Capital Structure Under Collusion

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Abstract

We analyze the financial leverage of firms that collude to soften product market competition by forming a cartel. We find that cartel firms have lower leverage during collusion periods. This is consistent with the idea that cartel firms strategically reduce leverage to make their cartels more stable, because high leverage makes deviations from a cartel agreement more attractive. Given that cartels have a large economic footprint, their study is also relevant for the capital structure literature, which has largely ignored the role of anti-competitive behavior.

Keywords: Capital Structure; Financial Leverage; Collusion; Cartels

JEL Classification: G32, L12

1. Introduction

A large and growing literature studies the interdependence of financial and product-market decisions. Some of this work studies whether leverage turns rival firms into less or more aggressive competitors,[1] while other work studies how a firm's competitive position affects its optimal financial structure.[2] This literature has focused on traditional product-market strategies, such as the choice of prices, output, and product quality. However, it has largely ignored *anti-competitive* product-market strategies, such as collusion with competitors by forming a cartel. In this paper, we provide evidence that when firms form a cartel, they strategically reduce their financial leverage to sustain collusion.

Cartels are illegal agreements in which firms collude with competitors to increase profits by softening competition. They distort market outcomes and cause significant deadweight losses (see Levenstein and Suslow 2006 for an overview). Cartels are more common than is widely known: Publicly traded U.S. firms convicted of cartel activity between 1990 and 2010 accounted for more than one fifth of the total U.S. market capitalization.[3]

Cartel agreements are difficult to enforce, since they are illegal. This makes them potentially unstable as each cartel firm has an incentive to deviate from the agreement and steal profits from the other cartel firms. Maksimovic (1988) analyzes theoretically how financial leverage affects this incentive to deviate. In his model, cartel firms use trigger strategies (Friedman 1971) to enforce a collusion agreement: Any deviation is punished by having all cartel firms revert to unfettered competition, and the threat of losing future collusion profits keeps cartel firms from deviating. Maksimovic (1988) shows that in such a setting, sufficiently large debt can make deviations attractive: The shareholders reap the rewards of a deviation (increased net profits in that period), while the lenders share in the cost of a deviation (lower future profits reduce a firm's ability to make the full promised debt payments). High leverage thus destabilizes cartels. The empirical prediction is that all else equal,

[1] Exogenously given debt can make firms more aggressive (Brander and Lewis (1986)), or less aggressive (Bolton and Scharfstein (1990), Maksimovic and Titman (1991), Chevalier and Scharfstein (1996)), or the effect may be ambiguous (Showalter (1995)); with endogenous borrowing, firms with debt are less aggressive (Povel and Raith (2004)). Empirically, Opler and Titman (1994), Chevalier (1995a,b), Kovenock and Phillips (1995, 1997), Khanna and Tice (2000), and Grullon, Kanatas and Kumar (2006) find that debt makes firms less aggressive; Zingales (1998) and Busse (2002) find the opposite; Phillips (1995) and Dasgupta and Titman (1998) find mixed results.

[2] See Maximovic and Zechner (1991) or MacKay and Phillips (2005).

[3] This is based on firms included in the Private International Cartels (see Section 3) and Compustat databases.

cartel firms should have lower leverage during collusion periods than other firms.

This prediction is cross-sectional in nature. However, drawing conclusive inferences from the data based on cross-sectional tests is challenging, given that cartel and non-cartel firms likely differ in many observed and unobserved characteristics that may also affect leverage choices. We overcome this challenge by testing our predictions using a difference-in-differences approach, which allows us to examine how leverage changes when firms start colluding (using within-firm variation), relative to a set of control observations coming from both non-collusion periods (for the cartel firms) and matched non-cartel firms. Key to this identification strategy is that when firms contemplate forming a cartel, they realize that they may have to reduce their leverage for strategic reasons (to prevent deviations). Our source of data on cartel firms and collusion periods is the Private International Cartels (PIC) database (see Section 3).

We find that cartel firms exhibit lower leverage during collusion periods. On average, leverage is reduced by three percentage points, compared with pre-collusion years. We obtain similar results both when including matched control firms (the difference-in-differences setup) and when including only firms that were cartel members at some time during the sample period (the "eventually treated" sample). The results are thus driven by leverage changes at the cartel firms, not by changes at non-cartel firms. These findings are consistent with the prediction that cartel firms *strategically* reduce their financial leverage during collusion periods, to make their cartels stable.

To better understand the role of strategic leverage reductions in supporting collusion agreements, we explore under what conditions we would expect more significant reductions in leverage. We derive four additional empirical predictions. First, we expect more significant reductions if firms face stronger competitive pressure, for example when products are homogenous, since it is then easier and more attractive to take away profits from other cartel firms by deviating. Second, leverage reductions should be more significant for firms that had high leverage before collusion started, since it is more likely that their leverage is above the level that makes their cartel unstable. Third, if firms expect a significant benefit from colluding, they should be more willing to make an extra effort to make their cartel stable, including bearing the opportunity cost of larger reductions in leverage (e.g., lost tax shields). Hence, we expect stronger leverage reductions when cartels are important. Fourth, leverage reductions should be less pronounced when the economy or industry is in a slump, as deviations are less attractive if aggregate profits are low.

To test these four predictions about conditions under which leverage reductions should be more pronounced, we use triple-differences setups. First, we find that the reduction in leverage is stronger when firms face stronger competitive pressure, consistent with our prediction. We consider three different measures of competitive pressure: Product-market fluidity (Hoberg, Phillips, and Prabhala 2014); R&D and advertising expenses (Sutton 1991); and industry homogeneity (Parrino 1997). The results are similar for all three. Second, we find that when cartel and control firms are highly levered before collusion starts, the reduction in leverage during collusion periods is stronger. This finding is consistent with our prediction, and it holds whether we study leverage in absolute terms or relative to a firm's industry. Third, we find stronger reductions in leverage when cartel firms likely benefited more from collusion, consistent with our prediction. We use two indirect proxies for the potential gains from cartel membership: The fines imposed on a cartel firm (fines are linked to the economic impact of collusion), and whether the cartel firm was acting as cartel leader and thus likely stood to gain more from the cartel. Fourth, we find that the reduction in leverage by cartel firms is smaller during economic downturns, again consistent with our prediction. We obtain similar findings when using recession years to identify economic downturns, or years with low GDP growth or low industry sales growth.

An alternative explanation for the reduction in financial leverage observed during collusion periods is that firms use their increased profits to retire debt. This alternative explanation, however, cannot explain our findings, as our results hold after controlling for lagged profitability. It is also hard to square this alternative hypothesis with all the results from our triple-difference tests. This last argument highlights the usefulness of the triple-differences results: besides shedding light on how leverage reductions are used strategically, they also raise the bar for alternative explanations.

The main contribution of our paper is to show that when firms decide to collude by forming a cartel, they also strategically reduce their leverage. The mechanism behind this has been analyzed on a theoretical level (see Maksimovic 1988 and Stenbacka 1994), but its empirical validity has remained unexplored. Prior studies have documented that once cartels are in place, they become less stable or more likely to break up when cartel firms are highly indebted (Lamoreaux 1985; Grossman and Paulson

Gjerde 2009; Levenstein and Suslow 2011).[4] Our study goes beyond this by documenting that firms internalize the destabilizing effects of debt and reduce it strategically when forming a cartel.

Our findings have implications for the literature studying the interdependence of financial and product-market decisions. The focus of this work is on traditional product-market strategies: output, pricing, product quality, advertising, etc. New insights can be gained by including explicit anti-competitive strategies in the analysis of product-market decisions. Given that cartels have a large economic footprint, our findings are also relevant for the capital structure literature: Anti-competitive behavior is an important factor that can explain capital structure decisions.

Our paper is also relevant for antitrust research because our findings support the idea that cartel firms use reductions in leverage to stabilize cartels. Our paper is related to recent work linking antitrust and finance issues. Dasgupta and Žaldokas (2019) test whether "strategic debt" (Brander and Lewis 1986) or the need for financial flexibility due to a threat of predation (Bolton and Scharfstein 1990) better explain leverage changes after changes in antitrust policies, but they do not study the role of capital structure in the functioning of a cartel. Dong et al. (2019) study how changes in antitrust policies affect profits and M&A activity, but they do not consider the effects on capital structure. Finally, Artiga et al. (2019) investigate cartel convictions from the perspective of corporate governance.

