The Dynamics of Mergers among (Ex)Co-Conspirators in the Shadow of Cartel Enforcement*

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We investigate cartelists' merger behavior using European Commission (EC) cartel decisions over a 28-year span and information on cartelists' merger activities over the last 30 years. We find that mergers occur frequently but that they cluster in a few particular industries and usually include only a minority of the cartel participants. The EC's leniency program appears to expedite mergers, while the EC's cartel settlement procedure appears to delay mergers. After cartel dissolution, co-conspirators merge at a substantially greater pace. The results shed light on the design of merger policies and enforcement programs against cartels.

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1. Introduction

Changes in ownership and control are frequent among cartel co-conspirators: in over one-third of the cartels discovered by the European Commission (hereafter "EC") between 1985 and 2012, conspirators sold majority shares or relocated major production and sales capacities to their co-conspirators. What motivated the conspirators to merge? The EC introduced a leniency program in July 1996, with the intent of destabilizing and deterring cartels. The program gave the first-in cartel confessor the opportunity to avoid sanctions (subject to certain

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conditions). In February 2002, the program was revised to guarantee automatic immunity for first-in confessors. While before July 1996 there was, on average, one merger or joint venture in every 40 cartels per year, the rate more than doubled during the period July 1996 to June 2008. In June 2008, the EC introduced a settlement procedure through which late confessors may obtain fine reductions outside the leniency program. Following the settlement introduction, merger and joint venture rates fell below the pre-leniency levels.

This paper is one of the first to provide a systematic empirical analysis of the dynamic nature of horizontal mergers among co-conspirators, from the date that a firm joins a cartel, to the time that it mergers with a co-conspirator, and up to the firm's liquidation, bankruptcy, or cessation of production. The main objectives are to describe the distribution of mergers over time and examine the determinants of mergers. Particular emphasis is placed on the effect on merger timing of the EC's leniency program and settlement procedure and the effect of cartel termination.

Despite extensive research on mergers and cartels, the link between the two forms of industrial organization has received little attention.[1] Two notable exceptions are Bittlingmayer (1985) and Kumar et al. (2013). Using a time series of historical US antitrust cases and merger activities, Bittlingmayer examines the impact on mergers of the Department of Justice's (hereafter "DOJ") early cartel enforcement. He finds that the DOJ's enforcement, measured as the number of case filings, is significantly correlated with the number of mergers. Kumar et al. present descriptive evidence of merger activities following cartel dissolution in eight of ten largest US manufacturing industries around the introduction of the 1980 Sherman Act. Using information on 55 European Commission cartel decisions between 2001 and 2010, they find mergers, acquisitions, or joint ventures after cartel dissolution in nearly half of the sampled cases. Other related earlier work includes Mueller (1996) who reviews the history of US antimerger policy, Boone (2006) who shows that firms may use inefficient merger to relax competition constraints, and Vasconcelos (2005) who studies the effect of merger on cartel stability in the presence of cost asymmetry.

Little systematic analysis has been performed regarding the importance of cartel dissolution for the variations in merger patterns over time and across firms and antitrust policies: A glance at the sample of mergers by the EC's convicted cartelists in period December 1983 to July 1996

[1]Kumar et al. (2013) make a similar remark on the inadequate attention that this issue receives in the literature.

reveals that nearly two-thirds of the mergers occurred *before* the conspirators ceased collusion; however, the pattern is reversed after July 1996, when the great majority of mergers took place *after* a conspirator left the cartel. Probing further into the data reveals that mergers usually included only a minority of the cartel members and clustered in a few particular industries. The empirical analysis of merger by cartelists so far has largely overlooked these variations.

Our research is motivated by the relatively meager understanding of the dynamics of merger activity among cartel members. We use reduced-form Poisson regression to test whether merger rates increase following leniency introduction and whether the rates fall following the introduction of a settlement procedure. We use multiple-spell discrete-time hazard regression to test whether the leniency programs expedite merger and whether the settlement procedure delays merger. The hazard models also enable us to test the effect of cartel breakdown on merger that is suggested by the recent theory (Kumar et al., 2013). We are able to control for economic conditions (Jovanovic and Rousseau, 2002; Alvarez and Stenbacka, 2006; Thijssen, 2005; Smit et al., 2005; Mason and Weeds, 2005; Rhodes-Kropf and Viswanathan, 2004; and Toxvaerd, 2008), market structure (Hackbarth and Miao, 2011), the EC's merger prohibition, remedies and withdrawal ratios (Duso, Gugler and Szücs, 2013) and other factors that are suggested by the literature to affect merger decisions.

Our data consist of the complete set of cartel decisions issued by the European Commission (hereafter "EC") and the European Court of Justice between December 18, 1985, and October 19, 2013. We merge the cartel data with a data set of conspirators' merger, acquisition and joint venture activities (hereafter they are collectively referred to as "merger" unless mentioned otherwise) between April 1983 and December 2012. The EC's leniency introduction on July 18, 1996, and the introduction of cartel settlement procedure on June 30, 2008, provide exogenous shocks that identify the effect of cartel enforcement innovations on merger.

Our first principal finding is that the EC's leniency programs increase the frequency and pace of mergers. The impacts are statistically significant, large in magnitude, and robust to various sample choices and specifications. The result is intuitive: firms prefer collusion until the policy environment makes it harder for cartels to survive, suggesting that the incremental profit of collusion over merger may have decreased following the 1996 leniency introduction.

Our second principal finding is that the EC's cartel settlement procedure decreased merger frequency and pace. As discussed above, the procedure grants second-in or subsequent leniency applicants fine reductions outside of the leniency program. This finding is consistent with our

first finding because it suggests that a punishment differential between the first and subsequent cartel confessors is an important component in conspirators' merger decisions.

Together, these findings suggest that it is worthwhile to explore explanations that posit monopoly gain as the reason for collusion and for merger induced by changes in the mode of market competition (e.g., Hackbarth and Miao, 2011; Kumar et al., 2013).

Finally, we find that cartel breakdown expedites merger. The result is consistent with the view that ex-conspirators often try to restore the status quo by merging to lessen competitive pressures (Evenett et al., 2001; Boone 2006; Levenstein, 2013). Our results suggests that vigilance against collusion should continue after cartel breakdown.

Our analysis is subject to the limitation that almost one-third of the sampled cartels affected the US markets and were investigated by the DOJ. Anecdotal evidence suggests that the DOJ's investigation often predates the EC's.[2] Due to data availability,[3] however, we have used the event of EC inspection to identify the impact of antitrust enforcement. In this way, our identification strategy might give rise to spurious estimates of the enforcement effect. To remedy this, at least in part, in robustness checks we exclude cartels with US market coverage.

Our approach closely follows that of Davies, Ormosi, and Graffenberger (2014), who study the dynamics of merger in a sample of EC detected cartels. We focus on the same set of cartels, but extend the time coverage of the sample. We differ in the treatment of the sample: when an EC decision covers multiple products, we treat these as separate cartels, whereas Davies, Ormosi, and Graffenberger (2014) view that as one cartel, which addresses an issue that affects our treatment of the same merger occurring in more than one cartel. By viewing these as separate cartels, we are able to capture the difference in the dissolution dates.[4] The separation also makes the definition of the rivals in our sample more accurate.[5] "[T]hese are the real

[2]See also Marx, Mezzetti, and Marshall (2014), especially footnote 11.

[3]The DOJ maintains strict confidentiality regarding the schedule of its cartel investigation. Although it is possible to find data or make inferences in some cases, more commonly the starting date of an investigation is unknowable from publicly available data.

[4]For example, there can be differences by customer (for Honda, the automotive wire harnesses cartel ended in 2009 and for Nissan in 2006), by product (the Vitamin C cartel ended in 1995, while the Vitamin D3 cartel ended in 1998), or by geography (the Thread cartel ended in the UK in 1996 and in Benelux and Nordic countries in 2001).

[5]For example, the Vitamin C cartel involved Roche, Solvay Pharm, BASF and Merck. The beta-carotene and carotinoids cartels involved only Roche and BASF. But because "[t]he only producers of these products

competitors in the defined product market as they are identified by the EC in its in-depth investigations" (Duso et al., 2013).[6] Davies, Ormosi, and Graffenberger (2014) focus on post-collusion periods, which focuses the analysis on activity that occurs after cartel dissolution.[7] We also consider mergers that take place before the dissolution of the cartel.[8] By doing so we increase the power of the inference because we can rule out a declining trend in merger activity that extends back into the cartel period.

Similar to Davies, Ormosi, and Graffenberger (2014), we discuss the impact of leniency on mergers. However, we use a different identification strategy. We use the date of the introduction of leniency to identify the impact of leniency, whereas Davies, Ormosi, and Graffenberger (2014) use the cause of the investigation. We chose to focus on the date of the introduction of leniency in order to avoid concerns about causality and measurement problems. For example, conspirators may apply for leniency long after a cartel dissolves (Gärtner and Zhou 2013).[9] These delayed applications may be affected by post-breakdown mergers. Moreover, "[f]ormer price-fixers often try to effectively restore the status quo ante by merging..." (Evenett et al., 2001). In such cases, a leniency program may affect a contemplated merger (by affecting the expected stability of the follow-up conspiracy) irrespective of the cause of investigation in previous infringements.

We follow Davies, Ormosi, and Graffenberger (2014) in using a duration analysis framework and modeling mergers as recurrent events. They use continuous-time methods to estimate the

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worldwide are Roche and BASF" (see Case COMP/E-1/37.512 Vitamins), Roche and BASF could not be rivals of Solvay and Merk on these markets.

[6] In a study of EU mergers, Duso, Gugler and Szücs (2013) similarly emphasize the importance of defining rivals by using the markets identified by the EC in its antitrust investigation.

[7] As described in Davies, Ormosi, and Graffenberger (2014), this choice has advantages in terms of avoiding confounding effects associated with using cartel-period data and mitigating issues of censoring as fewer firms are available to merge over time.

[8] Our calculation based on EC decisions on 151 cartels issued over the period 1985–2013 and information on 135 mergers and joint ventures shows that over one-third of the sampled mergers and joint ventures took place before the cartel dissolved.

[9] Using information on 105 EC detected cartels, Gärtner and Zhou (2013) find that more than three quarters of the leniency applications by first-in applicants took place after a cartel breakdown; about 40 percent of the applications by first-in applicants postdated the cartel breakdown by at least a year. More than half of the applications by first-in applicants arrived after the "dawn raids", by which time the EC was already aware of the cartels' existence.

merger hazard,[10] whereas we instead use discrete-time methods.[11] This allows us to address the issue of "ties." A large number of conspirators in the EC decisions participated in multiple cartels whose product markets differ.[12] When a merger involves such multi-cartel conspirators, survival times may be "tied," i.e., the merger spells have the same duration across the different cartels. The discrete-time specification gives consistent estimates in the presence of ties, whereas in a continuous-time specification, the presence of ties causes biased coefficient estimates and standard errors (Cox and Oakes, 1984, p.99; Kalbfleisch and Prentice, 1980, p.75). As another difference in methodologies, Davies, Ormosi, and Graffenberger (2014) fit models with a Weibull baseline hazard, despite lack of theoretical support for any particular shape. We fit a model involving flexible step-function approximations to the baseline hazard function where the shape of the function is not imposed a priori but determined by the data. By doing so, we hope to avoid the problem of inconsistent estimates that parametric specifications generate when the assumed baseline hazard is incorrect (Meyer, 1990, p.769).[13]

In our regression model,[14] we augment the set of variables in Davies, Ormosi, and Graffenberger (2014) to include variables for macroeconomic conditions. The existing literature on merger and cartel suggests that economic fluctuations can be important determinants of merger decisions (Jovanovic and Rousseau, 2002; Alvarez and Stenbacka, 2006; Thijssen, 2005; Smit et al., 2005; Mason and Weeds, 2005; Rhodes-Kropf and Viswanathan, 2004; and Toxvaerd,

[10]Other application of the continuous-time methods in the industrial organization literature include, among others, Zimmerman and Connor (2005), Levenstein and Suslow (2006, 2011), Brenner (2009) and Zhou (2012), who study the dynamics of cartel dissolution and Gärtner and Zhou (2013), who study the timing of leniency applications by cartel confessors.

[11]See Willett and Singer (1995), which illustrates the technique within a logistic regression framework. There are several competing methods for discrete-time duration modelling including mixed Poisson models (Nagin and Land 1993), log-linear models (Vermunt, 1997), multilevel ordered multinomial regression (Hedeker et al., 2000), and discrete-time Markov chain models (Masyn, 2008; and Langeheine and van de Pol 1990).

[12]Our calculation based on the EC cartel decisions issued over the period 1985-2013 shows that 129 firms— about 18 percent of the total firms— participated in cartels on different product markets.

[13]The advantages of adopting a flexible functional form for the baseline hazard in analyzing time to event data have been widely recognized. See, e.g., Kalbfleisch and Prentice (1980), Meyer (1990), and Perperoglou (2005).

[14]The regression model presented here belongs to the large class of duration models (e.g., Allison, 1982; Blossfeld et al. 1989; Cox, 1972; Heckman and Singer, 1984; Kalbfleish and Prentice, 1980; Laird and Oliver 1981; Miller, 1981; Prentice and Gloeckler 1978; Singer and Willett 1993, 2003; Willett and Singer 1993, 1995).

