Deterrence of Horizontal Mergers: Empirical Evidence from U.S. Industries

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Abstract: We estimate the deterrence effects of U.S. merger policy tools with respect to the composition of future merger notifications. Data from the Annual Reports by the U.S. DOJ and FTC allow industry based measures over the 1986-1999 period of the conditional probabilities for detection (eliciting an investigation), punishment (eliciting an antitrust action), and severity of punishment (eliciting prohibitions versus remedies) – deterrence variables somewhat akin to the traditional deterrence variables from the crime and punishment literature. We find both the conditional probability of detection and even more so the conditional probability of punishment to yield deterrence effects; however, the conditional probability of eliciting a severe punishment (prohibitions versus remedies) does not indicate significant deterrence. These results suggest that second-request-investigations and antitrust actions (remedies and prohibitions) generally involve deterrence; yet, prohibitions do not involve significantly more deterrence than do remedies.

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I. Introduction

> "We firmly believe that deterrence is perhaps the single most important ultimate outcome of the Division's work … [but] … we have not attempted to value either the "spillover" effects or the deterrent effects of our successful enforcement efforts, though we and those who have written on the subject believe that such effects exist and are significant". (Antitrust Division, U.S. Department of Justice in Nelson & Sun, 2001: pp. 939-940)

As the above quotation indicates, merger policy entails more than just direct regulatory effects but also indirect deterrence effects, since effective policy puts a premium on encouraging firms to internalize antitrust rules in their decision making. Merger policy rules should then create incentives that shape the behavior of both firms found – and not-found – in violation of these rules, as no policy can be effective if its every application has to be policed (Wilks, 1996; Baker, 2003). Accordingly, the effects of merger policy are not limited to the specific firms targeted by an antitrust investigation, but include all firms whose behavior and performance will be affected in the future by the decisions in specific cases. Joskow (2002: 99-100) takes the next step by noting "that the test of a good legal rule is not primarily whether it leads to the correct decision in a particular case, but rather whether it does a good job deterring anticompetitive behavior".

Yet, measuring the deterrence effects of merger policy has proven to be quite challenging. A number of scholars (e.g., Allen, 1984; Eckbo, 1989; Nelson & Sun, 2001; Crandall & Winston, 2003) have pointed out the difficulties involved with eliciting deterrence effects – including the challenge of identifying counterfactuals (e.g., mergers not proposed or not proposed in a certain fashion due to the existence of antitrust). Nevertheless, a few different approaches have been employed in order to attempt to capture the deterrence effects of merger policy: (1) considering changes in the composition – horizontals versus verticals and conglomerates – of proposed mergers (e.g., Stigler, 1966; Allen, 1984); (2) detecting differences in the stock-prices of rival firms (e.g., Eckbo and Wier, 1985; Eckbo, 1992), (3) surveying antitrust lawyers (Deloitte & Touch, 2007; Twynstra Gudde, 2005); (4) discerning departures from the merger wave as a manifestation of deterrence (Seldeslachts, Clougherty and Barros, 2009). However, these different approaches suffer from various limitations including the proclivity to make broad comparisons: the having/not-having a merger policy comparison, and the before/after a regime shift comparison. Accordingly, the U.S. Department of Justice (DOJ) and U.S. Federal Trade Commission (FTC) have not factored any beneficial deterrence effects resulting from merger policy as they have been unable to approach such a measurement (Nelson & Sun, 2001). This measurement omission is all the more striking when one considers that the two antitrust agencies firmly believe the deterrence of anti-competitive merger filings to be considerable (see the introductory quote above); and moreover, the agencies are required by the 'Government Performance and Results Act' (GPRA) to estimate the consumer savings derived from antitrust policies.

Despite the importance of this subject, we are unaware of any scholarship that attempts to measure merger policy deterrence while employing the dominant deterrence methodology from the crime and punishment literature spawned by Becker (1968). Such an omission is a pity in that the conditional probabilities methodology from the economics of crime literature rests on theoretical foundations (Becker, 1968; Ehrlich, 1973), has been subject to a great deal of scholarship (see Cameron, 1988, 1994; Grogger, 1990; and Cloninger & Marchesini, 2006 for reviews), and has elicited a healthy amount – particularly with regard to capital punishment and the deterrence of homicides – of criticism (e.g., Passel & Taylor, 1977; Berk, 2005; Donohue & Wolfers, 2005). Accordingly, the methodological framework from the economics of crime literature provides a sound means to test whether U.S. merger policy involves significant deterrence effects—a means that goes beyond the ad-hoc methods noted above that have been previously employed to capture merger policy deterrence, and a means that allows for more specific analysis of merger policy instruments as opposed to the before/after and having/not-having comparisons indicative of that previous literature. In particular, the conditional probabilities of detection and punishment (as well as severity of punishment) from the economics of crime literature lend themselves well to the realm of merger policy with its somewhat equivalent conditional probabilities of eliciting an investigation, an antitrust action, and a prohibition.

We propose then to employ the conditional probabilities methodology from the economics of crime literature to elicit whether different merger policy instruments (investigations, remedies and prohibitions) entail deterrence effects with regard to the composition of proposed merger activity in industrial sectors. Using two-digit industrial sector data reported by the DOJ and FTC on the number of second-request-investigations, remedies, prohibitions, horizontal mergers and total mergers over the 1986-1999 period, we can employ panel-data techniques to infer whether the conditional probability of eliciting an investigation, an antitrust action, or a prohibition lead to relatively fewer horizontal merger proposals in a particular industrial sector. Our results suggest that increasing the detection rate (i.e., the probability of eliciting a second-request-investigation) and more so increasing the punishment rate (i.e., the conditional probability of eliciting an antitrust action with respect to eliciting a second-request-investigation) do lead in subsequent years to both relatively and absolutely fewer horizontal mergers in an industrial sector. The results also suggest that increasing the severity of punishment (i.e., the conditional probability of eliciting a prohibition with respect to eliciting an antitrust action) does not involve significant deterrence effects—in other words, prohibitions do not generate any additional deterrence than do remedies.

In order to support our analysis, the structure of our paper is as follows. Section II respectively reviews the literature on merger policy deterrence and the literature on crime and punishment deterrence in order to point out the practices on which we either build or improve upon in our empirical analysis. Section III describes the industrial sector data on antitrust and merger activity. Section IV describes issues and techniques with regard to our dynamic panel data estimation. Section V presents empirical results. Section VI discusses and concludes.

II. Background

Since our aim is to employ the conditional probabilities methodology from the economics of crime literature to empirically capture the deterrence effects of the different policy instruments available to U.S. antitrust officials, it behooves us to review the respective merger-policy deterrence and crime-and-punishment deterrence literatures for any relevant features that we either build or improve upon in our empirical analysis.

