Price-Fixing Hits Home: An Empirical Study of U.S. Price Fixing Conspiracies

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Abstract

This paper analyzes all Section 1, Sherman Act price fixing cases brought by the U.S. Department of Justice between 1961 and 2013. Over 500 cartels were prosecuted during this period. The determinants of cartel formation and cartel breakup are estimated, including analysis of the impact of the discount rate, business cycles, and antitrust policy. We find that cartels are more likely to breakup during periods of high real interest rates, presumably because higher interest rates are associated with greater impatience. The adoption of a stronger amnesty policy has no significant impact on cartel breakup over this period, although the results suggest some association with lower cartel formation rates.

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1. Introduction

More than a century after the passage of the Sherman Antitrust Act and twenty years after the adoption of more aggressive enforcement by the U.S. Department of Justice (DOJ), cartels continue to affect US markets. Fifty new criminal cases, most of which were price fixing, were filed in 2013; over a billion dollars in criminal fines were levied.[1] In this paper, we study the formation and breakup of all price-fixing cartels convicted of operating in the United States in the last half-century. Understanding how and why these cartels continue to form and persist is critical to developing better models of firm behavior and better competition policy.

In this paper, we present analysis of data that include all Section 1, Sherman Act price fixing cases between 1961 and 2013. The long timespan of these data allows us to examine a number of issues. Approximately half-way through the sample period, the Justice Department strengthened its leniency policy. This policy change increased firms' incentive to turn in cartels in which they were participating. These data allow us to examine the effectiveness of this policy at encouraging cartel breakup and deterring cartel formation.

One of the few areas of consensus flowing from theoretical research on collusion is that it requires patience. In general there is a monotonic relationship between the ability of a cartel to sustain collusion and the discount rate of its members. There have been very few empirical studies of the relationship between the discount rate and collusion; there is a small experimental literature, discussed below, that has studied this question. Previously, using a sample of

[1] Baer and Hosko (2013), p. 2.

international cartel prosecutions by the DOJ and European Commission (EC), we found that market interest rates had little impact on cartel stability, but that firm-level financial weakness, which could raise individual discount rates, was an important determinant of cartel breakup (Levenstein and Suslow 2011). In this paper, we continue to pursue the study of the relationship between patience and cartel stability. We consider both economy-wide determinants of the discount rate and firm-level leverage and liquidity in industries with cartels.

In contrast, the theoretical literature is quite conflicted about how the business cycle may affect collusion. This question has been the focus of much of the prior empirical research on cartel breakup (see Levenstein and Suslow 2006a, 2011 for discussion of this literature). We examine the impact of business cycle fluctuations on cartel breakup. Our previous research suggests that economy-wide fluctuations in demand have had little impact on cartel breakup in contemporary cartels (Levenstein and Suslow 2011). That analysis covered a period of remarkable macroeconomic stability in the United States so may not provide a good setting for addressing this question. The data analyzed in this paper reach back over a number of business cycles and provide a better basis for analysis.

We also examine the cyclicality of cartel formation. Competition observers have often claimed that cartels are more likely to form during recessions, but there has been no systematic study of this question. Admittedly measuring cartel formation is challenging. Our examination of prior research on cartel formation with NBER business cycle dates suggests no obvious pattern (Levenstein and Suslow 2015, 446-449). Cartels were found to form during recessions, but not disproportionately so.

2. Amnesty, Discount Rates, and Business Cycles: Explaining Cartel Formation and Breakup

The familiar cooperative participation constraint for price-setting firms, in an infinitely repeated game with perfect information, illuminates the economic decision that firms face in deciding whether to begin or end participation in a collusive arrangement, as well as the decision to apply for leniency. Collusion can be sustained if the expected profit from colluding today outweighs the expected profit of defecting from a cooperative agreement: [2]

$$\sum_{t=0}^{\infty} \delta^t \Pi^i(p_{i,t}^M, p_{-i,t}^M) - \theta\Omega > \Pi^i(p_{i,0}^D, p_{-i,0}^M) + \sum_{t=1}^{\infty} \delta^t \Pi^i(p_{i,t}^C, p_{-i,t}^C) - \mathcal{L}$$

where

$p_{i,t}^M$ is the collusive price charged by firm *i* in period *t*,

$p_{i,t}^D$ is the price charged by firm *i* if it chooses to defect from the collusive agreement,

$p_{i,t}^C$ is the price charged by firm *i* in the continuation equilibrium after a defection by one firm,

Π^i is the profit earned by firm *i* in a single period,

-i indicates firms other than firm *i*,

δ^t is the discount factor in period *t*, with $\delta^t = e^{-r\tau}$ where *r* is the instantaneous rate of time preference and τ is the real time between periods,

θ is the probability that the antitrust authorities detect the cartel,

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[2] We follow Tirole's (1988, 245-53) representation of the classic cartel dilemma (Stigler 1964).

Ω is the penalty imposed on a cartel member who does not defect, and

$\mathcal{L}$ is any legal liability associated with a leniency application (which we assume will accompany defection).

Note that this specification makes explicit the possibility of legal liability whether one colludes or defects, so we assume that $\Omega > \mathcal{L}$; that is, an application for leniency is associated with a reduction in fines. The introduction of the Department of Justice leniency policy in 1993 increases θ and decreases $\mathcal{L}$ (as well as reducing uncertainty regarding $\mathcal{L}$). This shift should have increased the likelihood of cartel breakup overall. Analogously, this policy change should decrease the likelihood of cartel formation. [3] We test each of these hypotheses below.

The model makes firm impatience a central determinant of cartel stability. Building on early work by Feinberg and Husted (1993) a small literature uses experimental design to test the impact of discount rates on collusion. Dal Bo (2005) and Bruttel (2009) both confirm the theoretical consensus that discounts rates, often proxied as the probability that the game will continue another period, matter to collusion. Sabater-Grande and Georgantzis (2002) come to a similar conclusion using risk aversion as a determinant of variation in the discount rate. Dal Bo

[3] Spagnolo (2000, 2007) shows that the impact of leniency is less obvious than the treatment here suggests. In particular, partial amnesty can increase the set of collusive equilibria and the potential collusive profits available to a cartel, making collusion easier and therefore presumably more durable. Aubert, Rey, and Kovacic (2006) discuss the relationship between the design of leniency programs and their impact on both cartel stability and firm performance. Miller (2009) discusses strategic responses to leniency. Harrington and Chang (2009) discuss the impact of leniency on both breakup and formation.

and Frechette (2011) find that players are more likely to learn to collude when the incremental profit to collusion and the probability of future interaction is large.[4]

Most of the literature assumes, for simplicity, that there is symmetry among cartel members, so that they all have the same discount rate.[5] Assuming complete and efficient financial markets, firms can borrow or lend at the market interest rate (plus any firm-specific risk premium), so that the market rate captures, and in fact influences, the common discount rate among firms. Fershtman and Pakes (2000) and Harrington (1989) provide models in which cartel duration is affected by heterogeneity in firm discount rates and financial strength. Our previous research suggests that asymmetry may be important in explaining cartel breakup (Levenstein and Suslow 2011), finding that firm-level discount rates were an important determinant of cartel breakup among international cartels in the last quarter-century. We ask here whether this result holds in a different sample of cartels.

