Smokescreen:
How managers behave when they have something to hide

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Abstract

We study financial reporting and corporate governance in 216 U.S. companies accused of price fixing by antitrust authorities. We document a range of strategies used by these firms when reporting financial results, including frequent earnings smoothing, segment reclassification, and restatements. In corporate governance, cartel firms favor outside directors who are likely to be inattentive monitors due to their status as foreign or "busy." When directors resign, they are often not replaced, and new auditors are rarely engaged. Cartel managers exercise their stock options faster than managers of other firms. While our results are based only upon firms engaged in price fixing, we expect that they should apply generally to all companies in which managers seek to conceal poor performance or personal wrongdoing.

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1. Introduction

In most financial frauds, a company tries to make its performance appear better than it really is, hoping to achieve a valuation that wouldn't be supported by its true cash flows. This paper looks at the opposite case. We investigate the disclosure and governance practices of more than 200 U.S. companies accused by government authorities of participating in price-fixing cartels. These firms earn strong cash flows, and continuation of their schemes requires obfuscation of the windfalls from regulators, analysts, customers, and at times, even their own boards of directors.

Connor and Helmers (2007) define a cartel as "an association of legally independent firms that aims to raise their joint profits through explicit agreements. Hard-core cartels aim to control prices or restrict supply (or both)." Decisions to join cartels are typically taken by a firm's very top managers and then implemented by the intermediate management (Harrington, 2006). The role of top management suggests that corporate governance may affect formation and continuation of a cartel. For example, cartels may occur more readily in firms with a high concentration of power at the top level, a weak or inattentive board of directors, or strong pay-

for-performance incentives (Spagnolo, 2005).[1] In addition, financial reporting strategies that cause signal-jamming, such as earnings smoothing and suspicious accrual patterns, might be expected to occur frequently with cartels. Cartelists have an ongoing need to deter both cheating and the entry into the industry of new firms (Levenstein and Suslow, 2006). If a cartel member deviates from a collusive agreement, its sudden jump in earnings might be detected by co-conspirators, who could start a price war that could destroy the cartel. Therefore, we would expect cartels to use financial reporting strategies that obscure year-to-year swings in profitability.

This paper investigates how cartel firms attempt to cover up their conspiracies. We use an empirical framework similar to that of Kedia and Philippon (2009), who investigate cover-up actions by firms involved in fraudulent accounting. We study a sample of 1,561 firm-years from 216 U.S. companies participating in hard-core cartels between 1986 and 2010. We define a cartel firm-year as year in which the given cartelist has been involved in price fixing. The starting point and duration of the cartels in our sample are identified by enforcement actions brought by government antitrust authorities. We compare these cartel firm-year observations to observations from a set of control firms matched on size and industry. As in other empirical studies on cartels, our sample is subject to a selection bias as we are only able to consider discovered and indicted cartels, which are believed to represent on a minority of the price-fixing conspiracies that occur worldwide (see Connor, 2010).

[1] Theoretical research shows that cartel formation may be motivated not only by the potential profits from price-fixing, but also by management incentives (e.g., Levenstein and Suslow, 2006). Spagnolo (2005) adds managerial incentives schemes to a supergame-theoretic model of dynamic competition and shows that when managers have a preference for smooth time paths, collusion with other firms becomes more likely. This could be caused by management bonus contracts that have capped incentive provisions. His model shows that even though income smoothing is costly, shareholders tolerate the cost in return for the higher collusive profits. Buccirossi and Spagnolo (2008) show in a classical model of repeated oligopoly that the stability of tacit collusive agreements is positively correlated with performance-based incentives provided to top management.

We document a range of accounting and governance strategies that cartel firms adopt in systematic patterns, apparently with an eye toward prolonging their conspiracies and evading legal liability. To mislead readers of financial statements, companies engage in earnings smoothing and frequently reclassify the industrial segments for which they report line-of-business results. They file abnormally large numbers of financial restatements. In corporate governance, cartel firms favor outside directors who are likely to be inattentive monitors due to their status as foreign or "busy" (belonging to a large number of boards). When directors resign, they are often not replaced, and new auditing firms are engaged significantly less often than expected. Cartel managers exercise their stock options faster than managers of other firms. While our results are based only upon firms engaged in price fixing, we expect that they should apply generally to all companies in which the managers seek to conceal poor performance or personal wrongdoing.

As an example, the pharmaceutical company Bristol-Myers Squibb Co. is one the more prominent firms in our sample. It was charged with participating in cartels in three different countries between 1998 and 2005. During this cartel period, Bristol-Myers engaged in many of the practices described in our analysis below. The company reclassified its line-of-business segments nearly every year, constantly reorganizing them into different subcategories and at one point eliminating them altogether for a two-year period. Five years' of earnings results were restated, including two years that were restated twice. The company was sued twice during this period for securities fraud. It retained the same auditing firm for the entire cartel period despite the outward signs of financial reporting problems. Three new outside directors joined the Bristol-Myers board between 1998-2005. All three fell into the "busy" category, with three or more board memberships, and one of the three was based in a foreign country. Five of the incumbent outside directors from the start of the cartel period also had busy status.

At the end of its cartel period in 2006, after pleading guilty to federal criminal charges related to one cartel, Bristol-Myers appears to have undergone a governance and financial reporting overhaul. It changed auditors, replacing PricewaterhouseCoopers with Deloitte & Touche, and added a law-and-order independent outsider to its board, the former FBI Director and federal judge Louis Freeh.

The remainder of the paper is organized as follows. Section 2 describes the sample and variables. Section 3 reports the results from the empirical analysis. Section 4 concludes.

2. Sample selection

2.1 Cartel firms

We use the U.S. firms included in an extended version of Connor's (2010) hand collected Private International Cartel dataset, which covers private cartels discovered, disclosed and sanctioned by regulators around the world between January 1986 and December 2010.[2] The dataset omits cartels protected by sovereignty or multilateral treaties, as well as those for which no sanctions were imposed within five years of the authorities' discovery. A total of 648 cartels involving 2,115 companies appear in the dataset, although in certain cases many more firms are sanctioned anonymously. Many companies are repeat offenders and participate in multiple cartels. The median cartel involves eight companies and lasts five years before discovery by regulators; the maximum values are considerably higher, with some cartels lasting for decades and involving dozens of companies or more.[3]

The dataset includes each firm's name, country of incorporation, the market(s) and continent(s) where collusion took place, the duration of the collusive agreement, and if known,

[2] The dataset in Connor (2010) covers the time period from January 1990 to December 2009.

