Capital Structure Under Collusion

Daniel Ferrés
Universidad de Montevideo

Gaizka Ormazabal
IESE Business School

Paul Povel*
University of Houston

Giorgo Sertsios
Universidad de los Andes, Chile

June 2019

* Corresponding author. Bauer College of Business, University of Houston, Houston, TX 77204-6021, U.S.A. Phone: [+1] 713 743 4759. Email: povel@uh.edu.

Gaizka Ormazabal acknowledges funding from the Ramon y Cajal and Marie Curie Fellowships and the Spanish Ministry of Science and Innovation grant ECO2015-63711-P. Sertsios acknowledges financial support from CGC UC and from Proyecto Fondecyt Regular #1190091. We thank Augusto Orellana for excellent research assistance. We thank Murillo Campello, Francesco D'Acunto, Mike Faulkender, Sandy Klasa, Steve McKeon, Vojislav Maksimovic, Will Mullins, Hernan Ortiz-Molina (discussant), Ana Elisa Pereira, Gordon Phillips, Francisco Urzúa and Alminas Žaldokas for very helpful comments. We are also indebted to participants at the 2017 Midwest Finance Association meeting; and to seminar participants at Baylor University, University of Arizona, University of Houston, University of Maryland, Universidad Católica, and Universidad de Chile.

Declarations of interest: none. The institutions that provided financial support had no influence on the conduct of this research or on the publication process.

Capital Structure Under Collusion

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, like collusion with competitors by forming a cartel. In this paper, we provide evidence that firms strategically change their financial leverage when they form a cartel.

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, because each cartel firm has an incentive to deviate from the agreement and steal profits from the other cartel firms. Maksimovic (1988) analyzes 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 financial leverage worsens the incentive to deviate: Firms focus on their payoffs net of debt payments, and if the debt payments are sufficiently large, the threat of destroying future profits loses weight, compared with the one-time profit from deviating. The main prediction is that a cartel is stable only if all cartel firms have sufficiently low leverage.

---

[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 4) and *Compustat* databases.

The main prediction in Maksimovic (1988) 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 problem 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 both from non-collusion periods (for the cartel firms) and from matched non-cartel firms. To identify cartel firms and collusion periods, we use the *Private International Cartels* (PIC) database (see Section 4).

The evidence supports the main prediction, that cartel firms have lower leverage during collusion periods, 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). So the results are driven by leverage changes at the cartel firms, not by changes at non-cartel firms.

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, because 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 product 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 during economic downturns, the reduction in leverage by cartel firms is smaller, 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.

The empirical results from the triple-differences setups lend support to the main prediction, that cartel firms strategically reduce their leverage to make their cartels more stable. They also raise the bar for possible alternative explanations. Given the limited scope for alternative explanations to jointly explain the main result and those from the triple-differences setups, we limit our discussion to two possible alternative explanations. Specifically, we examine whether the drop in leverage during collusion periods may be due to changes in profitability, or whether it may be due to cartel firms facing a tighter credit environment. None of these alternative explanations is supported by our tests (see Section 6).

The main contribution of our paper is to show that when firms decide to collude by forming a cartel, this decision affects their optimal capital structure, leading many cartel firms to 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 that 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 some 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) and Campello et al. (2017) investigate cartel convictions from the perspective of corporate governance.

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. This generates higher profits, but 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

[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.)

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. The incremental profits would be lost for all firms, but if this threat prevents deviations, then in equilibrium it is never executed, and the profit losses do not arise.

Not all cartels need to rely on trigger strategies to sustain collusion. For example, a cartel may be able to monitor every cartel member's product market decisions and to directly punish (and thus prevent) deviations. However, not all cartels are this stable, and for those less stable cartels, trigger strategies may work.

Maksimovic (1988) shows that for such cartels that have to rely on trigger strategies, financial leverage makes the threat of deviations worse and thus makes cartels inherently less stable. The intuition is that with sufficiently high debt, shareholders care less about losing future collusion profits or possibly going bankrupt once unfettered competition resumes, while the one-time profit from deviating is, in relative terms (net of debt payments), more significant. The debt payment obligations reduce all of these profits by similar amounts (possible bankruptcy makes the effect more extreme), compared to a debt-free setting, and it thus magnifies the relative importance of the deviation profit compared with the collusion profit or unfettered-competition profit (which may be zero with sufficiently high debt).

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 main result in Maksimovic (1988) is that for cartels that have to rely on trigger strategies, cartel stability requires that all cartel firms have sufficiently low leverage. This leads to the cross-sectional

[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.

prediction that cartel firms have lower leverage than non-cartel firms.

H1: *Actively colluding cartel firms have lower leverage than other firms.*

Testing this hypothesis is not straightforward. Cartel and non-cartel firms might differ in many observed and unobserved characteristics, making it hard to draw inferences from a cross-sectional comparison. 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 4).

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), because 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 regarding the strategic use of leverage during collusion.

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, cartel firms should find it more tempting to deviate from a collusion agreement, because it is easier to take market share away from the other cartel firms (see, e.g., Chang 1991). 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.

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 for firms with relatively high leverage in years during which they are not colluding, we should observe that leverage reductions are more likely, or larger. 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 all 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. Examples of Changes to Financial Leverage Around the Start of Collusion

Since cartels are illegal, it is challenging to find information about their operations. Even when cartel firms can actually be identified (because of lawsuits or antitrust investigations, see the description of our data in Section 4), it is difficult to find any information about how the they operate, including the strategic use of financial leverage. We now briefly discuss three examples of cartel firms and their capital structure changes during their (alleged) collusion periods. The information is obtained from SEC filings and court documents. While there are no explicit statements that the reductions in leverage had the purpose of making a collusion agreement stable (something the cartel firms would have kept secret), the leverage reductions in these cases are striking, and they are supportive of the intuition described in Section 2.

