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Capital Structure Under Collusion

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Capital Structure Under Collusion

Abstract

We study the financial leverage of firms that collude by forming a cartel. We find that cartel firms have lower leverage ratios during collusion periods, consistent with the idea that reductions in leverage help increase cartel stability. Cartel firms have a surprisingly large economic footprint (they represent more than 20% of the total market capitalization in the U.S.), so understanding their decisions is relevant. Our findings show that anti-competitive behavior has a significant effect on capital structure choices. They also shed new light on the relation between profitability and financial leverage.

Keywords: Capital Structure; Financial Leverage; Financial Policies; Collusion; Cartels; Trigger Strategies

JEL Classification: G32, L12

1. Introduction

We study the financial decisions of firms that join cartels — collusive agreements to raise prices in the output markets. Cartel firms represent a significant fraction of the economy: Over the last two decades, publicly traded U.S. firms convicted of cartel activity accounted for more than one fifth of the total market capitalization (we describe our data in more detail below). But despite this large economic footprint (and the deadweight efficiency losses they cause, see Connor and Bolotova 2006), there is no empirical work asking whether collusion matters for a firm's financial policies.

This is an important gap in the literature that studies financial and product market decisions. Some of the existing work studies whether debt turns firms into less or more aggressive competitors,[1] or, conversely, how a firm's financial structure is affected by its competitive position.[2] In all of this work, the focus is on how traditional product market strategies (choices of output, prices, advertising, product quality, etc.) are determined, or on how variation in the intensity of competition affects financial policies.[3] We show that the set of product market strategies considered in this type of analysis should be extended to include anti-competitive behavior, i.e., outright cartel formation. Using this approach, we can explain interesting patterns in capital structure and other financial policies.

We find that during periods of collusion, cartel firms reduce their leverage. This is at first surprising, since based on the Trade-off Theory intuition, one would expect that higher profits from collusion make it attractive to *increase* financial leverage. However, a countervailing force dominates this effect: Reductions in financial leverage can make cartel agreements more stable, as argued in Maksimovic (1988). The building stone of his model is that cartel agreements are not

[1] The theoretical predictions are ambiguous: 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 and output decisions, firms with debt are less aggressive (Povel and Raith (2004)). Not surprisingly, the empirical findings are inconsistent. 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] Other work studies the interaction of a firm's competitive situation with more broadly defined financial policies, including cash holdings, payout, or investment. See, e.g., Haushalter et al. (2007), Hoberg and Phillips (2010), or Frésard and Valta (2016).

legally enforceable, and each cartel member has an incentive to "cheat" by undercutting the agreed cartel price. In this setting, high leverage makes deviations more attractive, so cartel firms looking for a stable collusive agreement should commit to keeping their leverage at moderate levels. This yields what we refer to as the "Commitment Hypothesis": During collusion periods, cartel firms should have lower financial leverage (see Section 2 for more details).

We study changes in leverage during collusion periods using a difference-in-difference approach. Information about cartel membership for U.S. firms, and the years in which they were explicitly colluding, is gathered from the PIC database, a comprehensive database on international price-fixing cartels (see Section 3 for details). We compare cartel firms with control firms, both during years in which cartel firms actively collude and years in which they do not. Consistent with the Commitment Hypothesis, we find that cartel firms reduce their leverage during periods in which they are actively colluding. We also show that the drop in leverage happens at the onset of collusion, and it is not driven by prior ongoing trends.

We corroborate our inferences about the Commitment Hypothesis using a series of triple-difference tests. We find that the effect we document is concentrated among cartel firms with higher leverage (the need to reduce leverage is stronger for them); cartel firms operating in more competitive environments (so deviations are a larger threat, requiring larger reductions in leverage to sustain collusion); cartel firms operating in years of economic booms (when increases in current profits are more attractive and the threat of reduced future profits has less bite); and cartel firms operating in environments with regulatory developments aimed at destabilizing cartels (stronger reductions in leverage are then needed to restore the stability of a cartel).

In additional tests we address the endogeneity of the timing of collusion. We instrument the collusion and post-collusion periods, using the intensity of cartel activity or cartel dissolutions in related industries. In the IV regressions we find an even larger reduction in leverage during collusion. We also show that the changes in leverage by cartel firms are not caused by changes in the cost of debt financing during collusion periods.

A comprehensive examination of a firm's capital structure decisions requires analyzing all major sources and uses of cash, including changes in payout, cash holdings, and investment. We therefore study these broader financial policies of cartel firms. We find that cartel firms have higher payout ratios during collusion periods, in the form of increased share repurchases. Consistent with the finding in Hoberg et al. (2014), that firms facing stronger competitive pressure tend to have lower payouts, we find that the payout increases are driven by cartel firms that face less competitive environments (i.e., where cartels are more stable and the risk of the cartel breaking up is likely lower). These are also the cartel firms that reduce their cash holdings during collusion periods, consistent with cash holdings for precautionary reasons being less important in less competitive environments (Haushalter et al. 2007). Finally, we find that firms do not increase their capital expenditure during collusion periods, not surprising given that capacity expansions would work against the cartel's goal of limiting output. Taken together, our tests suggest that cartel firms make strategic changes to their broadly defined financial strategies during collusion periods. This is important, as additional evidence that firms strategically change their financial policies during collusion periods further substantiates our inferences about the Commitment Hypothesis.

We make two contributions to the literature. First, our findings extend the literature (discussed above) that studies the interdependence of financial strategies and product market strategies. This literature has so far ignored — with the exception of Maksimovic (1988) — cartels and less formal product market coordination strategies ("tacit collusion").[4] The existing work mostly focuses on particular product market strategies that include pricing, output, and similar decisions. We show that financial decisions also interact with more broadly defined product market strategies that include anti-competitive behavior.

Our results are important, because they apply to a large part of the economy. According to our data, the economic footprint of cartel firms is large, as cartel activity includes many large firms.[5] Focusing on U.S. firms included in *Compustat*, and aggregating over the years 1990 to 2010, firms convicted of membership in at least one international cartel represent 16.6% of the total in terms of assets, 18.5% in terms of sales, and 22.7% in terms in terms of market cap. Even if we compare only the firm-years during which cartels were actively colluding, the proportions are high: 6.0% of assets, 5.2% of sales, and 6.3% of market cap. The importance of colluding firms is likely larger than that because according to the literature, many cartels remain undetected

[4] Phillips and Sertsios (2013) and Busse (2002) show that firms with higher financial leverage make more aggressive product market decisions (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.

[5] That is a standard finding, see Levenstein and Suslow (2006); for a rationale, see Bos and Harrington (2010).

by the authorities (see Connor 2011, 2014, and Bryant and Eckard 1991), and because our data does not include cases of "tacit collusion", i.e., anti-competitive behavior that functions without an explicit collusive agreement and is thus hard to detect and prosecute.[6] Moreover, our data (described in Section 3) does not include cartels that were limited to one country.[7]

A second contribution of our paper is that it sheds new light on the relation between firm profitability and financial leverage. The Trade-off Theory predicts that cartel firms should increase their leverage when they collude, as they increase their profitability. However, we find that when firms enter into a collusive agreement and increase their profits, they reduce their financial leverage, and this results in a negative association between profitability and leverage. Thus, our results can offer a new rationale for the negative relation between profitability and leverage that the literature has long found puzzling (Myers 1993; Parsons and Titman 2008; Graham and Leary 2011).

Specifically, our results suggest that in order to understand the relation between profitability and leverage, it is useful to study the sources of variation in profitability. The benefit of our focus on cartel membership is that it represents a very direct, positive shock to a firm's profitability, rooted in anti-competitive behavior that goes beyond what accounting measures of profitability can capture. An alternative approach is taken in Xu (2012), who studies shocks to a firm's competitive environment, specifically, changes in import restrictions. She finds a *positive* relation between profitability and leverage (as predicted by the Trade-off Theory). The difference between her results and our results is due to the focus on different drivers of profitability, which supports our argument that it is important to identify what causes changes in profitability.

Obviously, our findings cannot explain why the relation between profitability and leverage should be negative for *all* firms; but just like other possible explanations (e.g., the dynamic Trade-off model in Hennessy and Whited (2005)), our findings help understand why the relation *can* be negative. Importantly, while our findings conflict with the intuition of the Trade-off Theory, this

[6] Trigger-strategy models of collusion do not require an explicit collusive agreement, so our findings should extend to cases of tacit collusion.
[7] Most recent studies we know of use the PIC data set. That is not surprising, given the challenges of collecting data on cartels, whose activities are illegal and need to be hidden (see Connor (2014)). Levenstein and Suslow (2015) and Miller (2009) collected data on prosecutions brought by the U.S.D.O.J. under the Sherman Act. Of those cases, some are included in our data set, while others involve privately held firms for which financial data is unavailable.

does not imply that the Trade-off Theory is *false*: Our interpretation is instead that for the cartel firms we study, the commitment effect dominates the effects captured by the Trade-off Theory. We also examine other possible explanations for the changes in leverage during collusion periods (besides the Commitment Hypothesis and the Trade-off Theory), and we show that they cannot explain our results.[8]

Our paper is also related to recent work that focuses on cartels and changes in antitrust policies. Dasgupta and Žaldokas (2016) 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 the cartel (i.e., the Commitment Hypothesis). Dong et al. (2016) 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. (2013) and Campello et al. (2015) investigate cartel convictions from the perspective of corporate governance.

The remainder of the paper proceeds as follows. Section 2 discusses the main hypotheses tested in our paper. Section 3 describes the data. Section 4 presents results of the tests of the hypotheses. Section 5 presents results about how cartel formation affects more broadly defined financial policies, including payout policy and cash holdings. Section 6 discusses possible alternative explanations. Section 7 concludes.

2. Hypotheses

Our main hypothesis is what we term the "Commitment Hypothesis," based on effects modeled in Maksimovic (1988). He studies how firms may be able to sustain high prices and low outputs without the ability to legally enforce any such agreements (since cartels are illegal), and how this is affected by financial leverage.

Collusion is feasible in a multi-period model. If firms know that they will interact repeatedly, they have an incentive to set the agreed high prices and resist the temptation to lower their prices for an instantaneous profit (at the expense of the other cartel members) if such

[8] This includes the Pecking Order theory (Myers 1984), strategic debt (Brander and Lewis 1986), growth options (Strebulaev 2007), and the dynamic trade-off theory (Hennessy and Whited 2005). See Section 6 for details.

deviations lead to costly punishment in later periods. A credible threat is created by "trigger strategies": Each firm plans to choose the collusion action (high prices, low output) in each period, indefinitely, but if one or more rival firms deviate and charge lower prices, all firms revert to a low-price equilibrium strategy in each period that follows.[9] That response is credible, and it is a punishment threat since it reduces future profits for all firms, including firms that deviated. (This punishment outcome is costly for all firms, but in equilibrium it can be avoided.) Collusion can be sustained if the collusion profits that are lost after a deviation are more valuable than the one-time profit that can be earned by deviating in any period. Note that an explicit collusive agreement is not needed: Such trigger strategies form an equilibrium and can arise in the form of "tacit collusion".