In a broader context, our paper contributes to the literature that studies the *strategic* use of financial leverage. Brander and Lewis (1986) show that, in a model with uncertainty, debt can make competitors more aggressive because of a limited liability effect. Bronars and Deere (1991) and Matsa (2010) show that firms can increase their leverage to make unions less aggressive in wage negotiations. Fulghieri and Nagarajan (1996) show that high leverage can help an incumbent monopolist deter entry. In takeover settings, strategic debt can benefit a target (Stulz 1988) or a hostile bidder (Müller and Panunzi 2004). We contribute to this research by documenting that cartel firms strategically reduce their leverage to make their cartels more stable.

[4] Busse (2002) and Phillips and Sertsios (2013) show that high leverage makes firms more aggressive (for example, they are more likely to start price wars). They argue (but do not show) that this could be due to the breakup of tacit collusive agreements. (Tacit collusion can also be supported by trigger strategies.)

2. Hypotheses

In trigger strategy models of collusion (see, e.g., Friedman 1971), firms repeatedly compete in the product markets, but they try to increase their profits by colluding, i.e., agreeing to produce less and charge higher prices than they otherwise would. While this generates higher profits, each firm has an incentive to deviate from such a cartel agreement: If all other firms follow the agreed policy, a deviating firm can increase its profit even more by stealing market share from the more restrained cartel firms. This threat of deviations destabilizes cartels, but such deviations can be prevented if the cartel firms agree to use trigger strategies, requiring all firms to follow the agreement in each period and to revert to unfettered competition if there is a deviation. This threat of destroying future collusion profits makes a deviation less attractive, as the incremental profits would be lost for all firms. However, if this threat prevents deviations, then in equilibrium it is never executed, and the profit losses do not arise.

Maksimovic (1988) expands this analysis by showing that high leverage increases the attractiveness of deviations and thus makes cartels less stable. The driving force is that shareholders enjoy the benefits of a deviation (i.e., increased profits), while lenders bear some of the cost, because when the cartel firms follow the trigger strategy and revert to full competition, the reduced profits do not cover the promised payments to the lenders. This effect from debt on product-market strategies is similar to the limited liability effect analyzed in Brander and Lewis (1986), in a model of competition with uncertain demand.

A key assumption in the Maksimovic (1988) model is that firms are aware that financial leverage can cause aggressive product-market behavior (the incentive constraints in the model compare payoffs net of debt payments).[5] This assumption finds support in detailed analyses of historical cartels (see, e.g., Lamoreaux 1985; and Grossman and Paulson Gjerde 2009) which suggest that the cartel firms were aware of the issue (see also Levenstein and Suslow 2011). The

[5] Maksimovic (1988) does not incorporate any costs of reducing leverage into the model, but he conjectures how the results would change if tax shields were incorporated. Stenbacka (1994) analyzes a model with corporate income taxes, showing that the results are not changed qualitatively. Piccolo and Spagnolo (2014) suggest that firms could appoint bankruptcy-averse CEOs to overcome commitment problems.

main result in Maksimovic (1988) is that cartel stability requires cartel firms to have sufficiently low leverage. This leads to the cross-sectional prediction that cartel firms have lower leverage than non-cartel firms.

Given that cartel and non-cartel firms likely differ in many observed and unobserved characteristics, it is difficult to interpret the results from cross-sectional comparisons. So instead of focusing on pure cross-sectional tests, we use a difference-in-differences approach: We examine how leverage changes between collusion and non-collusion periods for cartel firms (within-firm variation), relative to a set of control observations coming from non-collusion periods or non-cartel firms (for details, see Section 3). We expect cartel firms to reduce their leverage during collusion periods relative to pre-collusion periods.

H1: *Cartel firms have lower leverage during collusion periods.*

Our interpretation of the model and results in Maksimovic (1988) is that low leverage serves as a commitment device to follow the collusion agreement (high prices, low output), since it mitigates the incentive to deviate from the agreement. When the incentive to deviate is stronger, a larger reduction in leverage may be necessary. We use this intuition to develop further predictions about strategic leverage reductions during collusion periods.

A first factor that influences the collusion and leverage decisions is the competitive environment in which firms operate. The aim of forming a cartel is to mitigate the profit-reducing effects of competition. Cartels are illegal, and we should expect that firms first use legal methods that mitigate those effects, if feasible. The industrial organization literature emphasizes the role of product differentiation (through easily recognized designs, brands, advertising, etc.) in mitigating competitive pressure. But product differentiation is not a feasible or effective strategy in all markets, for example if products are by nature homogenous, and some firms may resort to collusion as a way to increase their profits.

Importantly, the strength of competitive pressure also affects the incentives to deviate from a collusion agreement, and thus the stability of a cartel. With undifferentiated products, it is easier to take market share away from the other cartel firms (see, e.g., Chang 1991), and cartel firms should therefore find it more tempting to deviate from a collusion agreement. Cartel instability is therefore a stronger concern when potential cartel firms face stronger competitive pressure, and we expect such cartels to more likely require reductions in leverage, or to require larger reductions as a commitment device.

H2: *The leverage reduction is more pronounced for cartel firms that face stronger competitive pressure.*

A second factor is the leverage ratio before collusion starts. If firms trying to form a cartel have high leverage, compared with the leverage ratio that makes the cartel stable, then a larger reduction is needed to ensure the cartel is stable. So we should observe that leverage reductions are more likely, or larger, for firms with relatively high leverage during pre-collusion years. Conversely, if their leverage during pre-collusion years is relatively low, a reduction in leverage should be less likely, or small.

H3: *The leverage reduction is more pronounced for cartel firms that had high leverage before collusion started.*

A third factor that influences the collusion and leverage decisions is the size of the incremental profit from collusion. If the incremental profit is not large, then the cost of a large reduction in leverage (lost tax shields, etc.) may outweigh the benefits (higher profits), and collusion may not be feasible (a cartel may not be formed, or it may fall apart quickly). Collusion with moderate incremental gains is feasible only if the required leverage reduction is small, i.e., if the leverage is optimally low in years during which cartel firms are not colluding. On the other hand, if collusion generates large incremental profits relative to a competitive scenario, then cartel firms should be willing to make an extra effort to ensure the cartel's stability, including a large reduction in leverage. That is, if leverage is optimally high in years during which firms are not colluding, then collusion is feasible only if the gains from collusion are sufficiently large. We thus expect larger reductions in leverage if a firm stands to gain more from forming a cartel.

H4: *The leverage reduction is more pronounced for cartels that let firms earn larger incremental profits.*

The state of the economy constitutes a fourth factor that influences the collusion and leverage decisions. If the economy is in a recession or the industry is experiencing a downturn, the costs and benefits of a deviation from the cartel agreement are different than if times are good. Rotemberg and Saloner (1986) analyze this in a trigger strategy collusion model in which demand is uncertain and changes from period to period, and firms can observe the current state of demand before making their product-market decisions. During a boom, aggregate profits are larger, so the short-term profits from deviating are larger, and therefore it is harder to sustain a cartel during booms. Stenbacka (1994) expands that model to analyze the role of debt. Consistent with Maksimovic (1988), he finds that higher debt levels make it harder to collude, and for a given debt level, collusion is harder to sustain in booms and easier to sustain in recessions. Hence, to avoid instability, cartel firms have to reduce their debt by more if a cartel is operating during times in which the economy is doing well, and smaller reductions are sufficient if a cartel operates during a recession.

H5: *The leverage reduction is less pronounced during recessions.*

3. Data and Variable Construction

Our analysis uses the Private International Cartels (PIC) database, which contains information on virtually all private international cartels detected by antitrust authorities between 1990 and 2012. This database is described in detail in Connor (2014). To our knowledge, it is the most inclusive data set on cartels available, and it is used in several recent papers on collusion.[6] Recent studies that do not use the PIC data include Levenstein and Suslow (2016) and Miller (2009), who collected data on prosecutions brought by the U.S.D.O.J. under the Sherman Act. Of those cases, virtually all publicly traded U.S. cartel firms are included in our data set.

The PIC data set covers only "private" cartels — government-sanctioned "public" cartels (for

[6] See the papers by Connor and various co-authors, as well as Artiga et al (2019), Dasgupta and Žaldokas (2019), Dong et al. (2019), and Han and Žaldokas (2016).

example, OPEC) are excluded because they are not at risk of prosecution. Furthermore, the data include only cartels with an "international" flavor, i.e., cartels that include firms from multiple countries, or cases in which an antitrust authority pursued foreign firms. This allows for a more complete coverage of cartel activity: for example, cartels involving U.S. firms may operate or be prosecuted outside the U.S. Excluding cartels whose participants and activities were limited to one country does not seem to limit the sample in a significant way, since large cartels tend to include foreign firms or operate in multiple countries.