2008) and firms' choice between competition and collusion (e.g., Rotemberg and Saloner 1986; Haltiwanger and Harrington, 1991; Harrington and Chang, 2009; Zimmerman and Connor, 2005; Levenstein and Suslow, 2006, 2011; Zhou, 2012). By including controls for the economic conditions, we hope to avoid an omitted variable bias in our analysis of merger timing by (ex)conspirators. Comparisons of our regression results with and without controls for the macroeconomic variables suggests value in including the additional variables.

Hueschelrath and Smuda (2013) test the effect of cartel breakdown on merger rates in the *post-leniency period* (1997–2013). Their empirical strategy differs from ours because their sampled mergers relate not to the (ex)conspirators alone but to all the firms in a NACE-3 or -4 digit industry to which a cartel-affected market belong. Inference in their framework may be frustrated by measurement problems because the majority of their sampled NACE markets lack cartel discoveries. For these markets, Hueschelrath and Smuda (2013) distinguish the treatment and control groups by using the dates of cartel breakdown in markets with cartel discoveries. It is not clear why merger decisions in a market might react to the cartel breakdown in a different market. Our approach may have advantages to the extent that the counterfactual is more cleanly observed. Moreover, Hueschelrath and Smuda's (2013) sample excludes mergers before the 1996 leniency introduction and precludes an analysis of the leniency's impact. The impact is studied in detail here. Finally, Hueschelrath and Smuda (2013) do not isolate the effect of cartel breakdown on merger from the confounding influences of cartel enforcement and economic fluctuations. These influences are isolated in the present paper by using multivariate regression.

Other related empirical work includes that of DiCenso (2005), Kastrinaki and Stoneman (2012), Jopp (2011) and Erdogan (2012), who analyze in continuous-time hazard frameworks the relationship between merger timing and various firm characteristics and financial factors.

It should be emphasized that our explanations do not provide a general theory of merger. Certainly, there are factors other than cartel policies, market competition and the other variables discussed here that influence merger decisions. Our primary goal is to show that a plausible foundation can be constructed for the view that changes in antitrust policy can cause large time, firm, and industry variations in merger activity. Our paper is not an attempt to explain horizontal mergers in general, and we do not rule out the possibility that some mergers would have been formed eventually even if the EC's leniency programs had never been in place.

The paper is organized as follows. Section 2 describes the data. Section 3 characterise the

distribution of mergers and JVs over time and examine the factors that influence conspirators' decision to merge. In particular, Section 3.1 examines the evolution over time of the frequency of merger and the elimination of competitors via merger, and Section 3.2 studies the pace of merger at cartel-level. Our sample of mergers and JVs by members of discovered cartels provides evidence that the merger dynamics are significantly and strongly affected by anti-cartel enforcement. Concluding remarks and possible extensions follow.

2. Data

2.1. Data Source

The primary data for our analysis are the EC cartel decisions and judgments on cartels by the Court of First Instance and the European Court of Justice from December 1985 to July 2013. The data contain a rich variety of firm-specific and cartel-specific information, including, among others, whether a conspirator underwent a merger or JV, the identity of the acquirer and the target, the date of the transaction and the percentage of ownership change, the degree of firms' involvement in a JV, the dates that a firm joins and leaves a cartel, and the date that the EC launches a surprise inspection at the firm's premise. In some cases, the EC reports firms' market shares or ranges of market shares near the end of an infringement. These are the key variables of interest in this paper.

Moreover, we search for transaction records in the time series of EC merger decisions (1990-2012) for transactions that are omitted or undisclosed from the cartel decisions. The EC merger decision data set includes all mergers and JVs where an Article 6(1)a, 6(1)b or 6(1)c decision was taken, provided that the documentation has become publicly available by December 31, 2012.[15] The merger data contain, among other things, the dates of merger notification and approval, the percentage of ownership change during a merger, and the degree of a firm's involvement in a JV. Furthermore, we search for transaction records from the following sources: (i) the national competition authorities; (ii) the conspirators' annual reports, press releases, investor information, and company histories, etc.; and (iii) business and financial intelligence such as Bloomberg and Mergerstat Review.

[15]The Article 6(1)a decisions pertain to concentrations that do not fall within the scope of the Merger Regulation; the Article 6(1)b decisions pertain to concentrations that do not "raise serious doubts as to their compatibility with the common market"; the Article 6(1)c decisions pertain to concentrations that raise serious doubts (and therefore proceed to Phase II).

2.2. The Sample

Rules of Selection The EC cartel data set currently includes 151 cartels. We select cartels for which we could verify whether a merger or JV occurred. We excluded 11 cartels due to this restriction.[16] To examine the effects of cartel investigations, we restrict the sample to cartels for which the date of the initiation of the EC's investigation is known. We drop one cartel due to this restriction. From the remaining cartels, we find 129 mergers and 18 JVs.

To estimate the probability of merger and JV over time, we consider mergers and JVs that are successful, result in a completed transaction, and for which the date of transaction and the degree of ownership and control changes are known. Moreover, because our analysis concerns the transfer of corporate decision rights, we consider only mergers and JVs that meet one of the following criteria: (i) the acquiring conspirator ends up with more than 50% of the shares of the acquired co-conspirator (or subsidiary), and the acquiring conspirator controlled less than 50% of the shares of the target before the announcement; (ii) two (or more) conspirators jointly acquired control of a non-conspirator that did not participate in the cartel prior to the announcement; (iii) a non-conspirator acquired more than 50% of the shares from two (or more) conspirators; (iv) a JV was set up to carry out more than 50% of the sales or production of at least one of the conspirators, and the JV carried out less than 50% of the conspirator's activity before the announcement.

Our requirements yield a sample of 135 successful transactions (118 mergers and 17 JVs). They were undertaken by 187 firms from 55 cartels. We refer to these cartels as our *cartel-level merger sample*. The first transaction took place in December 1983 and the last in October 2012.

There were 84 cartels that did not go through a merger or set up a JV, or went through a transaction but failed criteria (i)-(iv). The estimation of merger timing must consider the censoring of the time-to-merger for cartels with no observed merger events. In these cases, we can only infer that the time-to-merger would have exceeded the observed time elapsed from the start of an infringement, but is censored at the time of data collection or the date of the firm's exit (because of liquidation, bankruptcy, or cessation of production). We refer to the cartel-level merger and non-merger samples together as the *cartel-level full sample*.

[16] These cartels are operated by trade associations or shipping conferences who are the addressees of the cartel decisions. The identity of the involved firms is usually undisclosed.

Variable Definition and Summary Statistics The main variables and model parameters are defined in Table 1, and the corresponding descriptive statistics are presented in Table 2. Column 1 of Table 2 gives descriptive statistics for the full sample. The remaining columns give descriptive statistics for the merger sample.

[Table 1 about here]

[Table 2 about here]

3. Empirical Analysis

The analysis starts by looking at changes in the number of mergers over time. At this step, our analysis runs at an aggregate level where mergers in different cartels and by different firms are grouped together and the only sample variation comes from changes through time. In the sections to follow, the analysis will consider cartel-level variation.

3.1. The Number of Mergers

3.1.1. Graphical Analysis

We start by graphing the distribution of merger activities over time. We create a series of six-month periods to track mergers. The periods before June 30, 2008, alternately begin on July 18 and January 18, so that they match the introduction of the EC's leniency program on July 18, 1996. The periods after June 30, 2008, alternately begin on June 30 and December 30, so that they match the introduction of the settlement procedure.

Figure 1 plots the total number of mergers and JVs per period. We have information for 87 periods. The vertical bars represent the introduction of the leniency program on July 18, 1996, and the introduction of the settlement procedure on June 30, 2008, respectively. The number of mergers or JVs does not evolve monotonically through time, but rather increases sharply after July 1996 when the EC leniency program was introduced, and falls in recent years. In our tests, we often use time-varying predictors (e.g., cartel policies, macroeconomic conditions, etc.) to take into account these changes.

[Figure 1 about here]

A potential caveat to the analysis above is that the time-varying pattern of merger rates in Figure 1 may be driven by the arrival of new cartels over time, with the latter periods including

more sampled cartels than the earlier periods. To see if this is the case, Panel A of Figure 2 plots the ratio of the total number of mergers and JVs in a period over the total number of sampled cartels in each period. The pattern is similar to that in Figure 1 and shows that the probability of a merger between July 1996 and June 2008 is significantly larger than the probabilities in the pre-leniency and post-settlement periods. Moreover, the high merger rates around the periods 1983 and 1989 should be regarded as outliers rather than the norm. In 24 out of the 28 pre-leniency periods, merger probabilities are below 0.06. Panel B plots the ratio of mergers and JVs over the number of sampled firms in each period. The pattern is similar to that in Panel B.

[Figure 2 about here]

3.1.2. Regression Analysis

A. Poisson Model

We use a reduced-form Poisson regression to test the effects of policy changes on merger numbers. In the model, the probability of observing that random variable Y_t, the number of mergers and JVs in period t, has the realization y_t is given by

$$\text{Prob}(Y_t = y_t \mid \mathbf{x}_t) = \frac{\exp(-\lambda_t)\lambda_t^{y_t}}{y_t!}, \qquad y_t = 0, 1, 2, ...,$$

where the conditional mean λ_t is specified as $\lambda_t = \exp(\mathbf{x}_t'\beta)$, where $\mathbf{x}_t$ is a vector of regressors and β is a vector of parameters, measuring the regressors' impact. Our main regressor, POLICY, is a categorical variable that indicates the policy environment where mergers occur. The categories are PRE-LENIENCY for the periods before July 18, 1996, LENIENCY for the periods between July 18, 1996, and June 30, 2008, and SETTLEMENT for the periods after June 30, 2008.[17]

We run two statistical tests. First, we examine whether the number of mergers and JVs increases following the introduction of leniency. The hypothesis seems plausible provided that the leniency programs have the intended effect of destabilizing cartels. When tension is created between the conspirators following an effective antitrust innovation (such as leniency), they face an increased need to facilitate coordination through eliminating competitors (in both an

[17]Valid inference from a Poisson model requires equidispersion, that is, equality of conditional variance and mean. For robustness, we run a negative binominal model and find that the results regarding the effects of leniency and settlement are similar to those obtained from the Poisson regression and that the dispersion parameter is close to zero.

ongoing infringement and a follow-up infringement after a cartel breakdown). The hypothesis is also plausible provided that leniency has the intended effect of deterring cartels. The increased competition intensity following the leniency introduction provides the ex-conspirators with an incentive to enlarge capacity through a merger so that they can compete more effectively against the rivals. Taking LENIENCY as the omitted category, the regression model generates an increase in mergers and JVs if the PRE-LENIENCY coefficient is negative. We therefore test the hypothesis:

$$H_0 : \beta_{PRE} \geq 0 \text{ versus } H_1 : \beta_{PRE} < 0,$$

where β_{PRE} denotes the PRE-LENIENCY coefficient. In the second test, we examine whether the number of mergers and JVs falls following the introduction of the settlement procedure. Because settlement gives late cartel confessors fine reductions outside of the leniency program, it may have the effect of weakening the effectiveness of the leniency program. Therefore, we hypothesize that the need for eliminating competitors through merger decreases following the settlement introduction. Taking LENIENCY as the omitted category, the regression model generates a decrease in mergers and JVs if the SETTLEMENT coefficient is negative. We therefore test the hypothesis:

$$H_0 : \beta_{SETTLEMENT} \geq 0 \text{ versus } H_1 : \beta_{SETTLEMENT} < 0,$$

where $\beta_{SETTLEMENT}$ denotes the settlement coefficient.

B. Regression Results

Table 2 reports the main regression results of the effect on the number of mergers and JVs of the antitrust policies we consider. In each regression from columns 1 to 4, the unit of observation is a six-month period and the dependent variable is the number of mergers and JVs. Column 1 includes only the policy indicator. Columns 2 adds the total number of sampled firms and cartels by the end of each period. Column 3 adds the macroeconomic variables. We have information on the macroeconomic fluctuations for only 84 periods. Column 4 includes all the control variables.

Starting with the effect of leniency introduction, the estimated PRE-LENIENCY coefficients are negative and statistically significant, suggesting that the introduction of the leniency program resulted in increased merger activity. The coefficients in the first row of columns 1 to

4 correspond to 4.47 times, 82.6 percent, 4.61 times, and 1.34 times increases in the number of mergers, respectively, when evaluated at the mean of the control variables.

Turning to the effect of settlement procedure introduction, the estimated SETTLEMENT coefficients are negative and statistically significant, suggesting that the introduction of the settlement program resulted in a decreased merger rate. The coefficients in the second row of columns 1 to 4 correspond to 3.22, 1.79, 4.76, and 3.08 times decreases in the number of mergers, respectively, when evaluated at the mean of the control variables.

In addition, the results lend some support for the theoretical findings that merger activity is linked to macroeconomic fluctuations.

Column 5 uses a 12-month period. The estimated policy coefficients remain statistically significant and have the same signs as those in the six-month period cases.