Maksimovic (1988) shows that financial leverage exacerbates the incentive to deviate from collusion and must therefore be kept in check. With significant debt, a cartel firm's future profits largely go to the creditors, so the threat of lower future profits has less bite (a shareholder's lowest possible future payoff is zero, due to limited liability); and the shareholders benefit if the firm can earn a large instantaneous profit (net of debt payments) by deviating. If several firms decide to form a cartel, they must therefore ensure that each cartel firm's leverage is below a certain level, thus strengthening its commitment to abide by the collusive agreement. The *Commitment Hypothesis* thus predicts that when firms form a cartel, the average leverage ratio should be reduced while profits increase.

An alternative hypothesis, with predictions that go against those of the Commitment Hypothesis, is based on the Trade-off Theory (Kraus and Litzenberger (1973), Bradley et al. (1984)). The intuition for how profitability should affect leverage is straightforward (see also Xu 2012): With higher profits, the threat of financial distress is reduced, and the tax benefits of corporate debt become more significant. Aligned, the two forces should lead to higher leverage. That should apply to cartel firms when collusion starts, since the goal of cartels is to increase profits. So the *Trade-off Theory Hypothesis* predicts that when firms form a cartel, their leverage should increase.

[9] The role of repeated interaction in making tacit collusion feasible was first described in Friedman (1971).

In Section 6 we discuss additional possible explanations of how profitability may affect financial leverage: The Pecking Order theory (Myers 1984), strategic debt (Brander and Lewis 1986), growth options (Strebulaev 2007), and the dynamic trade-off theory (Hennessy and Whited 2005). As we show, none of them can explain the leverage reductions during collusion periods.

3. Data and Variable Construction

Our analysis uses the Private International Cartels (PIC) database, which contains information on virtually all private international price-fixing cartels detected by antitrust authorities between 1990 and 2012.[10] The database is described in detail in Connor (2014). The focus is on "private" cartels, since government-sanctioned "public" cartels are not at risk of prosecution; and the data include only cartels with an "international" flavor, i.e., cartels that include firms from multiple countries, or if an antitrust authority pursued firms registered abroad.

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% 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.

From this database we collect the following information for each cartel firm: Name, country of incorporation, markets and locations where collusion took place, and the start and end dates of the collusive agreements. 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 216 firms. We exclude firms involved in more than one cartel simultaneously, since otherwise the start and end dates of collusion would be arbitrary. The final sample includes 1,429 firm-years for 93 cartel firms.[11]

[10] The data includes a few cartels discovered before 1990. Where possible, we use data from 1985 onwards in our tests.

[11] The cartel firms in our sample belonged to 58 different cartels. The other cartel members included international firms, as well as publicly traded and privately held U.S. firms.

In several of our tests we include "non-cartel" firms as controls. These are U.S. firms included in *Compustat* that were *not* cartel firms, i.e., they were not in the PIC database. We exclude non-cartel firms that operate in the same 4-digit SIC code as a given cartel firm, since Leary and Roberts (2014) show that firms imitate their rivals' decisions to some extent. If so, the tests could fail to detect leverage changes merely because same-industry non-cartel rivals changed their leverage too. As a robustness check, we replicate our tests including same-industry rivals. Our results continue to hold, suggesting that the pattern we document does not reflect a general variation in industry conditions.

The sample of non-cartel firms includes 128,188 firm-years for 12,999 firms. An alternative use of control firms would be to construct matched samples. The drawback of such an approach is that the most similar firms (in terms of observable characteristics) would also most likely be undetected members of cartels (for example, size is a strong predictor of cartel membership; see also Dong et al. 2016).

Table 1 presents descriptive statistics for selected variables. Panel A presents summary statistics for the data set including both cartel and non-cartel firms. Panel B presents separately the means for cartel and non-cartel firms, and the significance levels for the differences. The main variables used in our analysis are *Profitability, Leverage*, *Total Payout*, *Dividend* and *Cash*. All five are scaled by the book value of assets; for more details on the construction of each variable see Appendix A. All financial variables from *Compustat* are winsorized at the 1% level.

Table 1

The statistics in Panel B of Table 1 show that cartel firms are larger and more profitable than non-cartel firms. They have lower leverage and cash holdings than non-cartel firms. Their overall payout to shareholders is higher, but that is driven by higher share repurchases — while cartels firms are more likely to pay dividends, the average dividend (scaled by total assets) is smaller. Finally, cartel firms exhibit low cash flow volatility and high asset tangibility.

How long a cartel is active varies across the sample: The average duration is over six years, and the median duration is just under five years; 12.7% lasted for less than a year, while 7.2% lasted 15 or more years (the maximum is 34 years). There is also variation in the number of firms that joined a cartel. The average is less than seven, the median five; just over 15% of the cartels consisted of only two firms, while 8.3% consisted of 15 or more firms (the maximum is 42 firms). These numbers are consistent with those in earlier studies (Levenstein and Suslow 2006).

4. Collusion and Financial Leverage

4.1 Empirical Design

To analyze the relation between collusion and financial policies we estimate variations of the following baseline empirical model:

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

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-difference 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.[12]

Our key parameter of interest is β. A positive estimated coefficient would be consistent with cartel firms raising leverage ratios during cartel years, in response to increased profits; a negative coefficient would be consistent with firms reducing leverage ratios during collusion years, to strengthen their commitment not to deviate from a collusive agreement.

We include a set of controls **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 cash flow volatility*. Firm and year fixed effects are represented by φ_i and μ_t, respectively.

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

In some specifications we use cartel firms only (i.e., the "eventually treated" sample). In this setup, non-collusion firm-years are the control sample. To the extent that the results are similar to those of the main specification, this specification helps to attribute changes in behavior to the treated group, rather than to the control group. 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)).

4.2 Leverage During and After Collusion Years

Table 2, Panel A, presents the results for our baseline empirical specification, Equation (1). 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 2.5 to 3 percentage points. These effects are statistically significant at the 5% level, and they are economically significant too: As shown in Table 1, the average leverage ratio of cartel firms is 27%, so the leverage ratio decreases by nearly a tenth. Furthermore, the effect is twice as large for high-leverage firms, as we discuss below (see Table 3). The coefficients for *Collusion* are very similar across the four columns, which suggests that the effect is driven by cartel firms reducing their leverage ratio, and not by leverage changes made by non-cartel firms (recall that Columns (3) and (4) use cartel firm data only). Overall, our findings are consistent with the Commitment Hypothesis.

Table 2

Table 2, Panel A, also suggests that leverage is somewhat reduced in the years after collusion ends (the coefficients on *Post Collusion* are negative, but they are not statistically significant). This may capture increases in the cost of debt financing due to negative reputation effects after having been convicted of cartel activities (see Section 4.6, where we show that cartel firms face a more difficult credit environment after cartels are dissolved).

Table 2, Panel B, shows corresponding regression results for the firms in the same industry as cartel firms, but *not* members of the cartel. That is, we study whether non-cartel firms also reduced their leverage when their industry rivals formed a cartel. We find no evidence for that, so our results are not driven by industry trends.

4.3 Can Leverage-Ratio Trends Explain the Results?

A possible concern with our estimates in Table 2, Panel A, is that the negative coefficients for *Collusion* could reflect a time trend in the financial policies of cartel firms that is unrelated to collusion but overlaps with the period of collusion. That could invalidate the "parallel trends" assumption that underlies our difference-in-difference strategy. Nevertheless, we address this concern empirically by modelling time trends in *Leverage* around the collusion period. Specifically, we estimate the following variation of Equation (1):

$$y_{it} = \alpha + \beta * Collusion_{it} + \sum \gamma_h * d_{ih} + \varphi_i + \mu_t + \varepsilon_{it}. \quad (2)$$

The subscript h indexes the years that immediately precede collusion years ($h \in \{-3, -2, -1\}$), years that immediately follow collusion years ($h \in \{1, 2, 3\}$), or years that are collusion years (h = 0 ("Col. yrs." in Figure 1 below) 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). For example, if a cartel started in (say) September 2001 and ended in May 2005, then $d_{i0^-} = 1$ in 2001 and zero otherwise; $d_{i0} = 1$ in 2002, 2003 and 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 years yields very similar results.

We plot the regression coefficients γ_h and their 95% confidence intervals in Figure 1. In Panel A, the coefficients are estimated using *Leverage* as the dependent variable. The results suggest that the decrease in *Leverage* is concentrated in the collusion years. In the years preceding or following collusion years, and in partial collusion years, the coefficients are not significantly different from zero. Overall, this suggests that the empirical pattern we document in Table 2, Panel A, is unlikely to be driven by time trends in the financial policies of firms that form cartels.

Figure 1

The goal of collusion is to increase the profits of the cartel firms. To confirm that profits do indeed increase during collusion periods, we re-estimate Equation (2) using *Profitability* (ROA) as the dependent variable, and we present the coefficients γ_h and their 95% confidence intervals in Panel B of Figure 1. *Profitability* declines in year $h = -1$, the year immediately preceding the formation of a cartel. This suggests that disappointing profits could be a motivation for the formation of a cartel. Profitability starts to increase during the first partial year of collusion ($h = 0^-$), but the increased profitability is more apparent during the full collusion years. Not surprisingly, *Profitability* drops substantially after the cartel is dissolved and competition resumes.[13]

While Figure 1 shows that leverage drops during collusion years (and not before), it does not show during *which* collusion years leverage falls. Understanding the exact timing of the drop in leverage is important, as the Commitment Hypothesis predicts that leverage should be reduced when the cartel is formed, in order to mitigate any risk of cartel members violating the agreement. We explore the timing of the leverage changes in more detail in Figure 2. Panel A shows the average leverage of cartel firms for the years preceding collusion and the first few years of collusion, with year 0 being the year in which collusion started. There is a clear drop in leverage during the first two collusion years, followed by a partial rebound. This is consistent with firms reducing their leverage aggressively at the beginning, when a cartel is formed, and with drops in this financial discipline as time proceeds, undermining the stability of the cartels and possibly leading to their break-up.[14] We find a similar pattern when running an extended version of equation (2), in which we differentiate the effects on leverage for the first three full collusion years from those of other collusion years. The coefficients are displayed in Figure A.3 in the appendix, which shows a strong reduction in leverage for the first two cartel years and a partial rebound afterwards.

[13] To confirm that profits change as expected when cartel firms collude, we compute the correlation between the profitability of the members of each cartel, and we display the averages of those correlations (for the three groups of firm-years) in Figure A1, in the Appendix. The average correlation is higher for collusion years, and it is lower (in fact, negative) for firm-years classified as post-collusion years.

[14] The drop in financial discipline is also observed if we define the end of collusion year as event year. See Figure A.2 in the appendix, which shows that leverage increases during the years prior to a cartel breakup.

In Panel B of Figure 2 we examine in more detail how the change in leverage during the first few collusion years happens, by displaying the evolution of both total debt and assets. The figure shows that the reduction in leverage is driven by a strong reduction in debt, which is larger than the reduction in assets observed for the same event years.

Figure 2

4.4 Specific Predictions of the Commitment Hypothesis: Triple-Differences Tests

We now present a battery of triple-differences tests aimed at sharpening identification. These tests exploit cross-sectional and time-series variation in our data to further check whether the decrease in leverage during collusion periods is consistent with the Commitment Hypothesis.