The information in the PIC database is collected from press releases issued by antitrust authorities such as the Department of Justice and the Federal Trade Commission in the U.S., the European Commission (Directorate-General for Competition), or Canada's Competition Bureau. Firms are included in the database if an antitrust authority imposed fines or if class action lawsuits were filed. The data does not include *all* cartels but only those that were *detected* (Connor (2014) estimates that only about 10-30% of all cartels are detected; see also Bryant and Eckard (1991)).

Given that the PIC database only includes firms that were actively colluding cartel firms, it does not allow us to test whether non-cartel firms were *contemplating* the formation of a cartel. We also do not know with certainty that firms not included in the database never were in a cartel. However, the data allows us to test predictions contingent on a firm being a cartel firm, and to attribute changes in their capital structure to their decision to join a cartel. This is how we formulated the hypotheses in Section 2.

From the PIC database, we collect the following information for each cartel firm: Name, country of incorporation, and the start and end dates of collusion. We restrict the sample to U.S. firms, since several of our tests use additional data sets that focus on U.S. firms. We require that these firms be included in Compustat, which is the case for 213 firms. We use Compustat data starting with the year 1985, since the first cartel in our data was formed in 1990 and we use data for up to five years before the formation of a cartel (and up to five years after a cartel is dissolved).

Many of those cartel firms participated in more than one cartel, and therefore they have multiple collusion periods that may overlap. This complicates the process of grouping firm years into pre-collusion, collusion, and post-collusion years. Since the timing of collusion is central to our identification, we resolve this issue by dropping firms that participated in more than one cartel during the sample period. The reduction in the sample size is significant but unavoidable. The final sample includes 1,368 firm-years for 90 cartel firms that participated in 56 cartels. Of these 1,368 firm-years, 569 observations correspond to active collusion years, 401 to pre-collusion years, and 398 to post-collusion years.

Table 1 here

Table 1 shows summary statistics for our sample of cartel firms (all variables are defined in the Appendix). The average duration of collusion is just under six years. On average there were nine cartel firms (but only few of these are included in our sample, since it excludes non-U.S. firms, privately held U.S. firms, and publicly traded U.S. firms that participated in two or more cartels). More than half of the cartel member firms are North American firms, but there are cartel member firms from across the world, and cartels were active (and prosecuted) in many countries. Fines were imposed on some cartel firms, and some executives received jail terms. The industry distribution (based on one-digit SIC codes, see Panel C of Table 1) does not show particular patterns. Panel D of Table 1 summarizes the start and end years of collusion.

In several of our tests we use a matched set of "non-cartel" firms as controls for our sample of single-cartel firms. We restrict the potential set of control firms to U.S. firms included in Compustat that were *not* cartel firms, i.e., they were not in the PIC database, and that operate in the same industries (4-digit SIC code) as the sample of single-cartel firms (56 industries).[7] To reduce the probability of control firms being "false negatives" (i.e., undetected firms that are part of international collusive agreements), we further restrict the set of candidate control firms to those without operations outside the U.S.

We use coarsened exact matching (CEM) (Blackwell et al. 2009; Iacus et al. 2011, 2012; Balsmeier et al. 2017) to obtain a set of control observations comparable to those from the single-cartel sample. In this matching approach, "treated" and "control" samples are divided into cells by

[7] In unreported tests, we find that non-cartel firms in industries with cartels reported in the PIC database have unchanged leverage during years of cartel activity for their industries. As peer effects do not appear to be significant, same-industry firms are good candidates for "control firms", especially since in the regressions we can control for common industry-level trends that may affect both cartel and control firms.

multivariate sorting and then matched within each cell. Within each of the 56 industries with cartel firms, we sort the observations by logarithm of assets (10 bins), logarithm of sales (10 bins), and cash flow volatility (100 cells). CEM first drops any observations from the sample that do not have at least one counterpart in the opposite group. Thus, CEM enforces common support between the treated and control groups. CEM then produces a weight for each observation that ensures joint covariate balance between the treated and control groups. A key advantage of CEM matching over other matching techniques is that there is no need to fix the control sample size ex ante (e.g., 1:N matching in the case of propensity score matching).

Table 2 reports the covariate differences between the cartel and control observations. Eighty-nine out of ninety firms found a matched pair with common support. The matching seems to work well in reducing imbalances in observables: The firm-level variables are not statistically different across the two groups (financial variables are constructed using Compustat data and winsorized at the 1% level).

Table 2 here

4. Empirical Analysis

4.1 Leverage During and After Collusion Years

To analyze the relation between collusion and financial policies we use the following baseline empirical model:

$$y_{it} = \alpha + \beta * Collusion_{it} + \gamma * PostCollusion_{it} + \boldsymbol{\Omega}'\boldsymbol{X_{it-1}} + \varphi_i + \mu_{jt} + \varepsilon_{it}. \qquad (1)$$

The subscript *i* indexes firms, *j* indexes industries, and *t* indexes years. Our main dependent variable, y_{it}, is book leverage. *Collusion* takes a value of 1 for cartel firms during collusion years, and 0 otherwise. This is a difference-in-differences strategy, using *Collusion* as the treatment. The parameter β captures changes in the average leverage ratio of cartel firms during collusion periods, compared with pre-collusion years, while controlling for contemporaneous changes in leverage by matched control firms. Under Hypothesis H1, β should be negative.

We also include the dummy *Post Collusion* in Equation (1). This variable takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise.[8] The parameter γ captures changes in the average leverage ratio of cartel firms after collusion periods end, compared with pre-collusion years, while controlling for contemporaneous changes in leverage by matched control firms. We include the *Post Collusion* indicator for two reasons. First, one might expect leverage to return to pre-collusion levels after collusion ends, since there is no benefit from keeping it low anymore, while there are costs (for example, lost interest tax shields). Finding a statistically insignificant coefficient for *Post Collusion* would thus reinforce the notion that leverage is reduced *strategically* during active collusion years. Second, leverage may be "sticky," and post-collusion leverage ratios may be different from pre-collusion leverage ratios if there are adjustment costs. If this is the case, then pooling the pre-collusion and post-collusion firm-years could qualitatively change the findings, while including the *Post Collusion* dummy alongside the *Collusion* dummy allows for a cleaner estimation of the effects.

We include a set of controls $\boldsymbol{X_{it-1}}$ that comprises variables commonly used in the capital structure literature: Lagged *Tangibility,* lagged *Profitability,* lagged *Sales*, and lagged C*ash Flow Volatility*. Firm and industry-by-year fixed effects are represented by φ_i and μ_{jt}, respectively.[9] Controlling for industry-by-year fixed effects allows us to control for industry-level trends that affect both cartel and non-cartel firms simultaneously. Given that the control sample was obtained using CEM, each observation is weighted using the weights obtained in the matching.

As an alternative to including matched control firms, we estimate the relation between collusion and capital structure using cartel firms only, i.e., the "eventually treated" sample (see Bertrand and Mullainathan (2003) for a similar approach). Given that different cartels are active during different periods, the non-collusion years of some cartel firms act as control observations for the collusion years of other cartel firms. Estimating the effects using the eventually treated sample helps explore whether changes in behavior can be attributed to the treated group, rather than to the control group. It also mitigates concerns regarding the comparability across cartel (treated) and non-cartel

[8] We do not include observations for the years following *Post Collusion*, so the default period consists only of pre-collusion years.

[9] In all our specifications we adjust standard errors for heteroscedasticity and industry clustering. We cluster standard errors at the industry level because firms compete and collude at this level of aggregation. This clustering strategy allows for three types of arbitrary correlations in the error term: (1) Error correlation across different firms in a given industry and year; (2) error correlation across different firms in a given industry over time; and (3) error correlation for a given firm over time (see Petersen 2009). Our sample contains 56 unique 4-digit SIC codes.

(control) firms in terms of *unobservables*, since all firms in the eventually treated sample chose to collude at some point in time (i.e., they share the decision to collude). The downside is a loss of power due to the reduced sample size, and that we can only include year fixed effects instead of industry-by-year fixed effects, which precludes us from controlling for industry-level trends that might affect cartel firms at the time of collusion. Since the eventually treated sample does not include matched control firms, each observation is equally weighted.