3.1.3. Robustness Checks

Our empirical strategy is implemented using a before-and-after comparison across groups where the date of leniency introduction, July 18, 1996, is viewed as an exogenous shock to identify the impact of leniency. The date of settlement introduction, June 30, 2008, is used to identify the impact of settlement procedures. One concern is that if alternative shocks, i.e., placebo policies, yield a better fit to the data, then the link between the antitrust policies and the time series of mergers might not be causal. To investigate, we re-run the regression in column 4 of Table 2 using alternative time breakpoints and compare the maximized log-likelihoods across the different specifications.[18]

Each point in the graphs of Figure 3 corresponds to the maximized log-likelihood of one Poisson regression. The point 0 on the horizontal axes marks the maximized log-likelihood when we impose the shocks at the dates of the leniency introduction. The points to the left (resp. right) of 0 represent the log-likelihoods when we impose the shocks before (resp. after) July 18, 1996. Panel A uses six-month periods. The maximized log-likelihood generated by leniency (-99.286) is greater than those generated by all but two placebo policies that postdate July 18, 1996. The offending placebo policy shocks are caused by a sharp increase in the number of mergers in the third and fourth periods after July 18, 1996. Panels B uses twelve-month periods. The results are largely similar to those in the six-month case. In the twelve-month

[18]Miller (2009) runs a similar test on the effects of US leniency programs against placebo policy shocks.

case, the model fit generated by leniency is greater than those generated by all but one placebo policy that postdates July 18, 1996.

Figure 4 plots the log-likelihoods produced by imposing shocks before and after the introduction of the EC's settlement procedure. When six-months periods are used (Panel A), the maximized log-likelihood produced by the prior leniency policy is greater than those produced by all but one placebo policy that predates the prior leniency policy by two years. The results are similar when three-month periods are used—there are only two offending placebo policies. Overall, these robustness checks provide some support for the empirical specification.

3.2. Cartel-Level Analysis of The Pace of Merger

3.2.1. Descriptive Statistics

Most of the sampled cartels do not experience a merger (61.2%), although it is not rare to observe cartels with more than one mergers (23.7%). In particular, among those with mergers, nearly 40% had at least three mergers, 16% have at least four mergers. At this stage of the cartel-level analysis, we organize our data in such a way that each of the n cartels in our sample contributes to a *Cartel-Event* data set in which there are N (with $N > n$) lines of data—one line for every transaction or censoring event for which each cartel was observed. For example, if cartel j has m_j merger events, then it has $m_j + 1$ records (lines) of data, where each of the m_j records corresponds to a merger or JV, and the last record corresponds to the censoring event (i.e., data collection).

3.2.2. Graphical Analysis

Figure 5 plots the distribution of mergers around the time of cartel breakdown. As shown, mergers are infrequent long before and after cartel breakdown. The likelihood of mergers peaks around the time of the cartel breakdown. One-third of the sampled mergers occurred in the three years before the cartel breakdown. Over one-third of the sampled mergers occurred in the three years following the cartel breakdown.

[Figure 5 about here]

Figure 6 explores the effect of the 1996 leniency program on the link between mergers and cartel dissolution. Panel A includes only mergers that occurred before July 18, 1996, the introduction of the EC's leniency program. As shown, the great majority (85.4%) of

mergers in this period occurred before a firm ended its cartel involvement. The pattern changed substantially after leniency was introduced, with over two-thirds of the mergers occurring after the firms' exit (Panel B). Together, these graphical analyses suggest that the link between mergers and collusion varies across policy regimes.

[Figure 6 about here]

Next, we graph the non-parametric Kaplan-Meier hazard functions. The empirical hazard is the ratio of the number of observations that merge in a month relative to the number that had not yet experienced a merger at the start of the month.[19] These functions plot rates of merger against the *analysis time,* described below.

Let the *spell start* for the first merger in a cartel be the cartel's birth date. For the second and subsequent mergers, the spell start is the time of the preceding merger. Let the *spell end* be the earlier of the time of censoring (i.e., data collection on December 31, 2012) and the time of the present merger. The hazards do not evolve monotonically over time (Figure 7). The hazard is higher between 0 and 35 months and then again between 90 and 120 months. The first jump is probably caused by the start of the cartel or a recent merger. The second jump is probably caused by the cartel dissolution: the sampled mergers lasted on average 81 months; the majority of cartels experienced less than two mergers, if merger ever occurred. These points are discussed in more detail later. Furthermore, temporarily putting aside the issue of confounding influence from the time-varying factors, the shape of the hazard function suggests that a flexible baseline hazard specification will probably fit the data better than a monotonic baseline hazard (e.g., Weibull).

In Figure 8, the analysis time of the pre-leniency hazard profile begins at the spell start and ends at the earlier of the spell end and July 18, 1996. The analysis time of the leniency hazard profile begins at the latter of July 18, 1996, and the spell start. It ends at the earlier of the spell end and June 30, 2008. The analysis time of the settlement hazard profile begins at the latter of June 30, 2008, and the spell start, and it ends at the spell end.

[Figure 7 about here]

[19] Formally, defining the risk set in month m, R_m, as the number of spells not experiencing a merger by the start of month m, and the number of mergers in month m as S_m, the Kaplan-Meier empirical hazard is defined as S_m/R_m.

Panel A of Figure 8 depicts the merger hazards before the 1996 leniency introduction, after the leniency introduction but before the settlement procedure introduction, and after the settlement introduction. As shown, the introduction of the leniency program immediately results in a hazard profile with higher probabilities of merger. The introduction of the settlement procedure immediately result in a hazard profile with lower probabilities of merger.

A potential caveat to the analysis above is that, by design, analysis time of the leniency treatment group (i.e., spells that started after July 18, 1996) is at most 145.5 months (i.e, the time span from July 18, 1996, to June 30, 2008); but a similar selection of short analysis times does not occur to the control group (i.e., spell durations before July 18, 1996). If the immediate increase in the hazard rates following the leniency introduction were due to such a statistical artefact, one could argue that our result on the relationship between leniency and the observed hazards is driven by misspecification.

To examine whether this is the case, we censor all the spells in the pre-leniency group at month 145.5. Panel B of Figure 7 plots the Kaplan-Meier hazard estimates after the exclusion of long spells. The predicated hazards after the leniency introduction again immediately exceed the pre-leniency levels. Long after the start of the spell, the post-leniency hazards readjust and fall below the pre-leniency levels. The overall effect of leniency introduction, judging from the entire sampling range, is that it expedites merger.

3.2.3. Regression Analysis

A. Empirical Framework

The timing of a merger decision is analyzed naturally in a hazard model framework. A *spell* in this context refers to a period of time elapsed until a merger or the censoring event (i.e., data collection on December 31, 2012) occurs. Twenty-three of the sampled cartels went through multiple mergers and/or JVs. In these cases, spells may be correlated with the cluster, violating the assumption of independent spells that is required in conventional hazard regression (e.g., Weibull). To address the issue, we consider a multiple-spell hazard specification and model the repeated mergers as "recurrent events," with the spell of the first event starting at the cartel's birth and the spells of the subsequent events starting at the time of the preceding event.

In addition, 129 sampled conspirators participated in multiple cartels whose product markets differ. Failure times may be tied across cartels when a multi-cartel conspirator merge. Ties are present if the different cartels share in common two (or more) adjacent events of interest.

In particular, a non-trivial proportion, 17.4%, of the sampled observations in the Cartel-Event data set are tied. Such a data structure makes discrete-time hazard models a natural candidate for our analysis because the discrete-time methods give consistent estimates of the coefficients and their associated variance in the presence of ties.[20]

The additional advantages of the discrete-time hazard models include the following: (i) they can easily be extended to account for unobserved individual heterogeneity, even if the sample size is large; (ii) by applying discrete-time hazard models, we can easily circumvent the rather restrictive proportional hazards assumption that sometimes plagues continuous time models (e,g., the Weibull model); and (iii) they can easily handle multiple time-varying covariates, which are of particular interest for the present research.

Let T_s be the continuous, non-negative random variable that measures the length of a particular spell s. In a discrete-time framework, the core of duration analysis is formed by the probability that a particular cartel undergoes a merger within a given period $[t_k; t_{k+1})$, where $k = 1, ..., K$ and $t_1 = 0$, conditional on not having undergone a merger at the beginning of the interval and given the explanatory variables included in the regression model. This conditional probability is termed the *discrete-time hazard rate*, and its function is given by

$$h_{sk} := \text{Prob}\{T_s < t_{k+1} \mid T_s \geq t_k, \mathbf{x}_{sk}\} = F(\mathbf{x}'_{sk}\beta + \gamma_k),$$

where $\mathbf{x}_{sk}$ is a vector of potentially time-varying covariates and γ_k is a function of time that allows the hazard rate to vary across different periods within a spell. $F(\cdot)$ is a cumulative distribution function ensuring that $0 \leq h_{sk} \leq 1$. Here, the subscript s denotes separate spells. The parameter vector β is the vector of coefficients, measuring the influence of observed characteristics. The term $\mathbf{x}'_{sk}\beta$ shifts the hazard function $F(\cdot)$, and a positive coefficient indicates that the observed characteristics increase the merger hazard and reduce the time to merger after a firm joins the cartel.

For each merger spell, the last month in which a merger was observed is recorded. For censored spells, we record the last month for which the data is collected. In the following, this terminal month is denoted k_s. The subscript s indicates that it may differ across spells. We introduce a binary variable $y_{sk} = 1$ if spell s is observed to cease during the kth month, and

[20] Alternative approaches to deal with tied failure times in continuous-time hazard models have been developed in the literature, e.g., Breslow (1974). While computationally undemanding, Breslow's method will be inaccurate if there are many ties in the data set, which happens to be our case.

let $y_{sk} = 0$ otherwise. The log-likelihood for the observed data is given by

$$\ln \mathcal{L} = \sum_{s=1}^{n} \sum_{k=1}^{k_s} \left[y_{sk} \ln(h_{sk}) + (1 - y_{sk}) \ln(1 - h_{sk}) \right].$$

Assuming that $F(\cdot)$ follows a logistic distribution, the parameters can be estimated using a logit model. [21]

We reformat our data for a discrete-time survival analysis. Although initially each cartel had one record (line) of data for each event of interest, we re-organize the data so that the n spells in the original sample contribute multiple independent observations to a *Cartel-Spell-Period* data set in which there are $\sum_s k_s$ lines of data—one observation for every time period within every spell for which each cartel in the original sample was observed. Each spell period is 30 days long.

Next, we create three types of new variables for the newly created Cartel-Spell-Period data set: (a) a time period identifier, (b) the event indicator variable, and (c) time-varying covariates. The time period identifier TIME_j^t is a sequence of positive integers. It equals one during the first 30-day period after a cartel's start, two during the second period, and so on. The event indicator MERGER_j^t is a period-specific binary variable. It equals one if a merge occurs in cartel j during the period $(t - 30$ days, $t]$, it equals 0 otherwise. Finally, we create a set of time-varying policy and macroeconomic covariates to track changes in the policy and economic environments. They are described in Table 1. The descriptive statistics for the newly created data set are given in Table 5.

Our first main regressor POLICY^t is a time-varying categorical variable that indicates the antitrust policy environment at time t. The categories are PRE-LEN^t for the periods before July 18, 1996, LEN^t for the periods between July 18, 1996, and June 30, 2008, and SETTLEM^t for the periods after June 30, 2008. Our second main regressor BREAKDOWN_j^t is a time-varying dummy variable that indicates whether cartel j has collapsed by t.

We run three statistical tests. First, we examine whether the pace of merger increases following the leniency introduction. Taking LEN^t as the omitted category, the regression model generates an increase in the pace of merger if the PRE-LEN^t coefficient is negative. We therefore test the hypothesis:

$$H_0 : \beta_{PRE\ LEN^t} \geq 0 \text{ versus } H_1 : \beta_{PRE\ LEN^t} < 0,$$

[21] We obtain similar results from a probit model.

where $\beta_{PRE\ LEN^t}$ denotes the PRE-LEN^t coefficient.

In the second test, we examine whether the pace of merger decreases following the introduction of the settlement procedure. Taking LEN^t as the omitted category, the regression model generates an increase in the pace of merger if the SETTLEM^t coefficient is negative. We therefore test the hypothesis:

$$H_0 : \beta_{SETTLEM^t} \geq 0 \text{ versus } H_1 : \beta_{SETTLEM^t} < 0,$$

where $\beta_{SETTLEM^t}$ denotes the settlement coefficient.

In the third test, we examine whether the pace of merger increases following a cartel breakdown. The regression model generates an increase in the merger pace if the BREAKDOWN$_j^t$ coefficient is positive. We therefore test the hypothesis:

$$H_0 : \beta_{BREAKDOWN_j^t} \leq 0 \text{ versus } H_1 : \beta_{BREAKDOWN_j^t} > 0,$$

where $\beta_{BREAKDOWN_j^t}$ denotes the breakdown coefficient.

For robustness, we estimate the multiple spell discrete-time hazard model controlling for potentially confounding influences. Duso, Gugler and Szücs (2013) suggest that the EC's 2004 merger reform may affect anticompetitive mergers, and we create a variable that proxy this factor. ECMR04 is a dummy that indicates the merger policy regime of a spell-period. The other included variables capture, in turn, the impact of EC investigation, changes in macroeconomic conditions, and cartel and market characteristics.

B. Regression Results

Table 5 reports the cartel-level regression results of the effect of antitrust enforcement on the pace of merger. Column 1 includes the POLICY^t and BREAKDOWN$_j^t$. Columns 2, 3, 4 ad 5 alternately include ECMR04^t, INSPECTION$_j^t$, the cartel and market characteristics variables, and the macroeconomic variables. We only have 32,558 observations for the macroeconomic variables. Column 6 includes all the control variables. In all the specifications, we include 11 duration-interval specific dummies to describe the overall temporal profile of risk. Together, the interval dummies represent the effect of TIME$_j^t$ on the merger hazards. Moreover, we include, in each specification, two predictors that are associated with the previous merger episode: NUM-EX-MERGERS$_j^s$ and DUR-EX-MERGER$_j^s$. They represent the dependency of the hazard rate on the cartel's previous history and are included to relax the conditional

independence assumption inherent in the multiple discrete-time hazard model (Allison 1982, p.93).

