plot average gross margin for firms that were affected by leniency law for the period of 2 years before to 3 years after the leniency law. As the control sample we consider the firms that were not affected by leniency law over the same period as the treated firm, i.e. control firms did not have leniency law introduced over 2 years before to 3 years after the introduction of leniency law for treated firm.

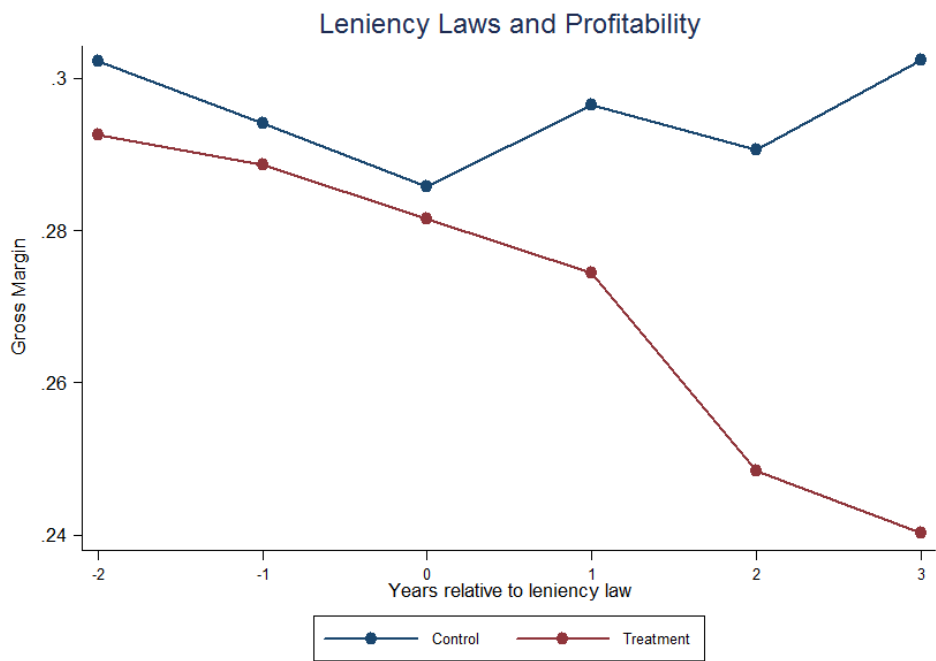
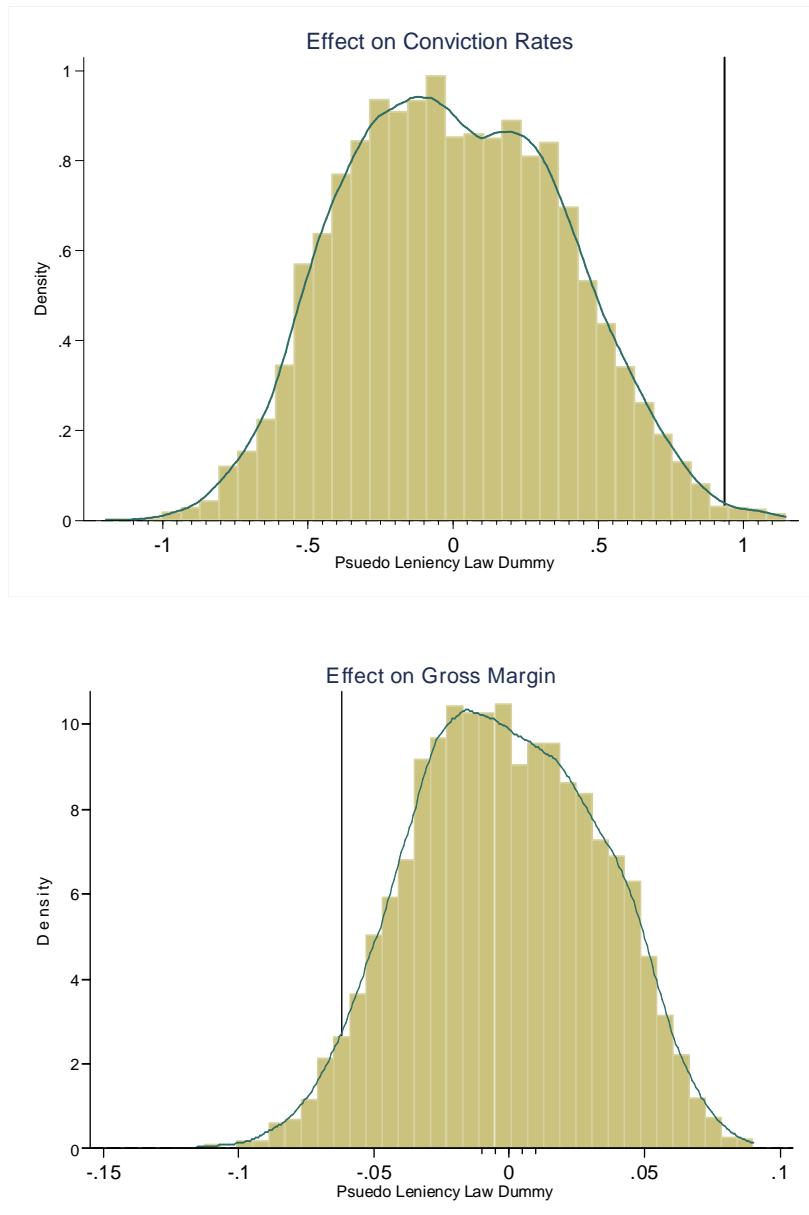