A. Merger Policy Deterrence

As already alluded to above, one characteristic of the scant literature on merger policy deterrence is the relatively broad level of analysis employed in those studies. For instance, Eckbo (1992) compares the U.S. merger population with the Canadian merger population (during a period lacking Canadian antitrust enforcement) to gather whether the stock-prices of non-merging (rival) firms in the U.S. are significantly less than those in Canada. He finds the rivals of Canadian mergers to have abnormal returns no greater than those of U.S. mergers, thus suggesting a lack of deterrence as Canadian mergers were no more anti-competitive than U.S. mergers. Stigler (1966) also looked for a change in the general composition of merger activity in the years subsequent to the 1950 anti-merger amendment to the Clayton Act; he finds a trend away from horizontal merger activity in the U.S. We will attempt to improve upon this previous work in merger policy deterrence by considering deterrence effects at the industrial sector level-of-analysis.

The broad level of analysis employed in the previous merger policy deterrence work also lent itself to empirical studies making broad comparisons. For instance in addition to the Eckbo (1992) U.S./Canada comparison, Eckbo and Wier (1985) make use of the period prior to and after the onset of the U.S. Hart-Scott-Rodino (HSR) Act to gather whether that antitrust statute led to the proposal of fewer anti-competitive mergers. Such results naturally generate implications for whether merger policy in general – or a particular shift in a policy regime –

yield more or less deterrence effects; yet, more targeted implications with regard to the effectiveness of different merger policy tools are challenging with such a set-up. Only the recent Seldeslachts, Clougherty, and Barros (2009) study considers the effectiveness of different merger policy tools (prohibitions and remedies in their case) with respect to deterring future merger behavior. Yet that study also suffers from a relatively broad nationwide level-of-analysis; e.g., the impact of a spike in antitrust activity for a nation in one year is considered on the overall number of national mergers in subsequent years. Accordingly, we will also consider the impact of prohibitions and remedies (as well as antitrust investigations) on future merger proclivities; however, we will be able to do so at a more narrow level-of-analysis. Hence, we will be able to tie the use of these different merger policy tools to future merger behavior in the particular industrial sector.

Another interesting feature from the previous literature is the different means via which researchers have attempted to measure deterrence effects. Aaronson (1992) points out that merger policy deterrence potentially manifests in two forms: (1) frequency-based deterrence, as merger plans are forsaken due to the existence (or enhancement) of antitrust; (2) composition-based deterrence, as future mergers are modified and shaped differently to conform with antitrust regulations. Beginning with Stigler (1966), a few researchers (Scherer, 1980; Allen, 1984) have considered the composition of proposed mergers (horizontal with respect to total mergers) to gather whether antitrust laws or administration changes yield deterrence in the form of altered merger proposals. B. Espen Eckbo's approach (Eckbo, 1992; Eckbo & Wier, 1985) is also firmly grounded in composition-based deterrence, as larger abnormal-returns for rival firms indicate more market-power based merger activity. On the other hand, the Seldeslachts, Clougherty and Barros (2009) study is firmly rooted in frequency-based deterrence, as they consider the impact of spikes in antitrust actions on the future level of merger notifications. We will initially follow the Stigler approach and consider the ratio of horizontal mergers to total mergers in an industrial sector. As an aside, we do not mean to argue here that all horizontal mergers are anti-competitive mergers, yet it is safe to state that the vast majority of anti-competitive mergers are indeed horizontal mergers; accordingly, the population of anti-competitive mergers resides within horizontal merger activity and not within vertical or conglomerate merger activity.[1] In addition to the Stigler approach, we will go beyond strictly considering composition effects to also consider the frequencies of horizontal and non-horizontal mergers in order to ensure that it is the deterrence of horizontals – and not the encouragement of non-horizontals – that is behind any measurable deterrence effects.

Last, despite the fact that mergers have long been realized to manifest as a wave-based phenomenon (Gort, 1969; Golbe and White, 1993), much of the research in economics has not considered merger activity in its proper wave-like context. Research in financial economics (Andrade & Stafford, 2004; Harford, 2005; Rhodes-Kropf et al., 2005), however, has recently advanced our understanding of the drivers behind merger waves. Furthermore, holding the merger-wave constant was a crucial feature in the Seldeslachts, Clougherty and Barros (2009) set-up, as their deterrence manifested as departures in the number of merger notifications from the merger wave. Our industrial sectors will also be subject to merger waves, accordingly we will control for common drivers of merger activity from the recent financial economics literature on merger waves.

In sum, our empirical approach to eliciting the deterrence effects of U.S. merger policy tools can be characterized as drawing and improving upon the following properties from the literature on merger policy deterrence. First, improving upon the broad level-of-analysis employed by previous work, we will analyze deterrence at the industrial sector level. Second, we will be able to make some inferences with regard to the deterrence effects of particular merger policy tools, as opposed the customary before/after and having/not-having comparisons. Third, we will be able to test for both composition-based deterrence (the ratio of horizontal

[1] In assembling our data on investigations, remedies and prohibitions by industrial sector, we read through all of the reports by the DOJ and FTC summarising their antitrust activities regarding merger control (1986-2005, although we use only 1986-1999 in our empirical analysis due to matching with industry data) and in only two cases did we find vertical concerns as being a rationale behind antitrust scrutiny.

mergers to total mergers) and frequency-based deterrence (the number of horizontal and non-horizontal mergers). Fourth, we will control for common drivers of merger waves to help ensure more robust causal inferences.

B. Crime and Punishment Deterrence

The classic works by Becker (1968) and Ehrlich (1973) on the economic approach to crime and punishment generated an extensive amount of empirical literature employing a choice-theoretic framework. While some variations in the design exist, most subsequent empirical pieces have crime depending upon the following conditional probabilities: detection over the number of crimes, conviction over the number of detections, punishment over the number of convictions, and then the severity of the punishment (e.g., Dezhbakhsh, Rubin & Shepherd, 2003; Katz, Levitt & Shustorovich, 2003; Mocan & Gittings, 2003; Zimmerman, 2004). This set-up derives from theory as the deterrence variables capture the relevant subjective probabilities that offenders are detected, convicted, punished and the severity of the punishment. In short, a crime supply equation is formulated as the deterrence variables play the role of prices with lower prices signaling a greater net relative gain from engaging in offences. While keeping in mind that the proposal of anti-competitive mergers is no "crime" in the strict sense, we will be able to formulate a somewhat similar equation for the provision of horizontal mergers employing three deterrence variables that conform with this dominant empirical approach to deterrence. Indeed, given that anti-competitive mergers are a subset of the number of proposed horizontal mergers – at least in the eyes of the DOJ and FTC – and that consequently U.S. merger policy actions are targeted at horizontal mergers almost exclusively, we formulate conditional probabilities for investigations over the number of horizontal mergers, for antitrust actions (remedies + prohibitions) over the number of investigations, and for prohibitions over the number of antitrust actions.[2] Accordingly, this yields a more theoretically consistent approach to measuring merger policy deterrence than the various ad-hoc means previously employed.