Table 3 presents the baseline results. Columns (1) and (2) show the results using both cartel and non-cartel firms. Columns (3) and (4) show the results using cartel firms only (i.e., the eventually treated sample). In Columns (1) and (3) we present the results from regressions without controls, while in Columns (2) and (4) we control for the set of variables in the vector $\boldsymbol{X}_{it-1}$. In all four regressions, *Collusion* has a significant negative effect on leverage, reducing it by around three percentage points. These effects are statistically significant at 5% levels, and they are economically significant too: The average leverage ratio of cartel firms is 27%, so the leverage ratio decreases by over a tenth. The coefficients for *Collusion* are very similar across the four columns, which suggests that the effect is driven by cartel firms reducing their leverage ratio, and not by leverage changes made by non-cartel firms (recall that Columns (3) and (4) use cartel firm data only). Overall, our findings are consistent with Hypothesis H1.

Table 3 here

The *Post Collusion* coefficients reported in Table 3 are statistically insignificant. The point estimates are positive (though economically small) in Columns (1) and (2), where the control firms are included in the estimation; and they are negative in Columns (3) and (4), where only the eventually-treated sample is included. One motivation for the inclusion of *Post Collusion* in all specifications was to prevent potentially "sticky" leverage ratios from confounding the results; the consistently insignificant coefficients suggest that this is not a major concern. The second motivation was to study whether leverage ratios revert to pre-collusion levels. The evidence suggests that this is the case. After collusion ends, there is no benefit from keeping leverage low, and the costs of keeping it low become relevant (for example, lost interest tax shields). Our finding that leverage reverts to pre-collusion levels thus lends support to our main prediction, that cartel firms *strategically* reduce their leverage during collusion periods.

4.2 Timing of the Leverage Reduction

We now study how leverage changes from year to year, focusing on the years around the start of collusion and the years around the end of collusion. We do this for two reasons. First, a year-by-year analysis allows us to analyze pre-collusion trends in leverage, and thus to test the parallel trends assumption that underlies difference-in-differences setups. Second, the timing of leverage changes is interesting in itself, since it sheds additional light on the cycle of leverage around collusion years. Specifically, we examine whether leverage reductions happen at the same time as the start of collusion, or whether they are delayed. We also examine when leverage reverts to pre-collusion levels (above we showed that leverage reverts to pre-collusion levels once collusion ends, but not exactly when this happens). Since we do not have a dynamic model of collusion and leverage decisions, there are no testable predictions about these timing decisions, so our analysis of leverage dynamics is, by nature, exploratory.

To examine the dynamics of leverage before, during, and after collusion periods, we estimate the following variation of Equation (1), using the full sample of cartel and matched non-cartel firms:

$$y_{it} = \alpha + \sum \beta_h * d_{ih} + \varphi_i + \mu_{jt} + \varepsilon_{it}. \qquad (2)$$

As before, y_{it} is *Leverage,* and φ_i and μ_{jt} are fixed effects. The dummy variable d_{ih} indexes years during and around collusion periods. The subscript h indexes the years that immediately precede collusion years ($h \in \{-2, -1\}$), years that immediately follow collusion years ($h \in \{1, 2,\}$), or years that are collusion years ($h \in \{$col1, col2, col3, oth. col$\}$ for full collusion years ("oth. col" includes the fourth and later years of collusion), $h = 0^-$ for a partial collusion year at the start of collusion, and $h = 0^+$ for a partial collusion year at the end of collusion). For example, if a cartel formed in September 2001 and dissolved in May 2005, then $d_{i0^-} = 1$ in 2001 and zero otherwise; $d_{icol1} = 1$ in 2002, $d_{icol2} = 1$ in 2003 and $d_{icol3} = 1$ in 2004 and zero otherwise; $d_{i0^+} = 1$ in 2005 and zero otherwise; and so on. We distinguish full years of collusion from partial years of collusion, since during partial years of collusion the effects are likely weaker, so this more granular data analysis is more informative. Pooling partial and full collusion years yields very similar results.

We plot the regression coefficients β_h and their 95% confidence intervals in Figure 1. The decrease in *Leverage* seems to be concentrated in the collusion years. In the preceding years, the coefficients are not significantly different from zero, so the reduction in leverage is likely not driven by pre-existing trends. Hence, the parallel trends assumption is reasonable in our setting.

Figure 1 here

Figure 1 also shows that the leverage reductions are most pronounced during the first two years of collusion, so they are delayed relative to the start of collusion. A possible explanation is that if a cartel is formed at short notice, practical reasons can make it difficult to pay off debt quickly. Furthermore, Figure 1 shows that leverage returns to pre-collusion levels by the time collusion ends. It is slightly higher in the later years of collusion (grouped in "oth. col" in Figure 1) than in the first three years of collusion. A possible explanation is that some cartels lose their discipline over time, and some cartel firms increase their leverage ratios closer to non-collusion levels (because of benefits from having debt financing). Such increases in leverage may have contributed to the cartel's demise, because of their destabilizing effect.

To shed further light on the leverage patterns in Figure 1, we examine whether the changes we observe are due to a drop in debt (the numerator) or an increase in assets (the denominator). Figure 2 displays the coefficients for the specification (Equation (2)) used to create Figure 1, but instead of using *Leverage* as the dependent variable it uses (separately) the logarithm of debt and the logarithm of assets as the dependent variables. The coefficients strongly suggest that reductions in debt levels are driving the decrease in leverage shown in Figure 2. There is no apparent change in the assets of cartel firms. This also mitigates a possible concern that the reduction in leverage we observe during collusion years may be caused by an increase in retained earnings, which in turn increases assets, and thus may mechanically reduce leverage ratios.

Figure 2 here

4.3 Triple-Differences Analyses

We now use a series of triple-differences analyses to study whether the changes in leverage are more pronounced in situations for which Hypotheses H2-H5 predict that it should be more likely or stronger. If the evidence is consistent with these hypotheses, we can be more confident with our interpretation that leverage is reduced strategically to stabilize cartels.

We first perform sample splits based on cross-sectional (time-invariant) firm characteristics, to test Hypotheses H2-H4. There are two possible concerns. First, the decision to form a cartel may be affected by the same firm-level characteristics that we use to split the sample. That is not a concern here because we test how firms change their leverage *conditional on* having decided to form a cartel (otherwise the firms would not be included in the PIC data). Second, our cartel sample is small. We do not have enough power to test for the significance of differences in the coefficients across subsamples, and our goal is therefore merely to show that the direction or magnitude of the effects across the sample splits follow the intuition derived from the hypotheses.

To test Hypothesis H5 we cannot rely on a sample split, as the prediction is not cross-sectional in nature: We study whether time-varying incentives to sustain collusion (or deviate from it) are related to the extent of the leverage reductions. We therefore use a triple-differences setting where we interact proxies for time-varying incentives to collude with the *Collusion* dummy variable.

4.3.1. Competitive Pressure

We now test Hypothesis H2, that the leverage reduction should be more likely or more significant for firms that face stronger competitive pressure. Measuring the competitive pressure a firm faces is not straightforward. The traditional approach is to use industry concentration (e.g., the Herfindahl Index), and to argue that concentrated industries are less competitive. However, industry structure is affected by entry and exit, and concentrated industries may be particularly competitive (Sutton 1991).[10] An alternative is to focus more directly on how firms can avoid direct competitive pressure through product differentiation, advertising, R&D, etc. We perform three sample splits based on different measures of competitive pressure.

Our first split is based on a novel measure that captures the concept of "product-market fluidity," developed in Hoberg et al. (2014).[11] It uses textual analysis of product descriptions found in SEC 10-K forms to estimate the intensity of a firm's product-market threats. A higher product-market fluidity measure means a firm's competitive environment changes frequently, and therefore it faces

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[10] Using the number of competitors in a market has the same shortcomings.

[11] We thank Jerry Hoberg, Gordon Phillips, and Nagpurnanand Prabhala for making their fluidity data available.

stronger competitive pressure.

We use product market fluidity to measure the competitive pressure faced by firms in our sample. The sample used in Hoberg et al. (2014) begins in the year 1997, so it does not cover all the years in our sample. However, fluidity seems to vary little over time, so we compute *Fluidity* as each firm's average fluidity, and we use those averages for our entire sample period. We split our sample of cartel firms into observations with above-median *Fluidity* (stronger competitive pressure) and below-median *Fluidity* (weaker competitive pressure) and then match each subsample to non-cartel firms. The matching procedure is the same as described in Section 3 for the overall sample (CEM within industries, based on size and cash flow volatility), but it includes firm-level product market fluidity as an additional matching variable, to ensure homogeneity between cartel and non-cartel firms on this dimension. Our matched low-*Fluidity* sample has 41 cartel firms and 220 matched control firms, while the high-*Fluidity* sample contains 41 cartel firms and 342 matched control firms. The matched sample size is smaller than before, since adding a matching criterion reduces the sample of cartel and control firms with common support in the covariate distribution.