Figure 2. Simulated distributions of leniency laws

We plot the distributions of the coefficients of the regressions based on randomized the passage of leniency laws. We randomize the passage of laws in 63 countries 5000 times. The upper figure shows the coefficients on conviction as in Table 3, Panel A, Column (1) while the lower figure shows the coefficients on profitability as in Table 3, Panel A, Column (1). Vertical lines indicate the coefficient from the actual regression.



Appendix 1. Leniency laws

This table reports leniency law passage by country. Our primary source of information is Cartel Regulation 2013, published by Getting the Deal Through. We complement this dataset using press releases and news articles.

Country	Year	Country	Year
Argentina	None	Lithuania	2008
Australia	2003	Luxembourg	2004
Austria	2006	Malaysia	2010
Belgium	2004	Mexico	2006
Brazil	2000	Netherlands	2002
Bulgaria	2003	New Zealand	2004
Canada	2000	Nigeria	None
Chile	2009	Norway	2005
China	2008	Oman	None
Colombia	2009	Pakistan	2007
Croatia	2010	Peru	2005
Cyprus	2011	Philippines	2009
Czech Republic	2001	Poland	2004
Denmark	2007	Portugal	2006
Ecuador	2011	Romania	2004
Estonia	2002	Russia	2007
Finland	2004	Singapore	2006
France	2001	Slovakia	2001
Germany	2000	Slovenia	2010
Greece	2006	South Africa	2004
Hong Kong	None	Spain	2008
Hungary	2003	Sweden	2002
Iceland	2005	Switzerland	2004
India	2009	Taiwan	2012
Indonesia	None	Thailand	None
Ireland	2001	Turkey	2009
Israel	2005	Ukraine	2012
Italy	2007	United Kingdom	1998
Japan	2005	USA	1993
Jordan	None	Venezuela	None
Korea	1997	Zambia	None
Latvia	2004		

Appendix 2. The Effects of Convicted Cartels on Firm Value

In this Appendix, we establish that breaking up cartels has a significant impact on the firm profitability and firm value.

In the first set of tests, we consider all the Compustat Global and North America firms over the 1990-2012 period. We are interested in whether the fact that the firm is convicted in a cartel case over the prior five years affects its profitability. We cluster the errors at the country*industry level as cartel conviction is likely to have correlated industry effects within the country. We report the results in Table A1, Panel A. In Column (1), we test the effect without any controls, except for firm- and time-fixed effects. We find that cartel conviction reduces gross margin by 3.6 percentage points.