In late 2006, *Nvidia Corp.* and one rival firm faced investigations by the USDOJ Antitrust division as well as civil lawsuits, alleging collusion in the market for Graphic Processing Units (GPU), *Nvidia*'s main product. The USDOJ investigations were eventually dropped, and the civil cases were settled. The GPU market was extremely competitive, with low margins, short product cycles, and significant investment requirements for design and R&D.

There was a drastic reduction in *Nvidia*'s long-term debt at the onset of the alleged collusion. A year after its IPO in 1999, in October 2000, *Nvidia* had issued convertible debt ($300m) and equity ($87.5m), with no specific purpose given in the prospectus. Some of that cash was used in December of that year, when *Nvidia* acquired the assets of a bankrupt rival, mostly to settle patent litigation, paying $70m in cash and 1m shares of common stock. The convertible debt was to mature in October 2007, and *Nvidia* had the right to redeem it on or after October 20, 2003. It redeemed the convertible debt on October 24, 2003, i.e., as soon as possible, near the end of the alleged first year of collusion. No reason was given for this early redemption, and there were no apparent benefits for the firm (aside from strategic benefits related to collusion): the debt was redeemed at 102.7% of the outstanding principal, while the debt was trading at a discount of 13% in January 2003; the convertibility option was far out of the money; the coupon of 4¾% was not particularly high (the 5-year T-Note rate on the redemption day was 3.13%), and the cumulative interest expense saved was offset by necessary write-offs caused by the redemption as well as income taxes due when marketable securities were liquidated. After that redemption, *Nvidia* had no long-term debt.

Integrated Silicon Solution Inc. also reduced its long-term debt to zero at the onset of collusion. It allegedly participated in a price-fixing cartel for static random access memory ("SRAM"), one of its two main products, during eight years. There was an investigation by a state attorney general (later dropped), and there were civil lawsuits that were settled. The firm gradually reduced its long-term debt to zero during the first two years of alleged collusion, and kept it at zero or very small levels thereafter.

Louisiana Pacific Corp. allegedly colluded with its rivals in the "oriented strand board (OSB)" market (low-priced lumber, similar to plywood). Class action lawsuits (begun in early 2006) alleging price fixing since mid-2002 were eventually settled. OSB is a commodity product, and the firms in this industry had been struggling to make profits during the precollusion period. *Louisiana Pacific* gradually reduced its debt during the first two years of alleged collusion, while also selling assets, with a significant debt reduction in 2004, resulting in a long-term debt level 46% lower than in the pre-collusion year.

Figure 1 here

Figure 1 shows the book leverage ratios for the three firms (see the Appendix for details on the construction of variables), for the years around the start of their alleged collusion. In all three cases, the pre-collusion leverage was considerable, and the reductions were significant. However, these are just three examples. In the following sections we examine whether the patterns found here also hold when analyzing a more comprehensive dataset on cartel firms.

4. 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 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, since cartels involving (say) 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. Since many cartels remain undetected (Connor (2014) estimates that only about 10-30%

[6] See the papers by Connor and various co-authors, as well as Artiga et al (2019), Campello et al. (2017), Dasgupta and Žaldokas (2019), Dong et al. (2019), and Han and Žaldokas (2016).

of all cartels are detected; see also Bryant and Eckard (1991)), the data does not include *all* cartels but only those that were detected and for which a conviction was possible.

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. That 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 are 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 start of a cartel (and up to five years after a cartel is dissolved).

Many of those cartel firms participated in more than one cartel, so 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.[7] The final sample includes 1,368 firm-years for 90 cartel firms that participated in 56 cartels. Out 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

[7] Multiple-cartel firms have similar characteristics to single-cartel firms. Their average leverage ratio is slightly lower (25.0% vs. 26.6%), consistent with the idea that collusion periods are associated with lower leverage.

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).[8] 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. 2016) 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 multivariate sorting, and then matched within each cell. We sort observations by logarithm of assets (10 bins), logarithm of sales (10 bins) and cash flow volatility (100 cells), within each of the 56 industry classifications of cartel firms. 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 as with 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

[8] 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.

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). The absence of discernible differences between the average leverage ratios suggests that the different leverage we might observe for cartel firms can be attributed to within-firm changes — that is, to changes made by cartel firms that transition from pre-collusion periods to collusion periods and then post-collusion periods.

Table 2 here

5. Empirical Analysis

5.1 Leverage During and After Collusion Years

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

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

The subscript *i* indexes firms, and *t* indexes years. Our main dependent variable, y_{it}, is book leverage. Our specification is essentially a difference-in-differences strategy with two treatments: *Collusion* and *Post Collusion*. *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. This research design compares differences between collusion years of cartel firms with both non-cartel firms and non-collusion years of cartel firms. It also differentiates years after collusion from other non-collusion years to capture the potential effects of a cartel's dissolution on its members' financial policies.[9] Given that the control sample was obtained using CEM, each observation is weighted using the weights obtained in the matching.

Our key parameter of interest is β, which under Hypothesis H1 should be negative. We include a set of controls $\boldsymbol{X}$ that comprises variables commonly used in the capital structure literature (e.g., Lemmon et al. 2008): Lagged *Tangibility,* lagged *Profitability,* lagged *Sales*, and lagged C*ash Flow*

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

Volatility. Firm and industry-by-year fixed effects are represented by φ_i and μ_{jt}, respectively.[10] Controlling for industry-by-year fixed effects allow us to control for industry-level trends that affect both cartel and non-cartel firms simultaneously.