4.4.1 Commitment is Easier for Low-Leverage Firms

The Commitment Hypothesis predicts that all else equal, cartel firms with relatively higher leverage need more dramatic leverage reductions during collusion periods, since they have a stronger incentive to deviate from the collusive agreement. To explore this prediction, we compute each firm's leverage ratio during the first year of data availability in our sample, and then we split the sample in two: Firms with high initial leverage (above the sample median), and firms with low initial leverage (below the sample median). We then estimate Equation (1) separately for the two subsamples. The goal is to distinguish firms that had high leverage in pre-collusion years from firms had low leverage, and by using the first year of data availability we can include the control firms in this test (for non-cartel firms, there is no well-defined "pre-collusion" period). We use the first year of data availability, since financial leverage seems to be "sticky" (Lemmon et al. 2008). We have repeated the same estimations both using each firm's average leverage during its first three years in the sample, and using the average of all firm-years available for a given firm, with very similar results.

We present the results in Columns (1) and (2) of Table 3. The decrease in leverage is significant for high-leverage firms, but it is insignificant for low-leverage firms. The decrease in leverage by high-leverage firms is economically large, at about six percentage points. This reduction is twice as large as that reported in Table 2 for the pooled sample. This supports the Commitment Hypothesis: high-leverage firms must reduce their leverage in order to add credibility to the collusive agreements, while low-leverage firms do not need large decreases in leverage. The leverage levels remain below the pre-collusion levels in the years after collusion ends for firms with high initial leverage. As we show below, the cost of debt financing increases after cartels are dissolved, which explains this result.

Table 3

4.4.2 Commitment is Easier with Weaker Competition

The second test exploits variation in competitive pressure across firms. With stronger competitive pressure, it is likely harder to sustain collusive agreements: The up-front profits from deviating (undercutting the cartel prices) seem large, compared with competitive profits; and the future losses due to any "punishment" by rival firms (executing the threat that is part of the trigger strategies) seem less threatening, in particular if shareholders consider the benefits of limited liability in case the punishment phase turns out to have severe consequences. The Commitment Hypothesis thus predicts that when firms face stronger competitive pressure, a more significant reduction in leverage is required to sustain collusive agreements.

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). An alternative is to focus more directly on how firms can avoid direct competitive pressure, in particular by differentiating their products, through design, advertising, etc. For example, if the firms in an industry sell differentiated products, they face lower competitive pressure, and cartel agreements are likely more stable. A new measure that captures this concept is "product-market fluidity," developed in Hoberg et al. (2014). 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.

We use product market fluidity to measure the competitive pressure faced by firms in our sample.[15] The sample used in Hoberg et al. (2014) covers the years 1997 to 2008, 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 into observations with above-median fluidity (stronger competitive pressure) and below-median fluidity (weaker competitive pressure) and then estimate Equation (1) separately for the two subsamples.[16]

The results are presented in Columns (3) and (4) of Table 3. We find that firms facing stronger competitive pressure (high fluidity) reduce their leverage substantially when colluding, while firms facing weaker competitive pressure (low fluidity) do not. Notice also that leverage remains low (or even decreases further) after the cartels are dissolved. With a return to competition, it seems reasonable that firms facing stronger competitive pressure maintain low leverage ratios: Presumably, profits are lower, and a firm's competitive situation is likely tougher. This would be consistent with the Trade-off Theory.

4.4.3 Commitment Is Easier During Recessions

The third test exploits time-series variation in the incentive to deviate from a collusive agreement. The incentive to deviate is stronger if the up-front benefits are larger and the profits lost if collusion ends smaller. Rotemberg and Saloner (1986) predict that collusion is harder to sustain during economic booms, when larger profits are generated in the aggregate and thus there are larger potential gains from deviating. Analogously, they predict that it is easier to sustain collusion during recessions, when the profits from any deviation are smaller. We use this intuition as the basis for our next test: Cartel firms should decrease their leverage by more during boom years, and by less during recession years.

Because this test is based on time-series variation in the data, we can also test whether our earlier findings might be driven by cross-sectional unobserved heterogeneity. For instance, it could be argued that cartel firms are more sensitive to changes in the competitive environment than non-

[15] We thank Jerry Hoberg and Gordon Phillips for making their fluidity data available.

[16] The subsamples are not of equal size as we compute the median fluidity using the overall product-market fluidity sample, including firm-years with missing data needed in our regressions. We find similar results if we first eliminate observations with missing data and then compute the sample median of fluidity to split the sample.

cartel firms, and this is why we observe decreases in leverage for the collusion and post-collusion periods. And if highly levered cartels firms (of cartel firms in highly competitive environments) are somehow even more sensitive to these changes, this could also explain the results from the previous triple-difference analyses.

Specifically, in this test the post-collusion years are used as a placebo. There should not be any significant differences in leverage in the post-collusion period for cartel firms, across recession and non-recession years. If that is what we find, then it is unlikely that our results are simply due to cartel firms being more sensitive to economic shocks. Furthermore, if we find differences in leverage in the collusion period but not in the post-collusion period, this would add support to the Commitment Hypothesis.

To test whether cartel firms decrease their leverage less during recession years, we conduct a triple-difference test by interacting *Recession Year*, an indicator variable for recession years, with both *Collusion* and *Post-Collusion*. Recession years are identified using NBER data. The results are presented in Table 4, Column (1).

As before, we find that there is a significant decrease in leverage during collusion years. Importantly, macroeconomic conditions seem to have a significant impact on changes in leverage: The interaction term of *Recession Year* and *Collusion* is positive. These coefficients all have significant economic magnitudes. In sum, cartel firms decrease their leverage by less (or not at all) during recession years, consistent with the intuition gained from Rotemberg and Saloner (1986) that sustaining collusion during recessions may be easier.

The coefficient of the interaction between *Recession year* and *Post Collusion* is insignificant. This suggests that cartel firms do not change their leverage differently when colluding during recession years simply because they are more sensitive to shocks (or they are somehow different in unobserved dimensions to non-cartel firms).

Table 4

4.4.4 Commitment is Easier in the Absence of Leniency Laws

The fourth test exploits time-series and cross-sectional variation in the stability of cartels, based on differences in the rewards for disclosing cartel activities to the antitrust authorities. Leniency laws are regarded as a significant threat to collusive agreements (see OECD 2002; Miller 2009; Dong et al. 2016; and Dasgupta and Žaldokas 2016). These laws offer immunity to the first member of a cartel who confesses to the authorities. Other cartel members get at best partial relief if they also confess and cooperate.[17] This creates an incentive to be the first to betray a cartel to the authorities, putting strain on a cartel's ability to sustain a collusive agreement.

Leniency laws are a recent legal innovation, adopted in many countries starting effectively in 1993. They have not been introduced in all countries, and the date of their passage varies across countries, i.e., the U.S. firms in our sample faced leniency laws (and their destabilizing effect on cartels) in some countries but not others, and the intensity of this exposure (the number of countries with leniency laws) varied over time. Thus, there is both cross-sectional variation and time-series variation in the exposure of cartels to leniency law regimes.

The passage of leniency laws in various countries offers us an additional identification strategy. It is reasonable to assume that the passage of these laws is an exogenous event when considering a given cartel: An as-yet undiscovered cartel is unlikely to have an effect on antitrust laws. This staggered introduction of leniency laws is also used for the identification strategy in Dasgupta and Žaldokas (2016) and Dong et al. (2016). The Commitment Hypothesis predicts that if a country introduces or has leniency laws, it should be harder to sustain collusive agreements, and thus stronger reductions in leverage ratios are needed to sustain collusion. The more countries covered by the cartel that have leniency laws, the stronger this effect should be.

We restrict our sample to cartel firms since information about which firm operates in which country is available only for cartel firms. We create a firm-year dummy *High Leniency*, which takes a value of one if a cartel operates in three or more countries with leniency laws, and zero otherwise. (The lowest number of countries is zero, the highest number is seven.)

[17] For details of leniency laws in various countries, see Lex Mundi (2013).

In our triple-differences specification we interact the dummy *High Leniency* with *Collusion* and *Post Collusion*. The Commitment Hypothesis predicts that the coefficient for *Collusion* should be negative, and that the coefficient for the interaction term with *High Leniency* should also be negative. There is no clear prediction for the coefficient of the interaction term of *High Leniency* and *Post Collusion.* On the one hand, cartel firms shouldn't change their leverage differently in the post-collusion period, as the incentives to reduce leverage during collusion periods are no longer in place. On the other hand, if a cartel is dissolved in a rigorous antitrust environment (which is likely the case in countries that have passed leniency laws), this may have a particularly strong effect on the firms' reputation and their subsequent ability to raise funds (we explore the cost-of-debt channel in Section 4.6). Cartel firms in the post-collusion period may thus keep their leverage low in *High-Leniency* environments.

We present the results in Table 4, Column (2). As in the main specification, the coefficient for *Collusion* is negative. Importantly, cartel firms more exposed to leniency laws exhibit more pronounced decreases in leverage during collusion years: The interaction term of *High Leniency* and *Collusion* is both economically and statistically significant. The results are thus consistent with the predictions of the Commitment Hypothesis. The interaction term of *High Leniency* and *Post Collusion* is negative, but only marginally statistically significant (at the 10% level). This is to be expected given the conflicting effects described above.[18]

4.5 Endogeneity of the Collusion Periods

A possible concern about our results is that whether a cartel is active or dissolved in a given year may be endogenous. That is, the formation of a new cartel may be driven by factors that also affect leverage choices. While our triple-difference results mitigate concerns regarding omitted variables or reverse causality, we now more directly address endogeneity concerns using an instrumental variables approach.

We construct instruments based on environmental factors that facilitate or impede cartel formation. The decision to form a cartel and the ability to sustain it depend on the power of antitrust authorities, including the likelihood that a cartel is detected and the penalties that can be imposed.

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[18] Only three firms in our cartel sample broke up their cartels by taking advantage of leniency deals. Our main results are robust to excluding those firms.

Symeonidis (2003) finds evidence that the likelihood of collusion is industry-specific. Differences in transparency and in the structure of competition across industries suggest that collusion may be easier to sustain in some industries than in others (obtaining information from customers or whistleblowers may be harder in concentrated industries, for example). Furthermore, there are differences in the power and goals of antitrust authorities, even within countries: In the U.S., antitrust concerns may be raised by either the FTC or the DOJ, depending on the industry (in addition, state-level authorities may initiate antitrust proceedings).

We construct two industry-level proxies for the probability of prosecution of a cartel in any given year. First, for a given firm-year, we count the number of firms with the same 2-digit SIC code that colluded during the same year or the preceding two years, excluding firms with the same 4-digit SIC code as the cartel firm. We denote this measure by *Cartels Active*. Firms with the same 4-digit SIC code are excluded to avoid a simple mechanical correlation between the instrument and the potentially endogenous regressor. Second, we construct a similar measure counting firms whose cartels were dissolved during the same year or the preceding two years, again considering only firms with the same 2-digit SIC code but excluding firms with the same 4-digit SIC code. We denote this measure by *Cartels Dissolved*. It seems plausible that a large number of prosecutions in a broadly defined industry means that collusion is generally harder to sustain, i.e., that a higher value of *Cartels Dissolved* makes it less likely that a cartel is formed. Similarly, it is plausible that the presence of many cartels in a broadly defined industry means that collusion is easier to sustain in that group of industries, i.e., that a higher value of *Cartels Active* makes it more likely that a cartel is formed.