Business cycle fluctuations affect cartel stability in multiple ways. Cartels may be more inclined to fall apart when demand declines (as in recessions), but they are also more likely to form when interest rates are low (as in recessions). The impact of declines in demand may depend on its observability.[6] Green and Porter (1984) consider unobservable shocks to demand,

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[4] For early contributions to this literature, see, for example, Roth and J. Keith Murnighan 1978; Murnighan and Roth 1983; Charles A. Holt 1985.

[5] There is a literature that focuses on the role of leaders (e.g., Saudi Arabia or U.S. Steel) in facilitating collusion and another strand that focuses on "maverick" firms as impediments to collusion (Baker 1989, 2002). Both of these strands presume that there is heterogeneity that affects collusive stability.

[6] See Levenstein and Suslow 2011, p. 478, for an overview of empirical examinations of the impact of demand fluctuations on cartel breakup. Experimental analysis in Feinberg and Snyder (2002) finds that observable demand

while Rotemberg and Saloner (1986) focus on observable shocks. In Levenstein and Suslow (2011), we find that observable fluctuations in GDP had no impact on cartel stability, while deviations from trend GDP were marginally significant (Table 6, pp. 483-4). The current study examines this issue over a much longer period with larger business cycle fluctuations.

While the theoretical literature has tried to explain cartel breakup, nothing in this theoretical approach explains why an industry that was not colluding yesterday would start colluding today.[7] Both d'Aspremont et al. (1983) and Bradburd and Over (1982) examine the conditions under which a cartel might or might not form, but there has to be an exogenous shock (i.e., a change in the fixed costs of formation) for an industry which was not colluding at a point a time to move to a new collusive equilibrium. Bos and Harrington (2010) provide an explicit model of cartel formation. Although theirs is a simple formulation (stochastic with fixed cost of forming cartel), it still an important advance in the literature.

There is a very small empirical literature on cartel formation.[8] Filson et al (2001) use interest group theory to examine the likelihood that agricultural producers will form a cartel ("marketing order" under the legal provisions for U.S. farmers to make cooperative output and

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downturns had little impact on the ability to sustain collusion, while unobservable negative shocks did undermine collusion.

[7] Part of the reason this remains a puzzle is that the theoretical literature does not generally address the question of when an industry would need to communicate explicitly in order to maintain a collusive equilibrium. Instead it asks whether collusion, either tacit or explicit, is an equilibrium. Our focus is on explicit collusive agreements.

[8] Alexander (1994) and Krepps (1997) examine the impact of changes in antitrust policy on cartel formation under the National Industrial Recovery Act. Levenstein and Suslow (2006a) summarize: "Many studies report that a cartel was formed during a period of falling prices, but this is not always, or even usually, associated with falling demand (either for the particular product or in the general economy). Instead, falling prices were often the result of entry or the integration of previously distinct markets" (p. 67). See also, Levenstein and Suslow (2011).

pricing decisions). They find that markets with less elastic demand and supply are more prone to form marketing orders. Concentration, both at the level of share of production in the state or region and among producers directly, also increased the likelihood that the industry would choose to collude. Schmitt and Weder (1998) use a case study analysis of the Swiss dye stuffs cartel to examine the impact of sunk costs and cyclical fluctuations on the incentive to form a cartel. They argue that industries that have high specific assets, such as R&D investments, will have an increased incentive to form a cartel when there is a downturn in demand, even if the industry is characterized by free entry that would deter cartel formation during normal macroeconomic periods. Potential entrants are deterred by high entry costs which cannot be justified during downturns. They note that the observed profits of the cartelized firms will be lower than prior to the downturn, despite the effectiveness of the cartel in raising profits about what they would be otherwise be. They find support for this hypothesis in the history of the formation of the dye stuffs cartel after World War I. In contrast, Andrew Dick's (1996) cross-sectional study of the formation of Webb-Pomerene Associations between 1918 and 1965 finds no association between the timing of the formation of export associations and fluctuations in export prices (215, Table 4). Comparing 125 export industries that established a W-P Association with 125 export industries that did not, he finds that cartels were more likely "to form in growing export markets where the United States had a large market share." Increases in capital intensity raised the probability of cartel formation, while increases in concentration had the opposite effect. (p. 213)

There are a variety of other factors which affect the formation and breakup of cartels, often considered under the rubric of "facilitating" practices or structural conditions. We choose

not to focus on these practices in the estimates presented here, as they are not likely to fluctuate over time or systematically with the business cycle. For example, neither the homogeneity of a good nor the transparency of a market is likely to change as a result of a recession. The analysis below takes advantage of the long time span of these data to advance our understanding of changes in the nature of competition in an industry over time.

3. Data Sample Construction and Description

We have created a new sample of cartels derived from Commerce Clearing House Trade Regulation reports. Drawing on over 1300 CCH reports, we find prosecutions of 524 distinct cartels during the period 1961-2013.[9] This sample builds on data provided by Bryant and Eckard (1991) for the early part of the period (1961-1987). [10] We collected data on all cases designated by the U.S. Justice Department as convictions under Section 1 of the Sherman Act, including those designated as price fixing, customer assignment, market division, and bid rigging.[11] In many cases, a cartel prosecution includes multiple CCH reports. There is variation over time in how indictments are reported and grouped, reflecting both changes in DOJ behavior and in CCH procedures. For the purposes of this study, a cartel is defined by one or more indictments relating to a specific product and geographic area. In some cases, a cartel was clearly local in nature and in others the evidence suggests that it was national.

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[9] Most prosecuted cartels have more than one CCH record because there are often separate convictions for different firms as well as individual defendants. We have consolidated these into cartel-level observations.

[10] We are extremely grateful to Bryant and Eckard for their willingness to share their data with us.

[11] The Byrant and Eckard sample excluded bid rigging cases. We have gone back to 1961 CCH records to collect data on the bid rigging cases, as described below. We have also added additional cases from that period that had been appealed and therefore were not included in their original sample.