Alternatively, we can estimate the effects of collusion on firms' capital structure using cartel firms only, i.e., the "eventually treated" sample (see Bertrand and Mullainathan (2003) for a similar approach). Given that cartel firms collude at different points in time, the non-colluding years of some cartels act as control observations for the colluding years of other cartels. Estimating the effects using the eventually treated sample helps to 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 (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 capital structure determinants previously used in the literature. In all four regressions, *Collusion* has a significant negative effect on leverage, reducing it by around 3 percentage points. These effects are statistically significant at 5% levels, and they are economically significant too: As shown in Table 2, 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

[10] 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.

data only). Overall, our findings are consistent with Hypothesis H1.

Table 3 here

5.2 Timing of the Leverage Reduction

We now study how leverage changes from year to year around and during the start and end of the collusion periods. We do this for two reasons. First, it is interesting to study whether the leverage reductions coincide with the start of collusion or whether they are delayed. Second, 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. In simpler setups with one-time events like changes in regulation, a figure displaying trends is often sufficient. However, in our setting, cartel firms are "treated" at different points of time, so a more formal test is needed.

To examine the evolution of firm leverage we examine whether there are changes in the dependent variable before (and after) the treatment period (in our case, the collusion period). Specifically, we estimate the following variation of Equation (1) using the full sample of cartel and matched non-cartel firms:

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

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 a cartel, and $h = 0^{+}$ for a partial collusion year at the end of a cartel). For example, if a cartel started in (say) September 2001 and ended 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 2, using *Leverage* as the dependent variable. The results suggest that the decrease in *Leverage* is concentrated in the collusion years. In the preceding years, the coefficients are not significantly different from zero, so the reduction in leverage is unlikely driven by pre-existing trends. Furthermore, leverage returns to average levels when collusion ends. Overall, these results are consistent with the intuition that firms strategically change their leverage to stabilize their cartel.

Figure 2 here

The timing of the leverage reduction during collusion is also of interest, even though there are no theoretical predictions of a dynamic nature about leverage around and during collusion periods. We observe that the reduction in leverage begins at the onset of collusion, and it is more pronounced two years into the collusion period. This could be consistent with debt reductions being delayed (relative to the start of collusion) for practical reasons, say, if a large reduction is required to gain stability. Furthermore, during the later years of collusion (grouped in "oth. col" in Figure 2), leverage seems to rebound. A possible interpretation is that some cartels become undisciplined and allow increases in leverage, thereby weakening the stability of the cartel.

To shed further light on the leverage ratio patterns in Figure 2, we examine whether the changes we observe are due to a drop in debt (the numerator) or an increase in assets (the denominator). Figure 3 displays the coefficients for the specification (Equation (2)) used to create Figure 2, but using separately the logarithm of debt and the logarithm of assets as the dependent variable. 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 increase assets, and thus may mechanically reduce leverage ratios.

Figure 3 here

5.3 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 those hypotheses, we can be more confident with our interpretation, that leverage is used 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 sample the 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.

5.3.1 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).[11] 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).[12] 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, so it faces stronger competitive pressure.

---

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

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

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 years in our sample. However, fluidity seems to vary little over time, so we compute each firm's average fluidity and 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 4 for the overall sample (CEM within industries, based on size and cash flow volatility), but including 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 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 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 expenses. 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 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.[13] As with all sample splits, we match each cartel subsample to control firms 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 for the previous two sample splits. Overall, the main message of Table 4 is that when competitive pressure is stronger, 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 Hypotheses H2.

5.3.2 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 sample median). We then match each subsample to control firms including 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

[13] We split the sample using partial correlation averages from pre-collusion years, excluding collusion years, since the partial correlations during collusion years are likely affected by cartel-induced synchronicity.

for high-leverage firms, but it is insignificant for low-leverage firms. The decrease in leverage by high-leverage firms is economically large, close to 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 that 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 non-cartel firms that are equally highly levered, so the drop in leverage cannot be ascribed to mean reversion. More to the point, if mean reversion was driving the results, we should 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).

5.3.3 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 Section 6 below). 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.[14]

[14] 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."

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 firms with no recorded fines did not reduce their leverage significantly. 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. Within the sample of firms with positive fines, we compute each firm's fine-to-revenue ratio, 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 fine-to-revenue ratios. 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-sales ratios decrease their leverage ratio during collusion by 12.8%, while firms with below-median fines-to-sales ratios decrease their leverage ratio during collusion by a more modest 2.6%.

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 furthermore, our results hold if we normalize the fines data, dividing the fines-to-revenues ratio by the number of years a cartel was active. Second, fines are lower in practice if a 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 those 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 measure how important a cartel was to a cartel firm is to focus on which firms were the leaders of a cartel. The PIC database classifies 29 of the 90 cartel firms in our sample as cartel leaders. Arguably, a firm is more likely to take a leadership role if it stands to gain more from the cartel. 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, while non-leader cartel firms do so by a statistically insignificant 1.8%. In sum, both measures of how important a cartel was to a firm support Hypothesis H4, that cartel firms are willing to make stronger reductions in leverage when they benefit more from collusion.

5.3.4. 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 5.3.1-5.3.3 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, interacting the *Collusion* dummy with a dummy variable for recession years, 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 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 firms that never joined a cartel, and whether this might explain the decreases in leverage during collusion periods (or the leverage rebounds when collusion ends). We explore this possibility by adding a second interaction to our triple-differences specification, between the *Post-Collusion* dummy and the dummy for recession years. The interaction between *Post-Collusion* and *Recession Year* 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 classify *Recession* years following 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* 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*, which 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

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

leverage is countercyclical, this 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* dummy is too coarse to fully capture differences in the economic environment. We tackle this concern by repeating 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* 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%, the control firms reduce their leverage by 1% (5%×0.200) relative to a scenario with no growth, and the 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 also 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.