[3] Connor (2010) reports one case of more than 2,000 unnamed construction companies accused of price fixing by authorities in the Netherlands.

the fines imposed, leniency granted by regulators, and estimated overcharges to consumers. Information is collected mainly from filings, documents, reports, and press releases from the antitrust authorities in different countries, as well as newspaper and magazine articles retrieved through search engines like Factiva or Lexis-Nexis. The sample generally includes more observations in recent years, with between 300 and 400 companies in each of the years 2005-2009. We do not know whether this pattern occurs due to better enforcement, more disclosure by regulators, more coverage by the press, or a greater tendency by companies to collude in price fixing or bid rigging, though Connor and Helmers (2007) estimate that only 10% to 30% of all price-fixing conspiracies are ever discovered. European companies comprise the majority of observations in the dataset, although many cartels are global in nature and involve multinational firms operating on several continents. Connor and Helmers (2007) estimate that by the early 2000s, worldwide corporate penalties for firms participating in cartels stabilized at or above $2 billion per year, with approximately 60% due to government fines (mainly from U.S. and European regulators) and 40% paid to settle private litigation.

To select our sample, we begin with 819 U.S. companies included in the international dataset. We exclude all cartels which started before 1986 and all firms not covered by Compustat, which substantially reduces the sample size to 216 firms that are involved in a total of 382 conspiracies. We obtain data for 1,561 cartel company-year observations (including part-years) for these 216 firms over the 1986-2010 period, with 67 of the 216 companies participating in more than one cartel. The mean and median cartel period is six years for our sample, and the maximum value is 22 years for a marine hose cartel involving the rubber manufacturers Goodyear and Parker Hannifin. ExxonMobil is involved in 13 individual cartels, the most of any company in the sample, followed by Johnson & Johnson with nine.

We augment the financial statement data from Compustat for our 216 firms with information from other financial and governance sources, including the Center for Research in Securities Prices (CRSP) stock price database, the RiskMetrics Governance and Directors databases, Standard and Poor's ExecuComp database, and the AuditAnalytics database. Missing values in these datasets reduce the observations available for some of our analysis below.

Figure 1 displays the mean and median ROA for our sample companies over a symmetric window of three years around the starting point of each cartel. ROA is calculated throughout the paper as the ratio of earnings before interest and taxes divided by total assets. Figure 1, which provides a reality check on the accuracy of regulators' identification of the cartels, shows that profitability decreases until the last year before a firm enters into a cartel, and then increases steadily over the subsequent four years, beginning in year 0, the first cartel year.

In our regression analysis below, we generally pool together the 1,561 cartel-firm observations with the much larger control sample of 53,418 observations described below, and we test the significance of an indicator variable for the cartel firm-year observations. However, the boundaries of our cartel-firm sample rely on governments' identification of the exact cartel periods. Because the start and end dates of a cartel may be quite ambiguous (even to the firms themselves), we also estimate all of our models including every Compustat firm-year that is available for our cartel firms. We find almost no difference in our regression estimates, and with the exception of financial restatements, no difference in the cartel firms' behavior compared to the control sample for any of our dependent variables. Therefore, we do not report the alternate set of results.

2.2. Control sample of matched firms

We construct a control sample of comparable companies following the approach used in Kedia and Philippon's (2009) study of financial fraud. For every cartelist, we create a group of non-cartelists including all companies that operate in the same two-digit SIC industry and are located in the same total assets quintile of Compustat in the year before each collusive agreement starts. If a conspiracy was already active in 1986, the beginning of our sample period, we use the first cartel firm-year in our sample to form the corresponding control group. No cartel firm is permitted also to enter the control sample.

Our final sample includes 1,561 cartel firm-years for 216 offending companies and a control group of 53,418 firm-years for 3,511 companies matched on size and industry. Table 1 presents summary statistics comparing the means and standard deviations across these two subsamples for all the variables used in our analysis below. The table shows that cartel firms are larger and more profitable than their counterparts in the control sample, and they also exhibit sharply higher rates of accounting problems such as restatements, securities fraud litigation, and discretionary accruals. Cartelists often are the largest firms in their industries, which seems logical since substantial market power would be required to fix prices successfully. Due to this pattern, we use the natural log of total assets as a control variable in all of our regressions.

Table 2 studies the growth of important financial variables for cartel firms compared to the control group. We use the methods in Kedia and Philippon's (2009) study of firms committing financial fraud, regressing the growth rates of different quantities against indicator variables that equal one for three periods that we label *Before*, *During*, and *After*. The *Before* variable equals 1 in the two years prior to initiation of a cartel, the *During* variable equals 1 while the cartel is active, and the *After* variable equals 1 for the two years subsequent to the

cartel. We estimate ordinary least squares panel regressions in which the dependent variable equals the change in growth rates of variables such as sales, from which we subtract the mean change in growth rates for the same variable in the control group of each cartel. We winsorize all these relative growth variables to limits of -1 and 1. We delete observations for which ROA exceeds 10 or is less than -10. To control for the dependence of the disturbance terms, we calculate standard errors using the cluster-robust variant of the Huber-White sandwich estimator.

Results in Table 2 show that cartel firms thrive as a result of their conspiracies, as estimates for the *During* variable indicate that sales grow 4.6% faster, return on assets grows 0.4% faster, and market capitalization rises 3.6% faster than the comparable growth rates for the control sample. These results are all significant at conventional levels and are generally close to zero and insignificant during both the *Before* or *After* years. Cartel firms seem to respond to their strong profits by hiring more workers and increasing their bases of fixed assets, as we also observe significant growth in both employment and property, plant and equipment. The bottom rows of the table report Wald *F*-tests for differences in coefficient estimates for each variable in the *Before* vs. *During* subperiods as well as *During* vs. *After*. The majority of these estimates are significant at conventional levels, and most of the others come close.

It is not clear why firms would systematically expand employment and fixed assets during the cartel period, especially since price-fixing strategies often involve restriction of output to maintain high prices. Many cartel firms agree either explicitly or implicitly not to encroach upon the market shares of rivals. These firms may expand to create a diversion for regulators, in line with many of the results found below, or they may use their cash flows to enter other lines of business as part of an empire building strategy, rather than returning the profits to stockholders.