This dataset builds on prior work, including Posner's (1970) landmark study of antitrust enforcement in the United States which catalogued 989 cases involving a horizontal conspiracy between 1890 and 1969. Posner, like others who have since studied U.S. price-fixing, used the CCH to create a picture of the breadth of cartel activity in the United States. Other previous research using CCH data includes Gallo et al. (2000) which coded 688 horizontal per se violations from 1955-1997 and Hay and Kelley (1974) which analyzed 65 cases between 1963 and 1972 in conjunction with internal reports of the U.S. Department of Justice. Bryant and Eckard (1991) includes all Section 1 convictions, except for bid rigging, between 1961 and 1987;[12] they identify 184 cartels during this period. [13] Miller (2009) examines 222 U.S. Department of Justice cartel indictments between 1985 and 2005.[14] While the cartels in our dataset are all U.S. convictions, the sample includes both strictly domestic cartels and cartels that have both foreign and domestic members. The latter begin to appear only after the DOJ began prosecuting "international cartels" around 1991.[15] It should be noted that, like these earlier datasets constructed from DOJ prosecutions, our data is not a random sample of cartels, but to the contrary is selected by prosecutors. It therefore reflects antitrust policies priorities that may have changed over time. The dates that we have for formation reflect the evidence that antitrust

[12] We have collected data on bid rigging convictions that are not currently included in this sample. Many of the bid rigging cases designate a specific auction as the target of the conspiracy rather than a time period during which the cartel members conspired. It is in principle possible to construct cartel duration from multiple convictions, but as different firms participated in different auctions, this is a non-trivial task.

[13] "We treat nolo contendere ("no contest") pleas as equivalent to guilty pleas and convictions in constituting evidence of a conspiracy." Bryant and Eckard (1991), 532.

[14] Our data set is, at least in principle, inclusive of all of the observations in Miller (2009).

[15] There are cases of earlier international cartel prosecutions (potash, shipping) but they are very rare and mostly pre-date World War II.

authorities were able to gather and willing to release. In many cases, the breakup of the cartel was the direct result of antitrust action, so that we expect breakup to be particularly sensitive to changes in antitrust policy which are not the primary focus of our economic model. We have no information about the cause of breakup or cartel detection in this sample.

Bid rigging prosecutions raise special considerations for data analysis because of the way that information about those conspiracies is sometimes reported. In particular, many CCH case reports for bid rigging cases identify a specific date or auction that was affected, rather than a time span for the entire conspiracy. It is possible, in some cases, to identify groups of cases that were, in all likelihood, part of an ongoing conspiracy, but there is more uncertainty and undoubtedly more measurement error, for these cartels, and thus we exclude them from the regression analysis of the determinants of duration. Due to the prevalence of bid rigging cases, however, we present descriptive statistics. They appear to have particularly pernicious effects on government procurement at local, state, and national levels, so are clearly important to understand and prevent.

Of the total 524 cartels, 195 were bid rigging and 329 were not (Table 1). We were able to identify start and end dates for 490. The average duration of collusion for these 490 was 6.15 years. There does not appear to be any difference in average duration of price fixing (6.11) and bid rigging cartels (6.22). The average cartel had five members. Again, there does not appear to be a large difference between the price fixing and bid rigging cartels.

There are a number of cartels for which we do not have an estimate of the number of members. Many of these cartels, especially in the 1960s and 1970s, involved trade associations.

The trade association was the target of the prosecution, so we do not know the number of firms involved. In many cases, it is clear that there were large numbers of member firms though presumably most were not active participants in cartel negotiations. Rather, industry associations were charged with restraining competition by promulgating rules regulating the behavior of all members. As we have argued elsewhere, industry associations can be critical to dampening the intensity of competition in less concentrated industries.[16] In recent decades, in the United States, industry associations have been much less likely to engage in such activities. In contrast, European trade associations continued to facilitate collusion, both implicitly and explicitly, well into the twenty-first century.[17]

The analysis in this paper focuses on a subsample of 247 non-bid rigging cartels for which we were able to obtain certain key variables: start and end dates, number of firms, NAICS code and industry-level financial information. Table 2 compares the sectoral distribution of cartels in the regression sample to the bid rigging cartels that are excluded from the regression analysis. Here there are differences between the bid-rigging cartels and others. About four-fifths of the regression sample was in manufacturing, while only about two-fifths of the bid rigging cases were in manufacturing. Almost a third of the bid rigging cartels are in construction, while these are rare among the price-fixing group. Virtually all segments of the economy are

[16] Levenstein and Suslow (2006a), "Another explanation for the prevalence of cartels in unconcentrated industries is the role played by trade associations. In the case studies and in contemporary international cartels, industry associations were involved whenever the number of cartel participants was large." (p. 60). See also Levenstein and Suslow (2006b), pp. 26-27, for discussion of the role of industry associations in US and European cartels over the last century.

[17] See Levenstein and Suslow (2006b), pp. 26-27, for discussion of the role of industry associations in US and European cartels over the last century.

represented in both types of cartels. While there are cartels that consist of the large, publicly traded, international firms that have made headlines in the last quarter-century, many cartels throughout the half-century covered in this study consisted of relatively small firms operating in local markets.

The average duration for this regression sample is 5.64 years (Table 3). This is very close to the duration of roughly five and a half years calculated for the cartels in Bryant and Eckard's original sample, suggesting that cartel duration has not changed over time.[18] Figure 1 presents the distribution of cartel duration for this set of cartels. This average is comparable to what has been found in previous studies of cartel duration (Levenstein and Suslow 2006a, 2011). And as in previous studies, there is considerable dispersion in cartel duration. A large number of cartels last only a year or two, but the longest-lived cartel endures 27 years. Figure 2 presents the probability of breakup as a function of the age of the cartel. Figure 3 shows the time series of cartel formation and Figure 4 a comparable series on cartel breakup.

The earliest formation in our sample was in 1955 and the last breakup was in 2011.[19] As is often the case, a large number of cartels break up very quickly, while others endure for decades. Figure 3 shows a fair amount of year-to-year variation in cartel formation. The average annual number of new cartels is 5.1. The figure shows recession years shaded in blue. We test this more formally below, but the graphical presentation does not suggest a strong relationship

[18] Calculated from data provide by Bryant and Eckard (1991).

[19] The first cartel in the full sample was formed in 1932, but was dropped for missing information.

between cartel formation and recessions. Similarly there does not appear to be any noticeable tendency for cartels to break up during recessions (Figure 4).