6. Alternative Explanations

In this section we address possible alternative explanations for our results, i.e., explanations not linked to the strategic use of leverage to stabilize cartels. Specifically, we consider the possible effects of changes in profitability, and whether cartel firms face a tighter credit environment during collusion periods. While additional alternative explanations are conceivable, we do not discuss them for brevity, noting that any other alternative explanation faces the hurdle of having to explain both our main findings (Section 5.1) and the results from the triple-difference tests (Section 5.3).

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

6.1. Profitability Changes and Leverage Changes

A common finding in the empirical corporate finance literature is the negative association between realized profitability and leverage (Heath and Sertsios 2019; Frank and Goyal 2015). Since the goal of colluding is to increase profits, one might thus predict a reduction in leverage during collusion periods, caused by an envisioned increase in profitability. We now show that profitability changes are unlikely to explain our findings, for several reasons.

First, our results do not change when we control for lagged *Profitability*. This is immediately apparent when comparing Columns (1) and (3) with Columns (2) and (4) in Table 3. So while leverage *may* be affected by profitability during collusion years, this cannot explain our findings.

Second, the most common interpretation for the negative association between profitability and leverage is the pecking order theory, which suggests that firms facing information asymmetry problems substitute internal funds for debt (Titman and Wessels 1998; Fama and French 2002). This makes changes in profitability a poor alternative explanation for the reduction in leverage during collusion periods, for two reasons. Cartel firms are large, so information asymmetry concerns are unlikely to be of first-order relevance. Furthermore, in unreported results we find that the reduction in leverage for cartel firms is stronger for low-R&D cartel firms than for high-R&D firms. One would expect high-R&D firms to more likely act according to the pecking order theory, since they are more likely to face problems of asymmetric information. Hence, the findings go against this alternative explanation.

Third, the profitability patterns around the years when collusion starts are not simple increases in profitability. The literature on cartels and collusion has shown that adverse economic shocks are frequently the motivation for the start of collusion; see, e.g., Lamoreaux (1985), Schmitt and Weder (1998), Connor (2011), or Herold and Paha (2018). For example, firms may form a cartel in response to the entry of new competitors or the removal of tariffs. The effect of collusion could then merely be that it offsets a decline in profitability caused by those external changes. If so, it is unlikely that increases in profitability can explain our findings, because what firms stand to gain from collusion is not well mapped into differences in observed or realized profitability.

To explore this in more detail, we estimate Equation (2) using *Profitability* as the dependent variable. We present the results in Figure 4. The evolution of pre-collusion profitability is in line with the case studies we just cited: There is a significant reduction in profitability shortly before collusion starts. Profitability reverts to average levels once collusion starts, and while the coefficients are positive for some of the collusion years, they are not statistically significant.

Figure 4 here

Next, we estimate Equation (1) with *Profitability* as the dependent variable. The results are presented in Table 8, Column (1). *Profitability* is increased by 0.3% during collusion periods, but the increase is not statistically significant. In Column (2) we present results from a specification that adds an indicator variable *Collusion*(*t-1*), which takes a value of one for the year prior to collusion for cartel firms and zero otherwise. The coefficient for *Collusion*(*t-1*) is negative and statistically significant, confirming that there is a drop in profitability before collusion starts. The difference in the coefficients between *Collusion* and *Collusion*(*t-1*) is statistically significant at the 5% level. Overall, these findings suggest that cartels are formed not to increase profits to abnormally high level, but instead to undo decreases in profitability.

Table 8 here

The findings in Figure 4 and Columns (1) and (2) of Table 8 suggest that the profitability of cartel firms during collusion years is on average not higher than during years preceding collusion. It then follows that changes in realized profitability cannot explain the significant reductions in leverage during collusion years. We test this conjecture explicitly by distinguishing cartel firms that experienced an increase in profitability from cartel firms that experienced a decline. Specifically, we compute the difference between the mean profitability for collusion and pre-collusion years and split the sample according to whether firms experienced negative or positive changes in profitability. In Columns (3) and (4) of Table 8 we present the results for the two subsamples. The coefficients for *Collusion* have similar magnitudes, but they are not statistically significant. This suggests that changes in observed profitability cannot explain the reduction in leverage we observe during collusion years.

6.2. Cost of Debt Financing

An alternative indirect link between the decision to form a cartel and the decision to reduce leverage could be constructed based on changes to the cost of borrowing. If being in a cartel is associated with worse debt financing terms for cartel firms, then they should naturally make less use of debt in their capital structure. It is unclear why a cartel firm should be viewed as a worse credit risk than a non-cartel firm, and more importantly, how a lender would be able to identify a cartel firm in the first place (because of the secrecy around cartels). Nevertheless, we entertain this possibility and explore how the credit environment changes when firms form a cartel.

We use data on private loan contracting terms from the Loan Pricing Corporation's (LPC) *Dealscan* database. The *Dealscan* database contains detailed loan information for U.S. and foreign commercial loans made to government entities and corporations.[17] Merging the *Dealscan* data with our main database causes significant sample attrition, since loan data is only available in years in which our sample firms signed new loan contracts. We are left with 518 cartel-firm observations for 84 cartel firms, which are then matched to control firm observations with available loan data using the approach described in Section 4.

To measure whether a cartel firm's credit environment has tightened, we focus on whether the coupon has increased, and whether a loan is more likely secured. We define *Spread* as the "all-in-drawn" spread (in basis points) over LIBOR, computed as the sum of coupon and annual fees on the loan in excess of six-month LIBOR. If a firm took out several loans during a given year, we use the average spread. Next, we define *Secured* as a dummy that takes a value of 1 if a firm took out a secured loan during a given year, and 0 otherwise. We estimate Equation (1) using either *Spread* or *Secured* as dependent variables. We use the same set of control variables as in the earlier leverage regressions, but we also report results for regressions that additionally include *Leverage* as an explanatory variable, since changes in the loan terms could be explained by the endogenous change in leverage we documented earlier.