In practice, large firms are more likely to join international cartels than small firms (Connor 2014; Dong et al. 2016). This allows us to refine our proxies for exogenous variation in the probability of cartel detection and prosecution. We sort the firms by size and create size quartile dummies that we then interact with the two prosecution-probability proxies, *Cartels Active* and *Cartels Dissolved*.

Columns (1) and (2) of Table 5 present the results of the first stage of the IV estimation. The results show that collusion and post-collusion periods are strongly associated with our instruments for cartel formation and prosecution. The signs of the coefficients are as expected:

Cartels Active is positively associated with *Collusion* and negatively associated with *Post Collusion*; while *Cartels Dissolved* is negatively associated with *Collusion* and positively associated with *Post Collusion.* Columns (3) and (4) of Table 5 present the corresponding results allowing for interaction of the two instruments with size quartiles. We find that the two instruments have a stronger effect for larger firms. Given that this second specification better exploits the heterogeneity of the treatment from the instruments, we use this specification as our first stage when estimating the second stage.

Instrumental variables need to satisfy two conditions: They need to be relevant (as opposed to weak), and they need to satisfy the exclusion restriction. The results in Columns (1)-(4) of Table 5 show the instruments are clearly relevant (the F-tests are all above 100). The exclusion restriction, on the other hand, cannot be tested. However, we believe it is likely to be satisfied in our setting, for the following reasons.

First, in Panel B of Table 2, we consider the leverage of the direct competitors of the cartel firms (same 4-digit SIC code), which does not seem to change during collusion periods in their respective industries. Thus, it is unlikely that cartel activity in different, but related industries (captured by *Cartels Active* and *Cartels Dissolved*) has a direct effect on the leverage of cartel firms, outside the likelihood-of-forming-cartels channel. Second, we directly study the effect of *Collusion* on the leverage of firms in related industries (i.e., firms with a different 4-digit SIC code than the colluding firms, but the same 2-digit SIC code). In unreported results we find that the coefficient for *Collusion* ranges between 0.4% and 0.7%, and it is always statistically insignificant (with p-values higher than 30%). This also speaks in favor of the exclusion restriction, because collusion in different but related industries fails to explain the changes in leverage. Nevertheless, we acknowledge that it is nearly impossible to rule out all possible channels through which cartel activity in related industries can have a direct effect on the leverage of colluding firms.[19]

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[19] Notice that the Gormley and Matsa (2014) critique of instrumental variables using industry averages does not apply to our setting (see their section 2.3.4). They consider the case of using as an instrument for an endogenous regressor X_i the average of that same variable in the *same* industry excluding firm "i." They argue that such an instrument is likely inconsistent in the presence of industry fixed effects as such a strategy exploits variation which is perfectly related with the endogenous regressor X_i. Our proposed instrument, in contrast, exploits variation from related industries after controlling for potential time-invariant unobserved firm heterogeneity (i.e., firm fixed effects). Thus the variation we exploit is not mechanically related to the endogenous regressor: We simply instrument time-series

The results from the estimation of the second stage are shown in Column (5) of Table 5. As before, we find that collusion has a negative and significant effect on financial leverage. The magnitude of the coefficient on *Collusion* is larger than in the earlier OLS specifications. The IV regressions thus further confirm our earlier findings, that cartel firms reduce their leverage ratios during collusion periods, consistent with the Commitment Hypothesis. Overall, the evidence in Table 5 suggests that the potential endogeneity of the start and end dates of a cartel is not driving our results.

Table 5

4.6. Collusion and the Cost of Debt Financing

Another possible concern about our results is that the decreases in leverage could be due to contemporaneous increases in the cost of debt financing (rather than strategic considerations). For example, banks may fear a reputation loss if a cartel is detected and prosecuted, and they may also worry about a convicted cartel member's ability to service its debt. This assumes that a bank can detect a cartel while antitrust authorities cannot; in fact, this may further increase a bank's required return if it could later be accused of being a co-conspirator or facilitator of a cartel. On the other hand, if lenders are kept uninformed about the cartel's existence, any reputational and payment-risk considerations should arise only in post-collusion years.

In order to capture the possible information effect during collusion years, we focus our analysis on relationship lending, using 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.[20] 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 close to 20,000 firm-year observations (570 of them correspond to cartel firm-years).

variation of a firm's likelihood of colluding using the time-series variation in the likelihood of observing cartel initiations and dissolutions in related industries.

[20] For a detailed description of this database see, for example, Chava and Roberts (2008).

We focus on two characteristics of loans that are associated with debt financing being more "costly": The coupon offered to the lender, and whether the loan is secured. Debt financing is less costly to a borrower if the coupon rate is lower and if the loan is not secured (offering collateral reduces a firm's debt capacity). 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. The average *Spread* in our sample is 191 basis points. We define *Secured* as a dummy that takes a value of 1 if the loan is secured, and 0 otherwise; 73% of the loan-years in our sample are secured.

If cartel formation leads to reduced leverage through a cost-of-debt channel, then cartel formation must (1) be observable to a lender and (2) reduce a cartel member's credit quality. The cost of debt financing and the use of collateral should then increase during collusion years. In contrast, the Commitment Hypothesis predicts that the cost of debt financing and the use of collateral should not be affected during collusion years, or that they decrease, since cartel firms reduce their leverage below the level they would otherwise find optimal. For the post-collusion years, we expect that if there are reputation effects, then cartel firms face a higher cost of debt financing and offer collateral more frequently. However, they may choose to borrow less, which may in turn mitigate that effect.

Table 6 presents the results. When estimating either *Spread* or *Secured*, the coefficient for *Collusion* is insignificant. So there is no evidence either that lenders are informed about a borrower's membership in a cartel, or that this has an adverse effect on their cost of debt financing. In other words, the evidence is inconsistent with the reduction in leverage during collusion years operating through a cost-of-debt channel. However, there are significant effects in post-collusion years: The coefficients for *Post Collusion* are both significant and large. This suggests that convictions for membership in a cartel have significant negative effects that operate through a cost-of-debt channel. This finding may explain why in our leverage regressions above, the coefficient for *Post Collusion* tended to be negative: Cartel firms had lower leverage after cartels were dissolved because of an increased cost of debt financing.[21]

[21] In unreported analyses we study the maturity of the loans and find it is unrelated to collusion. The same holds for the maturity structure of all debt, based on *Compustat* data.

Table 6

5. Collusion and Other Financial Policies

In this section, we analyze a broader set of financial policies. This allows us to shed light on other dimensions of the strategic behavior of cartel firms, which in turn can further substantiate our inferences about the Commitment Hypothesis. Specifically, we ask how cartel firms use their increased profits, by studying the payout policies and cash holdings of cartel firms during collusion and post-collusion years. We also discuss changes in capital expenditure and R&D, because they represent important uses of cash. Furthermore, these additional results help us rule out possible alternative explanations for our findings (which we address in Section 6) based on links between profitability and leverage that are different from the Commitment Hypothesis or the Trade-off Theory.

5.1. Collusion and Payout Policy

As described in Table 1, cartel firms have higher total payout ratios than other firms. It is thus likely that the profit increases during collusion years are passed on to shareholders. It is furthermore likely that the cartel firms would use share repurchases instead of dividend increases, for several reasons. First, while cartel firms are more likely to pay dividends than other firms, the average dividend paid is relatively low (see Table 1, Panel B). The higher payout is achieved by having more active share repurchase programs than other firms. Second, the need to keep a cartel agreement (and its effects) secret may let cartel firms favor share repurchases, since open-market share repurchases are not easily observed. Third, if a cartel seems unstable and may break up in the near future, then cartel firms should regard the collusion profits as temporary and thus favor repurchases over dividend increases.

We define *Total Payout* as the sum of the dividends paid and the amounts of common and preferred stock repurchased, divided by the lagged value of total assets. We then estimate Equation (1) with *Total Payout* as a dependent variable (instead of *Leverage*). Following prior work on the determinants of payout policies (see, e.g., Fama and French (2001) or Hoberg et al. (2014)) we use the following control variables: *Lagged profitability*, *lagged sales*, *cash flow volatility*, and the *market to book ratio (MB).* Next, we distinguish dividends from repurchases. We define

Dividends and *Repurchases* as the cash dividends divided by the lagged value of total assets and the amounts of common and preferred stock repurchased divided by the lagged value of total assets, respectively.

Table 7 shows the results of estimating Equation (1) with *Total Payout* as a dependent variable. Column (1) shows the results without using controls while Column (2) shows the results using the controls listed above. The coefficients for *Collusion* are statistically and economically significant: Cartel firms increase their total payout by 0.7%-0.8% of their assets, a large increase considering that the average payout is 1.3% of total assets (see Table 1 above). Columns (3) and (4) of Table 7 show the corresponding results distinguishing dividends from repurchases. The increase in total payout is clearly driven by increases in share repurchases. Dividends decrease after a cartel is dissolved, which is not surprising if this reintroduces competition to the industry and thus reduces profits.

Table 7

We also examine whether the competitive environment affects the payout of cartel firms during collusion periods. Hoberg et al. (2014) show that the strength of the competitive pressure a firm faces negatively affects how much of its profits it pays out. This is consistent with the findings in Haushalter et al. (2007), that firms in more competitive environments hoard cash for precautionary reasons (to protect themselves against predation, or to exploit new opportunities), which means that the payout to shareholders is likely lower. If this intuition is valid, we expect that any increases in payout during collusion periods should be more pronounced in less competitive environments, where cartels are more stable and the risk of the cartel breaking up is likely lower.

We test this prediction by splitting the sample into subsamples with above-median and below-median *Product-Market Fluidity*, as we did in Section 4.4.2 above. We present the results in Table 8, Columns (1) and (2). We find that firms facing weaker competitive pressure (low fluidity) substantially increase their payout during collusion, whereas firms facing stronger competitive pressure (higher fluidity) do not. This is consistent with our predictions: Firms in more competitive environments face more competitive threats; their cartels are likely less stable, and if collusion breaks down, they likely face stronger competition. Thus, they are less likely to distribute cash to shareholders during collusion periods and instead retain cash for precautionary reasons.

Table 8

Overall, the evidence suggests that cartels change their payout during collusion years, and that they choose these changes strategically. This is consistent with the Commitment Hypothesis, in the sense that cartel firms strategically change their overall financial strategies during collusion periods.

5.2. Collusion and Cash Holdings

We now examine how collusion affects the cash holding decisions of cartel firms. A priori, it is not clear what effect collusion should have on cash balances. On the one hand, cash holdings should decrease during collusion, as collusion by design reduces or eliminates competitive pressure. Thus, there is less need to build up cash balances for precautionary reasons. On the other hand, collusion leads to higher profits, making it possible to quickly accumulate significant cash balances.