To capture the market discount rate, we calculate a long-term, real rate equal to the interest rate on a ten-year U.S. Treasury bond minus last year's inflation rate as measured by the Consumer Price Index. Fluctuations in these rates presumably reflect changes in borrowing costs for most firms during normal macroeconomic periods. To capture the idiosyncratic impatience of individual cartels, we use firm-level financial data from Compustat. We create two variables (both measured annually): 1) firm leverage ratio is defined as total liabilities divided by total assets, and 2) firm interest coverage is defined as available cash divided by required interest payments, which captures whether the firm has sufficient liquid resources to cover required interest payments.[20] Ideally, we would calculate these variables for each cartel member. However, most of the firms in this sample are privately held, and their financial information is not included in Compustat. Thus cartel-level leverage and interest coverage are proxied by using every firm in Compustat in the relevant four-digit NAICS category. Rather than taking an industry average, which might well be irrelevant to cartel stability, we calculate the 75th percentile of leverage and the 25th percentile of interest coverage.[21] Our hypothesis is that a previously stable cartel can be destabilized by increases in leverage or decreases in interest coverage of its financially most precarious member because they may lead to the violation of the

[20] We use these two measures in our previous study of the determinants of international cartel breakup (Levenstein and Suslow 2011) and find that they have a significant impact.

[21] Note that this differs somewhat from the measure used in Levenstein and Suslow (2011). Most of the firms in that study were publicly traded, so it was possible to obtain financial information for the specific firms in the cartel. That was not possible for this sample, so instead we rely on data on firms in the industry.

participation constraint for a cartel member. Thus while we are unable to observe the financial situation of cartel members directly, if the distribution of financial precariousness of cartel members is similar to that of publicly held firms, this proxy will capture fluctuations in the proximity of the cartel to the participation constraint.

Finally, to capture the effects of macroeconomic downturns we include two measures: a dummy variable equal to one for all years in which the NBER considered the U.S. economy to be in recession for any part of the year[22] and deviations of GDP from trend, as measured with the Hodrick-Prescott (HP) filter.[23]

4. Empirical Model and Results

We test these hypotheses by examining the relationship between the date of cartel formation or breakup and various proxies for market and industry discount rates. For breakup, we estimate a proportional hazard model, specifying the probability of cartel breakdown as a function of variables that influence the stability of collusion. The hazard rate is the probability that an event occurs (that is, the cartel ends) at time *t*, given that it has not already occurred. A proportional hazard model with a vector of covariates, **x**, can be written as

$$\lambda(t;\mathbf{x}) = \kappa(\mathbf{x})\lambda_0(t),$$

[22] Recession years are defined by the NBER business cycle reference dates, from 1857 to the present, can be found at US Business Cycle Expansions and Contractions, National Bureau of Economics Research, www.nber.org/cycles.html.

[23] The HP filter fits a smooth nonlinear trend curve to a time series by decomposing it into a nonstationary trend component and a stationary cyclical component.

where $\kappa(.)>0$ is a nonnegative function of x and $\lambda_0(t)>0$ is the underlying or baseline hazard. The baseline hazard is common to all subjects in the population. It is invariant across cartels, but can be any separable function of time. Individual hazard functions differ proportionately based on a function $\kappa(\mathbf{x})$ of observed covariates. Typically, $\kappa(.)$ is parameterized as $\kappa(\mathbf{x})=\exp(\mathbf{x\beta})$, where $\boldsymbol{\beta}$ is a vector of parameters and $\exp(x\boldsymbol{\beta})$ is a shift factor that depends on economic variables. Taking logs of both sides yields:

$$\log \lambda(t;\mathbf{x}) = \mathbf{x\beta} + \log \lambda_0(t),$$

where β_j measures the semi-elasticity of the hazard with respect to x_j.

We are interested in how the covariates shift the hazard function, in which case the estimation of λ_0 is not necessary. We estimate the probability that a cartel that has lived to year $t - 1$ breaks up in year t as a function of the parameter vector $\boldsymbol{\beta}$ which includes characteristics of the cartel, the market, and the economic environment in year t. A coefficient greater than one indicates that the event (cartel end) is more likely to occur, and a coefficient less than one indicates that it is less likely.

Our estimates of the determinants of cartel breakup are reported in Table 4. Unlike our earlier work on international cartels (Levenstein and Suslow 2011), increases in the market interest rate significantly increase the probability of cartel breakup. If one compares the time series pattern of cartel breakups (Figure 4) with the graph of real interest rates over the same period (Figure 5), they have a similar pattern with the exception of the spike in cartel breakups in the mid-1970s, when nominal interest rates were high, but high inflation, especially high oil

prices, meant that the real interest rate was negative. This is consonant with the message from the repeated game literature that firms must be sufficiently patient in order to maintain collusion. Neither of the measures of firm-specific financial fragility, leverage and interest coverage, is statistically significant. In our earlier work on international cartels, only proxies of firm-specific discount rates, in particular interest coverage, affected cartel stability. It may be that the longer time span of this sample, over which there was much greater variation in market interest rates, allows us to observe their impact on firm impatience and cartel stability. It is also possible that our reliance on industry level measures of leverage and interest coverage do not capture fluctuations in firm-level financial precariousness for cartel members, so that the market interest rate remains relevant in explaining firm discount rates. Finally, breakups in these data are largely driven by prosecutorial trends. In the post-amnesty period, we would expect applications for leniency to be driven by economic factors, but it is not surprising that these measures of firm-specific impatience are less salient in the pre-amnesty period.

We do not find any relationship between the business cycle and cartel breakup. The coefficient on the HP Gap variable in Table 4 is not significant. In estimates not reported here, we replaced the HP Gap variable with a discrete measure of whether there was a recession during the year, based on the NBER classification, but it was also statistically insignificant. This is consistent with the graphical representation discussed above (Figure 4), and reinforces earlier findings that business cycles fluctuations are not systematically related to cartel stability.

Finally, the estimates suggest that the Department of Justice's adoption of a stronger amnesty policy in 1993 is not associated with a change in cartel breakup. This finding suggests that, when one looks at the impact of amnesty policy with a long-term perspective, it may not

have led to an increase in the probability of breakup. How does one reconcile this with our previous research and that of others (e.g., Miller 2009)? One explanation is that the "success" of amnesty in generating certain kinds of cartel cases has led to a shift in enforcement resources. In the decades prior to the adoption of the amnesty policy, there were waves of enforcement that targeted different types of conspiracies. Figure 4 indicates that there were many cartels broken up by the authorities during the 1970s and 1980s. These cases were more likely to target local or regional cartels of small firms, for example in highway construction and other local government procurement. In the post-amnesty period, international cartels of large multinationals were more frequently the defendants in cartel cases, and local cartels were less likely to be discovered through active antitrust enforcement.[24]

We also use these data to provide initial estimates of the determinants of cartel formation. We estimate the number of births in each year in each 2-digit NAICS first in an OLS specification (Table 5a) and then with a Poisson regression (Table 5b). We offer these estimates as a spur to future research, both theoretical and empirical, on cartel formation. Understanding cartel formation is challenging because it always hidden, but is important in evaluating any deterrent effects of the increased anti-cartel enforcement efforts of the last two decades. We estimate formation using the same variables as in the breakup estimates because there is not a theoretical literature on cartel formation that would suggest a distinct set of variables. Where there was more than one cartel in a 2-digit NAICS, the leverage and interest coverage variables

[24] Harrington and Chang (2012) examine the impact of leniency on enforcement when the antitrust authority has limited resources.

have been averaged across the 4-digit industry group. Leverage and amnesty seem to be the most salient determinants of cartel formation. Increases in leverage are associated with a statistically significant decline in cartel formation. While not statistically significant, the point estimate on the amnesty variable suggests a decline in cartel births. There is no statistically significant effect of interest coverage, market interest rates, or HP Gap.