3. Empirical results

We study the behavior of cartel firms in two broad areas: corporate governance and financial reporting. Our analysis in each subsection below investigates a variety of strategies that these firms may use to evade scrutiny from internal and external monitors, including changes in board composition, changes in auditors, and various financial reporting strategies such as restatements and earnings smoothing. We also investigate top managers' exercises of stock options and the frequency with which cartel firms become targets of class action shareholder litigation for securities fraud.

3.1 Director turnover and replacement

A company could participate in a cartel either with or without the knowledge of its board of directors, and for research purposes it may be difficult to predict which types of boards are more likely to be associated with collusive behavior. However, we can make more straightforward predictions about changes in the board. Companies should be reluctant to replace directors who resign or retire, because recruiting a new monitor from outside the company creates a risk of the cartel being exposed or halted.

Table 3 presents an analysis of board turnover and changes in board size for our cartel firms compared to companies in the control sample. We collect data on individual directors from the RiskMetrics database, and the limited coverage of this source causes a large reduction in our sample size. Our regressions include control variables for board size, Tobin's Q, the growth rate of ROA, industry fixed effects, and year fixed effects. As an alternative specification, we drop the year fixed effects in columns 2 and 4 and instead use an indicator

variable that equals one beginning in 2002, the first-year in which the Sarbanes-Oxley Act and other regulations required companies to begin implementing changes in board structure.

Table 3 shows two clear patterns. Directors resign or retire more frequently in cartel firms compared to companies in the control sample, and when they leave, cartel firms are more likely to allow the board to shrink rather than replacing them. Reasons for director resignations are not clear, but one clear possibility is that board members who become aware of wrongdoing leave quietly to evade future legal liability or to signal disagreement with management's actions (see Fahlenbrach, Low, and Stulz, 2012). The motive for cartel firms not to replace exiting directors seems more clear: by leaving a board seat vacant, the company avoids the possibility of being monitored by a new individual from outside the firm.

Table 4 analyzes the roles of busy and foreign directors on the boards of cartel firms compared to companies in the control sample. Both of these types of directors have been shown in recent papers to perform poorly as monitors, due to such factors as distraction, distance, and their unfamiliarity with U.S. accounting rules. Busy directors, defined as those serving on three or more boards simultaneously, are studied by Fich and Shivdasani (2006). Foreign independent directors are the subject of a recent paper by Masulis, Wang, and Xie (2012), who kindly shared their sample with us for use in this study. Estimates in Table 4 indicate that cartel firms have a greater proportion of busy outside directors than usual, and are also more likely to have at least one foreign outside director, compared to companies in the control sample. When appointing new directors, cartels are more likely to select busy outsiders, consistent with a conjecture that management nominates new board members who are unlikely to monitor aggressively. All of these results hold in models with controls for board size, profitability, board independence, and year and industry fixed effects.

3.2 Changes in auditors

We investigate cartel firms' changes in auditors to explore whether a pattern exists similar to that for boards of directors, with management exhibiting reluctance to bring in new outsiders who might monitor aggressively and become aware of the firm's illegal conduct. We use Compustat to identify changes in auditors and create an indicator variable that equals one for years in which the database reports a different auditor than the previous year. A limited number of auditing firms exit the industry due to mergers or liquidation (including, most famously, Arthur Andersen). In these cases, when an auditor change is mandatory and beyond the control of the firm, we set the auditor change indicator equal to missing.

Summary statistics in Table 1 indicate that auditor changes occur far less frequently for cartel firms than for companies in the control sample, with annual frequencies of 3.3% vs. 6.2%, respectively. In Table 5 we report estimates for probit regressions in which the auditor change indicator is regressed against the cartel indicator as well as control variables for firm size and profitability; these controls are used because larger firms are known to change auditors less often than smaller ones, and successful firms are also likely to change less often than unsuccessful ones for whom audit conflicts may occur with greater frequency. We include year fixed effects in the model on the left side of the table, and both year and industry fixed effects in the model shown on the right side.

Estimates for the cartel firm indicator confirm the results found in the simple comparison of sample means: cartel firms change auditors significantly less often than other companies, controlling for company size, performance, industry membership, and time period. The results are consistent with those found above documenting unusually slow replacement of directors for cartel companies, and they suggest that management attempts to reduce external scrutiny by restricting access by new monitors.

3.3 Accounting restatements

We investigate the propensity of cartel firms to engage in misleading financial reporting by analyzing their patterns of financial restatements. We use the Audit Analytics database to download information about restatements filed by all of our sample firms between 2000 and 2010. The database covers restatements filed electronically with the SEC since January 1, 2001, including restatements for past years filed since that date. We exclude years earlier than 2000, because the database's coverage of their entire restatement history is likely to be incomplete.

Table 6 presents our regression analysis of restatements for cartel firms and the matched sample. In columns 1 and 2, the dependent variable is a binary indicator for whether a fiscal year's results are eventually restated. These models are estimated in a probit framework, and column 2's specification includes industry and year fixed effects. In columns 3 and 4, the dependent variable is the number of times that a given year's results are restated, with the models estimated in a Poisson maximum likelihood framework. The control variables in all models follow those used by Larcker, Richardson, and Tuna (2007). They include the book-to-market ratio of common equity lagged one year, the log of the market value of equity lagged one year, a measure of external financing equal to net equity plus net debt issued deflated by the lagged market value of equity, acquisition spending over the lagged market value of equity, and a measure of free cash flow calculated as the difference of operating cash flow and average capital expenditure over the three prior years, deflated by lagged market value of equity. All control variables are winsorized at the 2^{nd} and 98^{th} percentiles.

Consistent with the summary statistics in Table 1 showing a 50% higher incidence of restatements among cartel firms, the estimates for the cartel firm indicator are positive in every

column and significant in three columns out of four, with the fourth result just falling short of the 10% threshold. The evidence shows that cartel firms are more likely to file restatements than firms in the control sample, consistent with a strategy of using misleading accounting in order to conceal the firm's true operating performance.

We analyze our results further in regressions similar to those in Table 2, with *Before*, *During*, and *After* indicator variables for the cartel companies. While cartel firms are significantly more likely than other firms to file financial restatements during the years in which the government accuses them of price-fixing behavior (the *During* years), they are significantly less likely than other firms to have restatements for those years either before or after the cartel period. This pattern indicates an interesting coincidence between government enforcement actions and firms' subsequent restatements. It is also unique in our set of dependent variables. We investigate the *Before-During-After* structure of indicator variables for all of our other analysis such as board changes and earnings management, and cartel firms' behavior in all other areas seems to persist for long periods that extend beyond the precise period in which the firm becomes targeted for enforcement.