The results are presented in Table 9. Column (1) shows that the loan spreads are narrower during collusion periods, not wider. Column (2) shows the same result when leverage is included as an independent variable. Columns (3) and (4) show that there is no significant change in the frequency of loans being secured during collusion periods. Overall, it does not seem that the credit environment is tighter for cartel firms.

---

[17] See Chava and Roberts (2008) for a description of this database.

Table 9 here

7. 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). We expand the set of predictions to better understand the effect, and to allow for sharper tests that raise the bar for both 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 patterns that otherwise have no convincing explanation.

Our findings also make an important contribution to the antitrust literature, by confirming empirically that leverage can be used strategically, to add stability to cartel agreements. Little is known about cartels and their operations, since they are illegal and secrecy seriously limits the access to reliable data. Our findings are consistent with the idea that the strategic use of leverage is one mechanism that cartels use to prevent cartel breakups. This is only one mechanism that cartels can use, of course, but our findings suggest that it should be taken more seriously, hopefully motivating further research into what strategies cartel firms use to make their cartels more stable.

References

Artiga, Tanja, Markus Schmid, and David Yermack, 2019. "Does Price Fixing Benefit Corporate Managers?" *Management Science*, forthcoming.

Balsmeier, Benjamin, Lee Fleming, and Gustavo Manso, 2016. "Independent Boards and Innovation." Working paper, UC Berkeley Haas.

Bertrand, Marianne, and Sendhil Mullainathan, 2003. "Enjoying the Quite Life? Corporate Governance and Managerial Preferences." *Journal of Political Economy* 111, 1043-1075.

Blackwell, Matthew, Stefano Iacus, Gary King, and Giuseppe Porro, 2009. "CEM: Coarsened Exact Matching in Stata." *Stata Journal* 9, 524-546.

Bolton, Patrick, and David Scharfstein, 1990. "A theory of predation based on agency problems in financial contracting." *American Economic Review* 80, 93-106.

Brander, James, and Tracy Lewis, 1986. "Oligopoly and Financial Structure: The Limited Liability Effect." *American Economic Review* 76, 956-970.

Bryant, Peter, and Woodrow Eckard, 1991. "Price Fixing: The Probability of Getting Caught." *Review of Economics and Statistics* 73, 531-536.

Busse, Meghan, 2002. "Firm Financial Condition and Airline Price Wars." *RAND Journal of Economics* 33, 298-318.

Campello, Murillo, Daniel Ferrés, and Gaizka Ormazabal, 2017. "Whistleblowers on the Board? The Role of Independent Directors in Cartel Prosecutions." *Journal of Law and Economics* 60, 241-268.

Chang, Myong-Hun, 1991. "The effects of product differentiation on collusive pricing." *International Journal of Industrial Organization* 9, 453-469.

Chava, Sudheer, and Michael Roberts, 2008. "How does financing impact investment? The role of debt covenant violations." *Journal of Finance* 63, 2085-2121.

Chevalier, Judith, 1995a. "Capital structure and product-market competition: Empirical evidence from the supermarket industry." *American Economic Review* 85, 415-435.

Chevalier, Judith, 1995b. “Do LBO supermarkets charge more? An empirical analysis of the effects of LBOs on supermarket pricing.” *Journal of Finance* 50, 1095-1112.

Chevalier, Judith, and David Scharfstein, 1996. “Capital market imperfections and countercyclical markups: Theory and evidence.” *American Economic Review* 86, 703-725.

Connor, John, 2011. “Cartel Detection and Duration Worldwide.” *Competition Policy International: Antitrust Chronicle* 9, 2-10.

Connor, John, 2014. “The Private International Cartels (PIC) Data Set: Guide and Summary Statistics, 1990−2013.” Working paper, available at http://ssrn.com/abstract=2478271

Dasgupta, Sudipto, and Sheridan Titman, 1998. “Pricing Strategy and Financial Policy.” *Review of Financial Studies* 11, 705-737.

Dasgupta, Sudipto, and Alminas Žaldokas, 2019. “Anti-Collusion Enforcement: Justice for Consumers and Equity for Firms.” Forthcoming, *Review of Financial Studies*.

Dong, Ailin, Massimo Massa, and Alminas Žaldokas, 2019. “The Effects of Global Leniency Programs on Margins and Mergers”. Forthcoming, *Rand Journal of Economics*.

Fama, Eugene, and Kenneth French, 2002. “Testing Trade-Off and Pecking Order Predictions About Dividends and Debt.” *Review of Financial Studies* 15, 1-33

Frank, Murray, and Vidhan Goyal, 2015. “The Profits–Leverage Puzzle Revisited.” *Review of Finance* 19, 1415-1453.

Friedman, James, 1971. “A noncooperative equilibrium for supergames.” *Review of Economic Studies* 38, 1-12.

Grossman, Peter, and Kathy Paulson Gjerde, 2009. “The JEC Revisited: Did Debt Undermine Stability?” *Atlantic Economic Journal* 37, 65-71.

Grullon, Gustavo, George Kanatas, and Pyush Kumar, 2006. “The Impact of Capital Structure on Advertising Competition: An Empirical Study.” *Journal of Business* 79, 3101-3124.

Halling, Michael, Jin Yu, and Josef Zechner, 2016. “Leverage dynamics over the business cycle.” *Journal of Financial Economics* 122, 21-41.

Han, Martijn, and Alminas Žaldokas, 2016. "CEO Short-Termism Can Enhance Product Market Collusion." Working Paper.

Heath, Davidson, and Giorgo Sertsios, 2019. "Not all profits are created equal: New evidence on the profits-leverage relation." Working paper.