[Table 5 about here]

Starting with the effect of leniency introduction, the estimated PRE-LEN coefficients are small and statistically insignificant in the specifications in columns 1, 2 and 3. After controlling for cartel and market characteristics and/or macroeconomic fluctuations (columns 4, 5 to 6), the PRE-LEN coefficients become statistically significant and larger in absolute value than the corresponding estimates in columns 1, 2 and 3. This results suggest that the introduction of leniency expedites merger and the effect of leniency is affected by both cartel characteristics and market and economic conditions.

Turning to the effect of settlement procedure introduction, the estimated SETTLEM coefficients are negative and statistically significant, suggesting that the settlement introduction delays merger.

Looking at the effect of cartel breakdown, the estimated BREAKDOWN coefficients in columns 1, 2, 3 and 5 are not significant at conventional levels. The coefficients become positive and statistically significant after controlling for the size of cartel membership and the market characteristics (columns 4 and 6), suggesting that mergers occur at a significantly greater pace after the cartel dissolution and the impact of dissolution is associated with and/or affected by the cartel and market characteristics.

To summarize briefly the effects of the other covariates (column (6)), we find little effect of an EC investigation, the expected demand changes (Δ GDP$_j^t$ and PEAK-TROUGH$_j^t$), the unexpected shocks (POS-SHOCK$_j^t$ and NEG-SHOCK$_j^t$), interest rates, the type of industries, the market scope (after controlling for macroeconomic fluctuations) and a cartel's merger history, but a significant decreased chance of merger for cartels with a small membership and after the 2004 EU merger policy reform.

Finally, the duration-interval-specific dummies describe the logit-hazard profile for the merger spell. The estimates suggest that chances of merger decline steadly over time. But the chances are higher relative to the trend in months 101-125 and then again in months 171-190. Notice that the corresponding standard error in the last interval dramatically increases, reflecting the decreasing precision that derives from the diminished risk set.

To this point, our discussion of the hazard model is fairly simple. We have made the baseline hazard rate a function of the explanatory variables and estimated a *single* duration elasticity.

A potential shortfall of the specification is that attribute variation, to the extent that it has any impact, is constrained to generate *parallel shifts* in the logit-hazard function.

However, it is important to consider the validity of the proportionality assumption. For example, the results of Davies, Ormosi, and Graffenberger (2014) suggest that the effect of cartel dissolution on the merger hazard may not be constant over time. The generalized model reported in the last column of Table 5 allows cartel dissolution and the EC's inspection to change both the intercept and the slope of the logit-hazard function. This is achieved by re-running the regression in column (6) of Table 5 but include an interaction between BREAKDOWN and the number of months elapsed from the cartel dissolution (TIME-AFTER-BREAKDOWN$_j^t$), and an interaction between INSPECTION and the months elapsed from the EC's investigation (TIME-AFTER-INSPECTION$_j^t$). The coefficients of the interactions gauge the effect of cartel dissolution and inspection on the slope, or duration elasticity, of the hazard function.

As in the proportional hazard specification (column (6), Table 5), the baseline coefficient of BREAKDOWN is positive, statistically significant and greater than the corresponding estimate from the simpler model. The coefficient of the interaction term is significantly negative, but small relative to its baseline coefficient. Consequently, BREAKDOWN shifts the hazard profile upwards over the relevant range of the data.[22]

There is a similar pattern associated with INSPECTION where the baseline and time-dependent effects work in opposition. The baseline coefficient of INSPECTION is positive and greater in absolute value than the corresponding estimate from the simpler models. The time-dependent coefficient of INSPECTION is negative and statistically significant. Taking together, a surprise investigation immediately results in a logit-hazard profile with a significantly larger intercept; but the time-dependent effect of inspection overcomes the baseline effect quickly. Inspection results in a hazard profile with lower probabilities of merger after approximately 48 ($\exp\{0.636/0.164\} \approx 48.3$) months, encompassing over 70 percent of the post-inspection observations.

3.2.4. Additional Robustness Checks

I. Leniency vs. Placebo Policy Shocks

[22]The time-dependent effect of BREAKDOWN does not overcome the baseline effect until TIME-AFTER-BREAKDOWN exceeds approximately 4,453 ($\exp\{1.151/0.137\} \approx 4,453$) months, encompassing all of the sampled observations.

Like in our analysis of merger frequency, our empirical strategy here is implemented using a before-and-after comparison across groups where we use the dates of policy introductions to identify the policies' impacts. Like before, our concern is that if alternative shocks, i.e., placebo policies, generate a better fit to the data, then the link between the antitrust policies and the pace of mergers might not be causal. To investigate, we re-run the regression in column 6 of Table 5 using alternative time breakpoints in the data and compare the maximized log-likelihoods across the different specifications.[23]

Each point on the graphs of Figure 9 corresponds to the maximized log-likelihood of one discrete-time hazard regression. The point 0 on the horizontal axes corresponds to the maximized log-likelihood when we impose the shocks at the dates of the leniency introduction. The points to the left (resp. right) of 0 represent the log-likelihoods when we impose the shocks before (resp. after) July 18, 1996. The maximized log-likelihood generated by leniency (-795.854) is greater than those generated by all the placebo policies.

II. Longer Pre-Leniency Observation Window

A potential caveat to the analysis in Table 5 is that the analysis time of the leniency treatment group is at most 145.5 months (i.e, the spell-periods from July 18, 1996, to June 30, 2008); but a similar restriction to short analysis times does not occur to the control group (i.e., the spell periods before July 18, 1996). If the immediate increase in the hazard rates following the leniency introduction were due to such a statistical artefact, one could argue that our result on the relationship between leniency and the observed hazards is driven by misspecification. To examine whether this is the case, we censor all the spells in the control group at month 145 and re-run the regression in column 6 of Table 5. Column 1 of Table 6 reports that the predicated hazards after the leniency introduction again immediately exceed the pre-leniency levels.

III. Potential Confounding Influences of DOJ Investigation

Over 30 percent of the sampled cartels affected the US markets. Anecdotal evidence suggests that the US cartel investigations often predate the EC investigations. In Table 5 we did not find a significant impact of cartel investigation on merger timing. To see whether this has been driven by the inclusion of US market cartels, in column (2) of Table 6 we exclude cartels

[23]Miller (2009) runs a similar test on the effects of US leniency programs against placebo policy shocks.

that affect the US markets. The INSPECTION$_j^t$ coefficient remains small and statistically insignificant.[24]

IV. Market Structure and Types of Infringement

Column (3) of Table 6 includes DISPERSION$_j$, the standard deviation of the cartel members' estimated market shares near the end of an infringement, as an additional control variable. The variable is constructed by using the EC's decision reports on cartelists' market shares near the end of an infringement.[25] We have data for 100 cartels on this variable. Column (4) of Table 6 includes the types of infringement (i.e, whether the cartel involves price-fixing (PRICE-FIXING$_j$=1 if yes;=0 otherwise), market allocation (MARKET ALLOC$_j$=1 if yes;=0 otherwise), and big rigging (BID RIGGING$_j$=1 if yes; =0 otherwise)) as additional control variables. The main point to extract from our analysis here is that the effects of leniency and settlement are robust to the inclusion of these additional control variables.

V. Random Effects Model

Column (5) of Table 6 reports the estimates from a random effects model. In this case, the odds of merger for a cartel whose unobserved (time-invariant) characteristics place it at one standard deviation beyond the mean are $(\exp(0.004) - 1) \times 100 \approx 0.4\%$ higher than for an average cartel with the same observed characteristics. However, the effect is not significant at conventional levels.

4. Conclusion

Mergers occur frequently among cartel members. This paper examines the nature and determinants of mergers among cartel members using a novel data set. Unlike previous empirical analysis of merger, our analysis explicitly recognizes the role of anti-cartel enforcement on cartels' incentive to merge. We find that the EC's leniency program expedites mergers. We find that the EC's settlement procedure discourages and delays merger. Moreover, we find that the forces behind the time series of merger change over time and across antitrust policies. The

[24]To our best effort, we are not able to find information of the dates of the DOJ's investigation initiation for the majority of cartels that affected the American markets.

[25]Sometimes, the EC reports a range of possible values of the market share. In this case, we use the mid-point of the range as a proxy of the market share.

increased efforts to increase concentration and reduce the coordination problems inherent in collusive relationships following the introduction of a more stringent antitrust policy suggest that the conspirators may have been trying to undo, at least partly, the leniency programs's adverse effect on collusive gains.

Our results may have important policy implications. Merger policies in the EU, the US, and many other jurisdictions may not adequately take into account the collusive history of merging firms. Because ex-conspirators often try to restore the status quo by merging or by taking other steps that lessen competitive pressures and raise prices, vigilance should not end with a cartel's punishment (Evenett et al., 2001). Our results imply that the authority may need a different set of policy tools to analyze the unilateral and coordinated effects of a merger in cartelized industries, including industries with low concentration.

There are many important policy issues that remain to be addressed, including the effects of different national antitrust policy changes, the effects of merger on market competition, and whether the new market structure post merger facilitates continued collusion. Given the long duration of some cartels, it would be interesting to see if some of the cartels use merger to create a new "balance" in the market to facilitate continued collusion after detection.

Table 1. Terms and Definitions

	Definition
Cartel	An agreement or a series of agreements between competing firms or associations of firms that constitutes a single infringement, according to the EC, of Art. 101 (formerly Art. 81 and Art. 85) of the EC treaty.
Merger	To be considered as a merger or a JV, the degree and nature of ownership changes should meet one of the following criteria: (i) the acquiring conspirator ends up with more than 50% the shares of the acquired co-conspirator (or subsidiary), and the acquiring conspirator control less than 50% of the shares of the target before the announcement; (ii) two (or more) co-conspirators jointly acquired control of a third firm who was not a co-conspirator until the announcement; (iii) a non-conspirator acquired more than 50% of the shares from two (or more) co-conspirators; (iv) A JV was set up to carry out more than 50% of sales or production of at least one of the conspirators, and the JV carried out less than 50% of the conspirator's activity before the announcement.
Merger date	= *the date of merger approval* by the EC (or other competition authorities); = *the date of merger becoming effective* (as reported in corporate or industry press or other sources) if the approval date is not reported in the authorities' official publications.
Censoring date	= December 31, 2012 (the date of data collection).
Variables	
TIME-TO-MERGER$_j^s$	= Number of months elapsed from cartel j's start until it experiences event s (i.e., a merger or censoring).
MERGER$_j^t$	= A dichotomous indicator of whether during the period (t-30 days, t] cartel j experiences a merge or a join venture. This is the dependent variable for our cartel level regression analysis.
TIME$_j^t$	Number of months since cartel j's start.
TIME$_j^t$-DUMMY	Duration-interval-specific dummies indicating the length of TIME$_j^t$, one for each spell interval at risk. The dummies are 1-15 months, 16-35 months, 36-55 months, 56-75 months, 81-110 months, 101-125 months, 126-150 months, 151-170 moths, 171-190 months, 191-220 months, $\geq$ 221 months.
POLICY^t	time-varying categorical variable that indicates the antitrust policy environment at time t. The categories are "PRE-LEN^t" (for the periods before July 18, 1996), "LEN^t" (for the periods between July 18, 1996 and June 30, 2008), and "SETTLEM^t" (for the periods after June 30, 2008).
BREAKDOWN$_j^t$	A dichotomous indicator = 1 if cartel j has ended by time t, = 0 otherwise.
INSPECTION$_j^t$	A dichotomous indicator = 1 if the EC has inspected cartel j by time t, = 0 otherwise.
ECMR04^t	time-varying dummy that indicates the merger policy regime at time t. = 1 for the periods that postdate May 1, 2004, = 0 otherwise.

(*continued overleaf*)

TABLE 1. (*Continued*)

	Definition
FIRMS_j	The total number of competitors in a cartel j during the cartel's entire course.
DISPERSION_j	Standard deviation of the members' market shares in cartel j.
PRICE-FIXING_j	=1 if cartel j fixed price and/or quality or restricted sales, production and/or capacity, =0 otherwise.
MARKET-ALLOC_j	=1 if cartel j divided customers or geographic markets, =0 otherwise.
BID-RIG_j	=1 if cartel j is a bid-rigging cartel, =0 otherwise.
INDUSTRY TYPE_j	Categorical variable indicating the type of industry where cartel j operates. The industry types are wholesale and retail trade; food, feed, tobacco and other agricultural products; chemicals; transport; primary material; machinery, equipment and metal products; and other products and services.
MARKET SCOPE_j	Categorical variable indicating the geographic scope of cartelized market. The scopes are national, multinational (but less than EU-wide), EEA-wide or EU-wide, and worldwide.
PEAK-TROUGH_j^t	=1 if in period t cartel j's market is in a peak-to-trough of a business cycle; =0 otherwise. If the relevant geographic market consisted of multiple economic areas in multiple countries, it is the weighted average of the indicators. The weight applied is the annual national GDP.
$\Delta\ \text{GDP}_j^t$	Annual growth rate of the real domestic product of the relevant geographic market (according to the EC) of a duration interval. If the relevant geographic market consisted of multiple economic areas in multiple countries, it is the weighted average of the rates. The weight applied is the annual national GDP.
POS-SHOCK_j^t	Positive deviation of real annual GDP from trend line (using the Hodrick-Prescott filter). If the relevant geographic market consisted of multiple economic areas in multiple countries, it is the weighted average of the deviations. The weight applied is the annual national GDP.
NEG-SHOCK_j^t	Negative deviation of real annual GDP from trend line (using the Hodrick-Prescott filter). If the relevant geographic market consisted of multiple economic areas in multiple countries, it is the weighted average of the deviations. The weight applied is the annual national GDP.
INTEREST_j^t	Annual average (real) short-term interest rates, 3-month maturity, of cartel j's relevant market at t. If the relevant geographic market consisted of multiple economic areas in multiple countries, it is the weighted average of the rates. The weight applied is the annual national GDP.
$\text{NUM-EX-MERGERS}_j^s$	The number of mergers that have occurred within a cartel prior to the current spell.
DUR-EX-MERGER_j^s	The duration of the prior spell within a cartel.