3.4 Earnings management

We expect cartel firms to engage in abnormally high levels of accounting earnings management. This strategy might serve either of two purposes: it could conceal the firm's rising profits from regulators and analysts, and it may signal to competing firms a desire to promote stable profits in the industry. We investigate two strategies for earnings management, the manipulation of discretionary accruals and deferred revenue.

Our study of discretionary accruals follows the approach of Bergstresser and Philippon (2006) and Klein (2002). We estimate a firm's discretionary accruals with a version of the

Jones (1991) and modified Jones (Dechow, Sloan, and Sweeney, 1995) models, which estimate nondiscretionary accruals as the fitted value from a regression. Discretionary accruals are then calculated as the residual difference of total accruals less nondiscretionary accruals.

Our methodology uses annual data from Compustat to calculate total accruals for firm *i* in year *t* as:

$$TA_{i,t} = \frac{\left(\Delta CA_{i,t} - \Delta CL_{i,t} - \Delta Cash_{i,t} + \Delta STD_{i,t} - Dep_{i,t}\right)}{A_{i,t-1}} \quad (1)$$

where

ΔCA = the change in current assets

ΔCL = the change in current liabilities

$\Delta Cash$ = the change in cash holdings

ΔSTD = the change in long term debt in current liabilities

ΔDep = the depreciation and amortization expense of the firm

ΔA = firm size (in assets)

We follow Klein (2002) by estimating expected accruals at the industry level by fitting the following regression each year across all Compustat firms in each two-digit SIC industry:

$$TA_{i,t} = \alpha_0 + \alpha_1 * \frac{1}{A_{i,t-1}} + \alpha_2 * \Delta Rev_{i,t} + \alpha_3 * PPE_{i,t} + \varepsilon_{i,t} \quad (2)$$

where ΔRev is the change in sales and *PPE* is gross property, plant and equipment, both deflated by lagged assets. We require a minimum of 12 observations per regression. Nondisicretionary accruals are then calculated for every firm-year observation as:

$$NDA_{i,t} = \tilde{\alpha}_0 + \tilde{\alpha}_1 * \frac{1}{A_{i,t-1}} + \tilde{\alpha}_2 * \Delta Rev_{i,t} + \tilde{\alpha}_3 * PPE_{i,t} \quad (3)$$

where $\tilde{\alpha}_0$, $\tilde{\alpha}_1$, $\tilde{\alpha}_2$ are the estimated parameters from equation (2). This leads to the calculation of discretionary accruals:

$$DA_{i,t} = TA_{i,t} - NDA_{i,t} \tag{4}$$

As an alternative, we use the modified Jones model. Instead of deriving the fitted values used to calculate *NDA* in equation (3) from the regression in equation (2), we estimate the following regression which substitutes the change in sales less the change in receivables for the change in sales, i.e.,

$$TA_{i,t} = \alpha_0 + \alpha_1 * \frac{1}{A_{i,t-1}} + \alpha_2 * \left(\Delta Rev_{i,t} - \Delta Rec_{i,t} \right) + \alpha_3 * PPE_{i,t} + \varepsilon_{i,t} \tag{5}$$

where ΔRec is the change in receivables deflated by lagged assets.

In Table 7 we present least-squares estimates of discretionary accruals for our cartel firms and the control sample. Other explanatory variables, following Klein (2002) and Bergstresser and Philippon (2006), include firm size (the log of total assets), leverage, Tobin's Q, the absolute value of the change in return on assets, and the natural log of firm age (based on the company's first inclusion in the CRSP database). Both models include year and industry fixed effects. Estimates in the left two columns of Table 7 are positive and significant for the cartel firm indicator using either of the dependent variables for discretionary accruals. This indicates that cartel firms are more aggressive about using accruals.

Table 7 continues with two regressions estimating cartel firms' propensity to record deferred revenue, an accounting entry that essentially pushes profits into a future period by balancing an increase in cash received with an entry on the liability side of the balance sheet; the liability is then converted into shareholders' equity, an act that increases the firm's profits, during the future period when the income is deemed to be earned. We calculate deferred revenue as the sum of the Compustat variables *DRC* (revenue that has not been earned but is

expected to be recognized in the current year) and *DRLT* (revenue that has not been earned and will be recognized in more than one year).

We present a Tobit analysis in column 3 of Table 7, with the dependent variable equal to deferred revenue scaled by net sales. In column 4, we estimate a binary probit model in which the dependent variable equals one if the firm has positive deferred revenue on its balance sheet in that year. We use the same explanatory variables that appear in our models of discretionary accruals. For either model, the estimate for the cartel firm indicator is negative and significant. The result is quite similar to that found for discretionary accruals, implying that cartel firms tend to report higher earnings in current periods. Interpreting these results is somewhat challenging. Our main thesis in this paper is that cartel firms actively try to conceal the extent of their success in order to prolong the benefits from collusion. This might suggest less aggressive earnings management and slower revenue recognition, but we find the opposite. This may occur for a number of reasons. Cartel firms may expect profits to rise in the future and see little need for establishing accounting reserves and delaying revenue recognition. Alternatively, cartelists may feel vulnerable to outside scrutiny if revenue deferrals and slow accruals lead to an overwhelming delay of profit reporting, and they may make no effort to delay favorable accounting news in order to keep the pipeline clear of future positive news that could raise a red flag for regulators.

3.5 Segment reclassification

Diversified companies are required to report line-of-business financial data in industry segments, providing information about key aggregates such as sales, operating profit, capital spending, and assets invested. However, companies have discretion to use their own judgment when setting the boundaries between their business entities. The financial statements of major

conglomerates routinely exhibit year-to-year changes in the number and relative size of business segments. These could occur due to acquisitions and divestitures, changes in operating performance, or accounting decisions made by management. Regardless of the motivation for segment reclassifications, they tend to obscure the clarity of a firm's financial results by making year-over-year performance comparisons difficult.