We present the results from the *Fluidity* sample splits in Columns (1) and (2) of Table 4. We find a strong negative association between leverage and collusion periods for firms facing stronger competitive pressure (high *Fluidity*) and an insignificant association for firms facing weaker competitive pressure (low *Fluidity*). The results thus support Hypothesis H2.

Table 4 here

Our second split is based on R&D and advertising expenses. Intuitively, investments in R&D and advertising allow firms to avoid competitive pressure through product differentiation (Sutton 1991). We compute *R&D and Advertising* as each firm's average (over the sample period) R&D and advertising expenses over total assets, and we split our sample of cartel firms into two groups: Cartel firms with below-median vs. above-median *R&D and Advertising*. As with the *Fluidity* subsamples, we match each cartel subsample to control firms adding the splitting variable as an additional matching criterion.

We present the results from the *R&D and Advertising* sample splits in Columns (3) and (4) of Table 4. We find a strong negative association between leverage and collusion periods for firms facing stronger competitive pressure (low *R&D and Advertising*) and an insignificant association for firms facing weaker competitive pressure (high *R&D and Advertising*). The results support Hypothesis H2.

Our third split is based on a measure of industry homogeneity introduced by Parrino (1997). He argues that industries are more homogeneous when the stock returns of an industry index and of the industry firms display a higher partial correlation, after controlling for their correlation with the market index. Intuitively, a stronger co-movement of firm and industry returns suggests that the firms and their products are more similar, and hence they face stronger competitive pressure. For each firm, we compute *Industry Homogeneity* as the monthly average of partial correlation in an industry for the pre-collusion years, and we split our sample of cartel firms into two groups: Cartel firms in industries with below-median vs. above-median *Industry Homogeneity*.[12] As with all sample splits, we match each cartel firms to control firms in each subsample by including the splitting variable as a matching criterion.

We present the results from the *Industry Homogeneity* sample splits in Columns (5) and (6) of Table 4. We find a stronger negative association between leverage and collusion periods for firms operating in more homogeneous industries. However, the differences are not as stark as in the previous two sample splits. Overall, the main message of Table 4 is that when competitive pressure is stronger, actively colluding firms reduce their leverage by more. This is consistent with the notion that a stronger reduction in leverage during collusion periods is needed when deviating from a cartel agreement is more tempting, as stated in Hypothesis H2.

4.3.2. Initial Leverage

Hypothesis H3 predicts that the leverage reduction should be more likely and more pronounced if a cartel firm had higher leverage in the pre-collusion years. To test this prediction, we estimate Equation (1) separately for cartel firms that had either high or low leverage before the start of collusion, and we compare the leverage reductions across the samples. Specifically, we compute each firm's leverage ratio during the initial year of data availability in our sample (*Initial Leverage*). Next, we split the sample of cartel firms into two: Firms with high vs. low *Initial Leverage* (above vs. below the

[12] We split the sample using partial correlation averages from pre-collusion years, not from collusion years, since the partial correlations during collusion years are likely affected by cartel-induced synchronicity.

sample median). When matching cartel firms with control firms in these subsamples, we include each firm's *Initial Leverage* as an additional matching criterion.

We present the results in Columns (1) and (2) of Table 5. The decrease in leverage is significant for high-leverage firms, but it is insignificant for low-leverage firms. The decrease in leverage by high-leverage firms is economically large (over five percentage points), and it is larger than that reported in Table 3 for the pooled sample. Note that the pronounced reduction in leverage during collusion periods for the highly levered sample is not mechanical, which would be the case if only cartel firms had high leverage, while control firms did not. If this was the case, it could be argued that cartel firms merely converge to the within-subsample leverage mean. However, our matching procedure ensures that the high-leverage sample is composed of highly-levered cartel firms and equally highly-levered non-cartel firms, so the drop in leverage cannot be ascribed to mean reversion. Moreover, if mean reversion was driving the results, we would expect the coefficient for *Post Collusion* to be similar to the coefficient for *Collusion* (or even more negative).

Table 5 here

For robustness, we repeat the analysis by focusing on each firm's leverage relative to that of its industry. We compute each firm's *Initial relative-to-industry leverage* by subtracting the sample mean leverage for each industry (4-digit SIC code) from each firm's initial leverage ratio. The results are displayed in Columns (3) and (4) of Table 5; they are very similar to the results displayed in Columns (1) and (2).

4.3.3. The Economic Benefit from Collusion

Hypothesis H4 predicts that cartel firms should be willing to accept larger reductions in leverage to safeguard the stability of their cartel if the cartel is more beneficial to them. Measuring how much a cartel benefited its members is difficult, since the counterfactual non-collusion performance cannot be observed and since cartels are sometimes formed to revert negative trends in profitability (see, e.g., Lamoreaux (1985), Schmitt and Weder (1998), Connor (2011), or Herold and Paha (2018)). We therefore use an indirect measure: The fines the cartel firms had to pay after being discovered and investigated. This is a measure of how important a cartel was to a cartel firm because (intuitively) fines tend to be higher if cartels caused greater market distortions and allowed cartel firms to earn larger profits.[13]

The PIC data includes data on fines imposed on cartel firms (see Panel A of Table 1). The data records zero fines for 63 firms and positive fines for 27 firms. We thus split the sample of cartel firms into firms with zero and positive fines, and we estimate Equation (1) for the two subsamples. We only include cartel firms in this analysis, since fines are pertinent to cartel firms only. (Note that the samples have unequal sizes, since around two thirds of the cartel firms paid zero fines according to the PIC data.)

The results are reported in Table 6, Columns (1) and (2). Firms with fines had large reductions in leverage (over 6 percentage points), while the reduction is not statistically significant for firms with no recorded fines. These results are consistent with Hypothesis H4.

Table 6 here

Given the large number of cartel firms with no reported fines, one could argue that those reported values of zero should be treated as missing values. We thus repeat the sample-split analysis after excluding zero-fine cartel firms. We compute the *Fines-to-Revenue* ratio for all cartel firms with positive fines, to create a normalized measure of how important a cartel was to its member firms (we divide a firm's fine by its sales revenue at the start of collusion). We then split the sample into subsamples with above-median and below-median *Fines-to-Revenue*. We present the results for these subsamples in Columns (3) and (4) of Table 6. We find that the results strengthen: Firms with above-median *Fines-to-Revenue* reduce their leverage ratio during collusion by 12.8%, while the reduction is not statistically significant for firms with below-median *Fines-to-Revenue*.

There are two possible concerns with these results. First, fines may be higher for longer-lived cartels, and higher fines may then proxy for the duration of a cartel more than its importance in any given year. This does not seem to be an important concern for our results: The correlation of *Fines* with *Cartel Duration* is weak at 0.11, and our results hold if we normalize the fines data, dividing *Fines-to-Revenue* by the number of years a cartel was active. Second, fines are lower in practice if a

[13] The fines in U.S. cartel cases are supposed to be linked to the economic impact of collusion. See the U.S. Sentencing Guidelines Manual (USSG §2R1.1, comment. (n.3)): "In selecting a fine for an organization within the guideline fine range, the court should consider both the gain to the organization from the offense and the loss caused by the organization."

cartel firm benefited from "leniency laws", i.e., if it informed the antitrust authorities of the illegal activities (see, e.g., Miller 2009; Dong et al. 2019; and Dasgupta and Žaldokas 2019). In these cases, a low fine would underestimate the importance of a cartel. However, only three cartel firms in our sample benefited from leniency laws, and dropping them does not change the results.

An alternative way to gauge how much a firm benefited from a cartel is to examine whether it was regarded as a leader in the cartel or just a member. Arguably, if a firm benefits more, it is more likely to initiate the negotiations to create a cartel and to coordinate its activities. The PIC database classifies 29 of the 90 cartel firms in our sample as cartel leaders. We estimate Equation (1) separately for leader and non-leader cartel firms, and we present the results in Columns (5) and (6) of Table 6. We find that cartel leaders reduce their leverage by a significant 6.3% during collusion periods, while non-leader cartel firms do so by a statistically insignificant 1.8%. These results support Hypothesis H4.[14]

4.3.4. Commitment During Recessions

We now test Hypothesis H5, that the leverage reduction by cartel firms is less pronounced during recessions. Unlike the tests of Hypotheses H2-H4 (in Sections 4.3.1-4.3.3 above), we use a triple-differences test. Performing a sample-split analysis (like in those earlier tests) based on a grouping of firm-years into recession years or non-recession years would be problematic in our setting, given that we identify the effects of collusion by exploiting within-firm (time-series) variation. If we partitioned the sample, each subsample would contain only partial data for each firm spread out through several interleaved periods. For instance, if we estimated the effects of collusion using the recession subsample, we would be dropping non-recession observations for the same firm, causing missing-data problems. For this reason, we set up our analysis as a triple-differences test: we interact the *Collusion* dummy with a dummy variable *Recession Year*, to study whether cartel firms reduce their leverage by less during collusion years that are also recession years.