5. Conclusion

This paper presents analysis of a new dataset of U.S. Department of Justice convictions of price fixing cartels from 1961 through 2013. Average cartel duration is similar to estimates in earlier studies, between five and six years. The empirical results on cartel breakup provide the strong evidence consistent with the pervasive theoretical result that market interest rates should affect cartel stability. A preliminary empirical analysis of cartel formation is also presented. As one would expect, increased cartel-level leverage is associated with significantly lower cartel formation. In contrast to cartel breakup, lower market interest rates do not appear to facilitate cartel formation. Our results provide some support for the U.S. Justice Department's now much emulated amnesty program. While amnesty policy is not associated with an increased probability of cartel breakup, the cartel formation results suggest the policy has had a deterrent effect on the formation of new cartels.

There are many caveats to this analysis. First, the dataset relies on cartel prosecutions; both start and end dates come from government cases and reflect the evidence available to prosecutors at the time and the resulting negotiated plea agreements. Both the number and the industry composition of prosecutions may thus reflect changes in policy that are not measured here. Second, the measures of idiosyncratic cartel impatience are based on data for all publicly

traded firms in the cartelized industry, not the firms that belonged to the cartel. While these data challenges pervade empirical research on cartels, the results presented here provide new insights and suggestions for further research in this area.

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Table 1

U.S. Department of Justice, Sherman Act Section 1 cases, 1961-2013

	All cartels	Price fixing cartels only	Bid rigging cartels only
Number of cartels	524	329	195
Number of members			
Mean	4.94	4.79	5.18
Median	3.5	4	3
Minimum	2	2	2
Maximum	37	27	37
Observations	462	287	175
Cartel Duration			
Mean	6.15	6.11	6.22
Median	5	5	5
Minimum	1	1	1
Maximum	41	41	36
Observations	490	305	185

Note: Observations indicate number of cartels used in estimate. The number varies because of missing data.

Table 2

Sectoral Distribution of Cartels, 1961-2013

		Regression sample		Bid Rigging cartels	
2-Digit NAICS Code	**Industry Definition***	**Frequency**	**Percentage**	**Frequency**	**Percentage**
21	Mining, Quarrying, and Oil and Gas Extraction	7	2.83%	0	0%
22	Utilities	0	0%	2	1.56%
23	Construction	2	0.81%	41	33.59%
31-33	Manufacturing	196	79.35%	47	36.72%
42	Wholesale Trade	4	1.62%	10	7.81%
44-45	Retail Trade	10	4.05%	4	3.13%
48-49	Transportation and Warehousing	15	6.07%	2	1.56%
51	Information	4	1.62%	6	4.69%
52	Finance and Insurance	1	0.40%	1	0.78%
53	Real Estate and Rental and Leasing	1	0.40%	4	3.13%
54	Professional, Scientific and Technical Services	0	0%	1	0.78%
56	Administrative, Support, Waste Management and Remediation Services	2	0.81%	5	3.91%
62	Health Care and Social Assistance	2	0.81%	0	0%
71	Arts Education and Recreation	0	0%	1	0.78%
72	Accommodation and Food Services	1	0.40%	1	0.78%
81	Other Services (Except Public Administration)	2	0.81%	1	0.78%
92	Public Administration	0	0%	2	1.56%
Total		247	100%	128	100%

Source: U.S. Department of Justice Sherman Act Section 1 cases, 1961-2013

Table 3

Descriptive Statistics: Regression Sample

Variable	Units	Obs	Mean	SD	Min	Max
Duration	Years per Cartel	247	5.64	4.34	1	27
Cartel births by NAICS	Number of new cartels per 2-digit NAICS per year	150	1.65	1.09	1	6
Cartel births per year	Number of new cartels per year	55	4.49	2.91	0	12
Number of Cartel Members	Firms per Cartel	247	4.65	3.51	2	27
Industry Leverage (75th Percentile)	Firm Average by NAICS Code by Cartel-Year	1392	0.61	0.17	0.14	2.02
Industry Interest Coverage (25th Percentile)	Firm Average by NAICS Code by Cartel-Year	1392	2.59	17.53	-261.61	90.24
Real interest rate	10-year US Treasury bond rates - Last year's annual rate of CPI inflation	1392	2.62	1.92	-2.51	9.26
NBER Indicator	Equals 1 if Recession Year	247	0.37	0.48	0.00	1.00
Deviation of GDP from trend, using Hodrick-Prescott Filter (λ=6.25)	Billions of USD per Cartel-Year	1392	0.000	0.015	-0.037	0.030
Amnesty Indicator	Equals 1 if Year Greater than or Equal to 1993	247	0.20	0.40	0.00	1.00

Notes:

U.S. Department of Justice, Sherman Act Section 1 cases, 1961-2013, excluding bid rigging cases and cases where industry, start and end dates, or number of members could not be determined.

For this set of cartels, median duration is 5 years, median number of firms is 4.

The Hodrick-Prescott filter is a weighted moving average, where the researcher must specify the weight, or smoothing parameter, commonly denoted by λ. The choice for λ recommended by Hodrick and Prescott (1997) varies with the frequency of the data. We have annual gross domestic product data, and thus we set λ equal to 6.25 (Ravn and Uhlig 2002).

Industry Leverage is calculated across all publicly listed firms in the industry by NAICS industry code and year, where Leverage Ratio = (Total Liabilities/Total Assets), and "Weighted" Leverage Multiplies by the expression by Total Assets.

Industry Interest Coverage is calculated in a similar way, with Interest Coverage = (Operating Income - Total Non-Operating Income Expense + Depreciation and Amortization) / (Interested and Related Expense). * Recession years given by NBER historical recession dates.

GDP (Real, Seasonally-Adjusted) and CPI obtained from St. Louis Federal Reserve Economic Data.