We study the extent to which the cartel firms in our sample reclassify their industrial segments on a year-to-year basis, since these accounting adjustments represent a further strategy for concealing the firm's financial results from external audiences. For our cartel firms and the control sample, we study annual changes in the intra-firm Herfindahl Hirschman Index as a way to infer segment reclassifications. We construct the index for each firm-year using data from the Compustat Segments database. We use total assets as a measure of the size of each industrial segment. The Herfindahal Hirschman Index is computed as the sum of the squares of each segment's assets as a proportion of the square of total assets for the company. For example, if a firm has only one segment, its index equals 1.0, and if it has 10 segments that each contribute 10% of its assets, its index equals 0.1. Hence, the index's value decreases as the degree of diversification increases.

In Table 8 we report results from regression estimations in which the dependent variable equals the absolute value of the annual change in the Herfindahl Hirschmann Index. We use the absolute value because we are concerned only with whether cartel firms adjust the relative sizes of their reporting segments on a year-to-year basis, rather than whether they increase or decrease their degree of diversification. As control variables, the model includes a specification based on Berger and Ofek's (1995) study that used segment financial data to calculate value losses from diversification, including the log of total assets, ROA (EBIT over total assets), and capital spending (CAPX over total assets). Year fixed effects are included in

both columns of the table, and the second column also includes industry fixed effects. We use a Tobit specification since the absolute value of the dependent variable is left-censored at zero.

Results shown in Table 8 indicate that cartel firms change the distribution of assets across segments more than matched control firms, as the cartel indicator variable has a positive and significant estimate of approximately 0.12 in both models. These estimates confirm our hypothesis that frequent segment reclassification helps these companies diminish the clarity of their performance by making more difficult year-over-year comparisons at the industry level.

3.6 Mangers' exercise of stock options

In Kedia and Philippon's (2009) study of companies that commit financial fraud, the authors document an abnormally rapid exercise of in-the-money stock options by those firms' managers. The obvious interpretation of their results is that managers attempt to withdraw equity compensation from the company when the per-share price is inflated above its likely long-term value. We investigate stock option exercises by cartel firms. If these managers also exercise stock options rapidly, the motivation may be more subtle than for executives in companies that commit fraud. A successful cartel may expect its stock price to rise over time, suggesting that the managers would be patient about withdrawing equity compensation. However, they may have concerns about whether the cartel can be sustained, and they may wish to withdraw their compensation before regulators discover the scheme. In addition, rapid stock option exercises may play a diversionary role, by communicating to outsiders that the managers do not expect future abnormal increases in the stock. It may also serve as a communication device by sending signals to managers of other firms in the cartel.

In Table 9 we present an analysis of stock option exercises by the CEO and the top five managers in our cartel firms and our control sample. Our data source for option exercises is the

S&P ExecuComp database, and relying on this database greatly reduces our sample size since it covers only about one-quarter of the companies on Compustat. Our regressions follow those reported by Kedia and Philippon (2009). We use two dependent variables: (i) the dollar value of option profits realized by managers, divided by the total amount by which options are in-the-money (their intrinsic value) at the start of the year, and (ii) a more simple calculation of the ratio between number of options exercised and the number that are vested and could theoretically have been exercised, whether in-the-money or not. Control variables include the size of the firm's total inventory of outstanding employee stock options, the exercise rate for all firms in the two-digit SIC industry, Tobin's *Q*, and the firm's stock return in the past year, along with fixed effects for year and industry.

Results of these estimations appear in Table 9. In every model estimates indicate that managers from cartel firms exercise their stock options more rapidly than managers from firms in the control sample, although the estimate in the right column just misses statistical significance at the 10 percent level. As noted above, these patterns of early option exercise could occur for a number of reasons, but they are consistent with an interpretation that managers wish to withdraw their equity compensation before some future date at which the cartel might be exposed and the firm's stock price could drop.

3.7 Securities fraud litigation

We use the Stanford Law School Securities Class Action database to identify companies accused of financial fraud in class action civil litigation brought by shareholders. Ultimately 60 of our 216 cartel firms are sued for securities fraud one or more times during the sample period, which does not begin until 1996, the starting date of the Stanford database. When scaled by the number of observations in the sample, the lawsuit frequency for the cartel firms exceeds 5

percent per year, almost three times higher than the frequency for companies in the control sample, as shown by the sample means reported in Table 1.

In Table 10 we present regression analysis of the incidence of securities fraud lawsuits. A binary probit model appears in the left column, with the dependent variable equal to one if the firm is sued in a given year. A Poisson maximum likelihood model in the right column is based on the number of lawsuits filed per year. As in our other regressions, control variables include firm size, ROA, and fixed effects for years and industries. We obtain strongly positive and significant estimates for the cartel indicator variable, confirming the result that these firms attract shareholder litigation for fraud at unusually high frequencies.

We reviewed many of the cases in the Stanford database and found that price fixing is rarely, if ever, the basis of shareholders' complaints. Cartel membership is not by itself an act of fraud, and shareholders may have difficulty arguing that they sustain damages when a firm successfully conspires to fix prices. Instead, the shareholder litigation may be a byproduct of the other signal-jamming strategies related to financial reporting that are illuminated by the regression results above, such as frequent earnings restatements, segment reclassifications, or abnormally high discretionary accruals.

4. Conclusions

We study the behavior of 216 U.S. public companies that are accused by governments of illegal cartel activity. We find that the sample firms engage in a range of practices designed to obscure their behavior from both internal and external audiences. Compared to a control sample of firms with similar size and industry, cartel firms appear to avoid new outside monitoring by not replacing board members who resign and changing auditors infrequently. When new directors are appointed, they tend to be busy or foreign-based directors who are

known to be inferior monitors. In financial reporting, cartel firms engage in unusually high levels of earnings smoothing and file high numbers of financial restatements for the cartel years. They also frequently reclassify their industrial segments year-to-year when reporting line-of-business data in annual financial statements, making annual comparisons of financial results more difficult. Top managers of cartel firms exercise stock options more frequently than executives in the control sample, perhaps attempting to withdraw their equity compensation before the cartel is exposed and the firm's stock price falls. Cartel firms are also sued at high rates for securities fraud, although this litigation is rarely connected to the price-fixing activity itself.

We believe our results create a template for understanding how companies behave when they wish to conceal aspects of their financial performance. The firms in our sample engage in a range of signal-jamming strategies when preparing their annual financial statements, and they appear to avoid inviting scrutiny from new directors or new auditors. Managers try to cash out their performance-based compensation earlier than would be expected. Multiple explanations may apply to some of our results. For example, earnings smoothing or a certain timing of the exercise of stock options may be a communication device to other firms in a cartel.