Another benefit from invoking the extensive literature on crime-and-punishment deterrence is the wealth of scholarship on the appropriate econometric practices with respect to measuring deterrence, as this can provide an informed basis upon which to structure our study. For one, Donohue and Wolfers (2005) point out that it has become standard practice in the deterrence literature to cluster standard errors by the relevant panel grouping. Accordingly, the Bertrand, Duflo and Mullainathan (2004) recommendation to employ robust standard errors clustered on the panel in order to factor serial correlation has taken hold in the deterrence literature. There seems then to be some implicit understanding in the crime-and-punishment literature that periods are inter-connected. Related to the previously noted wave-like properties of merger activity, we are particularly conscious of the potential for serial correlation. Beyond simply clustering the standard errors, we will attempt to more directly address this concern by including common drivers of merger waves and using a dynamic panel data framework. While including lagged dependent variables can control for serial correlation in a series of data, dynamic panel data models lead to biased and inconsistent estimates due to the obvious correlation of the lagged dependent variable(s) with the error term. Accordingly, we will employ the system generalized method of moments (System GMM) estimator proposed by Arellano and Bover (1995) for dynamic panel data. This GMM estimator instruments for the lagged dependent variables – as well as all other potentially endogenous variables – and results in unbiased and consistent estimators; thus, it yields good results when dealing with serial

[2] In our empirical setting (where the Hart-Scott-Rodino merger review process operates), convictions are not an immediate deterrence factor as a premium is put on speedy resolution of the matter with either a negotiated settlement being found between the merging parties and the antitrust authorities (a remedy) or a prohibition intent is announced and the merging parties then abandon the merger transaction. Court cases and the consequent convictions (or verdicts) come about when the merging firms and the government cannot come to an agreement – such cases occur, but are not so frequent and moreover are subsequent to the investigation and initial antitrust action.

correlation in panel data. In fact, Bertrand, Duflo and Mullainathan (2004: 274) state in their conclusion that “We also hope that our study will contribute in generating further work on alternative estimation methods … such as GLS or GMM estimation of dynamic panel data models”. Accordingly, our controlling for merger waves, invoking a dynamic panel data model, and employing a GMM estimator improve upon the efforts in the crime-and-punishment deterrence literature by more properly taking into account the inter-connectedness of observations over time.

One critique of the crime-and-punishment deterrence literature has been that the deterrence formulations make unreasonable demands on the computational skills of prospective criminals—in particular, prospective murderers (Berk, 2005). For instance due to the lags involved with the criminal justice system, the punishment probability is often executions at time t over number of convictions at time t-6 (e.g., Dezhbakhsh, Rubin & Shepherd, 2003; Mocan & Gittings, 2003). Katz, Levitt and Shustorovich (2003) point out that the high discount rate of murders and the fact that killings are often under the influence of drugs and alcohol (which further shorten time horizons) suggests that it is tough to believe that punishment with such long delays would be effective. Unsurprisingly then, some studies (e.g., Katz, Levitt and Shustorovich, 2003; Donohue & Wolfers, 2005) find the execution rate coefficient to be extremely sensitive to econometric specification choice. Furthermore, Zimmerman (2004) finds the deterrent effect of capital punishment to only exist in a current year and to dampen rather quickly with lags. He concludes that only an announcement effect appears to be present where information on policing efforts and executions is disseminated through channels such as the media and word-of-mouth, and potential offenders respond to these signals but not to changes in unobservable judicial probabilities. A number of other empirical researchers (Grogger, 1990; Shepherd, 2004; Berk, 2005; Dezhbakhsh & Shepherd, 2006) drop the conditional probabilities approach and rely on the absolute number of detections, convictions and punishments due to similar concerns. A substantial merit with our empirical setting is that we have subjects (firms) that are likely to be far more rational than potential criminals; further, these firms have every incentive and resource in which to undertake an estimation of their probability of eliciting different types of antitrust actions. In other words, the subjective probabilities by firms attached to eliciting various antitrust actions are more likely to conform to the conditional probabilities than would be the case with criminals. We would dare argue then that the merger policy setting is a far better setting to test the economic theory of deterrence than that of the relationship between executions and future homicides.

In sum, our empirical approach to eliciting the deterrence effects of U.S. merger policy tools can be characterized as drawing and improving upon the following properties from the literature on crime-and-punishment deterrence. First, drawing from the crime-and-punishment deterrence literature yields a more theoretically consistent empirical set-up focusing on the conditional probabilities of investigation, punishment, and severity of punishment. Second, the extensive empirical literature on crime-and-punishment deterrence yields a number of best empirical practices, including the need to consider the inter-connectedness of data observations over time. There, we can use the current state-of-the-art deterrence practice to cluster standard errors over a grouping when appropriate, but also improve upon that practice by employing dynamic panel data models and introducing merger wave driver variables. Third, we will employ the conditional probabilities approach in a setting (merger policy and resulting merger activity) where the agents are more likely to be rational than the dominant setting (capital punishment and resulting homicide activity) used in the economic theory of deterrence literature.

III. Dataset

The data are panel in nature and consist of matching observations from two separate sources: the DOJ and FTC's combined 'Annual Report to Congress on Hart-Scott-Rodino Antitrust Enforcement'; and Compustat's North American database. The above data sources were compiled to yield measures of U.S. merger activity, merger policy actions and economic conditions at the two-digit SIC sector level (seventy sectors) on an annual basis (the 1986–1999 period). Accordingly, each panel consists of a two-digit SIC sector; for instance, 'Tobacco Products' is one distinct panel consisting of fourteen annual observations (1986–1999). While more specific sector data (such as 4-digit SIC data) would be desired, U.S. antitrust authorities publicly report data at only the 2-digit level. Hence, the above represents the best publicly available data on U.S. merger enforcement suitable for a deterrence study.[3]

First, the FTC and DOJ data yield measures of merger activity and merger policy actions for U.S. industrial sectors. With regard to merger activity, we have the annual number of horizontal mergers, non-horizontal merges and total mergers – where total is composed of both horizontal and non-horizontal mergers –by industrial sector (hereafter respectively referred to as Horizontal, Non-Horizontal and Total Mergers).[4] In addition to the measures of merger activity by industry/year, we have two-digit level data on the total number of DOJ and FTC second request investigations, remedies and prohibitions.[5] Yet as already alluded to in previous sections, mergers in industrial sectors evolve in waves. Figure 1 – based on the eventual observations employed in the empirical estimations – charts the average number of Total Mergers per sector from 1989-1999 and thus illustrates the wave-like pattern in which mergers manifest. The wave-like nature of merger activity will be important when setting up our empirical specification; hence, the importance of our second source of data.