Herold, Daniel, and Johannes Paha, 2018. "Cartels as Defensive Devices: Evidence from Decisions of the European Commission 2001–2010." *Review of Law & Economics* 14, 1-31.

Hoberg, Gerard, and Gordon Phillips, 2010. "Product market synergies in mergers and acquisitions: A text based analysis." *Review of Financial Studies* 23, 3773–3811.

Hoberg, Gerard, Gordon Phillips, and Nagpurnanand Prabhala, 2014. "Product Market Threats, Payouts, and Financial Flexibility." *Journal of Finance* 69, 293-324.

Iacus, Stefano, Gary King, and Giuseppe Porro, 2011. "Multivariate Matching Methods That are Monotonic Imbalance Bounding." *Journal of the American Statistical Association* 106, 345-361.

Iacus, Stefano, Gary King, and Giuseppe Porro, 2012. "Causal Inference without Balance Checking: Coarsened Exact Matching." *Political Analysis* 20, 1-24.

Khanna, Naveen., and Sheri Tice, 2000. "Strategic responses of incumbents to new entry: The effect of ownership structure, capital structure, and focus." *Review of Financial Studies* 13, 749-779.

Kovenock, Dan, and Gordon Phillips, 1995. "Capital structure and product-market rivalry: How do we reconcile theory and evidence?" *American Economic Review* 85, 403-408.

Kovenock, Dan, and Gordon M. Phillips, 1997. "Capital structure and product market behavior: An examination of plant exit and investment decisions." *Review of Financial Studies* 10, 767-803.

Lamoreaux, Naomi, 1985. The great merger movement in American business, 1895-1904. Cambridge University Press.

Lemmon, Michael, Michael Roberts, and Jaime Zender, 2008. "Back to the beginning: Persistence and the cross-section of corporate capital structure." *Journal of Finance* 63, 1575-1608.

Levenstein, Margaret, and Valerie Suslow, 2006. "What Determines Cartel Success?" *Journal of Economic Literature* 44, 43-95.

Levenstein, Margaret, and Valerie Suslow, 2011. "Breaking Up Is Hard to Do: Determinants of Cartel Duration." *Journal of Law and Economics* 54, 455-492.

Levenstein, Margaret, and Valerie Suslow, 2016. "Price-Fixing Hits Home: An Empirical Study of U.S. Price Fixing Conspiracies." *Review of Industrial Organization* 48, 361-379.

MacKay, Peter, and Gordon Phillips, 2005. "How Does Industry Affect Firm Financial Structure?" *Review of Financial Studies* 18, 1433-1466.

Maksimovic, Vojislav, 1988. "Capital Structure in Repeated Oligopolies." *RAND Journal of Economics* 19, 389-407.

Maksimovic, Vojislav, and Sheridan Titman, 1991. "Financial policy and reputation for product quality." *Review of Financial Studies* 4, 175-200.

Maksimovic, Vojislav, and Josef Zechner, 1991. "Debt, Agency Costs, and Industry Equilibrium." *Journal of Finance* 46, 1619-1643.

Miller, Nathan, 2009. "Strategic Leniency and Cartel Enforcement." *American Economic Review* 99, 750-768.

Opler, Tim, and Sheridan Titman, 1994. "Financial distress and corporate performance." *Journal of Finance* 49, 1015-1040.

Parrino, Robert, 1997. "CEO turnover and outside succession a cross-sectional analysis." *Journal of Financial Economics* 46, 165-197.

Petersen, Mitchell, 2009. "Estimating Standard Errors in Finance Panel Data Sets: Comparing Approaches." *Review of Financial Studies* 22, 435-480.

Piccolo, Salvatore, and Giancarlo Spagnolo, 2014. "Debt, Managers and Cartels". Working paper.

Phillips, Gordon, 1995. "Increased debt and industry product markets: An empirical analysis." *Journal of Financial Economics* 37, 189-238.

Phillips, Gordon, and Giorgo Sertsios, 2013. "How Do Firm Financial Conditions Affect Product Quality and Pricing?" *Management Science* 59, 1764-1782.

Povel, Paul, and Michael Raith, 2004. "Financial Constraints and Product Market Competition: Ex-ante vs. Ex-post Incentives." *International Journal of Industrial Organization* 22, 917-949.

Rotemberg, Julio, and Garth Saloner, 1986. "A supergame-theoretic model of business cycles and price wars during booms." *American Economic Review* 76, 390-407.

Schmitt, Nicolas, and Rolf Weder, 1998. "Sunk costs and cartel formation: Theory and application to the dyestuff industry." *Journal of Economic Behavior & Organization* 36, 197-220.

Showalter, Dean, 1995. "Oligopoly and financial structure: Comment." *American Economic Review* 85, 647-653.

Stenbacka, Rune, 1994. "Financial structure and tacit collusion with repeated oligopoly competition." *Journal of Economic Behavior and Organization* 25, 281-292.

Sutton, John, 1991. Sunk Costs and Market Structure. Cambridge, MA: MIT Press.

Titman, Sheridan, and Roberto Wessels, 1998. "The Determinants of Capital Structure Choice." *Journal of Finance* 43, 1-19.

Zingales, Luigi, 1998. "Survival of the fittest or fattest? Exit and financing in the trucking industry." *Journal of Finance* 53, 905-938.

FIGURES AND TABLES

Figure 1

This figure plots *Leverage* for three cartel firms, around the year they allegedly started colluding. Each of the three firms reduced its leverage considerably at or near the onset of collusion.