Our results may extend to other situations, both benign and malign, in which companies actively try to conceal information from their own monitors and from outside audiences. Firms may not want to give clear pictures of their capital investment or new product development spending, for instance, and may seek to obscure unusual spending patterns through strategies such as segment reclassification or earnings smoothing. More concerning would be cases in which managers scheme to embezzle funds or mislead creditors about the firm's financial health. By understanding the playbook of strategies outline in this paper, these and other problems might become apparent earlier to shareholders, analysts, and regulators.

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Table 1

The table shows descriptive statistics for a sample of 1,561 annual observations for 216 cartel participants and a control sample of 53,418 observations for 3,511 Compustat firms matched on size and industry. Cartel firms are identified from the dataset of Connor (2010). Board of directors data is tabulated from the RiskMetrics Directors database, and busy independent directors are those serving on three of more boards. Foreign independent directors are identified from the sample of Masulis, Wang, and Xie (2012). Financial statement data is obtained from Compustat, which is also used to identify auditor changes. Stock option exercise data is obtained from ExecuComp, securities fraud lawsuits are tabulated from the Stanford securities clearinghouse website, and financial restatements are reported on the Audit Analytics database.

	Cartel firms			Control sample			
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Difference In Means
Board becomes smaller (indicator)	0.328	0.470	679	0.248	0.432	8.029	0.080 [a]
% Directors leaving	0.104	0.105	760	0.089	0.110	9,565	0.015 [a]
% Busy independent directors on board	0.418	0.230	709	0.261	0.233	9,610	0.157 [a]
New appointments busy (indicator)	0.290	0.628	672	0.158	0.434	8,752	0.132 [a]
At least one foreign independent director on board (indicator)	0.202	0.402	630	0.102	0.303	7,150	0.099 [a]
New appointment of foreign independent directors (indicator)	0.035	0.200	606	0.017	0.142	6,504	0.017 [a]
Auditor change indicator	0.033	0.178	1,462	0.062	0.241	44,716	-0.029 [a]
Value realized / intrinsic value of exercisable options (CEO)	0.211	0.300	916	0.188	0.295	13,672	0.023 [b]
Option exercised / exercisable options (CEO)	0.115	0.207	1,017	0.118	0.222	15,369	-0.003
Value realized / intrinsic value of exercisable options (Top5)	0.231	0.261	969	0.226	0.272	14,946	0.005
Option exercised / exercisable options (Top5)	0.122	0.162	1,031	0.139	0.197	16,072	-0.017 [a]
Restatement indicator	0.161	0.368	907	0.138	0.345	21,924	0.023 [b]
Number of restatements	0.208	0.521	907	0.170	0.472	21,924	0.038 [b]
Discretionary accruals (Jones)	0.010	0.116	1,279	-0.001	0.143	41,721	0.012 [a]
Discretionary accruals (Modified Jones)	0.003	0.128	1,279	-0.004	0.151	41,713	0.008 [c]
Deferred revenues	0.014	0.055	691	0.083	0.258	15,258	-0.069 [a]
Deferred revenue indicator	0.236	0.425	691	0.453	0.498	15,258	-0.217 [a]
Change in Herfindahl Hirschman Index	0.043	0.101	999	0.029	0.086	33,041	0.013 [a]
Securities fraud lawsuit indicator	0.052	0.222	1,153	0.018	0.133	32,528	0.034 [a]
Number of frauds	0.059	0.267	1,153	0.018	0.138	32,528	0.041 [a]
Board size	11.255	2.920	793	9.497	2.940	11,034	1.757 [a]

% independent directors on board	0.709	0.155	796	0.682	0.176	11,120	0.028 [a]
Total assets	62,411	192,805	1,558	6,957	38,566	51,801	55,454 [a]
Market value	27,734	56,220	1,459	2,810	10,591	46,147	24,924 [a]
Sales	19,130	37,564	1,555	2,182	5,948	51,530	16,948 [a]
ROA (EBIT / total assets)	0.095	0.079	1,553	0.040	0.169	50,211	0.056 [a]
Book-to-market	0.496	0.412	1,459	0.578	0.489	45,833	-0.082 [a]
Tobin's Q	1.882	1.373	1,461	1.996	2.331	45,885	-0.114 [c]
Leverage	0.669	0.216	1,549	0.622	0.485	51,704	0.047 [a]
Options outstanding	54.862	101.196	944	13.570	50.724	17,197	41.290 [a]
Annual return	1.658	56.376	1,421	0.713	27.484	43,047	0.945
External financing	0.009	0.230	814	0.013	0.229	18,204	-0.004
Acquisitions / market capitalization	0.029	0.077	1,257	0.034	0.091	38,173	-0.005 [c]
Free Cash Flow	0.027	0.198	1,288	0.007	0.201	33,897	0.020 [a]
Firm age	35.706	24.759	1,356	16.930	16.199	38,928	18.780 [a]
Absolute value of change in EBIT	1.031	9.188	1,508	1.870	27.768	46,665	-0.839
CAPX / total assets	0.051	0.040	1,511	0.061	0.075	48,159	-0.010 [a]

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 2
Growth dynamics for cartel firms and matched sample
The table reports OLS regression results of adjusted growth dynamics for a variety of financial and employment data. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010. Each dependent variable in Columns 1-4 is a growth rate (one year log differences) relative to the mean growth rate of a control group of 3,511 Compustat firms matched on size and industry, and in Column 5, the dependent variable is the one-year difference in return on assets (ROA) relative to the mean first difference of ROA for the control group. The indicator variable *Before* equals 1 for the two years preceding each cartel, *During* equals 1 during the cartel period, and *After* equals 1 for the two years after the cartel ends. *t*-statistics in parentheses are based on the cluster-robust variant of the Huber-White sandwich estimator. The table reports *p*-values of Wald tests for the hypotheses that β_{During} equals β_{Before} and β_{During} equals β_{After}.