We estimate Equation (1), with *Cash Holdings* as a dependent variable. We present the results in Table 9. Column (1) presents the results without controls, while Column (2) present the results controlling for variables commonly used in the cash holdings literature (see, e.g., Bates et al. 2009). The results indicate that cash holdings decrease by 1-1.4% of assets, a significant decrease given average cash holdings (for cartel firms) of 7% (see Table 1, Panel B); but the coefficients are either marginally significant (p-value of 8.1%) or statistically insignificant (p-value of 11.7%). These results suggest that collusion may cause decreases in the cash holdings of cartel firms, but the results are not as strong as our earlier results. Given the ambiguous predictions, that is not surprising.

As discussed above, the accumulation of cash balances for precautionary reasons (Haushalter et al. 2007) should be less beneficial for firms that face weaker competitive pressure. We thus split the sample by *Product-Market Fluidity*, as before, and repeat the regressions for the two subsamples. The results are in Columns (3) and (4) of Table 9. There are no significant changes in cash holdings during collusion years or afterwards for firms that face strong competitive pressure (high fluidity). Importantly, the cash holdings of cartel firms facing weak competitive pressure (low fluidity) decrease during collusion years. This is consistent with the empirical findings in Haushalter et al. (2007) and with our payout results. These results further highlight that firms change their financial policies for strategic reasons during collusion periods, and thus that our findings are unlikely to be driven by other confounding factors.

Table 9

5.3. Collusion, Capital Expenditure and R&D Expense

We next study whether cartel firms change their investment decisions during collusion periods. Specifically, we study changes in capital expenditure and R&D, since they are important uses of cash, and since they can shed light on the inner workings of cartels. Table 10 presents the results. Columns (1) and (2) show that during collusion periods, capital expenditure (normalized by assets) does not change in a significant way. This is not surprising: The purpose of the cartels is to limit supply and raise prices, so investments in expanding capacity would be counter-productive since they threaten the stability of a cartel. (A firm with larger spare capacity would find it easier and more attractive to undercut its fellow cartel members' prices.)

Table 10

We find that R&D expenses (also normalized by assets) are higher during the collusion periods, see Columns (3) to (5) in Table 10. In the regression reported in Columns (3) and (4), we include all firm-years, substituting missing R&D values by zero. (As is common in the literature,[22] we interpret the absence of R&D expense in the data as indicating that R&D expense is zero or negligible.) In the regression reported in Column (5) we only use firms for which R&D data is originally available in at least one year. That R&D is higher during collusion periods is consistent with the model of Dasgupta and Stiglitz (1980). They predict that firms facing reduced competition invest more in R&D, since it is more likely that they can internalize the benefits from such investments in those environments.

[22] See e.g., Fee, Hadlock and Thomas (2006) and Kale and Shahrur (2007).

6. Alternative Explanations

6.1. Long-run risk

One possible concern with our findings is that the formation of a cartel may somehow increase the risk faced by a cartel firm. For example, one could argue that the threat of the cartel failing and competition resuming influences the decisions firms make during collusion periods. If cartel formation does indeed increase risk, then a natural response would be to reduce a cartel firm's financial leverage.

This alternative explanation, however, conflicts with several of our results. Firms do not seem to prepare for bad outcomes. They do not increase cash holdings but instead decrease them or leave them unchanged. Also inconsistent with the risk explanation is that cartel firms increase their payout during collusion periods, and that during recessions the reduction in leverage is less pronounced in collusion periods. Furthermore, we find that capital expenditure does not change, and that R&D expenses increase, both inconsistent with firms being exposed to more risk.

The timing of the reduction in leverage is also inconsistent with a risk explanation: Leverage falls strongly at the beginning of the collusion period, and according to a risk-based explanation it should fall later on, when the risk of cartel breakup is more imminent. Finally, our IV regressions, which are less affected by such potential unobserved confounding factors, also show a reduction in leverage. Overall, there is no reason to believe that the threat of possible resumption of competition affects financial leverage choices during collusion periods.

6.2. Strategic Debt

The presence of "strategic debt" can create a link between profitability and leverage, possibly leading to an alternative explanation of our finding that leverage is lower during collusion periods (when profits are particularly high). Brander and Lewis (1986) show that financial leverage can make firms aggressive in their product markets, due to a risk-shifting effect. They show that for an individual firm it may be optimal to lever up strategically, expecting other firms to cut back their output in response. The model then predicts that more profitable firms have higher leverage. But that prediction is inconsistent with the evidence we find.

However, if all firms in an industry can take on "strategic debt", the outcome may resemble the "bad equilibrium" in a prisoners' dilemma game, with all firms producing high outputs, and with correspondingly lower prices and profits. If so, the model predicts that high-leverage firms have *lower* profits. To the extent that collusion periods are somehow negatively associated with episodes of such "bad equilibria," that would be consistent with some of our findings. However, given that the predictions about the relation between leverage and profits are ambiguous (and thus consistent with *any* empirical findings), the model in Brander and Lewis (1986) cannot help explain our findings.

The results in Brander and Lewis (1986) are derived for a model in which revenues are risky (their intuition is that risk shifting drives the effects). One might thus argue that the results should be stronger for firms that make risky investments, for example R&D-intensive firms. Note that cartel firms have a lower R&D intensity than other firms (see Table 1, Panel B), so it is unlikely that risky investments are driving our results. To test this more thoroughly, we split our sample according to each firm's R&D intensity, and run separate regressions using leverage as dependent variable. We present the results in Table 11. We find that the coefficient for *Collusion* is insignificant for high-R&D cartel firms, while it is significant for low-R&D cartel firms. Thus, the possibility of making risky investments is not the driver of our findings.[23] That the evidence does not support Brander and Lewis (1986) is perhaps unsurprising since subsequent work has shown that the predictions of that model are not robust (see Showalter 1995 and Povel and Raith 2004) and there are doubts about the empirical relevance of risk shifting explanations (see Hernández-Lagos et al., 2016).

Table 11

6.3. Pecking Order

The Pecking Order Theory (Myers 1984) suggests that when it comes to financing new investment, firms prefer using retained cash to issuing more debt, and they prefer issuing more debt to issuing more equity. If profits increase for exogenous reasons, one would expect firms to reduce their leverage somewhat, leading to a negative relation between profitability and leverage.

[23] If we include only firms with R&D data available and then split the sample by R&D intensity, the coefficient for *Collusion* is insignificant for both subsamples.

However, higher profits may also lead to increased investment if a budget constraint has been relaxed. This may moderate or even reverse the reduction in leverage. If such an effect is present, we are controlling for it in our regressions, since they include lagged profits as controls. So it is unlikely that pecking-order effects are driving our results.

The key element in the Pecking Order Theory is asymmetric information: The less well informed investors are about a firm's prospects and investment opportunities, the more costly it is to use sources of funds farther down the pecking order. Frank and Goyal (2003) and Leary and Roberts (2010) show that information asymmetry proxies do a poor job at explaining debt and equity issuance as predicted by the Pecking Order Theory.[24] Nevertheless, we can ask what effect asymmetric information has on the leverage choices of cartel firms. Arguably, asymmetric information is more likely to be a concern for R&D-intensive firms. Thus, to the extent that the Pecking Order Theory is somewhat related to our findings on collusion and leverage, we should find that the reduction in leverage during collusion periods is stronger for R&D-intensive firms. However, we find the opposite (see Table 11), which further suggests that the Pecking Order Theory is unrelated to our findings.

The Pecking Order Theory does not make very specific predictions. For example, it is unclear whether high-leverage firms should decrease their leverage more strongly if profits increase. It is plausible, however, that high-leverage firms are financially constrained and issue significant amounts of debt to finance attractive investment opportunities. If more internal funds become available, then the primary effect may be that capital expenditure increases. However, we find that capital expenditure does not increase for highly levered firms (see Table A.1 in the Appendix).

In sum, the predictions of the Pecking Order Theory cannot explain our findings, or the predictions are ambiguous and thus are unrelated to our findings.

[24] Transaction costs could also generate a pecking order. But proxies for transaction costs also align poorly with the predictions of the pecking order theory. For additional evidence against the pecking order theory explaining the negative association between profits and leverage see Fama and French (2005) or Rauh and Sufi (2010).

6.4. Agency Problems

Managerial agency problems can affect the relation between profitability and leverage. Kovenock and Phillips (1995, 1997) argue that if agency problems cause overinvestment, and debt mitigates those agency problems (Jensen 1986), then high-debt firms should be more efficient. In other words, there may be a positive relation between profitability and financial leverage. That cannot explain our findings, however, since we find a *negative* relation between collusion (high profits) and leverage.

The free cash flow problem analyzed in Jensen (1986) is more likely to be a concern if governance is weak, including pressure to make firms efficient coming from product market competition. In other words, if firms face stronger competitive threats, the free cash flow problem is not likely to be a concern. We find stronger results for firms with higher product market fluidity, so (again) the free cash flow problem cannot explain our findings.

In unreported tests we used sample splits to analyze whether changes in standard measures of corporate governance (the G-Index (Gompers et al. 2003); the percentage of independent directors; and the presence of a staggered board) affect leverage during collusion years, or whether there are changes in these variables during collusion years. We found no significant effects in any of those regressions, which suggests that agency problems cannot explain our results.

6.5. Growth Opportunities

As modeled in Strebulaev (2007), a negative relation between profitability and leverage may arise in a dynamic model: Positive profitability shocks increase the value of a firm (by increasing the present value of its growth options) and, if the firm does not continuously rebalance its capital structure, this reduces the firm's leverage ratio. Note that this only affects the firm's market value leverage ratio, but not its book value leverage ratio. In our tests, we measure leverage using book values, precisely to avoid mechanical effects caused by changes in expectations and corresponding changes in valuations.

How growth opportunities affect *book value* leverage is analyzed in Barclay et al. (2006). They show that investments in long-term growth options have a negative debt capacity, since the threat of underinvestment caused by debt overhang increases the cost of using debt, and since it is less likely that the free cash flow problem (Jensen 1986) causes efficiency losses (and so the benefits of having debt are reduced). Consequently, firms with more growth options should have lower book leverage. This could potentially explain our findings if growth options increase during collusion periods. However, this scenario is unlikely since leverage stays low after collusion ends (when growth options likely dissipate) and since the reduction in leverage is statistically significant only for firms with low R&D (see Table 11 above), which are arguably the firms with fewer growth options available.[25]

6.6. Dynamic Trade-off Theory

An alternative to the static Trade-off theory is the dynamic Trade-off Theory model proposed by Hennessy and Whited (2005). They study a firm's decisions over multiple periods: Given the realized cash flow, it chooses its investment, dividends, share repurchases, and changes in debt outstanding. This model can generate a negative association between profitability and leverage and thus could potentially explain the reduction in leverage we find during collusion periods. However, in their model, leverage is path dependent and exhibits hysteresis, i.e., firms with larger debt in place reduce their leverage by less if they experience a positive profit shock, since they have a larger financing gap. This prediction conflicts with our findings in Table 3, that firms with high initial leverage display *larger* reductions in leverage during collusion years. The Hennessy and Whited (2005) model also does not explain why firms reduce their leverage by more during boom periods and by less during recessions (see Table 4), and why the reduction in leverage is stronger for firms that face larger competitive threats (firms with higher product market fluidity, see Table 3).