This analysis, based on time-varying incentives to collude, also allows us to introduce a placebo

[14] That non-leaders do not reduce leverage does not necessarily pose a threat to cartel stability. Cartel leaders tend to be large firms in cartels where firm sizes are heterogeneous. In such settings, leaders can directly enforce non-leaders' compliance through monitoring and punishment (Davis and De 2013), or they can afford to ignore smaller firms (Bos and Harrington 2010).

test, to examine whether our main results might be driven by unobserved cross-sectional heterogeneity. Specifically, we test whether cartel firms are (for unexplained reasons) more sensitive to changes in the economic environment (including their competitive environment) than non-cartel firms, and whether this might explain the decreases in leverage during collusion periods (or the leverage rebounds when collusion ends). We explore this possibility by augmenting our triple-differences specification with an interaction between the *Post-Collusion* dummy and *Recession Year*. This additional interaction acts as a placebo: If we do not find differential effects during post-collusion years across recession and non-recession years, then it is unlikely that our earlier results are simply due to cartel firms being more sensitive to changes in their economic environment.

We construct *Recession Year* using NBER data.[15] The results are presented in Table 7, Column (1). As before, we find that there is a significant decrease in leverage during collusion years. The macroeconomic conditions have the predicted impact: The interaction term of *Recession Year* and *Collusion* is positive, so the leverage reduction is smaller (or absent) during recessions. Both coefficients have significant economic magnitudes, so the evidence supports Hypothesis H5.

Table 7 here

Regarding the placebo test, we find a small and insignificant coefficient for the interaction between *Recession Year* and *Post Collusion*. This suggests that our findings so far are not driven by an unexplained higher sensitivity of cartel firms to economic shocks, or by other unobserved differences to non-cartel firms. We can thus be more confident that the leverage reductions are strategic, consistent with the hypotheses developed in Section 2.

There are two possible concerns with the findings reported in Column (1) of Table 7. First, Halling et al. (2016) show that leverage ratios are countercyclical, and hence a smaller reduction in leverage during recession years could have been expected. However, this is not driving our results. Equation (1) includes industry-by-year fixed effects, so if higher leverage ratios are observed during recessions on average, the mean difference in leverage across economic cycles for all firms would be absorbed by those fixed effects. To examine this further, in Column (2) we present results from a specification that drops industry-by-year fixed effects and includes the dummy *Recession Year*, which

[15] See http://www.nber.org/cycles/cyclesmain.html.

previously could not be included due to perfect collinearity. Consistent with the findings in the literature, we find that leverage significantly increases during recessions: Leverage is 1.8% higher, and this difference is statistically significant. These changes do not alter our earlier result, however: The leverage reduction by cartel firms during collusion years remains significant; this reduction is mitigated during recession years; and the post-collusion leverage ratios are unaffected by economic cycles. While leverage is countercyclical, this effect is not driving our results.

A second possible concern relates to the use of an economy-wide recession indicator variable. It could be argued that the *Recession Year* dummy is too coarse to fully capture differences in the economic environment. We repeat the tests using two alternative measures of the economic environment in which cartel firms operate. We first replicate the specification shown in Column (2), after replacing the *Recession Year* indicator by yearly U.S. GDP growth rates.[16] The results are shown in Column (3) of Table 7. Consistent with leverage ratios being countercyclical, we find a positive coefficient for *GDP Growth*. Consistent with Hypothesis H5, we find a negative coefficient for the interaction between *GDP Growth* and *Collusion*. In terms of economic magnitudes, if the GDP growth rate is 0%, the leverage of cartel firms does not change during collusion years (the *Collusion* coefficient is 0.000); if the GDP growth rate in a year is 5%, control firms reduce their leverage by 1% (5%×0.200) relative to a scenario with no growth, and cartel firms reduce their leverage by 4.9% (5%×0.200 + 5%×0.789). So the leverage reduction by cartel firms is 3.9 percentage points larger during collusion years with 5% GDP growth.

We repeat the tests using an industry-specific measure of the economic environment. We use yearly industry sales growth instead of GDP growth rates, and we present the results in Column (4) of Table 7. The results are consistent with those presented in Columns (1)-(3). Overall, the results from Table 7 are consistent with Hypothesis H5.

5. Conclusions

The goal of this paper is to test whether cartel firms reduce their leverage during collusion periods, in an attempt to make their cartel more stable. The intuition is modeled in Maksimovic (1988). Expanding the set of predictions helps us to better understand the effect, and it allows for sharper tests

[16] See https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?locations=US.

that raise the bar both for this explanation and for possible alternative explanations. We find that cartel firms (on average) reduce their leverage when collusion starts, or soon thereafter. The changes in leverage are more pronounced in settings for which intuition suggests that they should be more pronounced, for instance settings in which cartels are inherently less stable. Overall, the evidence is consistent with the intuition from Maksimovic (1988).

Our results are important for the capital structure literature. Some of this work analyzes the interdependence of financial decisions and product-market decisions, but this analysis has focused on traditional product market strategies like product pricing, output choices, quality differentiation, advertising, etc. Our findings suggest that new insights can be gained by expanding this set of product-market strategies: Studying anti-competitive behavior could help explain capital structure patterns that otherwise have no convincing explanation.

Our findings also make an important contribution to the antitrust literature, by confirming empirically that leverage reductions can be used strategically to make cartels stable. Little is known about cartels and their operations, since they are illegal and therefore surrounded by secrecy. While some cartels are able to use other methods to prevent deviations, the evidence in this paper suggests that leverage reductions also play an important role in cartel stability. Our findings will hopefully motivate further research into what strategies cartel firms use to make their cartels stable.

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FIGURES AND TABLES

Figure 1

This figure plots the coefficients β_h and their 95% confidence intervals for the regression

$$y_{it} = \alpha + \sum \beta_h * d_{ih} + \varphi_i + \mu_{jt} + \varepsilon_{it},$$

using book leverage as the dependent variable. The subscript h indexes the years that immediately precede collusion years ($h \in \{-2, -1\}$), years that immediately follow collusion years ($h \in \{1, 2\}$), or years that are collusion years ($h \in$ {col1,col2,col3,oth.col} for full collusion years, $h = 0^-$ for a partial collusion year at the start of collusion, and $h = 0^+$ for a partial collusion year at the end of collusion). The indicator variable d_{ih} takes a value of 1 if a firm operates in one of those years, and 0 otherwise. The results suggest that cartel firms reduce their leverage during collusion years, that the changes are strongest in the first two full years of collusion, that there is no apparent reduction in leverage immediately before the onset of collusion, and that leverage rebounds to average levels when collusion ends.

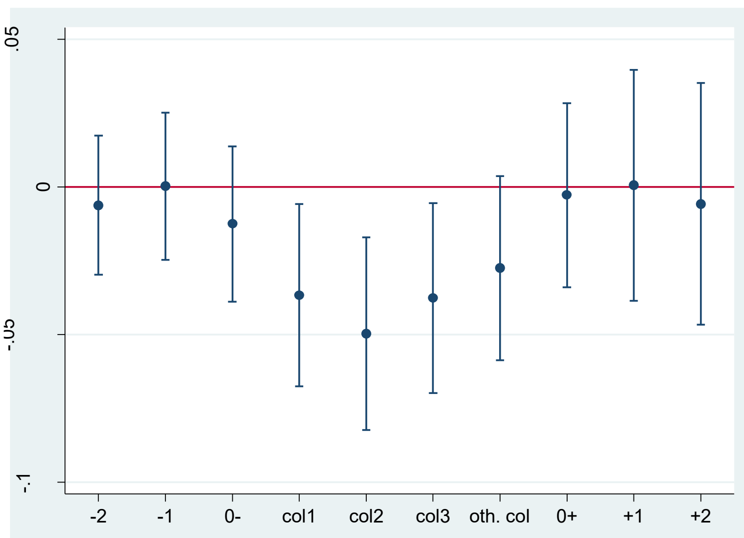

Figure 2

This figure plots the coefficients β_h for the regression

$$y_{it} = \alpha + \sum \beta_h * d_{ih} + \varphi_i + \mu_{jt} + \varepsilon_{it},$$

using the logarithm of debt and the logarithm of assets as the dependent variables. The subscript h indexes the years that immediately precede collusion years ($h \in \{-2, -1\}$), years that immediately follow collusion years ($h \in \{1, 2\}$), or years that are collusion years ($h \in$ {col1, col2, col3, oth.col} for full collusion years, $h = 0^-$ for a partial collusion year at the start of collusion, and $h = 0^+$ for a partial collusion year at the end of collusion). The indicator variable d_{ih} takes a value of 1 if a firm operates in one of those years, and 0 otherwise. The results suggest that changes in leverage are driven by changes in the debt levels of cartel firms, since the changes to their assets are minor in comparison to changes in debt.