Table 2. Poisson Regression Results

	6-month period				*12-month period*
	(1)	(2)	(3)	(4)	(5)
Antitrust policy cateogries					
PRE-LENIENCY	−1.700***	−0.602*	−1.724***	−0.850**	−0.865**
	(0.240)	(0.353)	(0.240)	(0.346)	(0.387)
SETTLEMENT	−1.440***	−1.026**	−1.751***	−1.405***	−1.112**
	(0.392)	(0.418)	(0.485)	(0.501)	(0.472)
Control variables					
FIRMS INVOLVED		0.025***		0.029***	0.031***
		(0.004)		(0.005)	(0.006)
CARTELS STARTED		−0.166***		−0.197***	−0.212***
		(0.031)		(0.042)	(0.048)
Δ GDP (%)			−0.316***	−0.430***	−0.145
			(0.116)	(0.130)	(0.138)
PEAK-THROUGH (=1 if yes)			−0.487**	−0.586**	−0.240
			(0.245)	(0.259)	(0.267)
POS-SHOCK (trillion dollars)			−0.003**	−0.001	0.002
			(0.001)	(0.002)	(0.001)
NEG-SHOCK (trillion dollars)			−0.007***	−0.008***	−0.005
			(0.003)	(0.003)	(0.003)
Constant	1.322***	−1.611***	2.627***	0.008	−0.489
	(0.115)	(0.543)	(0.442)	(0.709)	(0.962)
Pseudo-R^2	0.260	0.394	0.280	0.407	0.505
Observations	87	87	84	84	42

Note.– Standard errors are robust to heteroskedasticity and are shown in parentheses. The table reports the Poisson regression results. The dependent variable is the number of mergers and JVs per period. The units of observation in columns 1, 2, 3, and 4 are six-month periods. The unit of observation in columns 5 is 12-month period. The omitted antitrust policy category is LENIENCY which includes the periods between July 18, 1996 and June 29, 2008. The category PRE-LENIENCY includes the periods that predate July 17, 1996. The category SETTLEMENT includes the periods that postdate June 30, 2008. The variable FIRMS INVOLVED is the number of firms that have joined a cartel by the end of the period; the variable CARTELS STARTED is the number of cartels that have started by the end of the period; Δ GDP is the is the annual growth rate of the real gross domestic product in the EU; PEAK-TROUGH=1 if the period is in a peak-to-trough of a business cycle, =0 otherwise. POS-SHOCK (resp. NEG-SHOCK) is the positive (resp. negative) deviation of real annual EU GDP from trend line (using the Hodrick-Prescott filter).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 3. Descriptive Statistics of The Cartel-Event Data Set

	Full Sample		*Merger Sample*	
	Mean (Std. Dev.)	Median	Mean (Std. Dev.)	Median
TIME-TO-MERGER (mon.)	181.79 (108.43)	171.98	161.29 (102.12)	148.29
SPELL-LENGTH (mon.)	120.01 (106.15)	94.13	72.62 (58.90)	62.24
FIRMS	11.20 (8.79)	8	13.76 (8.88)	11
NUM-EX-MERGERS	1.69 (2.83)	1	2.40 (3.11)	1
DUR-EX-MERGER (mon.)	29.19 (46.48)	2.24	40.86 (50.36)	17.79
PRICE FIXING (1=yes)	0.94 (0.24)	1	0.95 (0.23)	1
MARKET ALLOC (1=yes)	0.77 (0.42)	1	0.77 (0.42)	1
BID RIGGING (1=yes)	0.11 (0.31)	0	0.10 (0.30)	0
	N	%	N	%
INDUSTRY TYPE				
Wholesale & retail trade	5	1.74	2	0.99
Food, feed, tobacco & other agr. products	11	0.04	4	0.02
Primary material	49	0.17	40	0.20
Chemicals	74	0.26	47	0.23
Machinery, equipment& metal products	60	0.21	51	0.25
Transport	37	0.13	28	0.14
Textiles & paper products	25	0.09	25	0.12
Other products & services	26	0.09	5	0.02
MARKET SCOPE				
National	49	17.07	31	15.35
Multinational	43	14.98	33	16.34
EU-wide or EEA-wide	127	44.25	102	50.50
Worldwide	68	23.69	36	17.82
Number of Observations	287		202	
Number of Cartels	114		39	
Number of Firms	506		119	

Source.– Authors' calculations based on decisions on 139 cartels and information by the European Commission and judgments of the Court of First Instance and the European Court of Justice for the period 1996-2012 and information on 135 mergers and joint ventures concluded by the cartel members between 1983 and 2010.

Note.– An "observation" corresponds to a Cartel-Spell combination. For the first merger, a spell starts at the time of cartel birth and ends at the time of the merger. For second and subsequent mergers, a spell starts at the time of the preceding merger and ends at the time of the present merger. Censored spells start at the time of cartel birth (for cartels with no merger) or the last merger (for cartels with mergers) and end at the time of data collection (December 31, 2012).

Table 4. Descriptive Statistics of The Cartel-Spell-Period Data Set

Panel A: Means and Standard Deviations

	Pre-Leniency			*Leniency*			*Settlement*			*All*		
	Mean	(Std. Dev.)	Obs.	Mean	(Std. Dev.)	Obs.	Mean	(Std. Dev.)	Obs.	Mean	(Std. Dev.)	Obs.
MERGER_j^t (1=yes)	0.00	(0.06)	9,876	0.01	(0.08)	17,361	0.00	(0.03)	7,202	0.00	(0.07)	34,439
ECMR04^t (1=yes)	0.00	(0.00)	9,876	0.39	(0.49)	17,361	1.00	(0.00)	7,202	0.40	(0.49)	34,439
BREAKDOWN_j^t (1=yes)	0.29	(0.45)	9,876	0.78	(0.41)	17,361	1.00	(0.05)	7,202	0.69	(0.46)	34,439
INSPECTION_j^t (1=yes)	0.28	(0.45)	9,876	0.68	(0.47)	17,361	0.98	(0.12)	7,202	0.63	(0.48)	34,439
FIRMS	9.52	(8.53)	9,876	7.19	(5.87)	17,361	7.10	(5.85)	7,202	7.84	(6.82)	34,439
Δ GDP (%)	2.39	(1.41)	9,834	2.49	(1.14)	17,361	0.06	(2.46)	7,201	1.95	(1.86)	34,396
PEAK-TROUGH (1=yes)	0.53	(0.46)	9,858	0.38	(0.46)	17,361	0.54	(0.48)	7,201	0.46	(0.47)	34,420
POS-SHOCK ($tn)	20,261.71	(45, 951.47)	9,858	69,747.30	(152, 333.70)	17,361	44,519.84	(98, 919.05)	7,201	50,296.63	(121, 704.70)	34,420
NEG-SHOCK ($tn)	29,646.70	(51, 375.78)	9,858	33,525.79	(71, 279.15)	17,361	120,191.50	(250, 903.20)	7,201	50,546.13	(133, 316.60)	34,420
INTEREST (%)	7.77	(2.84)	8,014	3.35	(1.10)	17,349	1.21	(1.10)	7,195	3.97	(2.89)	32,558
NUM-EX-MERGERS	0.32	(1.33)	9,876	0.74	(1.75)	17,361	1.11	(2.25)	7,202	0.70	(1.79)	34,439
DUR-EX-MERGER (mon.)	8.20	(27.13)	9,876	21.56	(43.64)	17,361	32.13	(53.10)	7,202	19.94	(42.81)	34,439

(*Continued overleaf*)

TABLE 4. (Continued)

Panel B: Medians

	Pre-Leniency		*Leniency*		*Settlement*		*All*	
	Median	Obs.	Median	Obs.	Median	Obs.	Median	Obs.
$MERGER_j^t$ (1=yes)	0.00	9,876	0.00	17,361	0.00	7,202	0.00	34,439
$ECMR04^t$ (1=yes)	0.00	9,876	1.00	17,361	1.00	7,202	0.00	34,439
$BREAKDOWN_j^t$	0.00	9,876	1.00	17,361	1.00	7,202	1.00	34,439
$INSPECTION_j^t$	0.00	9,876	1.00	17,361	1.00	7,202	1.00	34,439
FIRMS	6.00	9,876	5.00	17,361	5.00	7,202	5.00	34,439
Δ GDP (%)	2.49	9,834	2.71	17,361	0.99	7,201	2.21	34,396
PEAK-TROUGH	0.82	9,858	0.00	17,361	0.90	7,201	0.17	34,420
POS-SHOCK	0.00	9,858	0.00	17,361	0.00	7,201	0.00	34,420
NEG-SHOCK	1,953.52	9,858	937.97	17,361	7,258.18	7,201	1,641.57	34,420
INTEREST (%)	7.39	8,014	3.32	17,349	0.82	7,195	3.39	32,558
NUM-EX-MERGERS	0.00	9,876	0.00	17,361	0.00	7,202	0.00	34,439
DUR-EX-MERGER (mon.)	0.00	9,876	0.00	17,361	0.00	7,202	0.00	34,439

SOURCE.– Authors' calculations based on decisions on 139 cartels and information by the European Commission and judgments of the Court of First Instance and the European Court of Justice for the period 1996-2012 and information on 135 mergers and joint ventures concluded by the cartel members between 1983 and 2010.

NOTE.– An "observation" corresponds to a Cartel-Spell-Period combination. A period is 30 days. For the first merger, a spell starts at the time of cartel birth and ends at the time of the merger. For second and subsequent mergers, a spell starts at the time of the preceding merger and ends at the time of the present merger. Censored spells start at the time of cartel birth (for cartels with no merger) or the last merger (for cartels with mergers) and end at the time of data collection (December 31, 2012).

Table 5. Cartel-Level Discrete-Time Hazard Regression Results

	MERGER_j^t						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
POLICY^t (ref is spell periods between July 18, 1996 to June 30, 2008)$_\dagger$							
PRE-LEN^t (spell periods before July 17, 1996)	−0.052	−0.254	−0.058	−0.780**	−0.817*	−1.160***	−1.209***
	(0.349)	(0.338)	(0.348)	(0.351)	(0.424)	(0.406)	(0.409)
SETTLEM^t (spell periods after July 1, 2008)	−1.806***	−1.284***	−1.791***	−1.615***	−1.092**	−0.909*	−0.752
	(0.392)	(0.424)	(0.400)	(0.410)	(0.477)	(0.540)	(0.550)
$\text{BREAKDOWN}_j^t{}_\dagger$	0.368	0.443	0.589	0.683**	0.446	0.713**	1.151***
	(0.349)	(0.332)	(0.369)	(0.329)	(0.338)	(0.320)	(0.384)
$\text{BREAKDOWN}_j^t \times \text{TIME-AFTER-BREAKDOWN}_j^t{}_\dagger$							−0.138*
							(0.073)
Control Variables							
$\text{ECMR04}^t{}_\dagger$		−0.884***				−0.674**	−0.605**
		(0.226)				(0.303)	(0.295)
$\text{INSPECTION}_j^t{}_\dagger$			−0.325			0.069	0.636*
			(0.291)			(0.305)	(0.351)
$\text{INSPECTION}_j^t \times \text{TIME-AFTER-INSPECTION}_j^t{}_\dagger$							−0.164*
							(0.099)
$\text{Log(FIRMS}_j\text{)}$				1.533***		1.431***	1.377***
				(0.229)		(0.244)	(0.238)
INDUSTRY TYPE_j (ref is agricultural products)				No		No	No
MARKET SCOPE_j (ref is national cartels)				Yes		Yes	No
$\text{Log}(\Delta\ \text{GDP}_j^t\text{+6.56})_\dagger$					0.474	0.004	0.083
					(0.541)	(0.566)	(0.588)
$\text{Log(PEAK-TROUGH}_j^t\text{+1)}_\dagger$					0.100	−0.198	−0.185
					(0.283)	(0.281)	(0.280)
$\text{Log(POS-SHOCK}_j^t\text{+1)}_\dagger$					−0.005	−0.059	−0.056
					(0.048)	(0.078)	(0.077)
$\text{Log(NEG-SHOCK}_j^t\text{+1)}_\dagger$					0.039	−0.050	−0.044
					(0.051)	(0.075)	(0.075)

(*Continued overleaf*)

TABLE 5. (*Continued*)