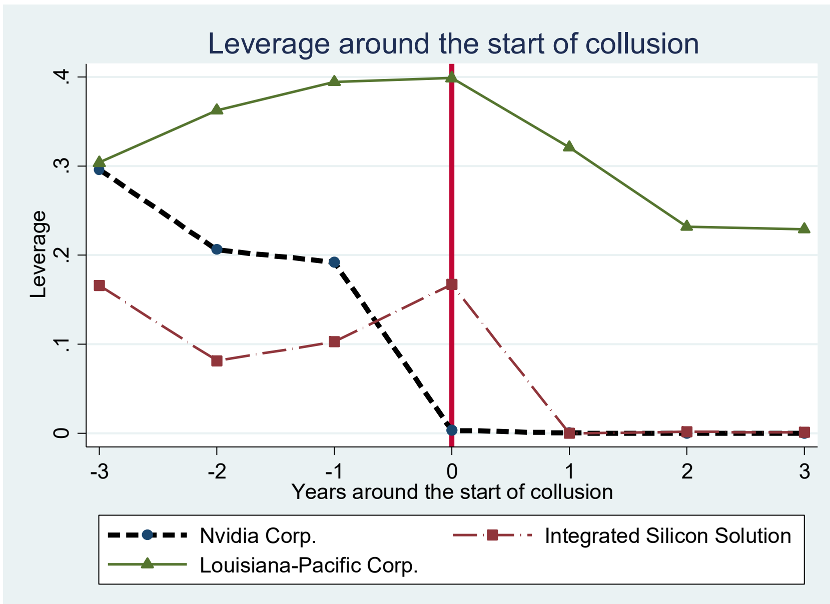

Figure 2

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 ($h \in \{\text{col1,col2,col3,oth.col}\}$ for full collusion years, $h = 0^-$ for a partial collusion year at the start of a cartel, and $h = 0^+$ for a partial collusion year at the end of a cartel). 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.

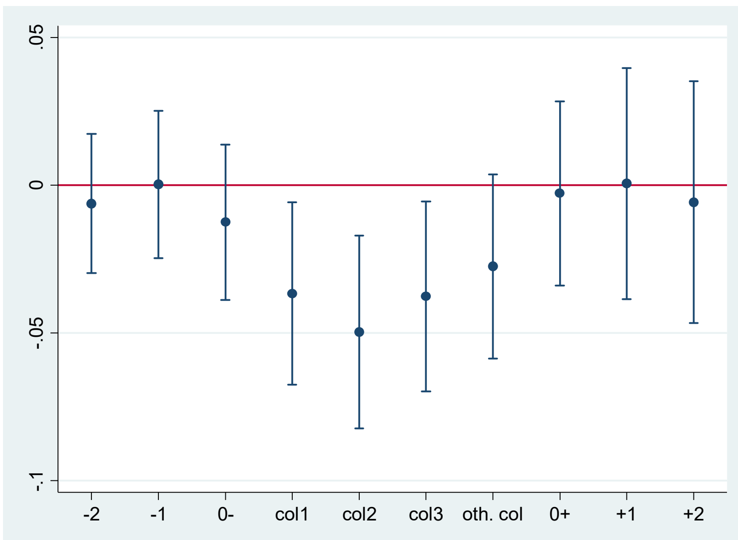

Figure 3

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 log(Debt) or log(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 a cartel, and $h = 0^+$ for a partial collusion year at the end of a cartel). 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.

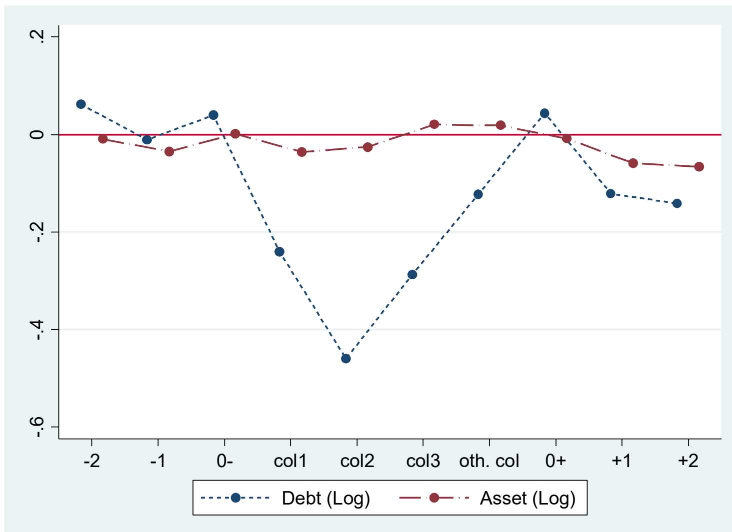

Figure 4

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 *Profitability* 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 ($h \in \{\text{col1, col2, col3, oth.col}\}$ for full collusion years, $h = 0^-$ for a partial collusion year at the start of a cartel, and $h = 0^+$ for a partial collusion year at the end of a cartel). 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 the onset of collusion is preceded by a negative shock to profitability, and that cartels are formed in response to such negative shocks.

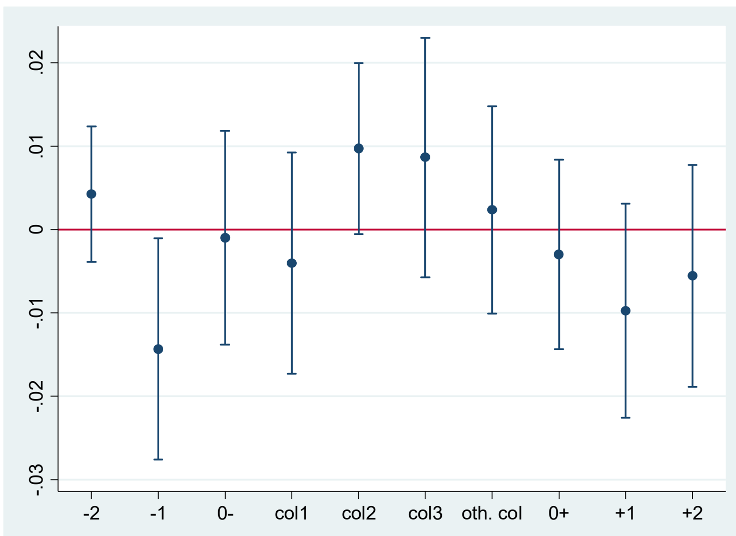

Table 1. Summary statistics

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 firms 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 (US $ million) > 0	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 (months) >0	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: Cartel 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 to 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 samples 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%.