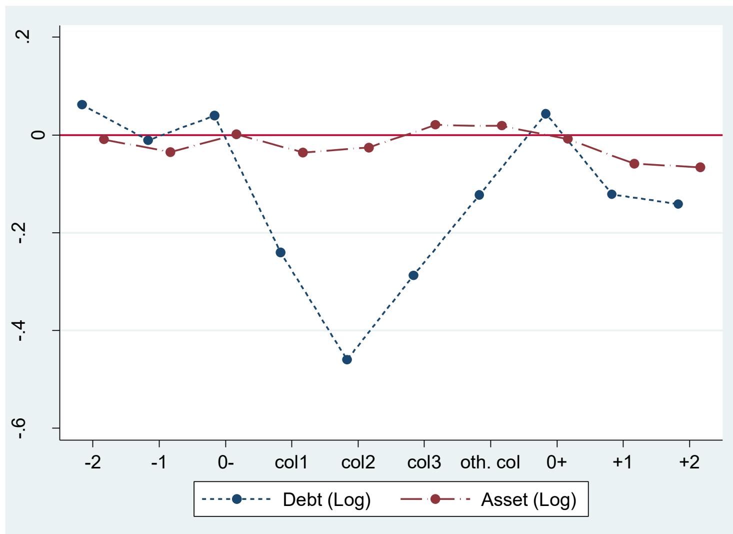

Table 1. Summary statistics

Panels A-D present summary statistics for the sample of U.S. publicly traded cartel firms that operated in a single cartel during the sample period. Cartel variables are obtained from the PIC database. See the Appendix for variable definitions.

Panel A: Cartel firm summary statistics

	Mean	P10	P25	P50	P75	P90	Total
Collusion Duration (months)	68.16	12.00	36.00	69.16	96.00	110.97	90
# Firms in Cartel	9.19	3.00	4.00	7.00	12.00	17.00	90
Cartel Leader	0.32	0.00	0.00	0.00	1.00	1.00	90
Fines (US$ million)	13.19	0.00	0.00	0.00	0.43	14.49	90
Fines > 0 (US$ million)	43.98	0.25	0.73	3.20	31.60	100.00	27
Prison for Executives (months)	5.17	0.00	0.00	0.00	0.00	0.00	90
Prison for Executives > 0 (months)	93.00	7.00	36.00	66.00	168.00	188.00	5
% North American Firms	0.63	0.00	0.00	1.00	1.00	1.00	90
% European Firms	0.22	0.00	0.00	0.00	0.33	1.00	90
% African Firms	0.01	0.00	0.00	0.00	0.00	0.00	90
% Asian/Oceanian Firms	0.08	0.00	0.00	0.00	0.00	0.27	90
% Latin American Firms	0.07	0.00	0.00	0.00	0.00	0.00	90

Panel B: *Lead Jurisdiction*

	Observations	Percentage
U.S.	41	45.56
U.S. + Other	9	10.00
U.S. Private	11	12.22
Other	29	32.22
Total	90	

Panel C: One-digit SIC codes

SIC	Observations	Percentage
0	2	2.22
1	3	3.33
2	27	30.00
3	29	32.22
4	15	16.67
5	9	10.00
7	5	5.56
Total	90	

Panel D: Start and end dates of collusion

Years	Collusion Started		Collusion Ended	
	Observations	Percentage	Observations	Percentage
1990-1995	26	28.9	1	1.1
1996-2000	24	26.7	13	14.4
2001-2005	32	35.6	36	40.0
2006-2010	8	8.9	34	37.8
2011-2015	0	0.0	6	6.7
Total	90		90	

Table 2. Single-cartel firms vs. matched non-cartel firms

This table reports descriptive statistics for our sample of firms prosecuted for cartel participation (including observations from up to five years prior to cartel formation and up to five years after the cartel is dissolved), and for the matched sample of cartel and control firms (control firms are matched U.S. firms from the same 56 industries as cartel firms, without foreign operations, included in Compustat, but not included in the PIC data set). Given that the matched sample was obtained using CEM, each observation in the matched sample is weighted using the weights obtained in the matching. Column 4 reports differences in selected characteristics of the matched cartel and control firms. See the Appendix for variable definitions. Differences significant at: *10%, **5%, and ***1%.

	(1)	(2)	(3)	(4): (2)-(3)
Variable	Cartel	Cartel (matched)	Non-cartel (matched)	Difference
Profitability (ROA)	0.139	0.139	0.140	-0.001
Tangibility	0.345	0.347	0.355	-0.008
Assets (log)	7.859	7.882	7.897	-0.016
Sales (log)	7.797	7.820	7.787	0.033
Cash Flow Volatility	0.026	0.024	0.025	-0.001
# Firms	90	89	1,596	
# Observations	1,368	1,308	11,862	

Table 3. Collusion and capital structure

This table presents the results of analyzing the association between collusion and leverage. The dependent variable is *Leverage*. Columns (1) and (2) present the estimation results using both cartel and control firms. Columns (3) and (4) present the estimation results using only cartel firms. *Collusion* takes a value of 1 for cartel firms during collusion years, and 0 otherwise; and *Post Collusion* takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise. The *Control Variables* include lagged *Profitability*, *Tangibility*, *Cash Flow Volatility*, and *Sales* (see the Appendix for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5%, and ***1%.

Independent variables	(1) *Leverage*	(2) *Leverage*	(3) *Leverage*	(4) *Leverage*
Collusion	-0.029**	-0.032**	-0.027**	-0.028**
	(0.013)	(0.013)	(0.014)	(0.013)
Post Collusion	0.010	0.003	-0.023	-0.030
	(0.020)	(0.019)	(0.027)	(0.026)
Observations	12,655	12,655	1,368	1,368
R-squared	0.802	0.805	0.674	0.688
# Cartel Firms	89	89	90	90
# Matched Control Firms	1,596	1,596	0	0
Control Variables	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	No	No
Year FE	No	No	Yes	Yes

Table 4. Competition sample split

This table presents the results of analyzing the association between collusion and leverage using sample splits. The dependent variable is *Leverage*. Columns (1) and (2) present the results of splitting the sample into firms with below-median and above-median *Fluidity*, a text-based measure of competitive pressure formulated by Hoberg et al. (2014) that uses product descriptions in SEC Form 10-K filings. Columns (3) and (4) present the results of splitting the sample into firms with below-median and above-median *R&D and Advertising*. Columns (5) and (6) present the results of splitting the sample into firms with low and high *Industry Homogeneity* (see Parrino 1997). *Collusion* takes a value of 1 for cartel firms during collusion years, and 0 otherwise; *Post Collusion* takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise. The *Control Variables* include lagged *Profitability*, *Tangibility*, *Cash Flow Volatility*, and *Sales* (see the Appendix for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5%, and ***1%.

	(1)	(2)	(3)	(4)	(5)	(6)
Independent variables	*Leverage*	*Leverage*	*Leverage*	*Leverage*	*Leverage*	*Leverage*
Collusion	-0.026	-0.054**	-0.041**	-0.017	-0.021	-0.031**
	(0.024)	(0.020)	(0.020)	(0.021)	(0.017)	(0.015)
Post Collusion	0.001	-0.056	-0.004	-0.001	0.025	-0.002
	(0.033)	(0.033)	(0.033)	(0.030)	(0.026)	(0.022)
Observations	2,546	2,258	5,752	5,830	9,664	5,072
R-squared	0.794	0.819	0.869	0.784	0.803	0.865
Sample Split	Low *Fluidity*	High *Fluidity*	Low *R&D & Adv.*	High *R&D & Adv.*	Low *Ind. Homogen.*	High *Ind. Homogen.*
# Cartel Firms	41	41	45	44	45	43
# Matched Control Firms	220	342	779	659	1348	559
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Initial leverage sample split

This table presents the results of analyzing the association between collusion and leverage using sample splits. The dependent variable is *Leverage*. Columns (1) and (2) present the results of splitting the sample into firms with below-median and above-median *Leverage* as measured in the first year of each firm's observations in the data. Columns (3) and (4) present the results of splitting the sample into firms with below-median and above-median leverage relative to industry leverage. *Collusion* takes a value of 1 for cartel firms during collusion years, and 0 otherwise; *Post Collusion* takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise. The *Control Variables* include lagged *Profitability*, *Tangibility*, *Cash Flow Volatility*, and *Sales* (see the Appendix for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5%, and ***1%.