We then provide robustness checks. In Column (2), we control for firm and country characteristics and we find that the effect is qualitatively and quantitatively similar. In Column (3), we cluster the standard errors at the country level. The effect also holds if we limit the sample to non-US firms, as in Column (4). In Column (5), we restrict the sample to the firms that do not change their headquarter countries, and cluster the standard errors at the country level.

In Column (6), we look at the effect of conviction over the prior three year period. We find a slightly smaller but still a statistically significant effect, suggesting that the effect of conviction on the firm profitability is persistent and has long term consequences. We finally look at the cases in which the conviction involves a firm that has been previously convicted in another cartel case during our sample period. In Column (7), we report that such recidivism is associated with a bigger drop in profitability. This can be explained with larger fines in recidivist cases as well as the fact that a convicted firm is more likely to be in the spotlight of regulatory authorities. It would therefore only get involved in the cartels that are particularly profitable and thus being convicted again would produce a bigger drop in profitability.

We note that some firms – e.g. the *customers of the convicted firms* – benefit from the cartel convictions. In Table A1, Panel B, we look at whether the conviction of a supplier leads to an increase in the firm profitability. We identify the supplier-customer relationships from OECD

Input-Output tables (2002 version), following Shahrur (2005) and Ahern and Harford (2014). In particular, a certain four-digit SIC code firm is defined as supplier if the domestic value of the inputs in the corresponding OECD industry category constitutes more than 10% of all the inputs of the firm's OECD industry category.¹⁵ We find that the supplier conviction over the prior three years leads to an increase in gross margin by 3.1 percentage points. This result is robust.

In Column (2), we control for firm and country characteristics. In Column (3), we limit the sample to non-US firms, while in Column (4) and Column (5) we use proxies for the importance of the cartel – i.e. the number of suppliers convicted as well as the log dollar value of cartel affected commerce. In these alternative additional specifications we also find that the customers benefit from supplier conviction.

Further, we show that conviction in price fixing has a direct effect on the firm stock value. This test focuses on the impact of the start of an investigation of price-fixing and the award of penalties on the stock price of the firms involved in the cartel. We employ an event study methodology, in which we compute the cumulative abnormal returns by using three alternative factor-pricing models, described in Section V.

We use two alternative estimation periods – 180 days, or 60 days – and four alternative windows around the event – i.e., 3 day [0,2], 5 day [0,4], 30 day [0,29] and 3 month [0,89]. We winsorize the abnormal returns at the 1% level. We define as event day either the day the first case was opened by any global antitrust authority (“First probe day”), or the day the first decision was made to award penalties by any global antitrust authority (“First penalty day”). The investigations by the cartel authorities should be secret, so the revelation of the probe should be the news to the market.

In Table A2, Panel A, we show that the stock price of the firm drops by 0.7% over three day period and on average by 2.8% in the first month after the probe has been initiated by the first antitrust authority. The latter – longer term – effect is even larger if we consider the resolution of

¹⁵ For instance, 15.4% of inputs in Pharmaceuticals industry in China come from Agriculture industry and 10.7% from Chemicals (excl. Pharmaceuticals) industry. We thus consider firms from these two industries in China to be suppliers for Chinese pharmaceutical firms.

the case. The stock price of the firm drops on average by 4.1% in the first month when the first penalty has been awarded. These effects on stock returns are consistent across different time periods and event windows.¹⁶

Next, we look at how the stock prices reacted to the opening of antitrust investigation of rival firms. We posit that the investigation of a cartel in a specific industry makes it more likely that existing – even if not investigated – cartels will break and new ones will not form as the industry is in the spotlight of the antitrust authorities.¹⁷ Also, we expect that, as some of the potential collaborators are now being investigated, the set of firms that could collude is smaller. The focus on the rival non-convicted firms also lets us estimate the market value that can be attributed to the future drop in profitability rather than fines and legal costs.

We focus on the rival firms which operate within the four-digit SIC industries and investigate separately the firms that are headquartered in the countries where the cartelized market is being investigated from those which are headquartered in other countries.