[3] See Coate, Higgins and McChesney (1990) and Coate (2005) for studies based on non-public data from internal U.S. antitrust files. While more specific in nature, such data is both unobtainable for those not employed by the antitrust agencies and further not necessarily suitable for a deterrence study.

[4] Horizontal mergers are defined as mergers where both the target and acquirer belong to the same 4-digit SIC industry (though in a few instances it is a 3-digit correspondence); therefore, even though the data is aggregated to the 2-digit level by U.S. authorities, the definition of a horizontal merger is usually at the 4-digit level. Non-horizontal mergers are defined as mergers where the acquirer belongs to a different 4-digit industry. See Table 1 for an exact definition of all the variables we employ.

[5] While the total numbers of second request investigations per industry are routinely reported by the FTC and DOJ, the number of remedies and prohibitions are not. We, therefore, went through the yearly reports and assigned to each antitrust action a two-digit SIC code. More precisely, we assigned an industry code to each remedy and prohibition where a complaint or injunctive relief was filed in a U.S. district court by either the FTC or DOJ.

We constructed industry-level control variables over the 1986-1999 period from Compustat's North America database – a database containing firm-specific information on about 22,000 publicly listed U.S. firms. Such control variables are pivotal for our analysis, as finance economics scholars (e.g., Andrade and Stafford, 2004; Harford, 2005) have recently found industry-factors to be important drivers of merger patterns. In keeping with this recent literature, we constructed for each industry/year a measure of concentration, as well as an annual average in sales growth and cash flow for the firms in that two-digit industry; an exact specification of these variables is given in the next Section. Including these industry specific variables should further control – in addition to employing a dynamic panel data framework – for the cyclical movements in merger behavior.

IV. Estimation and choice of variables

Composition effects

Our main goal is to investigate whether different merger policy tools (investigations, remedies and prohibitions) have an impact on the composition of future merger activity. Following Stigler's (1966) seminal work and given that U.S. antitrust authorities almost exclusively target horizontal mergers, the relevant question in more precise terms should be whether merger policy actions in targeted sectors lead to relative reductions in horizontal merger activity in those particular sectors. Therefore, our main construct of interest is the annual number of horizontal mergers relative to total number of mergers ('RelHoriz') in an industrial sector.

As previously argued, any study of merger behavior should take into account that mergers occur in waves, as is the case in our sample given that it includes the merger wave of the late 1990s. Furthermore, Figure 2 illustrates that the cyclical pattern of merger activity is by and large driven by horizontal mergers; i.e., merger waves are composed of horizontal – not non-horizontal – mergers. Thus, our main construct of interest, RelHoriz, also shows a wave-like pattern. We, therefore, include as right-hand side variables lagged terms of RelHoriz;

hence, current merger behavior is partly explained by past merger behavior. We also include year dummies to capture additional period-specific shocks. Further, given that merger waves can be partly explained by industry factors, we include relevant measures as indicated by Andrade and Stafford (2004). The Andrade and Stafford setup is most suited for our purposes, as they consider the factors driving industry-level patterns of merger intensity. In particular, their panel regressions find industry concentration – as well as industry averages for sales growth and cash flow – to be robustly significant drivers of merger activity.[6] Accordingly, we construct annual two-digit level measures for HHI ('HHI'), sales growth ('Growth') and cash flow ('Cash') – see Table 1 for an exact definition of these variables. In our empirical specification, we lag these measures by one year for two reasons. First, due to the matching of different datasets and slightly different year bases (fiscal year versus calendar year), it is the easiest means to ensure that the control variables precede the dependent variable. Second and related, it is a first step in correcting for the potential endogeneity of the control variables; for example, industry concentration may go up due to increased (horizontal) merger activity.

For our main explanatory variables, we adapt the conditional probability approach from the crime and punishment literature to the context of U.S. merger policy. At the two-digit level, we construct three conditional probabilities (the three deterrence variables); first, the number of investigations over the number of horizontal mergers ('Detect'); second, the number of antitrust actions (remedies + prohibitions) over the number of investigations ('Punish'); and third, the number of prohibitions over the number of antitrust actions ('Degree'). Given that Detect contains horizontal mergers in the denominator, taking lags for this variable might be a sound step to avoid endogeneity problems. Further, given that antitrust actions undertaken are a function of the number of mergers, the Punish and Degree variables are also likely to be endogenous. Accordingly as a first step in controlling for the potential endogeneity of our deterrence variables, we lag the three antitrust probabilities to mitigate endogeneity concerns

[6] See Andrade and Stafford (2004), Table 3 (b) and (c) on page 12-13, where the 'industry adjusted' regressions are fixed effects estimations for panel data.

with any contemporaneous relationship[7]. More specifically, we employ a lagged two-year average for our conditional probabilities.[8] In support of the two-year average, the FTC considers its enforcement efforts to involve a two-year lag in terms of benefits (Davies & Majumdar, 2002). An additional advantage of this variable definition is that it de-sensitizes the deterrence variables to yearly variations. Table 2 reports summary statistics – based on the observations employed in the empirical estimations – for the merger, deterrence and control variables.

Summarizing the above, we estimate how the ratio of horizontal over total mergers – Relative Horizontals or 'RelHoriz' for short – depends on past merger ratios (the previous two years),[9] conditional merger policy probabilities and industry controls:

$$\begin{aligned}(\mathrm{RelHoriz})_{i,t} = \alpha_0 + \sum_{k=1}^{2} \alpha_k (\mathrm{RelHoriz})_{i,t-k} + \alpha_3 \mathrm{Detect}_{i,t-1} + \alpha_4 \mathrm{Punish}_{i,t-1} + \alpha_5 \mathrm{Degree}_{i,t-1} \\ + \delta \mathrm{Controls}_{i,t-1} + \omega_i + \lambda_t + \varepsilon_{i,t},\end{aligned} \quad (1)$$

where i indexes the two-digit SIC industries, t indexes time (year), and k allows for convenient expressions. The merger policy actions consist of two-year averages of the (conditional) probabilities Detect, Punish and Degree – all lagged. The vector Controls represents the vector of lagged industry control variables: industry concentration (HHI), sales growth (Growth) and cash flow (Cash). Finally, ω_i represents the unobserved industry-specific effect, λ_t are the year dummies and $\varepsilon_{i,t}$ the disturbances.