Table 4

Determinants of Cartel Breakup

Variable	Hazard Ratio	Std. Err.	z	P>z	[95% Conf.	Interval]
Leverage (75^{th} percentile)	1.59	0.60	1.23	0.22	0.76	3.32
Interest coverage (25^{th} percentile)	1.00	0.00	-1.45	0.15	0.99	1.00
Real interest rate	1.08	0.04	2.15	0.03	1.01	1.15
HP Gap	0.08	0.35	-0.56	0.58	0.00	680.27
Amnesty	1.08	0.18	0.48	0.63	0.78	1.50

N = 1392 observations on 247 cartels

U.S. Department of Justice, Sherman Act Section 1 cases, 1961-2013, excluding bid rigging cases and cases where industry, start and end dates, or number of members could not be determined.
Observation is at the cartel-year level.

Table 5a

Annual Cartel Births in Two-Digit NAICS

OLS Estimates

Variable	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
Leverage (75th percentile)	-0.77	0.26	-2.98	0.00	-1.28	-0.26
Interest coverage (25th percentile)	0.00	0.00	0.14	0.89	-0.01	0.01
Real interest rate	0.07	0.05	1.23	0.22	-0.04	0.17
HP Gap	-5.42	8.23	-0.66	0.51	-21.68	10.83
Amnesty	-0.31	0.17	-1.82	0.07	-0.65	0.03
Constant	2.01	0.27	7.47	0.00	1.48	2.54
N=150 year-2digit NAICS						

Observations are at the 2-digit NAICS/year level.