Dependent variables (annual growth rates):	Market value	Sales	Employment	PPE	ROA
Before	0.010	0.002	0.016	0.017	-0.002
(indicator for two years prior to cartel)	(0.537)	(0.132)	(1.450)	(1.173)	(-0.450)
During	0.036 [a]	0.046 [a]	0.029 [a]	0.053 [a]	0.004 [b]
(indicator for years in cartel period)	(2.795)	(4.462)	(3.335)	(4.560)	(2.077)
After	-0.032	-0.002	-0.010	0.016	-0.005
(indicator for two years following cartel)	(-1.571)	(-0.142)	(-0.698)	(1.175)	(-1.071)
Observations	3,288	3,453	3,316	3,383	3,465
r-squared	0.004	0.012	0.006	0.011	0.001
Wald test: $\beta_{During} = \beta_{Before}$	0.202	0.000 [a]	0.291	0.010 [a]	0.125
Wald test: $\beta_{During} = \beta_{After}$	0.002 [a]	0.000 [a]	0.003 [a]	0.012 [b]	0.033 [b]

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 3
Retention and replacement of directors

The first two columns reports results from probit regressions in which the dependent variable equals 1 if board size decreases in a given year. The right two columns report the results from Tobit regressions of the percentage of directors leaving the board in a given year. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. ln(board size) is the natural log of the number of directors on the board. Board of directors data is obtained from the RiskMetrics Directors database. Tobin's *Q* is defined as total assets minus the book value of equity plus the market value of equity over total assets. Past growth in ROA is the three-year growth rate in return on assets. SOX is an indicator variable which equals for the years after enactment of the Sarbanes-Oxley Act (2002 and after). *z*-statistics and *t*-statistics appear in parentheses.

Dependent variable:	Board becomes smaller (Probit)		Percentage of directors leaving (Tobit)	
Cartel firm indicator	0.293 [a]	0.304 [a]	0.016 [b]	0.023 [a]
	(5.189)	(5.432)	(2.087)	(3.027)
ln(board size)	-0.306 [a]	-0.307 [a]	0.116 [a]	0.110 [a]
	(-5.099)	(-5.154)	(15.071)	(14.356)
Tobin's *Q*	-0.030 [a]	-0.029 [a]	-0.008 [a]	-0.007 [a]
	(-2.784)	(-2.762)	(-6.026)	(-5.313)
Past growth in ROA	5.323 [b]	5.486 [b]	0.043	0.047
	(2.261)	(2.280)	(0.542)	(0.594)
SOX		-0.060 [b]		-0.007 [c]
		(-1.966)		(-1.859)
Year fixed effects	Yes	No	Yes	No
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	8,504	8,504	10,046	10,046
Pseudo *r*-squared	0.025	0.023		
LR Chi-squared			577.16	397.00
Prob > Chi-squared			0.000	0.000

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 4
Busy and foreign outside directors
The table shows regression estimates of models of the presence and appointment of busy and foreign outside directors to the board. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. Busy directors are defined as those holding three or more board seats. Data on foreign outside directors is obtained from Masulis et. al (2012). Other board of directors data is obtained from the RiskMetrics Directors database. ln(board size) is the natural log of the number of directors on the board. *z*-statistics and *t*-statistics appear in parentheses.

Dependent variable:	Busy outside directors		Foreign outside directors	
	Percentage on board (Tobit)	New appointments (Poisson)	At least one on board (Probit)	New appointments (Poisson)
Cartel firm indicator	0.138 [a]	0.319 [a]	0.149 [b]	0.227
	(11.603)	(3.903)	(2.223)	(0.898)
ROA	-0.028	-0.061	-0.220	0.041
	(-0.917)	(-0.204)	(-1.054)	(0.042)
ln(board size)	0.256 [a]	1.535 [a]	0.991 [a]	1.865 [a]
	(21.642)	(14.196)	(12.340)	(5.112)
% Independent directors	0.438 [a]	2.158 [a]	1.118 [a]	1.636 [a]
	(21.858)	(11.367)	(8.512)	(2.818)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	10,125	9,258	7,698	7,035
Pseudo *r*-squared		0.056	0.086	0.063
LR Chi-squared	1638.66			
Prob > Chi-squared	0.000			

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 5
Changes in auditors
The table reports the results from probit regressions of a dummy variable that equals 1 when the firm changes auditors in a given year. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. Auditor changes are identified from Compustat. Regressions include return on assets and firm size (the log of total assets) as control variables. *z*-statistics appear in parentheses.

Dependent variable:	Company changes auditor	
Cartel firm indicator	-0.183 [a]	-0.133 [c]
	(-2.712)	(-1.864)
ROA	-0.183 [a]	-0.117 [c]
	(-3.219)	(-1.922)
ln(Total assets)	-0.050 [a]	-0.082 [a]
	(-9.708)	(-12.525)
Year fixed effects	Yes	Yes
Industry fixed effects	No	Yes
Observations	45,626	45,626
Pseudo *r*-squared	0.045	0.057

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 6
Restatements

The table reports probit regression estimates for whether a firm restates its audited financial statements in a given year and Poisson regression estimates of the number of restatements per year. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. Regressions include control variables for the book-to-market ratio (lagged), the natural logarithm of market capitalization (lagged), external financing, acquisitions scaled by market capitalization, and free cash flow. Data for restatements is obtained from the Audit Analytics database, which covers restatements disclosed since January 1, 2001, and due to the coverage of this database we use observations only for the years 2000-2010. All control variables are defined as in Larcker, Richardson, and Tuna (2007). *z*-statistics appear in parentheses.

Dependent variable:	Restatements (Probit)		Number of times restated (Poisson)	
Cartel firm indicator	0.208 **b**	0.171 **c**	0.312 **a**	0.190
	(2.533)	(1.948)	(2.555)	(1.488)
Book-to-market ratio (lagged)	-0.009	0.073 **c**	0.030	0.178 **a**
	(-0.228)	(1.678)	(0.508)	(2.609)
Ln(market capitalization) (lagged)	-0.012	0.001	0.001	0.042 **b**
	(-1.239)	(0.083)	(0.094)	(2.293)
External financing	-0.060	-0.114	-0.083	-0.148
	(-0.667)	(-1.171)	(-0.592)	(-0.944)
Acquisitions / market capitalization	0.123	0.037	0.320	0.205
	(0.498)	(0.140)	(0.837)	(0.509)
Free Cash Flow	-0.048	-0.158	-0.080	-0.204
	(-0.506)	(-1.552)	(-0.537)	(-1.284)
Year fixed effects	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes
Observations	8,141	8,103	8,141	8,141
Pseudo *r*-squared	0.001	0.060	0.001	0.064