Decomposition

After analyzing the effects of the deterrence variables on the composition of proposed mergers, one can potentially trace back how merger policy tools affect specific types of merger behavior.

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[7] Right from the start of the economic theory of deterrence literature, this endogeneity issue between the dependent variable and conditional probabilities has been recognized. Ehrlich (1973) noted that the probability and severity of punishment are not exogenous variables in the sense that they are determined by the level of crime itself. Further, he observed that expenditure on law enforcement is affected by the rate of crime.

[8] Accordingly, for example, the value for Detect in one particular observation year is the following: $((\mathrm{Investigations}_{t-1} + \mathrm{Investigations}_{t-2}) / (\mathrm{Horizontal\text{-}Mergers}_{t-1} + \mathrm{Horizontal\text{-}Mergers}_{t-2}))$.

[9] Regressions indicate that the model with two merger lags yields the best results.

In other words, if merger policy tools have a deterrence effect on the composition of proposed mergers, it is possible that this composition-based deterrence effect owes to firms – in response to a higher likelihood of policy intervention – proposing fewer horizontal mergers and/or more non-horizontal mergers. Therefore, in a second step, we consider whether merger policy tools have an impact on horizontal and non-horizontal mergers separately. In other words, we regress two equations

$$Horiz_{i,t} = \beta_0 + \sum_{k=1}^{2} \beta_k Horiz_{i,t-k} + \beta_3 Detect_{i,t-1} + \beta_4 Punish_{i,t-1} + \beta_5 Degree_{i,t-1} + \delta Controls_{i,t-1} + \omega_i + \lambda_t + \varepsilon_{i,t}, \quad (2)$$

$$NonHoriz_{i,t} = \gamma_0 + \sum_{k=1}^{2} \gamma_k NonHoriz_{i,t-k} + \gamma_3 Detect_{i,t-1} + \gamma_4 Punish_{i,t-1} + \beta_5 Degree_{i,t-1} + \delta Controls_{i,t-1} + \omega_i + \lambda_t + \varepsilon_{i,t}, \quad (3)$$

where both Horiz and NonHoriz are log-transformed.[10] Donohue and Wolfers (2005) point out that deterrence variables require the consideration of scaling issues; hence, we log-transform our merger frequency variables (Horizontal and Non-Horizontal) to yield some additional estimation advantages. First, log-transforming helps moderate – or cancel out – potential size differences between the different industries via the estimation of a log-linear regression model (recall that our conditional probabilities are not in logs). Second, log-transforming also addresses to some extent the count nature of the data on merger frequencies by making the variable constructs more continuous.

Estimation Strategy

For all three specifications, it behooves us to employ the methodology of dynamic panel data models (see Bond, 2002, for an overview), as we include autoregressive dynamics of the dependent variable (RelHoriz, Horiz or NonHoriz) on the right-hand side. The serial correlation

[10] It behooves us to include lags of non-horizontal mergers as explanatory variables for reasons beyond simply consistency with the other regression models. Tests show that, although non-horizontal mergers do not move in waves (see Figure 2), the number of non-horizontal mergers still correlates over time.

in all mergers series implies that a least-squares or within-groups estimation would result in biased and inconsistent estimates. For this reason, we estimate our expression instrumenting for our lagged dependent variable – as well as all other potentially endogenous variables – using the system generalized method of moments (System GMM) estimator proposed by Arellano and Bover (1995). Dynamic panel data methods are specially designed to properly control for wave-contexts: Bond (2002: 142) states that “allowing for dynamics in the underlying process [a merger wave] may be crucial for recovering consistent estimates of other parameters [the deterrence variables]”. Given that papers both in the literatures of crime and punishment and merger waves do not use this methodology, employing the appropriate dynamic panel method represents a merit of this paper.

Arellano and Bond (1991) developed a GMM estimator that treats the model as a system of equations – one for each time period – where the predetermined and endogenous variables in first differences are instrumented with suitable lags of their own levels. A problem with the original Arellano-Bond estimator is that lagged levels are often poor instruments for first differences. Adding an equation in levels to be estimated with the equation in first differences (namely, estimating a system of equations) improves the performance of the estimator. Arellano and Bover (1995) described how – by adding the original equations in levels – additional moment conditions could be brought to bear to increase efficiency and reduce finite sample bias; hence, we employ Stata’s procedure for System GMM. Two testable assumptions are required for the use of these estimators. First, in order to reach identification, the disturbances $\varepsilon_{i,t}$ must be serially uncorrelated. This is equivalent to having no second-order serial correlation in the first differenced residuals, and can thus be directly tested in the first-differenced model. Second, the instruments must be uncorrelated with the first-differenced residuals, which can be tested using the Hansen test of over-identifying restrictions.

Accordingly, it behooves us to instrument for all potentially endogenous and predetermined variables. In particular, we treat the lagged dependent variables as endogenous (as the methodology of dynamic panel data prescribes). Second, recall that our employment of lagged antitrust probabilities as explanatory variables mitigates the endogeneity problems due to contemporaneous relationships; the Detect probability directly through its denominator containing horizontal mergers, the Punish and Degree probabilities indirectly through investigations and antitrust actions possibly being a subset of the number of notified mergers in that year. Nevertheless, our lagged merger policy probabilities Punish and Degree may be correlated with past merger notification shocks when an antitrust authority does not come to a definite decision in the same year as the merger notification. Further, although it is unlikely that merger investigations are initiated later than a year after their notification, the inclusion of lagged merger variables introduces issues of multi-collinearity between the lagged merger and deterrence variables, which potentially makes the interpretation of the Detect coefficient and size of standard errors unreliable. Also our industry control variables may still be predetermined – despite including these variables lagged one year – given that merger shocks may propagate slowly through to sales and profits. Accordingly, we instrument – using system GMM – for the deterrence variables and the industry control variables.