Frank and Goyal (2015) also consider a dynamic model of leverage choices, incorporating the costs of changing debt and equity levels. They conclude that the relation between profitability and leverage may be negative. They find that profit increases lead to both debt increases and equity increases, and that leverage decreases because the equity increases are larger. However, this

[25] Furthermore, investments in R&D often require follow-up investments in future years, thus tying up future cash flows and reducing a firm's debt capacity. For a dynamic model of financing and investment, see Lambrecht and Myers (2016).

cannot explain our findings: We show in Figure 2 (Panel B) that the debt levels of cartel firms *decrease* when collusion starts, i.e., when their profits increase.

7. Conclusions

Cartels are widespread, even in developed market economies, and their economic footprint is surprisingly large. As we show in this paper, cartel firms change their financial policies during collusion periods. In particular, they reduce their leverage ratios in a significant way. Our findings are consistent with the idea developed in Maksimovic (1988) that reductions in financial leverage help make cartels more stable.

We obtain our findings by analyzing the most comprehensive data set on cartel activity that is currently available. We perform a series of robustness tests to address possible concerns about our empirical setup and to strengthen our inferences. We also test a number of possible alternative explanations, none of which is consistent with our findings.

Our findings have several implications. First, research that analyzes the linkages between financial decisions and product market strategies needs to expand the definition of what constitutes a product market strategy. It is important to go beyond the usual choices of prices, output, advertising, quality, etc., and to incorporate active anti-competitive behavior, either in the form of formal cartel agreements, or as tacit collusion. We show that cartel firms change their financial decisions in a way that makes it easier to support collusion, an effect that a focus on narrower product market strategies could not explain.

Second, the reduction in leverage during collusion periods goes against the intuition of the Trade-off Theory. Specifically, a negative relation between profitability and leverage is inconsistent with the Trade-off theory, which predicts a positive relation. A negative relation is the standard finding in the literature, but researchers have struggled to explain that. Our findings show that it is important to study specific shocks to profitability when analyzing why empiricists tend to find a negative relation between profitability and leverage.

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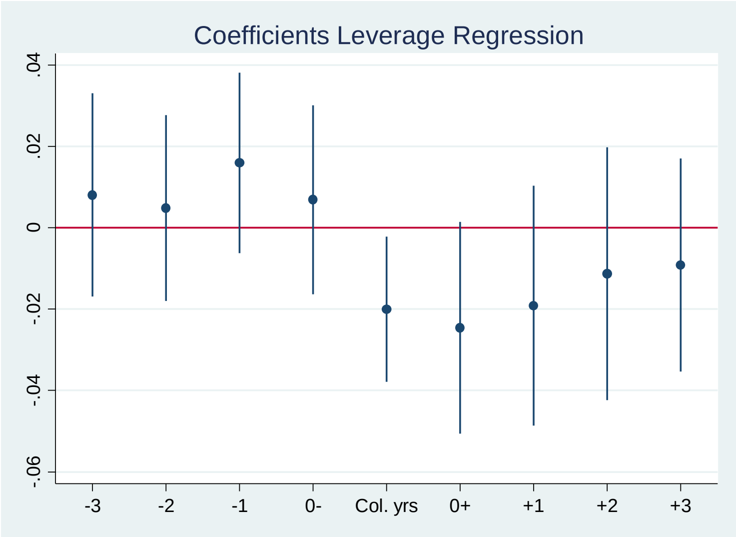
Figure 1

Panels A and B plot the coefficients, and 95% confidence intervals, of the following regression:

$$y_{it} = \alpha + \beta * Collusion_{it} + \sum \gamma_h * d_{ih} + \varphi_i + \mu_t + \varepsilon_{it},$$

The subscript h indexes the years that immediately precede collusion years ($h \in \{-3, -2, -1\}$), years that immediately follow collusion years ($h \in \{1, 2, 3\}$), or years that are collusion years (h = 0 (Col. yrs) 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. Panel A plots regression coefficients γ_h using book leverage as the dependent variable, while Panel B plots regression coefficients γ_h using ROA as the dependent variable.

Panel A: Coefficients γ_h when y = *Leverage*



Panel B: Coefficients γ_h when y = *ROA*

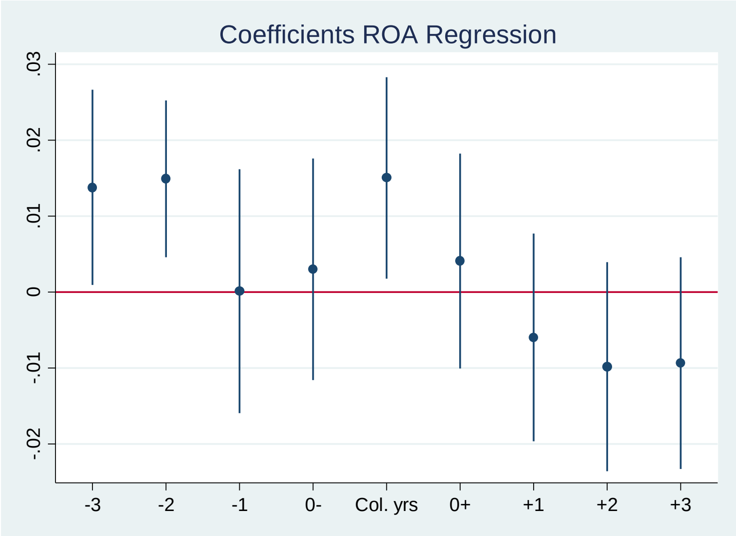
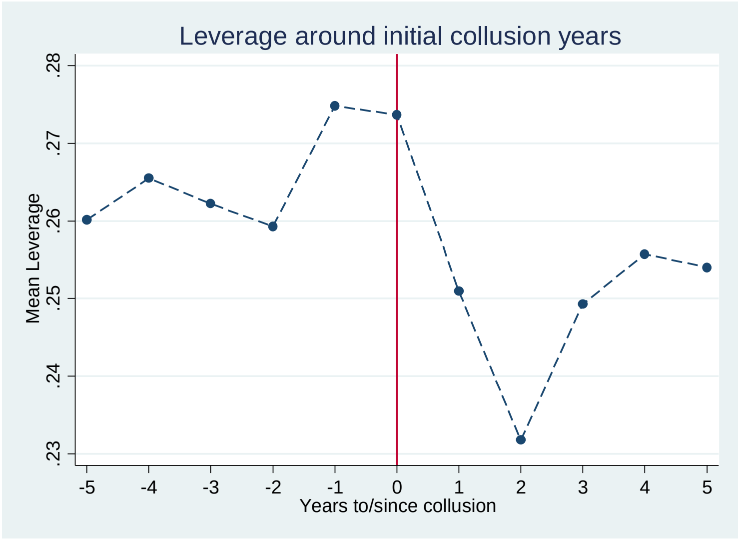


Figure 2

Panel A of this figure plots the average leverage ratios of cartel firms, separately for each year immediately before the start of a collusion period, and for each of the first few years of a collusion period. There is an apparent drop in leverage during the first two years of the colluding periods. Panel B plots the numerator and denominator of the leverage ratio, debt and total assets (with different scales), for the same years, showing that while both decrease during collusion periods, the initial drop in leverage is driven by a more significant drop in average debt.

Panel A: Leverage around Initial Collusion Years



Panel B: Debt and Total Assets around Initial Collusion Years

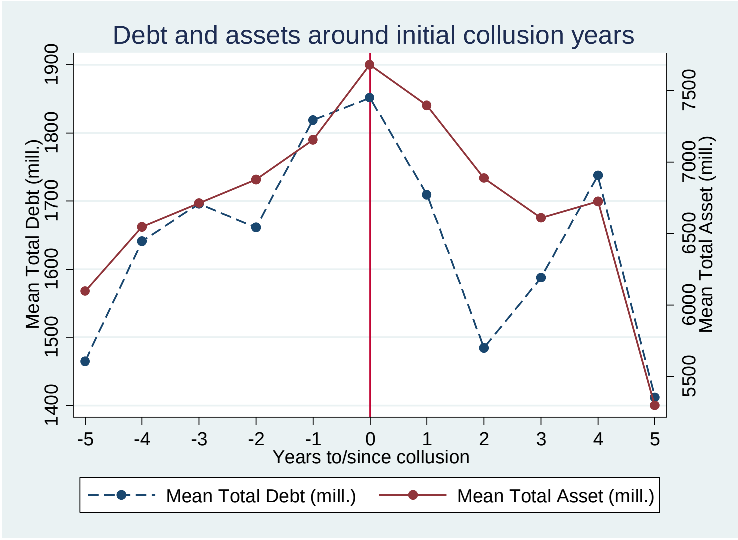


Table 1. Descriptive Statistics

This table reports descriptive statistics for our sample of firms prosecuted for cartel participation, and for the control firms. Panel A presents detailed descriptive statistics for all cartel and control firms (pooled). Panel B reports differences in selected characteristics of the cartel and the control firms. See Appendix A for variable definitions. Differences significant at: *10%, **5% and ***1%.

Panel A: Summary Statistics

Variable	*Mean*	*p10*	*p90*	*sd*	*N*
Leverage	0.33	0.00	0.66	0.47	129,617
Total Payout	0.013	0.000	0.031	0.04	123,541
Dividend	0.00049	0.0000	0.0002	0.00	123,541
Dividend Payer	0.39	0.00	1.0000	0.49	129,617
Equity Repurchases	0.01	0.00	0.03	0.04	123,541
Profitability	0.03	-0.29	0.22	0.23	129,617
Negative Prof	0.25	0.00	1.00	0.43	129,617
Tangibility	0.29	0.03	0.71	0.25	129,617
Sales (million)	1,025	2	2,386	2,833	129,617
Cash Flow Volatility	0.081	0.007	0.18	0.14	124,433
Cash	0.12	0.00	0.34	0.17	123,210
Assets (million)	1,436	3	3,076	4,389	129,617
MB	4.14	0.84	4.37	39.60	107,926
R&D	0.12	0.00	0.25	0.51	68,190
Net Working Capital	-0.09	-0.14	0.59	7.62	125,449

Panel B: Univariate Mean Analysis, Cartel vs. Non-cartel Firms

Variable	*Cartel*	*Non-Cartel*	*Difference (Cartel – Non-Cartel)*
Leverage	0.27	0.33	-0.06***
Total Payout	0.023	0.013	0.011***
Dividend	0.00002	0.0005	-0.00047***
Dividend Payer	0.73	0.38	-0.35***
Equity Repurchases	0.02	0.01	0.01***
Profitability	0.14	0.02	0.12***
Negative Prof	0.03	0.25	-0,22***
Tangibility	0.34	0.29	0.05***
Sales (million)	6,223	967	5,256***
Cash Flow Volatility	0.026	0.082	-0.056***
Cash	0.07	0.13	-0.06***
Assets (million)	7,663	1,367	6,296***
MB	2.02	4.17	-2.15**
R&D	0.03	0.12	-0.09***
Net Working Capital	0.16	-0.09	0.25