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 7
Earnings management
The table reports ordinary least squares regression estimates of discretionary accruals and revenue recognition. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. Following Bergstresser and Philippon (2006) and Klein (2002), the discretionary accruals are constructed according to the cross-sectional Jones (Column 1) and the cross-sectional modified Jones models (Column 2). The measure of deferred revenue is the ratio of Compustat items DRC+DRLT divided by net sales. DRC is revenue which has not yet been earned, but is expected to be classified as earned during the current year, while DRLT is revenue which has not yet been earned. The model in the fourth column uses a binary dependent variable that equals 1 if the sum of the sum of DRC+DRLT is greater than zero. Regressions include controls for firm size (the log of total assets), leverage lagged by one year, the log of firm age, Tobin's *Q*, and the absolute value of the change in earnings before interest and taxes over total assets. *t*-statistics appear in parentheses and in the first two columns are based on the cluster-robust variant of the Huber-White sandwich estimator.

Dependent variable:	Discretionary accruals (Jones)	Discretionary accruals (Modified Jones)	Deferred revenue	Revenue indicator
	(OLS)	(OLS)	(Tobit)	(Probit)
Cartel firm indicator	0.012 [a]	0.008 [b]	-0.125 [a]	-0.438 [a]
	(3.528)	(2.242)	(-5.291)	(-6.381)
ln(total assets)	-0.002 [a]	-0.002 [a]	-0.015 [a]	0.012
	(-2.988)	(-2.696)	(-5.669)	(1.419)
Leverage	0.009 [b]	0.006	-0.012	-0.295 [a]
	(2.199)	(1.380)	(-1.119)	(-6.952)
ln(firm age)	0.004 [a]	0.002 [c]	-0.053 [a]	-0.120 [a]
	(4.432)	(1.870)	(-11.371)	(-8.289)
Tobin's *Q*	-0.003 [a]	-0.001 [a]	0.029 [a]	0.112 [a]
	(-4.141)	(-4.141)	(12.374)	(12.489)
Absolute value of change in EBIT	-0.053 [b]	-0.060 [b]	0.014	0.619
	(-2.302)	(-2.400)	(0.196)	(1.429)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	30,975	30,969	12,373	12,208
r-squared	0.015	0.011		
Pseudo *r*-squared				0.084
LR Chi-squared			2911.46	
Prob > Chi-squared			0.000	

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 8
Change in Herfindahl Hirschman Index
The table reports the results from tobit regressions of the absolute value of the annual change in the intra-firm Herfindahl Hirschman Index based on total assets. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. The index is calculated from annual industry segment data reported by each company. *t*-statistics appear in parentheses.

Dependent variable:	Change in HHI (total assets)	
Cartel firm indicator	0.012 [a]	0.011 [b]
	(2.674)	(2.308)
ROA	0.026 [a]	0.015 [a]
	(5.031)	(2.801)
CAPX / total assets	-0.122 [a]	-0.158 [a]
	(-9.886)	(-11.361)
ln(Total assets)	0.013 [a]	0.013 [a]
	(28.024)	(23.576)
Year fixed effects	Yes	Yes
Industry fixed effects	No	Yes
Observations	33,303	33,313
LR Chi-squared	1,775.22	2,574.91
Prob > Chi-squared	0.000	0.000

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 9
Stock option exercises by top executives
The table reports Tobit regression estimates for the value realized from options exercised over the intrinsic value of exercisable options (Columns 1 and 3) and the number of options exercised over total exercisable options (Columns 2 and 4). In the right two columns, the dependent variables are calculated as the mean within the group of top five executives for each company. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. Option exercise data is obtained from ExecuComp, while information about the inventory and value of exercisable options is obtained from Compustat. Regressions include control variables for options outstanding, past stock performance, Tobin's *Q* and average industry exercises, which is calculated as the average value of the dependent variable for all firms in the same two-digit SIC industry with data on ExecuComp in that year. All control variables are lagged by one year. All regressions include year fixed effects. *t*-statistics appear in parentheses.

	CEO only		Top 5 executives	
Dependent variable:	Value realized / intrinsic value of exercisable options	Options exercised / exercisable options	Value realized / intrinsic value of exercisable options	Options exercised / exercisable options
Cartel firm indicator	0.108 [a]	0.058 [a]	0.048 [a]	0.016
	(4.315)	(3.215)	(3.258)	(1.621)
Average industry exercises	0.159	0.522 [a]	-0.004	0.393 [a]
	(1.333)	(4.544)	(-0.064)	(6.364)
Options outstanding	0.071	-0.085	0.034	-0.121 [a]
	(0.756)	(-1.231)	(0.632)	(-3.238)
Past year returns	-0.003	0.002	-0.002	0.002
	(-0.898)	(1.098)	(-1.169)	(1.331)
Tobin's *Q*	0.026 [a]	0.028 [a]	0.015 [a]	0.019 [a]
	(8.681)	(13.100)	(8.364)	(15.635)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	8,280	9,498	8,968	9,737
LR Chi-squared	312.34	505.89	541.93	815.50
Prob > Chi-squared	0.000	0.000	0.000	0.000

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Table 10
Securities fraud litigation
The table reports the results from regressions for the incidence of securities fraud lawsuits filed during the period 1996-2010, based on the Stanford Law School Securities Class Action Clearinghouse. The sample includes 216 U.S. firms identified by regulators as cartel participants between 1986-2010, as well as a control sample of Compustat firms matched on size and industry. *t*-statistics appear in parentheses.

Dependent variable:	Fraud lawsuit indicator (Probit)	Number of fraud lawsuits (Poisson)
Cartel firm indicator	0.209 [a]	0.530 [a]
	(2.753)	(3.516)
ROA	-0.995 [a]	-2.338 [a]
	(-9.965)	(-11.247)
ln(Total assets)	0.168 [a]	0.372 [a]
	(14.166)	(14.602)
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Observations	31,728	32,632
Pseudo *r*-squared	0.057	0.088

Significant at 1% (**a**), 5% (**b**) and 10% (**c**) levels.

Figure 1
Return on assets around the year of cartel formation
The figure shows mean annual return on assets (ROA) for 216 U.S. companies participating in price-fixing cartels. Year 0 represents the beginning of each cartel according to government antitrust regulators. The sample is identified by Connor (2010) from government filings, press releases, and news reports