A downside of the proposed methodology is that – although the number of valid moment conditions increases with the number of periods and these improve efficiency – the system GMM estimator may use too many moment conditions with respect to the number of available observations. Put simply, too many instruments may lead to over-fitting the instrumented variables and bias the results. Accordingly, it behooves us to estimate – as a robustness check – our regression equation by only instrumenting for the lagged dependent and deterrence variables, while treating the industry control variables as exogenous. By doing so, we can keep the number of instruments relatively low and mitigate the over-fitting bias. Still, it could be that the efficiency gains from system GMM are also small. Therefore – keeping in mind that fixed-effects estimations potentially suffer from correlation between the (transformed) lagged dependent variables and the (transformed) error term – we also report fixed effects results with standard errors clustered at the industry level, which mitigates to some extent the serial correlation of the merger series. This specification has the added advantage that it is the currently preferred specification in the crime-and-punishment deterrence papers. Although the GMM specification seems most suited to deal with a wave-like phenomenon such as mergers, the fixed effects specification serves as a robustness check and allows us to make the link with the bulk of the deterrence literature. Finally, as another robustness check, we also employ Tobit fixed effects panel data estimation.[11] Given that we deal with the annual number of horizontal mergers in a particular two-digit industry, it is possible that our dependent merger variable shows a truncated distribution, i.e. our merger variable may be left-censored at zero. Although only about 10% of the observations of our dependent merger variable are actually zero, we still report results for the Tobit panel estimation. This specification has the added advantage that it can be more directly compared to Andrade and Stafford's (2004) contribution: where they have a panel data estimation on horizontal mergers that we build upon by including deterrence variables in addition to the industry drivers of merger waves.[12]

Our main empirical results for each of our three empirical setups – the ratio of horizontal to total mergers (specification 1), the number of horizontal mergers (specification 2) and the number of non-horizontal mergers (specification 3) – consist of four regression specifications that attempt to take the above issues into account. Each setup involves four regressions that all involve fixed period-specific effects and clustered standard errors when the GMM procedure is not invoked. Regression #1 reports the results of the fixed panel-and-period specific effects procedure. Regression #2 reports the results of the Tobit unconditional fixed panel-and-period specific effects procedure. Regression #3 reports the results of a GMM estimation where only the lagged dependent variables and antitrust probabilities are instrumented for. Regression #4 reports a GMM estimation where also the industry variables are instrumented for.

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[11] In particular, we use an unconditional fixed-effects Tobit model with clustered standard errors. As with the normal fixed effects estimator in our model, it must be kept in mind that unconditional fixed-effects coefficient estimates may be biased due to the untreated endogeneity in the lagged dependent variables.

[12] Their industries include more 'zero' observations as they employ a different industry definition.

V. Empirical results

Table 3 reports the estimation results for the four regression specifications where the composition of merger activity (Relative-Horizontals) is the dependent variables. Before discussing the constructs of primary interest, we comment on the adequateness of the model. First, the Hansen test of over-identifying restrictions yields evidence in both GMM estimations (regressions' #3 & #4) that one cannot reject the hypothesis of no correlation between instruments and error terms. Second, the null hypothesis of no second order autocorrelation on the error differences also cannot be rejected, suggesting that serial autocorrelation does not exist in error levels (the smallest of both estimations reports Pr>z=0.66). Third, the R-squared term in Regression #1 is 0.41 and the log likelihood of the Tobit Regression #2 is 30.704. Accordingly, the regression model passes the necessary diagnostics and appears to be well-specified. We comment now on the control variables:

The two lags of horizontal over total mergers (Relative-Horizontals) seem to be relevant: the first is positive and highly significant in all four estimations; the second is positive – although only significant for the two GMM specifications – yet its inclusion is appropriate as the serial correlation in the error term only vanishes when including the second lag.

All three industry control variables have the same sign as in Andrade and Stafford (2004): HHI is negative, Growth is positive and Cash is positive, although Cash is never significant. A negative impact of HHI on the relative number of horizontal mergers may indicate that these industries are more closely watched by the DOJ and FTC; hence, relatively fewer horizontal mergers are proposed in highly concentrated industries. The positive and significant impact of Growth shows that horizontal mergers in general are more likely in growing industries – a result in line with both prior empirical evidence and theoretical predictions (see Banal-Estanol et al., 2008, for an overview).

We can now look at the results for the variables of primary interest: the relationship between the deterrence variables and the composition of future merger activity. First, the probability that a proposed merger is investigated (Detect) has a statistically significant and negative impact on the ratio of future horizontal mergers in three out of four regression equations. Second, the conditional probability of applying an action once investigated (Punish) has a statistically significant and negative impact on the ratio of future horizontal mergers in all four regression equations, though the impact is smaller in size than the Detect probability. Third, the conditional probability of applying prohibitions (Degree) is negative but only significant in one out of the four regression equations. The consistent impact of the Punishment variable suggests that spikes in the relative use of antitrust actions sends a clear signal of toughness by antitrust authorities that appears to be internalized by firms, as it significantly reduces the relative number of horizontal mergers. Furthermore, the probability of investigations also has a negative impact that is relatively robust and negative. Yet, the use of more severe antitrust actions – i.e., employing more prohibitions with respect to remedies – does not appear to involve robust deterrence.

In order to ensure that the deterrence effects elicited above are reflective of reduced horizontal merger activity and not increased non-horizontal merger activity (since both changes could be behind relatively fewer horizontal mergers), we investigate the underlying merger patterns to respectively consider the impact of the deterrence variables on the absolute number of horizontal and non-horizontal mergers. In other words, we now attempt to factor the underlying frequency-based deterrence effects in order to ensure that we are correctly interpreting the composition-based deterrence effects. We do so simply by employing the same regression specification, but with the relative-horizontals dependent variable being respectively replaced with the number of horizontal and non-horizontal mergers. In terms of empirical modeling, both the horizontal and non-horizontal models seem to be well specified.[13] Further, for the horizontal mergers the industry variables Growth (positive) and HHI (negative) are strongly significant and with a larger coefficient in absolute size than in our main model; for the non-horizontal mergers, the industry control variables are never significant. This result should not surprise, as recent merger wave papers have shown that (i) waves are composed of horizontal mergers – see also our Figure 2 – and (ii) industry variables are important in explaining these waves.[14]

We can now look at the causal relations between our significant deterrence variables of the main regression (Detect and Punish). First, the Detect probabibility has a non-significant impact on both horizontal and non-horizontal mergers – a result which diminishes our confidence in the robustness of the above findings concerning the impact of Detect on Relative-Horizontals. Nevertheless, given that the detect probability seems to have a less negative (and sometimes even positive) impact on non-horizontal than on horizontal mergers, it may be that the detection probability (despite having no impact on each type of merger behavior separately) does have a resulting significant net impact on the relative number of horizontal mergers proposed. Second, the conditional probability of applying an additional action (Punish) has a robust negative impact on horizontal mergers, and seems to have no significant impact on non-horizontal mergers. This result suggests the robustness of the Punishment variable, as increasing the probability of investigations leading to actual Antitrust Actions appears to reduce the number of horizontal mergers in both relative and absolute terms.

[13] First, the Hansen test of over-identifying restrictions yields evidence in all GMM estimations that one cannot reject the hypothesis of no correlation between instruments and error terms (the lowest value is p=>0.73). Second, the null hypothesis of no second order autocorrelation on the error differences cannot be rejected, suggesting that serial autocorrelation does not exist in the error terms (see regressions' #3 & #4 in Tables 3 and 4). The R-squared for the fixed effects regressions is 73% and 75% respectively (see regressions #1 in Tables 3 and 4) and the log-likelihoods of the Tobit regressions are -438 and -583 (see regressions #2 in Tables 3 and 4).