	MERGER_j^t						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Log}(\text{INTEREST}_j^t+4.9)_\dagger$					1.938***	0.909	0.939
					(0.623)	(0.749)	(0.714)
Duration-Interval-Specific-Dummy$_\dagger$							
[0, 15] months	−5.566***	−5.526***	−5.570***	−7.937***	−10.990***	−9.101***	−9.451***
	(0.392)	(0.394)	(0.393)	(0.948)	(2.158)	(2.266)	(2.320)
[16, 35]	−5.760***	−5.685***	−5.759***	−8.064***	−11.122***	−9.119***	−9.396***
	(0.333)	(0.337)	(0.337)	(0.849)	(2.180)	(2.277)	(2.315)
[36, 55]	−6.083***	−5.958***	−6.088***	−8.441***	−11.533***	−9.532***	−9.778***
	(0.353)	(0.340)	(0.358)	(0.918)	(2.172)	(2.287)	(2.313)
[56, 75]	−6.063***	−5.853***	−6.060***	−8.488***	−11.426***	−9.467***	−9.742***
	(0.387)	(0.361)	(0.384)	(0.981)	(2.151)	(2.297)	(2.336)
[76, 100]	−5.773***	−5.512***	−5.751***	−8.184***	−11.175***	−9.185***	−9.385***
	(0.397)	(0.375)	(0.399)	(0.907)	(2.145)	(2.247)	(2.277)
[101, 125]	−5.275***	−5.068***	−5.227***	−7.676***	−10.636***	−8.667***	−8.828***
	(0.370)	(0.347)	(0.369)	(0.914)	(2.168)	(2.290)	(2.308)
[126, 150]	−6.455***	−6.288***	−6.394***	−8.858***	−11.802***	−9.881***	−10.018***
	(0.615)	(0.602)	(0.616)	(1.028)	(2.314)	(2.412)	(2.417)
[151, 170]	−6.716***	−6.511***	−6.641***	−9.191***	−12.081***	−10.182***	−10.282***
	(0.734)	(0.708)	(0.744)	(1.091)	(2.111)	(2.180)	(2.191)
[171, 190]	−5.858***	−5.589***	−5.777***	−8.284***	−11.910***	−9.961***	−9.958***
	(0.746)	(0.717)	(0.740)	(1.232)	(2.233)	(2.299)	(2.291)
[191, 220]	−6.665***	−6.452***	−6.582***	−9.208***	−12.039***	−10.300***	−10.253***
	(0.778)	(0.760)	(0.781)	(1.205)	(2.189)	(2.206)	(2.211)
$\geq$ 221 months	−8.347***	−8.100***	−8.270***	−11.194***	−13.713***	−12.137***	−11.977***
	(1.050)	(1.041)	(1.049)	(1.437)	(2.347)	(2.428)	(2.423)

(*Continued overleaf*)

TABLE 5. (*Continued*)

	MERGER_j^t						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Log(NUM-EX-MERGERS}_j^t\text{+1)}_\ddagger$	0.966***	0.980***	1.004***	−0.228	0.921***	−0.144	0.046
	(0.101)	(0.095)	(0.105)	(0.191)	(0.096)	(0.209)	(0.199)
$\text{Log(DU-EX-MERGER}_j^t\text{+1)}_\ddagger$	−0.034	−0.003	−0.031	−0.010	−0.030	0.007	0.019
	(0.054)	(0.052)	(0.053)	(0.059)	(0.055)	(0.059)	(0.058)
Number of Observations	34,439	34,439	34,439	34,439	32,558	32,558	32,558
Number of Spells	287	287	287	287	287	287	287
Number of Cartels	139	139	139	139	139	139	139
Log-pseudo likelihood	-877.058	-869.677	-876.337	-833.817	-835.026	-795.854	-792.125

NOTE.– Standard errors are robust to heteroskedasticity, clustered by cartel and are shown in parentheses. The table reports the cartel-level discrete-time hazard regression results. The dependent variable MERGER_j^t is a dichotomous indicator of whether during the period (t-30 days, t] a merge or JV occurs in cartel j. The omitted antitrust policy category is LEN^t which includes cartel j's spell-periods between July 18, 1996 and June 29, 2008. The category PRE-LEN^t includes the spell-periods that predate July 17, 1996. The category SETTLEM^t includes the spell-periods that postdate June 30, 2008. BREAKDOWN_j^t=1 if the first exit in cartel j took place by time t, =0 otherwise. † indicates a time-varying variable; ‡ indicates a variable that relates to the previous merger episode.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 6. Cartel-Level Discrete-Time Hazard Regression Results (Robustness Checks)

	MERGER_j^t				
	(1)	(2)	(3)	(4)	(5)
POLICY^t (ref is spell periods between July 18, 1996 to June 30, 2008)$_\dagger$					
PRE-LEN^t (spell periods before July 17, 1996)	−1.099**	−1.133***	−1.587***	−1.233***	−1.160***
	(0.430)	(0.439)	(0.599)	(0.420)	(0.369)
SETTLEM^t (spell periods after July 1, 2008)	−0.939*	−0.909	−0.852	−0.869*	−0.909*
	(0.571)	(0.655)	(0.604)	(0.553)	(0.495)
$\text{BREAKDOWN}_j^t{}_\dagger$	0.694**	0.400	0.632	0.644**	0.713**
	(0.326)	(0.355)	(0.476)	(0.327)	(0.295)
Control Variables					
$\text{ECMR04}^t{}_\dagger$	−0.649**	−0.855***	−1.056**	−0.699**	−0.674**
	(0.319)	(0.303)	(0.420)	(0.299)	(0.283)
$\text{INSPECTION}_j^t{}_\dagger$	0.143	0.504	−0.005	0.103	0.069
	(0.317)	(0.352)	(0.463)	(0.331)	(0.271)
$\text{Log(FIRMS}_j\text{)}$	1.476***	1.326***	1.565***	1.458***	1.431***
	(0.265)	(0.267)	(0.374)	(0.247)	(0.221)
INDUSTRY TYPE_j (ref is agricultural products)	No	No	No	No	No
MARKET SCOPE_j (ref is national cartel)	No	No	No	No	Yes
$\text{Log}(\Delta\ \text{GDP}_j^t+6.56)_\dagger$	−0.099	−0.100	−0.326	0.009	0.004
	(0.569)	(0.581)	(0.606)	(0.579)	(0.561)
$\text{Log(PEAK-TROUGH}_j^t+1)_\dagger$	−0.249	−0.522*	−0.315	−0.241	−0.198
	(0.288)	(0.277)	(0.301)	(0.281)	(0.312)
$\text{Log(POS-SHOCK}_j^t+1)_\dagger$	−0.078	−0.058	0.068	−0.060	−0.059
	(0.079)	(0.080)	(0.091)	(0.080)	(0.075)
$\text{Log(NEG-SHOCK}_j^t+1)_\dagger$	−0.073	−0.049	0.075	−0.052	−0.050
	(0.075)	(0.077)	(0.084)	(0.078)	(0.077)
$\text{Log(INTEREST}_j^t+4.9)_\dagger$	0.719	0.808	1.557*	1.063**	0.909
	(0.800)	(0.820)	(0.945)	(0.762)	(0.703)
Duration-Interval-Specific-Dummy$_\dagger$					
[0, 15] months	−8.379***	−8.382***	−10.065***	−8.721***	−9.101***
	(2.341)	(2.500)	(2.600)	(2.519)	(2.280)
[16, 35]	−8.404***	−8.506***	−10.495***	−8.738***	−9.119***
	(2.348)	(2.503)	(2.638)	(2.506)	(2.274)
[36, 55]	−8.818***	−8.832***	−10.468***	−9.142***	−9.532***
	(2.366)	(2.533)	(2.665)	(2.537)	(2.293)
[56, 75]	−8.770***	−8.705***	−10.615***	−9.050***	−9.467***
	(2.394)	(2.523)	(2.695)	(2.561)	(2.296)
[76, 100]	−8.475***	−8.495***	−10.072***	−8.744***	−9.185***
	(2.339)	(2.469)	(2.614)	(2.537)	(2.295)
[101, 125]	−7.955***	−8.165***	−9.429***	−8.327***	−8.667***
	(2.390)	(2.576)	(2.603)	(2.585)	(2.303)
[126, 150]	−8.960***	−9.289***	−10.604***	−9.439***	−9.881***
	(2.510)	(2.683)	(2.857)	(2.699)	(2.337)
[151, 170]	–	−9.383***	−11.279***	−9.748***	−10.182***
	–	(2.360)	(2.816)	(2.403)	(2.399)

(*continued overleaf*)

TABLE 5. (*Continued*)

	MERGER_j^t				
	(1)	(2)	(3)	(4)	(5)
[171, 190]	–	−9.139***	−10.206***	−9.541***	−9.961***
	–	(2.493)	(2.714)	(2.610)	(2.396)
[191, 220]	–	−9.556***	−11.167***	−9.876***	−10.300***
	–	(2.400)	(2.726)	(2.502)	(2.409)
$\geq$ 221 months	–	–	−12.642***	−11.688***	−12.137***
	–	–	(2.886)	(2.689)	(2.498)
$\text{Log(NUM-EX-MERGERS}_j^t\text{+1)}_\ddagger$	−0.227	−0.200	−0.447	−0.125	−0.144
	(0.224)	(0.233)	(0.355)	(0.212)	(0.208)
$\text{Log(DUR-EX-MERGER}_j^t\text{+1)}_\ddagger$	0.026	0.025	0.137	0.010	0.007
	(0.060)	(0.063)	(0.092)	(0.064)	(0.057)
	(2.293)	(2.346)	(2.751)	(2.461)	(2.456)
DISPERSION_j			−1.950		
			(2.710)		
Price-Fix_j				−0.699**	
				(0.356)	
Market-Alloc_j				−0.225	
				(0.305)	
Big Rigging_j				0.158	
				(0.403)	
Cartel-level random effect SD					0.004
					(0.043)
Number of Observations	21,968	19,955	23,559	32,413	32,558
Number of Spells	286	223	192	284	287
Number of Cartels	129	95	100	138	139
Log-pseudo likelihood	-741.603	-662.412	-512.053	-785.184	-795.854

NOTE.– Standard errors are robust to heteroskedasticity, clustered by cartel and are shown in parentheses. The table reports the cartel-level discrete-time hazard regression results. The dependent variable MERGER_j^t is a dichotomous indicator of whether during the period (t-30 days, t] a merge or JV occurs in cartel j. BREAKDOWN_j^t=1 if the first exit in cartel j took place by time t, =0 otherwise. Specification (1) censors the spells at 145.5 months for spell periods before July 18, 1996. Specification (2) excludes cartels with US market coverage. Specification (3) includes DISPERSION as a control variable. Specification (4) includes infringement types as control variables. Specification (5) reports the estimates from a random effects model.

† indicates a time-varying variable; ‡ indicates a variable that relates to the previous merger episode.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

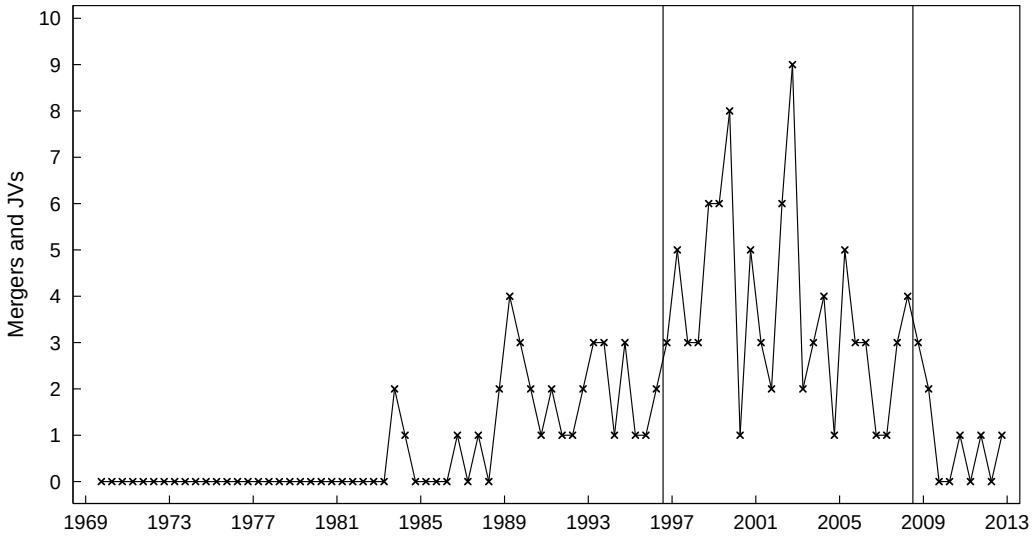


FIGURE 1. SEMI-ANNUAL NUMBER OF MERGERS AND JVS

Notes: The sample runs from March 1969 to December 2013. The figure plots the total number of mergers and JVs in each six-month period and over time. The vertical bars mark the introduction of the leniency program on July 18, 1996 and the introduction of the settlement procedure on June 30, 2008, respectively.