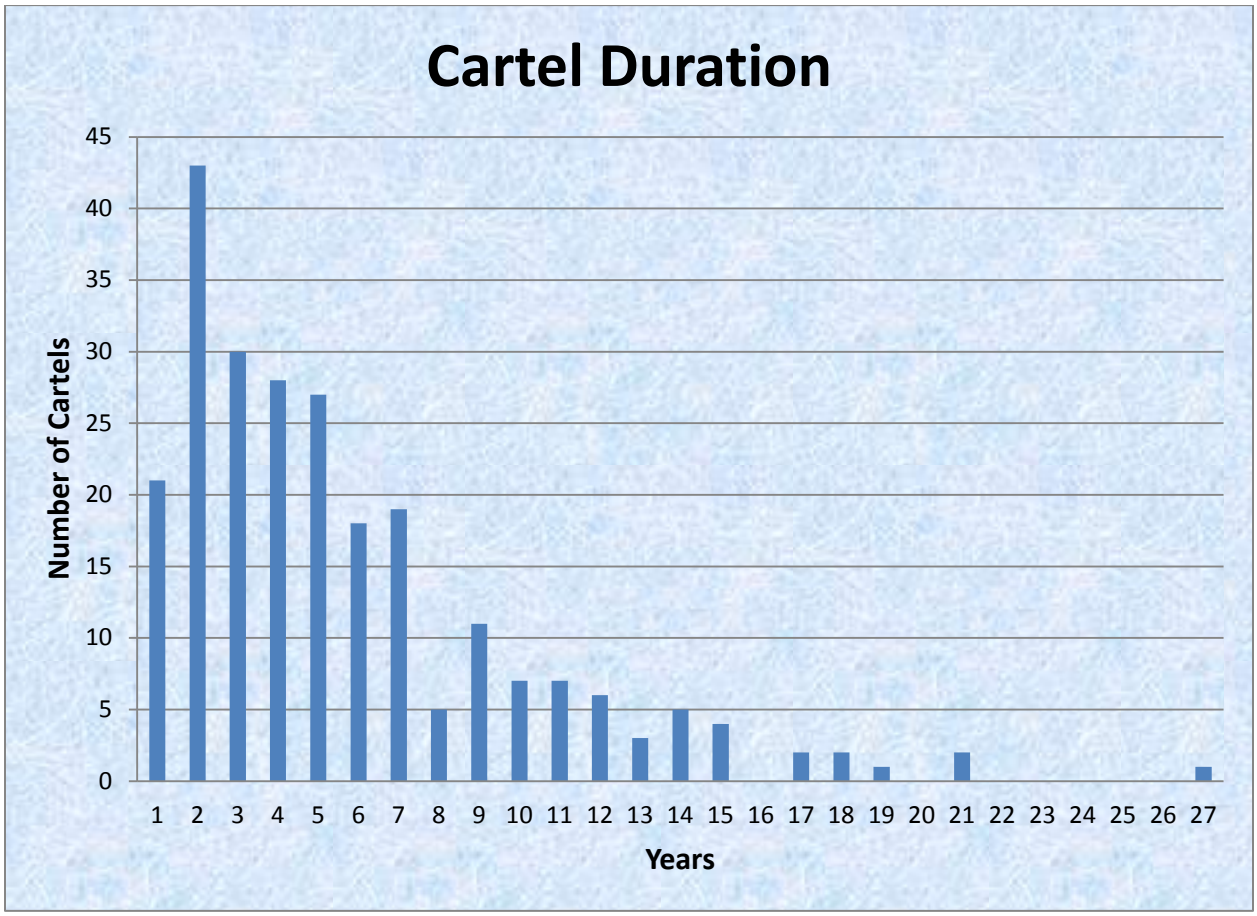
Table 5b

Annual Cartel Births in Two-Digit NAICS

POISSON estimates

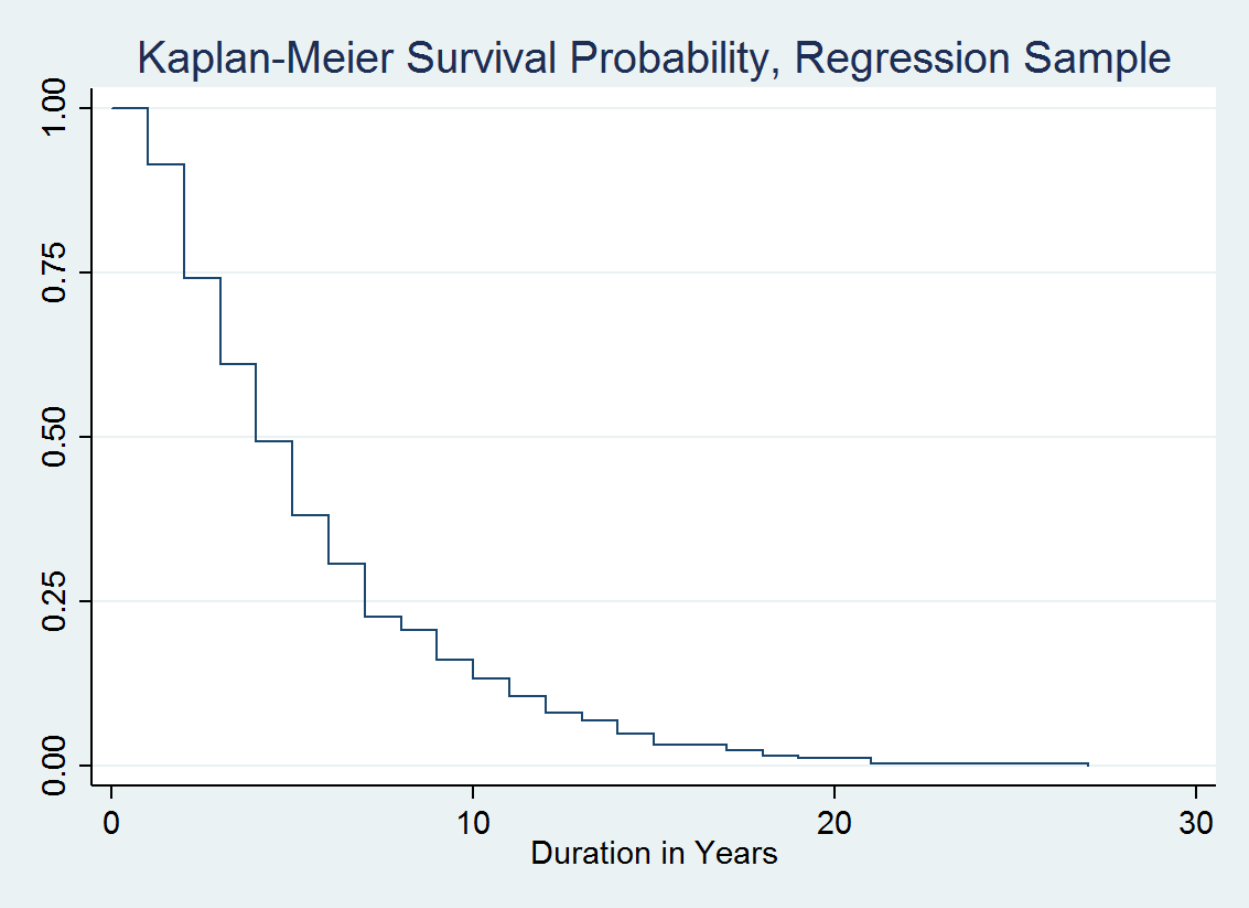
Variable	Coef.	Std. Err.	z	P>z	[95% Conf.	Interval]
Leverage (75th percentile)	-0.54	0.18	-3.11	0.00	-0.89	-0.20
Interest coverage (25th percentile)	0.00	0.00	-0.01	0.99	0.00	0.00
Real interest rate	0.04	0.03	1.38	0.17	-0.02	0.10
HP Gap	-3.13	4.73	-0.66	0.51	-12.40	6.13
Amnesty	-0.20	0.12	-1.66	0.10	-0.43	0.04
Constant	0.76	0.16	4.75	0.00	0.44	1.07
N=150 year-2digit NAICS						

Figure 1



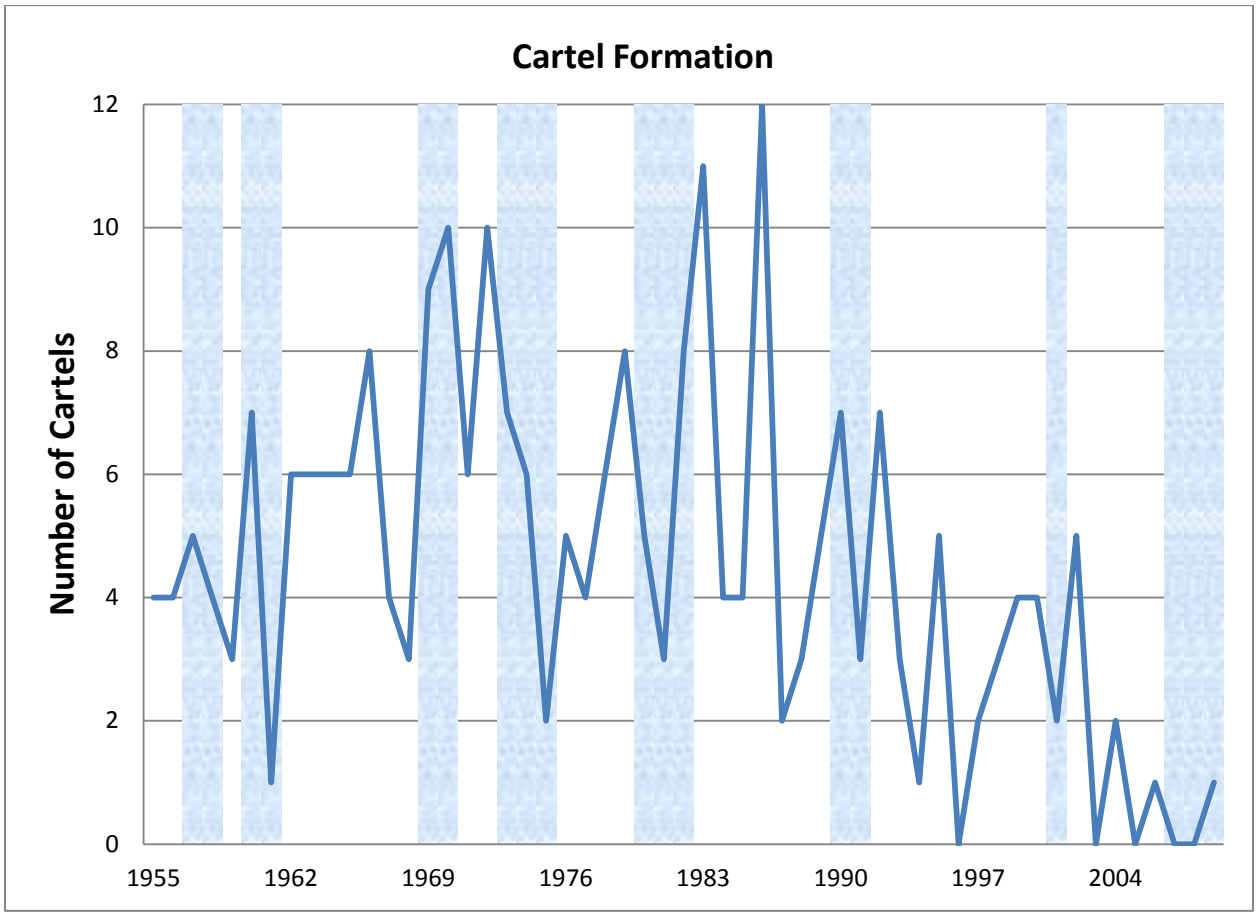
Note: 247 cartels convicted by the U.S. Department of Justice for Sherman Act, Section 1 violations, in cases brought between 1961 and 2013.