[14] Further evidence for this can be found in the lagged dependent variables: whereas for the horizontal-merger specification the two lags are positive and significant, for the non-horizontal mergers only the first lag is consistently significant, and furthermore that first lag is relatively smaller in absolute values and not robust in sign for the non-horizontal merger regression equations.

In sum, the probability of investigating mergers (Detect) has a negative impact on the future ratio of horizontal to total mergers; yet this negative relationship cannot be traced back when dismantling this composition-based deterrence effect into horizontal and non-horizontal mergers. Second, the conditional probability of applying an antitrust action given an investigation has a robust negative impact, as this effect can be traced back to having a stronger negative impact on future horizontal mergers and having no significant impact on non-horizontal mergers. Finally, the conditional probability of applying relatively more prohibitions, given that an antitrust action is used, has a negative but not statistically robust impact, both on the relative and absolute number of proposed horizontal merger notifications.

VI. Conclusion

Merger policy deterrence has generally gone under-studied by law and economics researchers. Accordingly, we use the established framework from the crime and punishment deterrence literature and adapt it to a merger-policy framework. In particular, we investigate how probabilities of detection (the number of second request investigations relative to proposed horizontal mergers), punishment (the number of antitrust actions – prohibitions and remedies – relative to the number of investigations) and degree of punishment (the number of prohibitions relative to the total number of antitrust actions) have a deterrence impact on the future composition of proposed mergers. Accordingly, we bring empirical evidence to bear on this issue by building a cross-industry data set spanning the 1986-1999 period that is composed of measures for U.S. merger activity, for U.S. merger policy, and for industry control variables capturing merger wave drivers. Our data are sufficiently rich and detailed to allow consideration of whether changes in the relevant conditional probabilities for merger policy enforcement impact both the composition of future merger notifications (the relative number of horizontal mergers) and the frequency of future merger notifications (the absolute number horizontal and non-horizontal mergers).

With regards to the composition of future mergers, we find the probability of eliciting an investigation (Detection) and even more so the conditional probability of eliciting an antitrust action (Punishment) by U.S. antitrust authorities to have a robust and negative impact on the relative number of horizontal mergers in subsequent years. However, the conditional probability of eliciting a prohibition with respect to an antitrust action (Severity) does not appear to entail significant composition-based deterrence effects.

With regards to the frequency of future mergers, we find the probability of eliciting an investigation (Detection) – and the conditional probability of eliciting a prohibition (Severity) – to not significantly impact either the absolute number of horizontal or non-horizontal mergers in subsequent years. Yet the conditional probability of eliciting an antitrust action (Punishment) does significantly reduce the proclivity of firms to submit horizontal mergers in subsequent years. Accordingly, the robustness of the Punishment variable is confirmed in the auxiliary regressions, while the robustness of the Detection variable is cast in more doubt.

In sum, our results indicate that the composition of horizontal merger activity is to some extent negatively influenced by past antitrust investigations, but is even more influenced by the application of past antitrust actions. Our ability to show that the conditional probability of eliciting an antitrust action (Punishment) deters future horizontal merger activity both in relative and absolute terms suggests that the application of antitrust actions involves a robust deterrence effect. Furthermore, the results suggest that while both prohibitions and remedies involve deterrence, there is no significant difference between both actions; i.e., prohibitions do not involve significantly more deterrence in the U.S. merger policy context than do remedies.

The empirical results also indicate that higher antitrust activity in a particular industry sector reduces the relative number of potentially anti-competitive mergers activity in that sector – when making the reasonable assumption that anti-competitive mergers are a subset of horizontal mergers. Given that there does not seem to be a shift towards non-horizontal mergers in the same sector (i.e., the deterrence variables do not encourage non-horizontal merger activity), it would be interesting to further investigate whether merger activity is moved to other less-scrutinized industries, or whether merger activity in the whole economy diminishes. We leave this for future work.

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Figures and Tables

Figure 1: The number of mergers per industry shows a wave-like pattern (# Total Mergers, averaged over seventy U.S. SIC2 industries, 1989-1999)

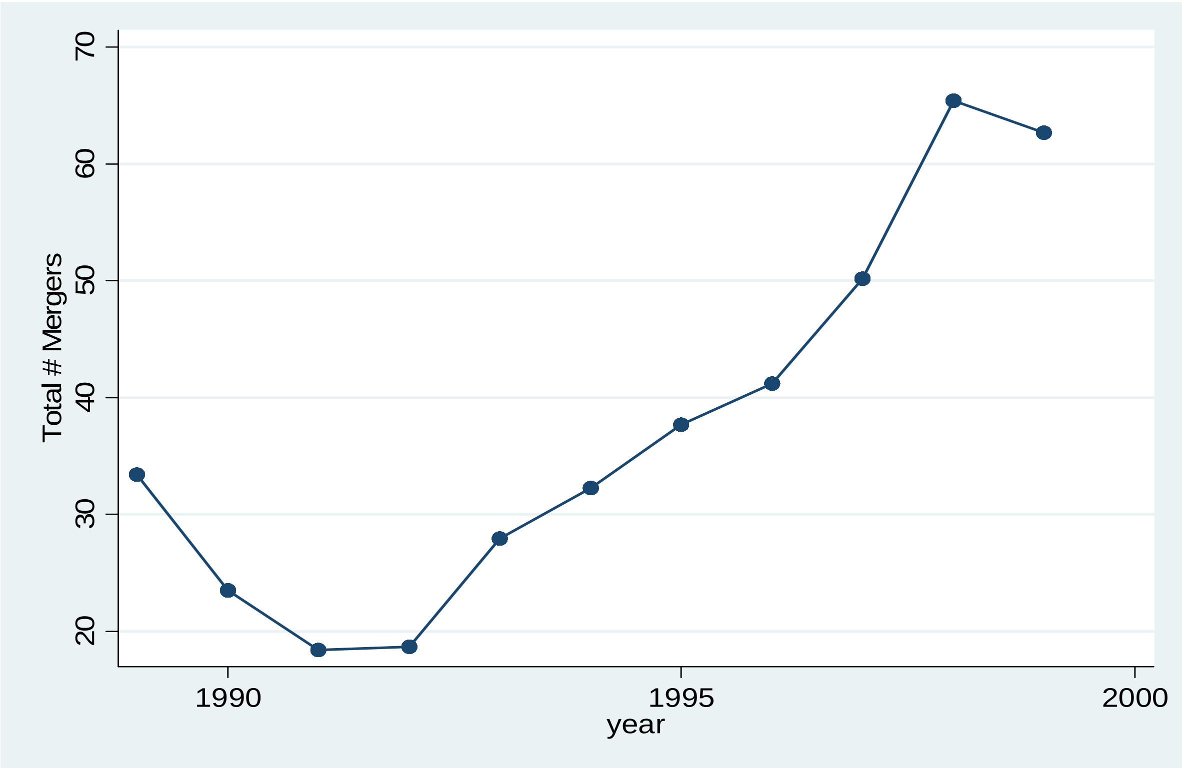

Figure 2: The Wave-like Pattern is Due to Horizontal Mergers
(All Merger measures averaged over seventy SIC2 industries)