Variable	(1) Cartel	(2) Cartel (matched)	(3) Non-cartel (matched)	(4): (2)-(3) Difference
Leverage	0.272	0.269	0.260	0.008
Profitability	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
MB	2.005	2.009	2.053	-0.044
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 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 *Product-Market Fluidity*. *Product-Market Fluidity* is 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* expenses. 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 homogen.	High 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 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 lev.	High initial lev.	Low initial rel-to-ind. lev.	High initial rel-to-ind. lev.
# 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 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-revenues*, where *Fines-to-revenues* 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 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*. In Column (1) we do not report the coefficient for *Recession*, 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* by *GDP Growth*. In Column (4) we present the results after replacing the indicator variable *Recession* by *Industry Growth*. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	(1)	(2)	(3)	(4)
Independent variables	*Leverage*	*Leverage*	*Leverage*	*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	0.030***	0.037***		
	(0.011)	(0.011)		
Post Collusion × Recession	-0.001	-0.006		
	(0.010)	(0.011)		
Recession		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

Table 8. Profitability

This table presents the results of analyzing the association between collusion and profitability (Columns 1 and 2), and collusion and leverage (Columns 3 and 4). Column (1) presents the estimation results with *Profitability* as the dependent variable (see the Appendix for variable definitions). Column (2) presents the estimation results after adding *Collusion*(*t-1*) as an independent variable. Columns (3) and (4) present the results of splitting the sample of cartel firms into firms that earned higher average profits during collusion years than during pre-collusion years, and firms that earned lower average profits during collusion years than during pre-collusion years. *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*. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

Independent variables	(1) *Profitability*	(2) *Profitability*	(3) *Leverage*	(4) *Leverage*
Collusion(*t-1*)		-0.016**		
		(0.007)		
Collusion	0.003	-0.001	-0.028	-0.021
	(0.005)	(0.005)	(0.020)	(0.015)
Post Collusion	-0.004	-0.008	-0.011	-0.034
	(0.005)	(0.005)	(0.048)	(0.022)
Observations	12,655	12,655	606	762
R-squared	0.805	0.805	0.691	0.683
Sample Split	All	All	$\Delta\pi \geq 0$	$\Delta\pi < 0$
# Cartel Firms	89	89	42	48
# Matched Control Firms	1,596	1,596	0	0
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes

Table 9. Loan contracting

This table presents results of analyzing the association between collusion and terms of loan contracting. In Columns (1)-(2), the dependent variable is *Spread.* This is the "All-in-Drawn" spread (in basis-points) over LIBOR, computed as the sum of coupon and annual fees on the loan in excess of six-month LIBOR (averaged, if a firm took out several loans in a year). In Columns (3)-(4), the dependent variable *Secured* is a firm-year dummy variable that takes a value of 1 if a firm obtained a secured loan (or multiple secured loans) in a year, and zero otherwise. *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). Columns (2) and (4) additionally control for *Leverage*. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

Independent variables	(1) *Spread*	(2) *Spread*	(3) *Secured*	(4) *Secured*
Collusion	-31.7**	-27.5*	0.07	0.08
	(15.245)	(14.006)	(0.131)	(0.133)
Post Collusion	-27.8	-23.0	0.25*	0.25*
	(17.453)	(16.044)	(0.143)	(0.143)
Leverage		76.1**		0.13
		(35.656)		(0.134)
Observations	2,686	2,686	1,344	1,344
R-squared	0.829	0.831	0.837	0.838
# Cartel Firms	84	84	78	78
# Matched Control Firms	815	815	692	692
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes

Appendix: Variable Definitions

Cartel Characteristics:

Collusion length (months)	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.
# Cartel firms	Number of global firms involved in a cartel.
Fines (US $ million)	Worldwide fines and recoveries from a cartel firm, in millions of U.S. dollars.
Fines (US $ million)>0	Worldwide fines and recoveries from a cartel firm, in millions of U.S. dollars, conditional on non-zero fines and recoveries being recorded.
Prision for executives (months)	Aggregate prison time sentences for all executives at a cartel firm.
Prision for executives (months) >0	Aggregate prison time sentences for all executives at a cartel firm, conditional of 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

MB	Sum of the market value of equity and total liabilities divided by book value of assets
Debt	Logarithm of the sum of short-term debt and long-term debt
Collusion	Indicator variable that takes the value of 1 for cartel firms during active collusion years, and 0 otherwise
Post Collusion	Indicator variable that takes a value of 1 for cartel firms during the 5 years after a cartel is dissolved, and 0 otherwise

Sample Splits:

Product-Market 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 & 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 every 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.
Recession Year	Indicator variable that takes a value of 1 in a recession year, and 0 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, for Compustat firms.
$\Delta \pi$	Change in average profitability, comparing collusion and pre-collusion years for cartel firms. The variable is positive when mean profitability during collusion years is higher than during pre-collusion years.

Loan Characteristics:

Spread	"All-in-Drawn" spread (in basis-points) over LIBOR, computed as the sum of coupon and annual fees on the loan in excess of six-month LIBOR.
Secured	Indicator variable that takes a value of 1 if the firm took out a secured loan in a year, and 0 otherwise