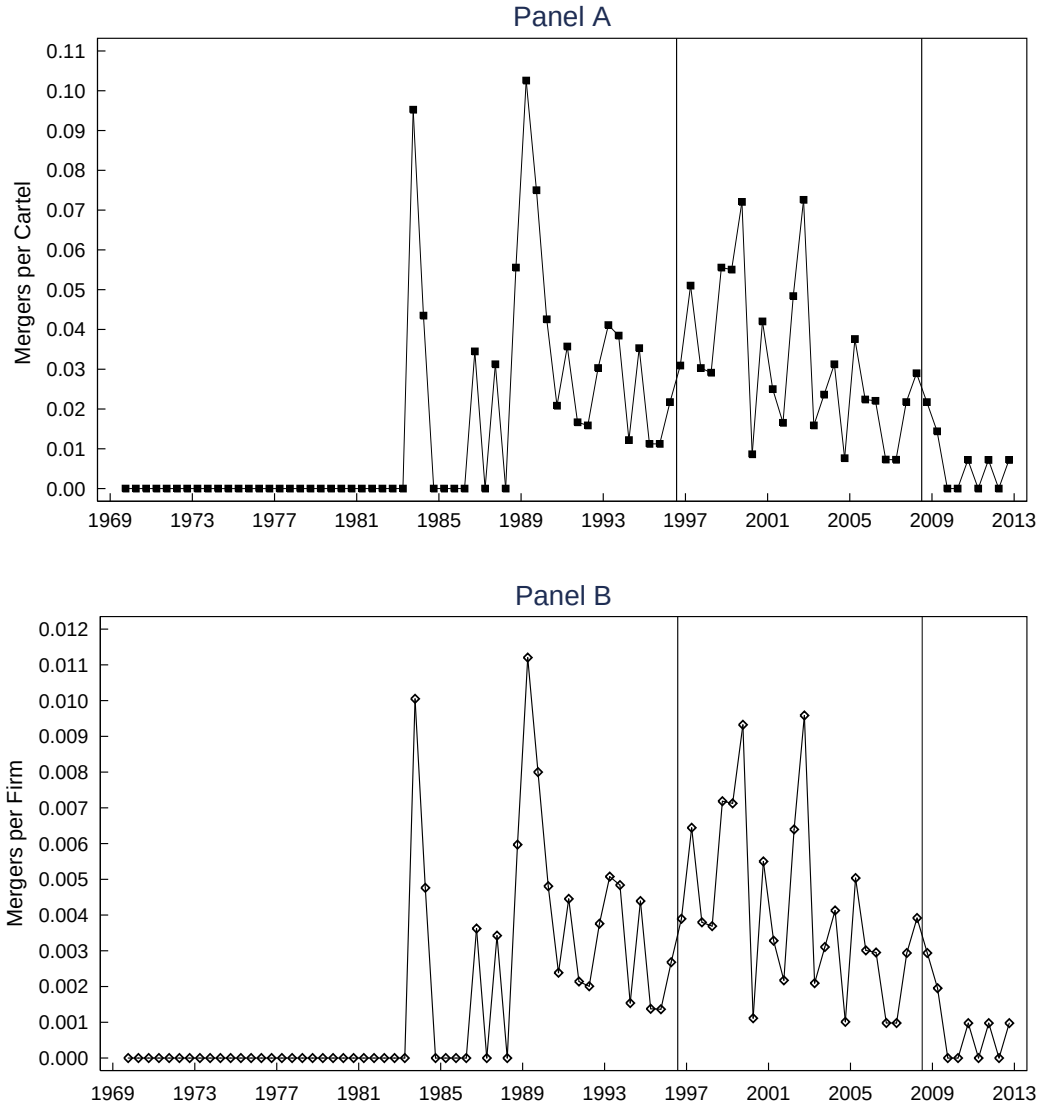


FIGURE 2. SEMI-ANNUAL MERGER RATES

Notes: The sample runs from March 1969 to December 2013. Panel A plots the number of mergers and JVs per sampled cartel in each six-month period and over time. Panel B plots the number of mergers and JVs per sampled firm. The vertical bars mark the introduction of the leniency program on July 18, 1996 and the introduction of the settlement procedure on June 30, 2008, respectively.

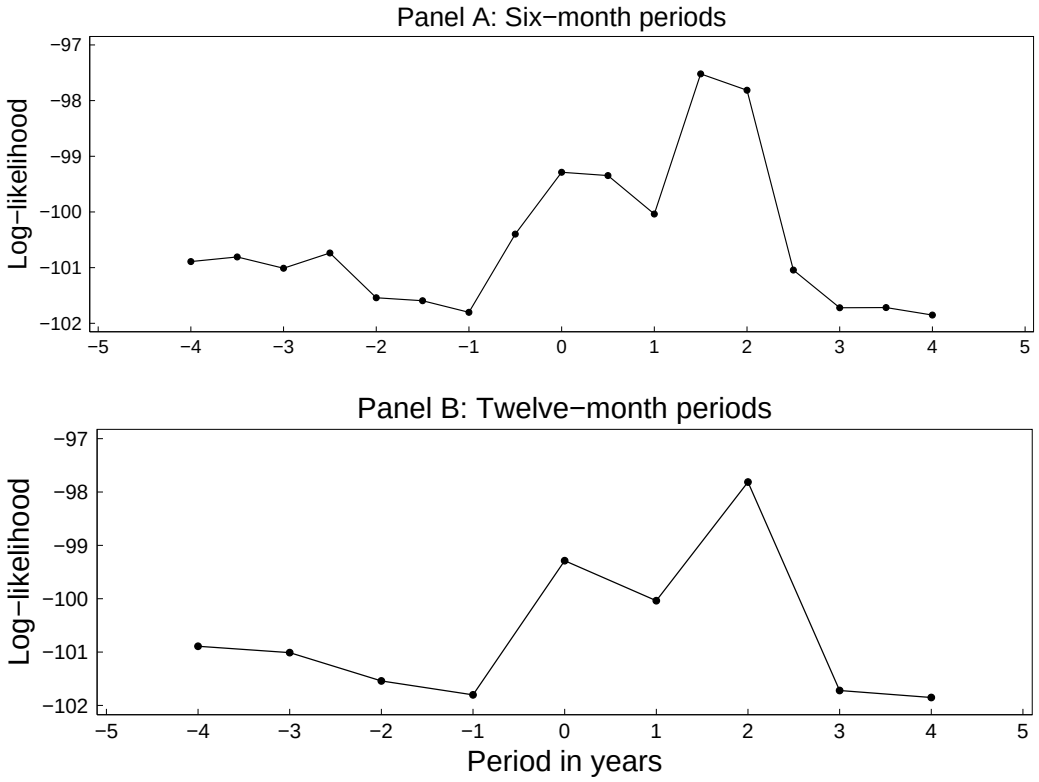


FIGURE 3. LENIENCY PROGRAMS VERSUS PLACEBO INTERVENTIONS

Notes: Each point corresponds to the maximized log-likelihood of a Poisson regression. The points at zero on the horizontal axes are generated by breakpoints that correspond to the introduction of the leniency program on July 18, 1996. The points to the left (right) of zero are generated by placebo interventions that predate (postdate) leniency introduction. Panel A features six-month periods; panel B features twelve-month periods.

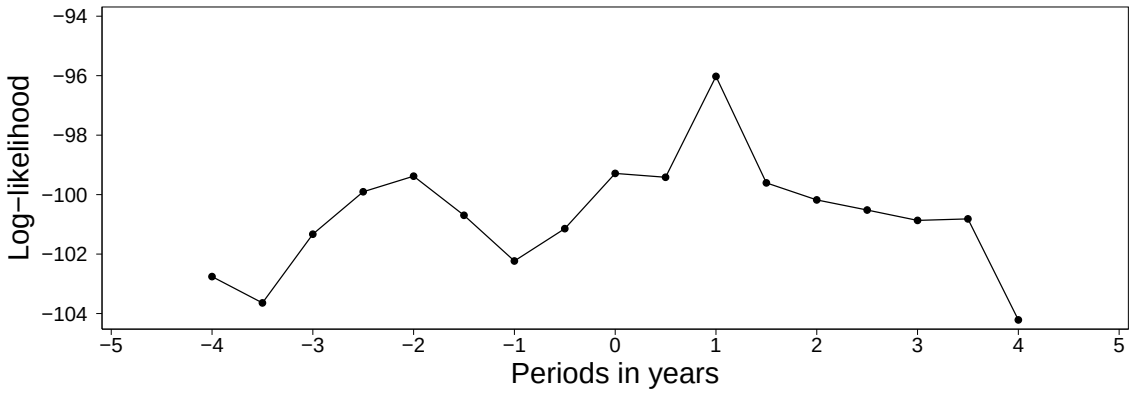


FIGURE 4. SETTLEMENT PROCEDURE VERSUS PLACEBO INTERVENTIONS

Notes: Each point corresponds to the maximized log-likelihood of a Poisson regression. The points at zero on the horizontal axes are generated by breakpoints that correspond to the introduction of the cartel settlement procedure on June 30, 2008. The points to the left (right) of zero are generated by placebo interventions that predate (postdate) the introduction of settlement procedure.

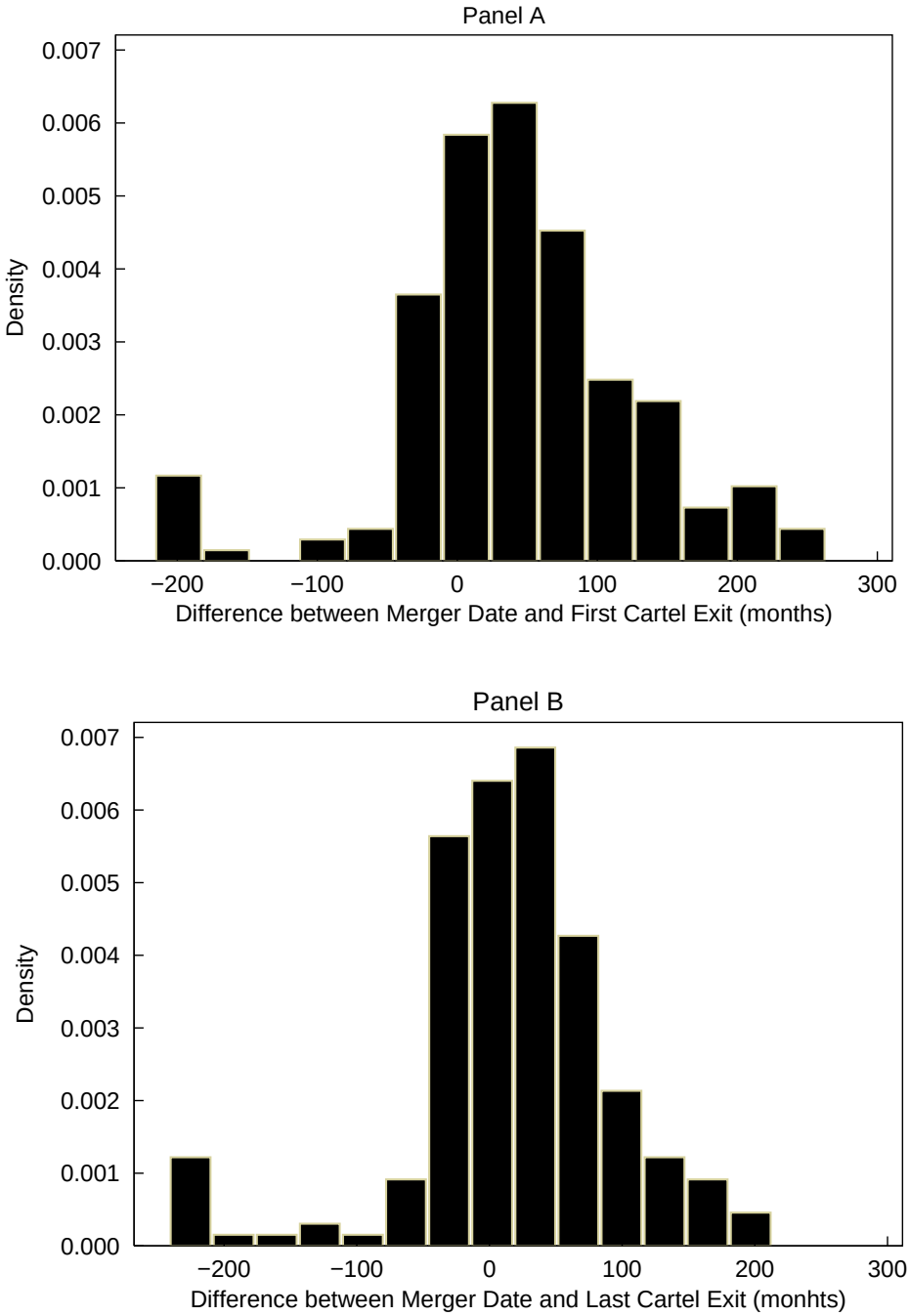
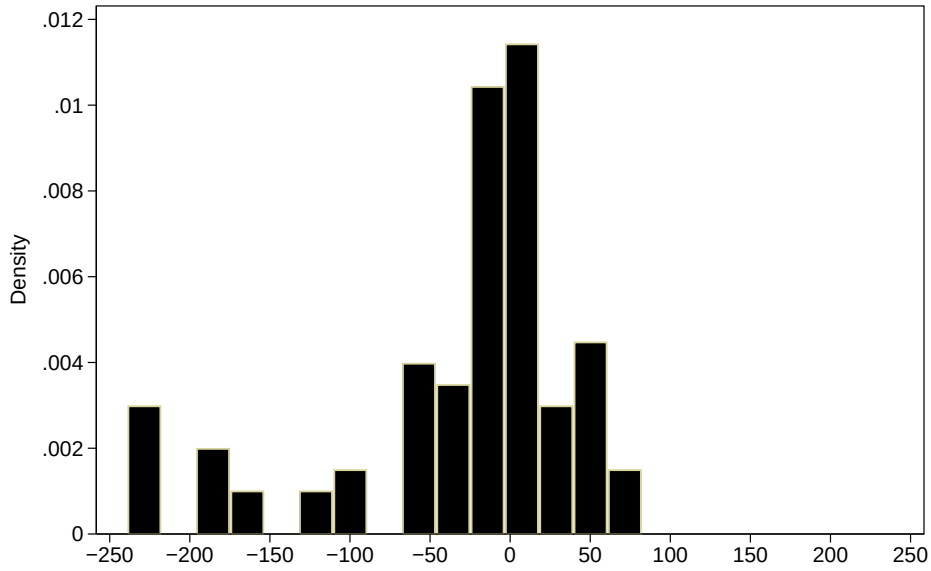


FIGURE 5. DISTRIBUTION OF MERGERS AROUND THE TIME OF CARTEL BREAKDOWN

Notes: The sample consists of 287 cartel-event combinations that are constructed using 135 mergers and JVs by members of EC detected cartels (Decisions 1985-2013) for the period December 1983 to October 2012. The figure plots the distribution of merger activities around the time that a cartel collapses. The observations located at zero on the horizontal axis are mergers or joint ventures that took place at the same time of a cartel breakdown. The observations to the left (right) of zero are mergers before (after) before a breakdown. Panel A uses the date that the first conspirator ends the infringement to mark the cartel's breakdown. Panel B uses the date that the last conspirator ends the infringement to mark the cartel's breakdown.