We report the results in Table A2, Panel B. For the sake of brevity, we only report the results for the abnormal returns around the first probe day. We document that the stock price of a firm reacts negatively to the opening of an investigation of the firm's rivals in the firm's country. The economic size is very similar to the stock price reaction experienced in the case of an opening of investigation on the firm itself. The stock price reaction of the rivals in other countries, as expected, is smaller but also significantly negative.

Taken together, the results of this Appendix provide evidence on the importance of cartels for firm value.

¹⁶ Bosch and Eckard (1991) estimate an average two-day drop in firm value of 1.08% following an indictment in the antitrust investigation in the US; Bizjak and Coles (1997) find a 0.6% two-day drop in defendant's value in private antitrust cases while Aguzzoni, Langus and Motta (2013) estimate the effect of EU surprise inspection to be 2.89% over 30 day window.

¹⁷ Eckbo (1983) studies a similar case how corporate events of rival firms affect future cartel stability. He looks at the rival reactions to horizontal mergers and finds that these generate positive abnormal returns as they increase the probability of successful collusion.

Table A1. Conviction effect on profitability

Panel A. Convicted firms

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

Our main variable of interest is a dummy variable whether the firm has been convicted in a cartel case over the last five years. In Column (1) we test the effect without any additional controls. In Column (2) we control for firm and country characteristics. In Column (3) we instead cluster standard errors at country level. In Column (4) we limit the sample to non-US firms. In Column (5) we restrict the sample to the firms that do not change their headquarter countries. In Column (6) our main variable of interest is whether the firm is convicted in a cartel case over the last three years. Column (7) splits Convicted variable into the first conviction for the firm in our sample and the recidivist conviction. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Convicted	-0.036***	-0.037***	-0.037**	-0.032**	-0.036***	-0.031***	
	-3.529	-3.022	-2.545	-2.203	-2.947	-3.468	
Recidivist convicted							-0.068***
							-4.128
First conviction							-0.025**
							-2.368
Assets		0.080***	0.080***	0.059***	0.079***		
		7.129	6.668	4.011	6.794		
Leverage		0	0	-0.001	0		
		-0.318	-0.397	-1.59	-0.286		
GDP per capita		-0.099***	-0.098***	-0.071***	-0.110***		
		-7.183	-4.102	-5.086	-6.814		
Unemployment		0.012***	0.012**	0.008***	0.012***		
		4.172	2.335	4.551	4.212		
Constant	0.268***	0.795***	0.795***	0.693***	0.914***	0.268***	0.268***
	27.255	7.789	3.45	7.201	7.644	27.3	27.226
R-squared	0.522	0.53	0.531	0.524	0.533	0.522	0.522
N	488980	404048	404107	237098	398114	488980	488980

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

Panel B. Customers of convicted firms

We consider all Compustat Global and North America firms over 1990-2012 that come from the countries for which we have OECD Input-Output tables. This table reports OLS regressions, where the dependent variable is gross margin. All regressions include firm fixed effects, time fixed effects. Standard errors are clustered at the industry level.

Our main variable of interest is a dummy variable whether the supplier of the firm has been convicted in a cartel case over the last three years. In Column (1) we test the effect without any additional controls. In Column (2) we control for firm and country characteristics. In Column (3) we restrict the sample to the firms that do not change their headquarter countries, and cluster standard errors at the country level. In Column (4) our main variable of interest is the number of supplier firms involved in cartels over the last three years. Column (5) our main variable of interest is log value of supplier cartel affected commerce over the last three years. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
Supplier convicted	0.031**	0.032**	0.030**		
	2.262	2.436	2.403		
Log number of suppliers involved in cartels				0.002**	
				2.012	
Log dollar value of supplier cartel affected commerce					0.005***
					3.632
Convicted	-0.042***	-0.042***	-0.042***	-0.041***	-0.042***
	-2.973	-2.829	-2.887	-2.809	-2.844
Assets		-0.105***	-0.114***	-0.111***	-0.107***
		-5.275	-5.103	-5.237	-5.194
Leverage		0.020***	0.020***	0.020***	0.020***
		4.058	4.07	4.44	4.135
GDP per capita		0.084***	0.084***	0.085***	0.084***
		5.988	5.8	5.887	5.975
Unemployment		0.001	0.001	0.001	0.001
		1.122	1.053	1.129	1.137
Constant	0.236***	0.800***	0.893***	0.864***	0.838***
	20.1	4.808	4.922	4.984	4.855
R-squared	0.533	0.536	0.539	0.536	0.536
N	237436	221965	218750	221912	221912