Figure 2



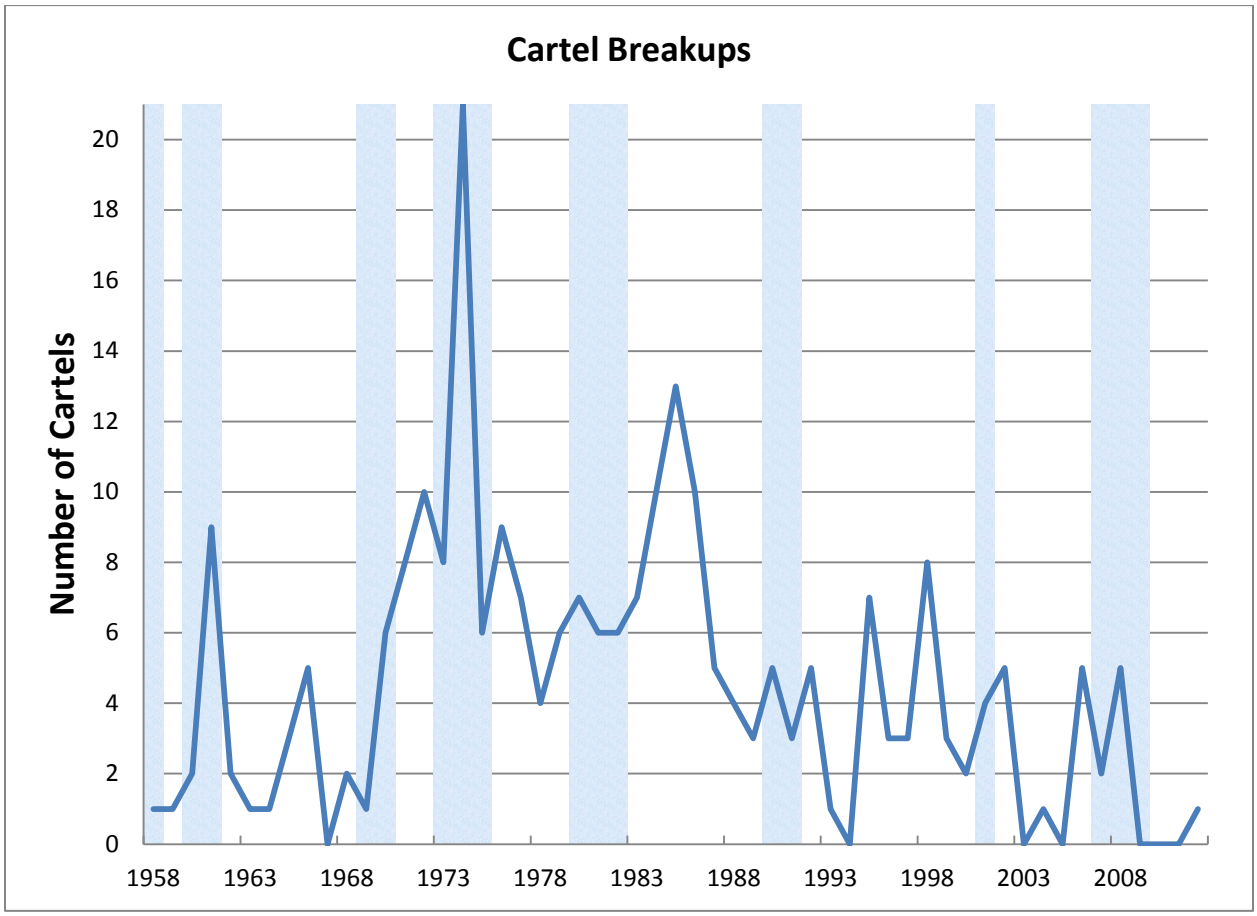
Note: 247 cartels convicted by the U.S. Department of Justice for Sherman Act, Section 1 violations, in cases brought between 1961 and 2013.

Figure 3



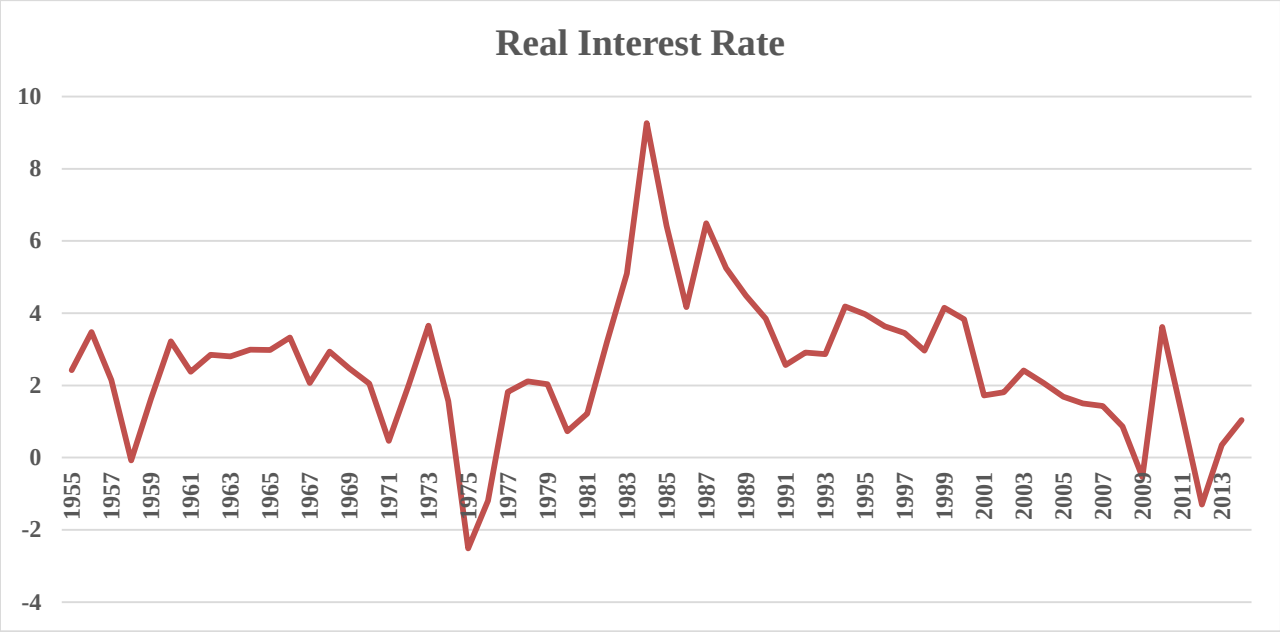
Note: 247 cartels convicted by the U.S. Department of Justice for Sherman Act, Section 1 violations, in cases brought between 1961 and 2013.

Figure 4



Note: 247 cartels convicted by the U.S. Department of Justice for Sherman Act, Section 1 violations, in cases brought between 1961 and 2013.

Figure 5


Real Interest Rate
10
8
6
4
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0
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-4
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