Panel A: Merger Distribution before July 18, 1996



Panel B: Merger Distribution after July 18, 1996

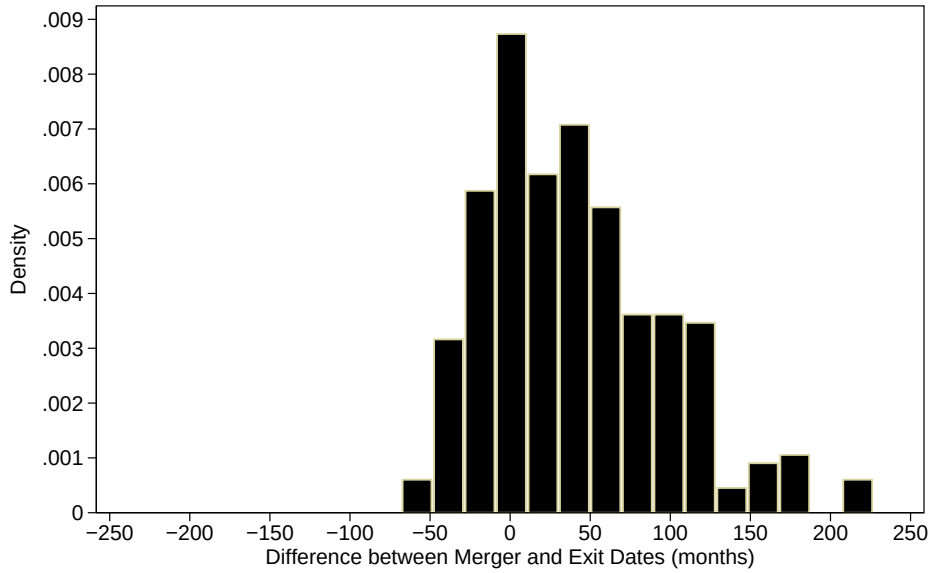


FIGURE 6. DISTRIBUTION OF MERGERS AROUND EXITING TIME BY CARTEL POLICIES

Notes: The sample consists of 287 cartel-event combinations that are constructed using 135 mergers and JVs by members of EC detected cartels (Decisions 1985-2013) for the period December 1983 to October 2012. The figure plots the distribution of merger activities around the time that a cartel collapses. The observations located at zero on the horizontal axis are mergers or joint ventures that took place at the same time of a cartel breakdown. The observations to the left (right) of zero are mergers before (after) before a breakdown. Panel A include all the sampled mergers. Panel B includes only mergers that completed before July 18, 1996. Panel C plots only mergers that completed after July 18, 1996.

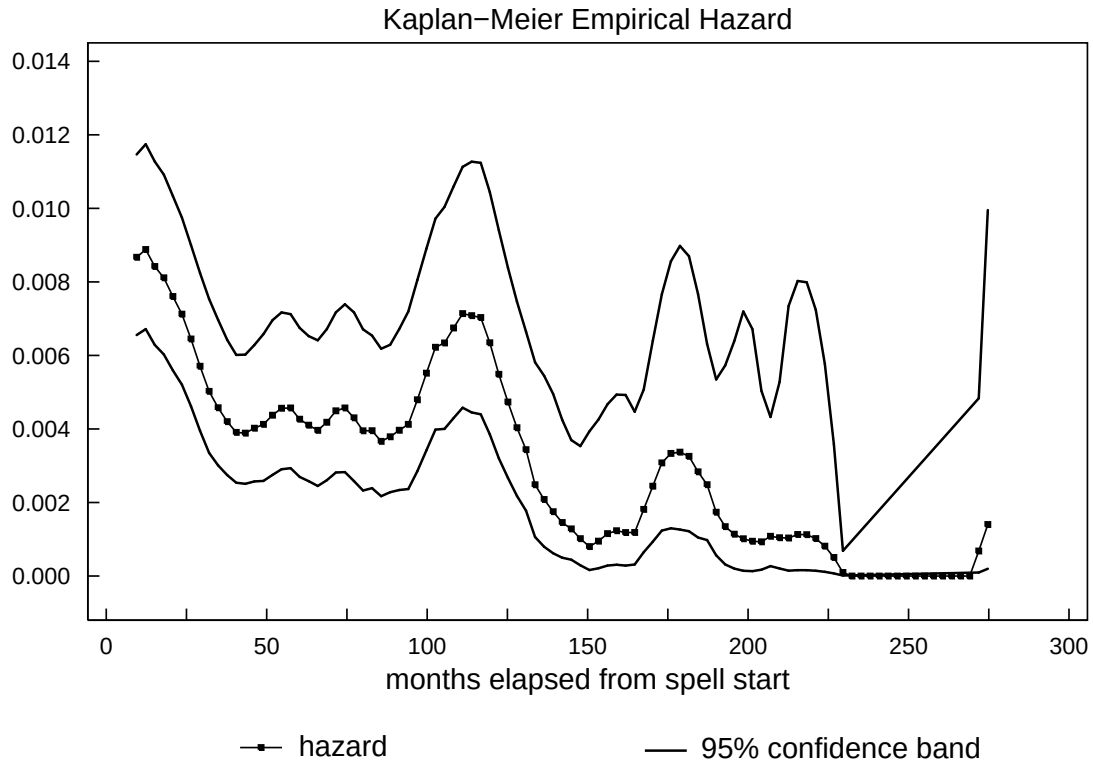


FIGURE 7. KAPLAN-MEIER EMPIRICAL HAZARD

Notes: The sample consists of 287 cartel-event combinations that the authors constructed using decisions on 139 cartels and information by the European Commission and judgments of the Court of First Instance and the European Court of Justice for the period 1996-2012 and information on 135 mergers and joint ventures concluded by the cartel members between 1983 and 2010.

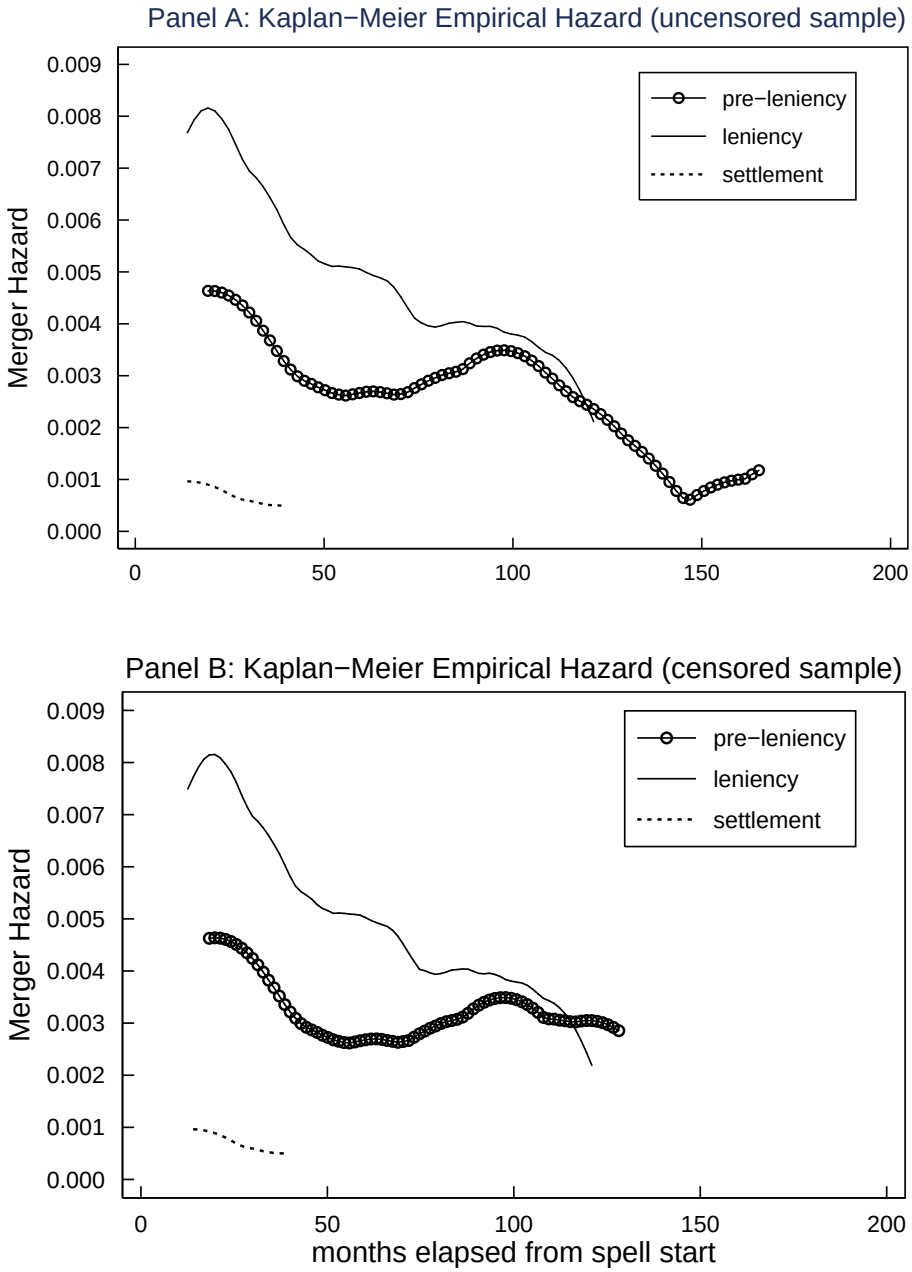


FIGURE 8. MERGER DYNAMICS BEFORE AND ANTITRUST POLICIES

Notes: The sample consists of 287 cartel-event combinations that the authors constructed using decisions on 139 cartels and information by the European Commission and judgments of the Court of First Instance and the European Court of Justice for the period 1996-2012 and information on 135 mergers and joint ventures concluded by the cartel members between 1983 and 2010. The circles correspond to merger hazards before July 18, 1996. The solid lines correspond to hazards between July 18, 1996 and June 30m , 2008. The dashed lines correspond to merger hazards after June 30, 2008. The duration spells in Panel A are not censored. Panel B censors the observations at 145.5 months for spell durations before July 18, 1996.

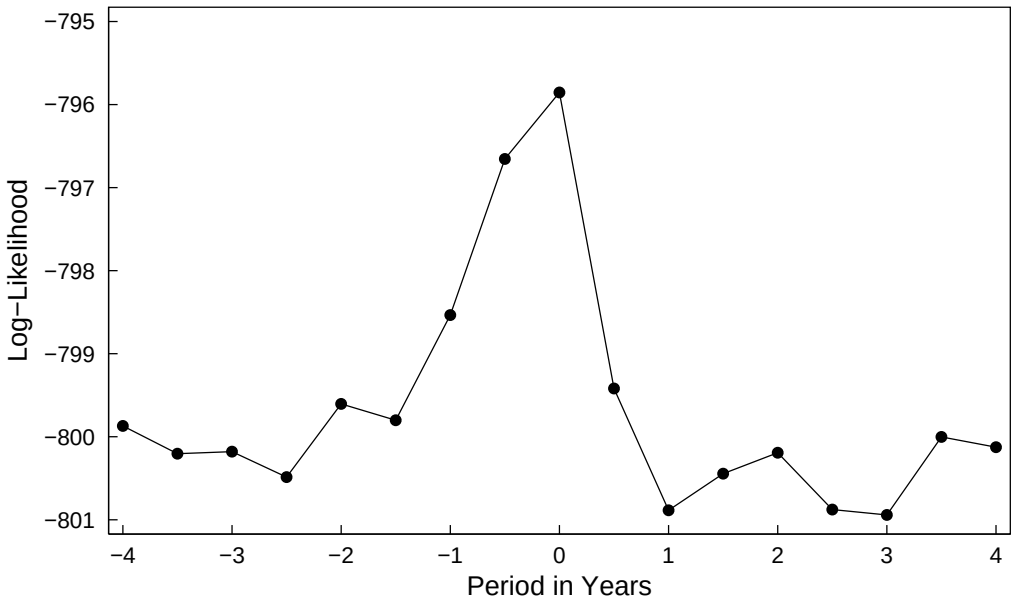


FIGURE 9. LENIENCY VERSUS PLACEBO INTERVENTIONS

Notes: Each point corresponds to the maximized log-likelihood of a hazard regression. The points at zero on the horizontal axes are generated by breakpoints that correspond to the introduction of the cartel settlement procedure on June 30, 2008. The points to the left (right) of zero are generated by placebo interventions that predate (postdate) the introduction of settlement procedure.

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