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

Table A2. Stock price reaction to conviction

Panel A. Convicted firms

This table reports univariate comparisons of cumulative abnormal returns on the stock of firm indicted in the collusion case. Data for years 1998-2010 is used in this analysis. Cumulative abnormal returns are estimated over either 3 day [0,2], 5 day [0,4], 30 day [0,29] and 3 month [0,89] window. Three models are used to estimate abnormal returns: Model 1 estimates expected returns using domestic country and global market returns; Model 2 estimates expected returns using three domestic factor (domestic market, SMB, HML) model; Model 3 estimates expected returns using three domestic factor (domestic market, SMB, HML) and three global factor (global market, SMB, HML) model. Either 180 day, or 60 day estimation periods are used. Abnormal returns are winsorized at 1%.

First probe refers to the day when the first case was opened by any global antitrust authority. First penalty refers to the day when the first decision has been made on the awarded penalties by any global antitrust authority. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	First probe			First penalty		
	Mean	t stat	N	Mean	t stat	N
CAR(0,2), Model 1, 180 day est.	-0.007***	-6.007	905	-0.006***	-4.259	827
CAR(0,4), Model 1, 180 day est.	-0.010***	-6.503	905	-0.012***	-6.713	827
CAR(0,29), Model 1, 180 day est.	-0.028***	-5.109	905	-0.041***	-7.092	827
CAR(0,89), Model 1, 180 day est.	-0.087***	-6.098	905	-0.120***	-7.537	827
CAR(0,2), Model 2, 180 day est.	-0.007***	-6.608	863	-0.006***	-4.232	797
CAR(0,2), Model 3, 180 day est.	-0.005***	-4.23	863	-0.002*	-1.647	797
CAR(0,2), Model 1, 60 day est.	-0.006***	-4.904	934	-0.006***	-3.742	850

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

Panel B. Rivals of convicted firms

This table reports univariate comparisons of cumulative abnormal returns on the stock of firms whose (same four-digit SIC code) rivals were indicted in the collusion case. Cumulative abnormal returns are estimated over either 3 day [0,2], 5 day [0,4], 30 day [0,29] and 3 month [0,89] window over the time when the first case on rivals was opened by any global antitrust authority. Three models are used to estimate abnormal returns: Model 1 estimates expected returns using domestic country and global market returns; Model 2 estimates expected returns using three domestic factor (domestic market, SMB, HML) model; Model 3 estimates expected returns using three domestic factor (domestic market, SMB, HML) and three global factor (global market, SMB, HML) model. Either 180 day, or 60 day estimation periods are used. Abnormal returns are winsorized at 1%.

Same country as convicted market refers to rivals that are headquartered in the country where the investigated cartel took place. Other countries refer to rivals that are not headquartered in the country where the investigated cartel took place. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Same country as convicted market			Other countries		
	Mean	t stat	N	Mean	t stat	N
CAR(0,2), Model 1, 180 day est.	-0.008***	-5.16	1421	-0.004***	-12.501	26067
CAR(0,4), Model 1, 180 day est.	-0.012***	-6.097	1421	-0.007***	-15.273	26067
CAR(0,29), Model 1, 180 day est.	-0.036***	-5.706	1421	-0.020***	-15.402	26067
CAR(0,89), Model 1, 180 day est.	-0.097***	-6.52	1421	-0.051***	-15.482	26067
CAR(0,2), Model 2, 180 day est.	-0.010***	-6.127	1344	-0.005***	-15.077	23340
CAR(0,2), Model 3, 180 day est.	-0.002	-1.323	1344	-0.002***	-5.045	23340
CAR(0,2), Model 1, 60 day est.	-0.006***	-3.845	1465	-0.003***	-9.35	26411

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