Table 2. Collusion and Capital Structure

This table presents the results of analyzing the association between collusion and leverage. The dependent variable is *Leverage*. Panel A presents results using cartel firms: 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. Panel B presents the results using only firms from industries in which there was collusion, but the firms were not cartel members (i.e., they were the cartel firms' non-colluding rivals). *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 Controls include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

Panel A: Cartel Firms and Control Firms

	Dependent variable: *Leverage*			
	Cartel and control firms		*Cartel firms only*	
Independent Variables:	(1)	(2)	(3)	(4)
Collusion	-0.0277**	-0.0283**	-0.0265**	-0.0278**
	(0.0115)	(0.0112)	(0.0131)	(0.0126)
Post Collusion	-0.0249	-0.0260	-0.0277	-0.0335
	(0.0199)	(0.0198)	(0.0234)	(0.0229)
Controls	No	Yes	No	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
SIC Cluster	Yes	Yes	Yes	Yes
R-squared (within)	0.0036	0.0280	0.0732	0.1059
N	129,617	129,616	1,429	1,429

Panel B: Non-Cartel Firms from Industries with Cartels

	Dependent variable: *Leverage*	
Independent Variables:	(1)	(2)
Collusion	0.0100	0.0072
	(0.0086)	(0.0086)
Post Collusion	0.0211**	0.0154
	(0.0104)	(0.0108)
Controls	No	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
SIC Cluster	Yes	Yes
R-squared (within)	0.0191	0.0523
N	16,450	15,523

Table 3. Collusion and Capital Structure: Sample Splits

This table presents results of analyzing the association between collusion and leverage, using sample splits. The dependent variable is *Leverage*. *Product-Market Fluidity* is a text-based measure of product market competitiveness formulated by Hoberg et al. (2014) that uses product descriptions in SEC Form 10-K filings. 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 *Product-Market Fluidity*. *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 Controls include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent variable: *Leverage*			
	Split by Initial Leverage		*Split by Product-Market Fluidity*	
	Below Median	*Above Median*	*Below Median*	*Above Median*
Independent Variables:	(1)	(2)	(3)	(4)
Collusion	-0.0128	-0.0561***	-0.0142	-0.0486**
	(0.0165)	(0.0158)	(0.0146)	(0.0207)
Post Collusion	-0.0319	-0.0483*	0.0103	-0.0774**
	(0.0291)	(0.0277)	(0.0224)	(0.0299)
Controls	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
SIC Cluster	Yes	Yes	Yes	Yes
R-squared (within)	0.0568	0.0253	0.0328	0.0338
N	64,628	64,720	43,725	33,033

Table 4. Collusion and Capital Structure: Triple Differences

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. *High Leniency* is a dummy variable that takes a value of 1 if in a given year a firm is operating a cartel that covers three or more countries with leniency laws. Column (1) presents the estimation results for the triple-difference analysis that exploits the variation in recession versus non-recession years. Column (2) presents the estimation results for the triple-difference analysis that exploits the variation in a firm's exposure to leniency laws. *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 Controls include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). We do not report the coefficient for *Recession*, since that dummy variable is collinear with the year fixed effects. Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent variable: *Leverage*	
	Recession vs Non-recession	*High vs low exposure to leniency laws*
Independent Variables:	(1)	(2)
Collusion	-0.0385***	-0.0280**
	(0.0107)	(0.0129)
Post Collusion	-0.0236	-0.0308
	(0.0201)	(0.0233)
*Recession Year*Collusion*	0.0392***	
	(0.0112)	
*Recession Year*Post Collusion*	-0.0079	
	(0.0119)	
High Leniency		0.0408
		(0.0360)
*High Leniency*Collusion*		-0.0583**
		(0.0263)
*High Leniency*Post Collusion*		-0.0831*
		(0.0448)
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
SIC Cluster	Yes	Yes
R-squared (within)	0.0280	0.1109
N	129,616	1,429

Table 5. Collusion and Capital Structure: Instrumental Variables

This table presents results of analyzing the impact of collusion on leverage, using an instrumental variables approach. In the first stage, the dependent variables are *Collusion* (takes a value of 1 for cartel firms during the cartel 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 instrumental variables for *Collusion* and *Post Collusion* are *Cartels Active* (using the number of firms colluding in years *t*, *t-1* and *t-2*, in the same 2-digit SIC code, but different 4-digit SIC code as a given firm) and *Cartels Dissolved* (using the number of firms ending a collusive agreement in years *t*, *t-1* and *t-2*, in the same 2-digit SIC code, but different 4-digit SIC code as a given firm). Columns (1) and (2) present the relation between the instruments and *Collusion* and *Post Collusion*. Additionally, we classify firms according to their assets in four quartile sizes, and interact quartile dummies with the instruments. Columns (3) and (4) show the results of this alternative first-stage specification. In the second stage, the dependent variable is *Leverage*. Column (5) presents the results of the second-stage estimation, using the specification in Columns (3) and (4) as the first-stage regressions. The *Controls* include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	First Stage				*Second Stage*
	Instruments: *Cartels Active and Cartels Dissolved*		Instruments: *Cartels Active and Cartels Dissolved Interacted with Firm Size Quartiles*		*Using all Instruments*
	Dep. Var: *Collusion*	Dep. Var: *Post Collusion*	Dep. Var: *Collusion*	Dep. Var: *Post Collusion*	Dep. Var: *Leverage*
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Collusion					-0.3665*
					(0.1978)
Post Collusion					-0.2647
					(0.1880)
Cartels Active	0.0142***	-0.0107***	0.0014*	0.0006	
	(0.0048)	(0.0040)	(0.0007)	(0.0011)	
*Cartels Active * Size_q2*			0.0026**	-0.0019***	
			(0.0010)	(0.0007)	
*Cartels Active * Size_q3*			0.0099**	-0.0120***	
			(0.0040)	(0.0043)	
*Cartels Active * Size_q4*			0.0457***	-0.0414***	
			(0.0135)	(0.0112)	

Cartels Dissolved	-0.0063**	0.0137***	0.0001	-0.0020***	
	(0.0030)	(0.0050)	(0.0005)	(0.0008)	
*Cartels Dissolved * Size_q2*			-0.0028*	0.0026*	
			(0.0015)	(0.0014)	
*Cartels Dissolved * Size_q3*			-0.0053	0.0119***	
			(0.0038)	(0.0045)	
*Cartels Dissolved * Size_q4*			-0.0263**	0.0560***	
			(0.0127)	(0.0178)	
Controls	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
SIC Cluster	Yes	Yes	Yes	Yes	Yes
N	115,658		115,658		115,658

Table 6. Collusion and Loan Contracting

This table presents results of analyzing the association between collusion and the terms of loan contracting. In Column (1) the dependent variable *Spread* 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. In Column (2), the dependent variable *Secured* is a dummy variable that takes a value of 1 if the (new or renewed) loan is secured, 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 Controls include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

Independent Variables:	Dep. Var: *Spread* (1)	Dep. Var: *Secured* (2)
Collusion	-3.2232	-0.0071
	(9.6709)	(0.0543)
Post Collusion	25.5873**	0.1452**
	(12.7741)	(0.0650)
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
SIC Cluster	Yes	Yes
R-squared (within)	0.2457	0.0395
N	19,653	14,679

Table 7. Collusion and Payout Policy

This table presents results of analyzing the association between collusion and payout policy. In Columns (1) and (2) the dependent variable *Total Payout* is the sum of dividends paid and the amounts of common and preferred stock repurchased, divided by the lagged value of total assets. Column (3) presents the estimation results when using dividends divided by lagged assets as the dependent variable (*Dividends*), while Column (4) uses the amounts of common and preferred stock repurchased divided by lagged assets as the dependent variable (*Repurchases*). *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 Controls in Columns (2)-(4) include *Profitability*, *Cash Flow Volatility*, *MB*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

			Breakdown of *Total Payout*	
	Dep. Var: *Total Payout*		Dep. Var: *Dividends*	Dep. Var: *Repurchases*
Independent Variables:	(1)	(2)	(3)	(4)
Collusion	0.0078***	0.0072**	-0.0000	0.0072**
	(0.0028)	(0.0028)	(0.0000)	(0.0028)
Post Collusion	0.0024	0.0029	-0.0001*	0.0030
	(0.0032)	(0.0034)	(0.0000)	(0.0034)
Controls	No	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
SIC Cluster	Yes	Yes	Yes	Yes
R-squared (within)	0.0105	0.0147	0.0125	0.0150
N	123,191	105,861	105,861	105,861

Table 8. Collusion and Payout Policy: Sample Split

This table presents results of analyzing the association between collusion and corporate payout policies, using a sample split. The dependent variable *Total Payout* is the sum of dividends paid and the amounts of common and preferred stock repurchased, divided by the lagged value of total assets. *Product-Market Fluidity* is a text-based measure of product market competitiveness formulated by Hoberg et al. (2014) that uses product descriptions in SEC Form 10-K filings. Columns (1) and (2) present the results of splitting the sample into firms with below-median and above-median *Product-Market Fluidity*. *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 Controls include *Profitability*, *Cash Flow Volatility*, *MB*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent Variable: *Total Payout*	
	Split by Product-Market Fluidity	
	Below Median	*Above Median*
Independent Variables:	(1)	(2)
Collusion	0.0080**	0.0035
	(0.0034)	(0.0045)
Post Collusion	0.0022	0.0039
	(0.0039)	(0.0064)
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
SIC Cluster	Yes	Yes
R-squared (within)	0.0258	0.0165
N	46,391	31,246

Table 9. Collusion and Cash Holdings

This table presents results of analyzing the association between collusion and cash holdings. The dependent variable *Cash* is cash and equivalents divided by total assets. Columns (1) and (2) show the coefficient for *Collusion* and *Post Collusion* when running regressions without and with controls. Columns (3) and (4) 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 product market competitiveness formulated by Hoberg et al. (2014) that uses product descriptions in SEC Form 10-K filings. *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 Controls included in Columns (2)-(4) are *MB*, *RD*, *Net Working Capital*, *Negative Prof*, *Dividend Payer*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent Variable: *Cash*			
			Split by Product-Market Fluidity	
	Pooled observations		*Below Median*	*Above Median*
Independent Variables:	(1)	(2)	(3)	(4)
Collusion	-0.0101*	-0.0138	-0.0174*	0.0064
	(0.0058)	(0.0088)	(0.0095)	(0.0211)
Post Collusion	0.0043	0.0029	0.0008	-0.0001
	(0.0088)	(0.0094)	(0.0113)	(0.0158)
Controls	No	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
SIC Cluster	Yes	Yes	Yes	Yes
R-squared (within)	0.0089	0.0604	0.0789	0.0786
N	123,210	56,481	24,876	18,568