Independent variables	(1) *Leverage*	(2) *Leverage*	(3) *Leverage*	(4) *Leverage*
Collusion	0.024	-0.052***	0.010	-0.055***
	(0.022)	(0.017)	(0.020)	(0.015)
Post Collusion	0.047	-0.003	0.048	-0.014
	(0.034)	(0.027)	(0.031)	(0.027)
Observations	5,525	4,473	4,690	4,857
R-squared	0.768	0.856	0.808	0.839
Sample Split	Low *Initial Leverage*	High *Initial Leverage*	Low *Initial Relative-to-Industry Leverage*	High Initial *Relative-to-Industry Leverage*
# Cartel Firms	44	44	44	45
# Matched Control Firms	661	605	549	702
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes

Table 6. Cartel relevance sample split

This table presents the results of analyzing the association between collusion and leverage using sample splits. The dependent variable is *Leverage*. Columns (1) and (2) present the results of splitting the sample into firms with zero and positive fines. Columns (3) and (4) present the results excluding firms with zero fines and splitting the sample into firms that paid below-median or above-median *Fines-to-Revenue*, where *Fines-to-Revenue* is the ratio of the fines imposed by antitrust authorities to the firm's revenue during the first year of collusion. Columns (5) and (6) present the results of splitting the sample into firms that are or are not classified as leaders of their cartel. *Collusion* takes a value of 1 for cartel firms during collusion years, and 0 otherwise; *Post Collusion* takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise. The *Control Variables* include lagged *Profitability*, *Tangibility*, *Cash Flow Volatility*, and *Sales* (see the Appendix for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5%, and ***1%.

Independent variables	(1) *Leverage*	(2) *Leverage*	(3) *Leverage*	(4) *Leverage*	(5) *Leverage*	(6) *Leverage*
Collusion	-0.014	-0.062**	-0.026	-0.128***	-0.018	-0.063***
	(0.014)	(0.022)	(0.024)	(0.041)	(0.016)	(0.016)
Post Collusion	-0.009	-0.069	-0.076	-0.081	-0.045	-0.027
	(0.029)	(0.042)	(0.047)	(0.056)	(0.027)	(0.041)
Observations	977	391	189	199	968	400
R-squared	0.666	0.775	0.894	0.706	0.706	0.721
Sample Split	No fine	Fine>0	Low fine>0	High fine>0	Not leader	Cartel leader
# Cartel Firms	63	27	14	13	61	29
# Matched Control Firms	0	0	0	0	0	0
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7. Economic prospects

This table presents the results of analyzing the association between collusion and leverage using a triple-differences approach. The dependent variable is *Leverage*. *Recession Year* is a dummy variable that takes a value of 1 in a recession year, as defined using the NBER recession year list, and 0 otherwise. *GDP Growth* is the yearly U.S. GDP growth as reported by the World Bank. *Industry Growth is the* median yearly industry sales growth net of inflation, for Compustat firms. *Collusion* takes a value of 1 for cartel firms during collusion years, and 0 otherwise; *Post Collusion* takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise. The *Control Variables* include lagged *Profitability*, *Tangibility*, *Cash Flow Volatility*, and *Sales* (see the Appendix for definitions). In Columns (1) and (2) the key interactions relate to the dummy *Recession Year*. In Column (1) we do not report the coefficient for *Recession Year*, since that dummy variable is collinear with the year fixed effects. We include it in Column (2), after dropping Industry-by-Year fixed effects. In Column (3) we present the results after replacing the indicator variable *Recession Year* by *GDP Growth*. In Column (4) we present the results after replacing the indicator variable *Recession Year* with *Industry Growth*. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5%, and ***1%.

Independent variables	(1) *Leverage*	(2) *Leverage*	(3) *Leverage*	(4) *Leverage*
Collusion	-0.037***	-0.027**	0.000	-0.016
	(0.012)	(0.011)	(0.013)	(0.012)
Post Collusion	0.003	-0.011	-0.014	-0.010
	(0.020)	(0.018)	(0.018)	(0.018)
Collusion × Recession Year	0.030***	0.037***		
	(0.011)	(0.011)		
Post Collusion × Recession Year	-0.001	-0.006		
	(0.010)	(0.011)		
Recession Year		0.018***		
		(0.004)		
Collusion × GDP Growth			-0.789**	
			(0.315)	
Post collusion × GDP Growth			0.029	
			(0.398)	
GDP Growth			-0.200**	
			(0.091)	
Collusion × Industry Growth				-0.188***
				(0.065)
Post Collusion × Industry Growth				-0.084
				(0.105)
Industry Growth				-0.076***
				(0.027)
Observations	12,655	12,901	12,901	12,901
R-squared	0.806	0.752	0.751	0.751
# Cartel Firms	89	89	89	89
# Matched Control Firms	1,596	1,596	1,596	1,596
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	No	No	No

Appendix: Variable Definitions

Cartel Characteristics:

Collusion Duration	Number of months from the beginning to the end of collusion according to the PIC database.
Cartel Leader	Indicator variable that takes a value of one if the firm is identified as the cartel leader in the PIC database, and zero otherwise.
# Firms in Cartel	Number of global firms involved in a cartel.
Fines	Worldwide fines and recoveries from a cartel firm, in millions of U.S. dollars.
Fines>0	Worldwide fines and recoveries from a cartel firm, in millions of U.S. dollars, conditional on non-zero fines and recoveries being recorded.
Prison for Executives	Aggregate prison time sentences (in months) for all executives at a cartel firm.
Prison for Executives >0	Aggregate prison time sentences (in months) for all executives at a cartel firm, conditional on a firm having at least one executive receiving prison time.
% North American Firms	Fraction of U.S. and Canadian firms involved in a cartel.
% European Firms	Fraction of European firms involved in a cartel.
% African Firms	Fraction of African firms involved in a cartel.
% Asian/Oceanian Firms	Fraction of Asian and Oceanian firms involved in a cartel.
% Latin American Firms	Fraction of Latin American firms involved in a cartel.
Lead Jurisdiction	Lead prosecution region or entity in a cartel case.

Firm Characteristics:

Leverage	Short-term debt plus long-term debt divided by book value of assets.
Profitability	Operating income before depreciation divided by book value of assets (ROA).
Tangibility	Net property, plant, and equipment divided by book value of assets.
Sales	Logarithm of the value of total sales measured in millions of U.S. dollars.
Cash Flow Volatility	Standard deviation of *Profitability* over the prior 3-year period.
Assets	Logarithm of the book value of assets measured in millions of U.S. dollars.

Debt	Logarithm of the sum of short-term debt and long-term debt.
Collusion	Indicator variable that takes the value of one for cartel firms during active collusion years, and zero otherwise.
Post Collusion	Indicator variable that takes a value of one for cartel firms during the five years after a cartel is dissolved, and zero otherwise.

Sample Splits:

Fluidity	Text-based measure of product market competitive pressure formulated by Hoberg et al. (2014) using product descriptions in 10-K filings. We use the firm-level average over time.
R&D and Advertising	Ratio of R&D plus advertising expenses divided by book value of assets. Missing values in both variables are replaced by zero.
Industry Homogeneity	Measure of industry homogeneity based on firms' returns formulated by Parrino (1997). For each firm in an industry (4-digit SIC), we compute the partial correlations of that firm's monthly returns with the industry returns, controlling for the correlation of the firm's returns with the market index. For each year, *Industry Homogeneity* is the average of the partial correlations across firms.
Initial Leverage	Firm leverage based on the first observation available in the data.
Initial Relative-to-Industry Leverage	Difference between firm leverage based on the first observation available in the data, and mean industry (4-digit SIC) leverage.
Fines-to-Revenue	Ratio of the fines imposed by antitrust authorities to the firm's revenue during the first year of collusion.
Recession Year	Indicator variable that takes a value of one in a f year, and zero otherwise, classified using the NBER recession year list.
GDP Growth	Yearly U.S. GDP growth as reported by the World Bank.
Industry Growth	Median yearly industry (4-digit SIC) sales growth net of inflation, using Compustat data.