Table 10. Collusion and Investment

This table presents the results of analyzing the association between collusion and investment, either in R&D or capital expenditure. The dependent variable in Columns (1) and (2) is capital expenditure divided by book value of assets; the dependent variable in Columns (3) to (5) is R&D divided by book value of assets. In Columns (3) and (4) we replace missing R&D values by zero, while Column (5) uses only firms for which R&D data is available in at least one year. *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 controls in Columns (2), (4) and (5) include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent Variable: *Capex*		Dependent Variable: *R&D*		Dep. Var: *R&D, if positive*
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Collusion	-0.0053	0.0039	0.0030**	0.0028**	0.0054***
	(0.0151)	(0.0143)	(0.0013)	(0.0013)	(0.0022)
Post Collusion	0.0138	0.0059	0.0017	0.0007	0.0031
	(0.0221)	(0.0205)	(0.0021)	(0.0020)	(0.0032)
Controls	No	Yes	No	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
SIC Cluster	Yes	Yes	Yes	Yes	Yes
R-squared (within)	0.0462	0.1686	0.0052	0.0353	0.0529
N	115,724	115,723	129,617	129,616	63,543

Table 11. Collusion and R&D Intensity

This table presents the results of analyzing the association between collusion and *Leverage*, using sample splits that distinguish firms for which R&D data is not available (Column (1)) from firms for which R&D data is available in at least one year (Column (2)). *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 Controls include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent variable: *Leverage*	
	Split by R&D Expense	
	No R&D	*Positive R&D*
Independent Variables:	(1)	(2)
Collusion	-0.0482***	-0.0161
	(0.0161)	(0.0142)
Post Collusion	-0.0475	-0.0148
	(0.0304)	(0.0235)
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
SIC Cluster	Yes	Yes
R-squared (within)	0.0258	0.0319
N	66073	63543

Appendix

Variable Definitions

Firm Characteristics:

Leverage	Short-term debt plus long-term debt divided by book value of assets
Total Payout	Dividends paid plus common and preferred stock purchased divided by lagged book value of assets
Dividends	Cash dividends divided by lagged book value of assets
Dividend Payer	Indicator variable that takes a value of one if *Dividends* is greater than zero, and zero otherwise
Repurchases	Value of common and preferred stock purchased divided by lagged book value of assets
Profitability	Operating income before depreciation divided by book value of assets (ROA)
Negative Prof	Indicator variable that takes a value of one if *Profitability* is less than zero, and zero otherwise
Tangibility	Net property, plant, and equipment divided by book value of assets
Sales	Value of total sales measured in millions of U.S. dollars
Cash Flow Volatility	Standard deviation of *Profitability* over the prior 3-year period
Cash	Cash and equivalents divided by book value of assets
Assets	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
RD	Research and development expenses divided by book value of assets
Net Working Capital	Difference between current assets and current liabilities divided by book value of assets

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

Sample Splits:

Collusion	Indicator variable that takes the value of 1 for cartel firms during the cartel 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
Product-Market Fluidity	Text-based measure of product market competitive pressure formulated by Hoberg et al. (2014), using product descriptions in 10-K filings
Recession Year	Indicator variable that takes a value of 1 in a recession year, and 0 otherwise, classified using the NBER recession year list
High Leniency	Indicator variable that takes a value of 1 if in a given year a cartel firm is operating in three or more countries with leniency laws in place, and 0 otherwise

Instrumental Variables:

Cartels Active	Natural logarithm of one plus the number of firms colluding in years *t*, *t-1* and *t-2*, in the same 2-digit SIC code, but different 4-digit SIC code as a given firm
Cartels Dissolved	Natural logarithm of one plus the number of firms ending a collusive agreement in years *t*, *t-1* and *t-2*, in the same 2-digit SIC code, but different 4-digit SIC code as a given firm

Figure A.1

This figure plots the mean of the correlation coefficient of the profitability of all members of a cartel, averaged over all cartels, distinguishing firm-years before collusion, during collusion, and after collusion.

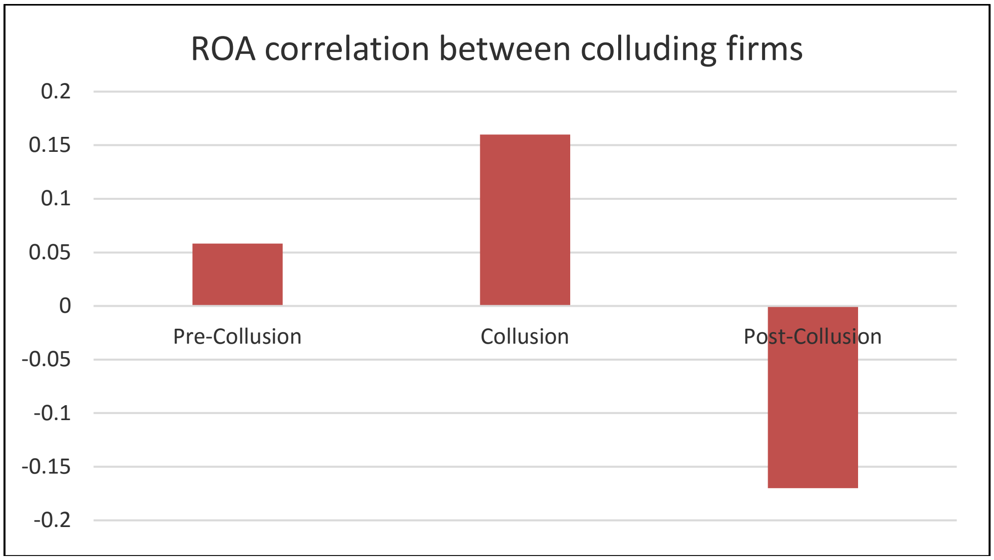
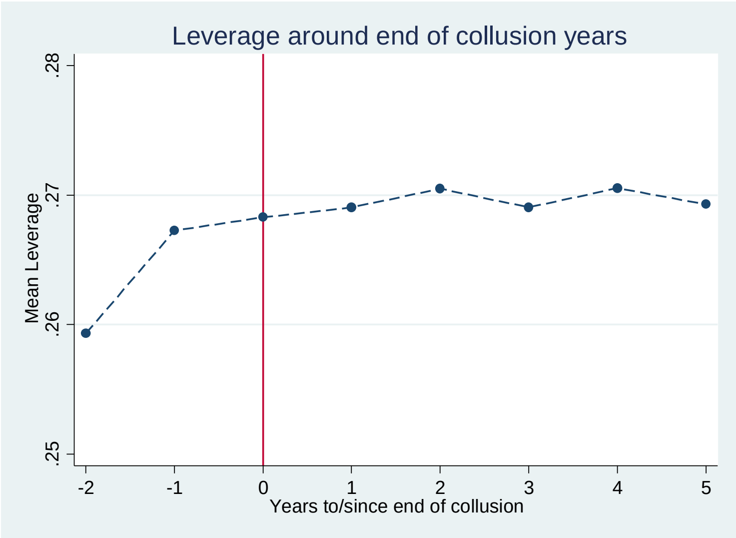


Figure A.2

This figure replicates Figure 2, but now using the year in which collusion ended as the event year, instead of the year in which collusion started. Panel A plots the average leverage, starting 2 years before a cartel breakup and ending 5 years after. Panel B plots the average debt and total assets for the same event years.

Panel A: Leverage around End of Collusion Year



Panel B: Debt and Total Assets around End of Collusion Year

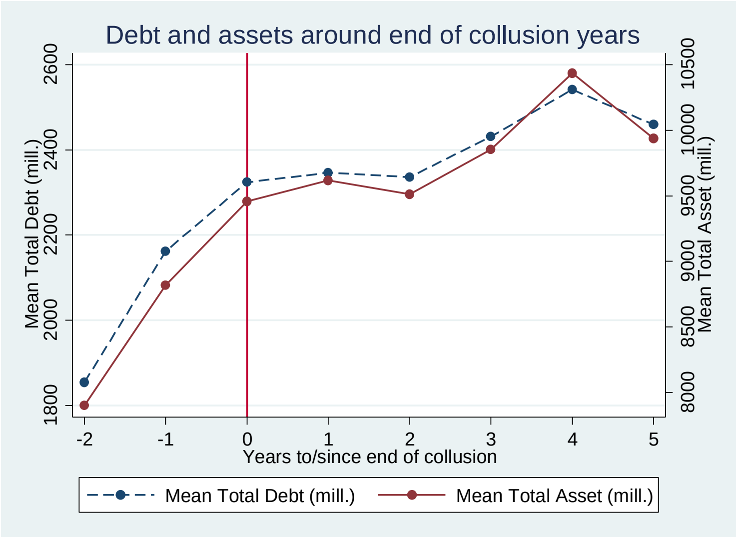


Figure A.3

This figure plots the coefficients, and 95% confidence intervals, of the following regression:

$$y_{it} = \alpha + \beta * Collusion_{it} + \sum \gamma_h * d_{ih} + \varphi_i + \mu_t + \varepsilon_{it},$$

The subscript h indexes the years that immediately precede collusion years ($h \in \{-3, -2, -1\}$), years that immediately follow collusion years ($h \in \{1, 2, 3\}$), 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 figure plots regression coefficients γ_h using book leverage as the dependent variable. The difference, compared with Figure 1, Panel A, is that here we decompose the full-year collusion years in the first full three colluding years, and the other colluding years.

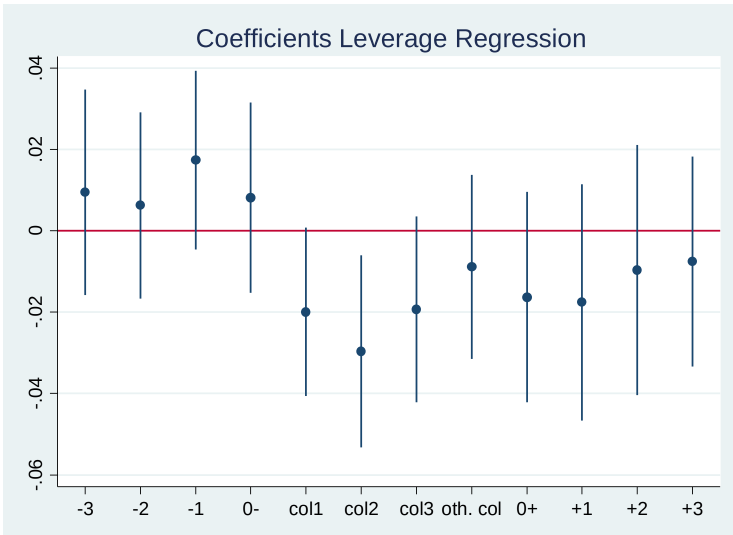


Table A.1. Capital Expenditure and Initial leverage

This table presents results of analyzing the association between collusion and capital expenditure, using sample splits. The dependent variable *Capital Expenditure* is capital expenditure divided by book value of assets. 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. *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 Controls include *Profitability*, *Tangibility*, *Cash Flow Volatility*, and the logarithm of *Sales* (see Appendix A for definitions). Standard errors (in parentheses) are adjusted for heteroscedasticity and clustered at the industry level. Significant at: *10%, **5% and ***1%.

	Dependent variable: Capital Expenditure	
	Below Median Leverage (1)	Above Median Leverage (2)
Collusion	0.0286	-0.0160
	(0.0256)	(0.0197)
Post Collusion	0.0245	-0.0032
	(0.0365)	(0.0206)
Controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
SIC Cluster	Yes	Yes
R-squared (within)	0.1915	0.1459
N	57,811	57,667