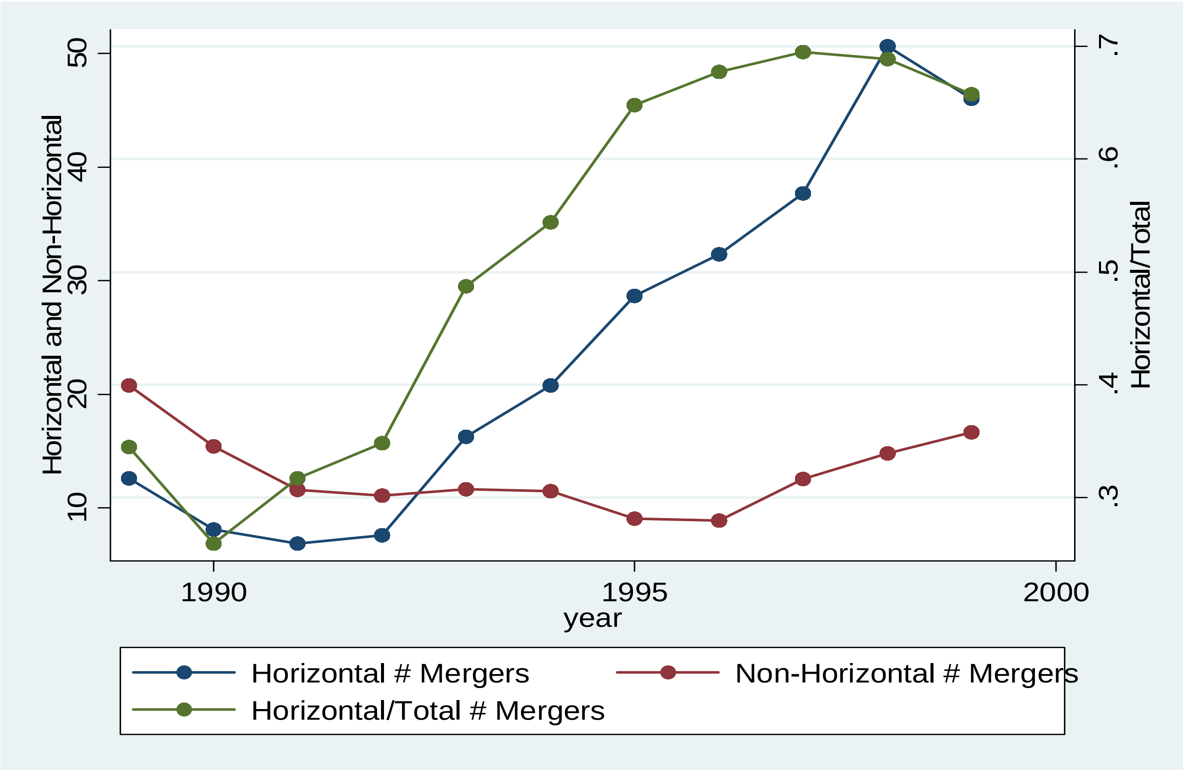

Table 1. Description of Variables Used in the Regressions

Variable	Description
HORIZONTAL	Log of the yearly number of horizontal mergers (+1 for zero) in a SIC2 industry *i*. 'Horizontal' defined as target and acquirer from same industry at the SIC4 level $\in$ SIC2 industry *i*.
NON-HORIZONTAL	Log of the yearly number of non-horizontal mergers (+1 for zero) in a SIC2 industry *i*. 'Non-horizontal' defined as acquirer coming from a different SIC4 industry than target's SIC4 industry $\in$ SIC2 industry *i*.
TOTAL	Log of the total yearly number of mergers (+1 for zero) in a SIC2 industry *i*. 'Total' defined as all mergers where targets belong to a SIC4 industry $\in$ SIC2 industry *i*.
REL-HORIZ	The yearly number of horizontal merger as a percentage of the yearly number of total mergers in a SIC2 industry *i*. 'Horizontal' defined as target and acquirer from same industry at the SIC4 level $\in$ SIC2 industry *i*. 'Total' defined as all mergers where targets belong to a SIC4 industry $\in$ SIC2 industry *i*.
DETECT	Two-year sum of FTC and DOJ second request investigations ('investig') over two-year sum of horizontal mergers in SIC2 industry i, $DETECT_{it} = (investig_{it} + investig_{it-1})/(horizontal_{it} + horizontal_{it-1})$ 'Horizontal' defined as target and acquirer from same industry at the SIC4 level $\in$ SIC2 industry *i*.
PUNISH	Two-year sum of FTC and DOJ remedies ('remed') and prohibitions ('proh') through a U.S. district court over two-year sum of second request investigations ('investig') in SIC2 industry *i*, $PUNISH_{it} = (remed_{it} + remed_{it-1} + proh_{it} + proh_{it-1})/(investig_{it} + investig_{it-1})$.
DEGREE	Two-year sum of FTC and DOJ prohibitions ('proh') through a U.S. district court over Two-year sum of FTC and DOJ remedies ('remed') and prohibitions ('proh') through a U.S. district court in SIC2 industry *i*, $DEGREE_{it} = (proh_{it} + proh_{it-1})/(remed_{it} + remed_{it-1} + proh_{it} + proh_{it-1})$
HHI	Log of the Herfindahl index and SIC2 industry based on sales; i.e., for all firms *j* that constitute industry *i*, $HHI_{it} = \log[\sum_{j \in i}(Sales_{jt} / TotalSales_{it})^2]$, for $Sales_{jt} > 0$.
GROWTH	Average sales growth over last two years in a given SIC2 industry *i*; i.e, for J firms *j* that constitute industry *i*, $GROWTH_{it} = \frac{1}{J}\sum_{j\in i}[(Sales_{jt} - Sales_{jt-2}) / Sales_{jt-2}]$ for $Sales_{jt}, Sales_{jt-2} > 0$.
CASH	Average earnings before interests, taxes, depreciation and amortization (EBITDA) over sales in a given SIC2 industry; i.e, for J firms *j* that constitute industry *i*, $CASH_{it} = \frac{1}{J}\sum_{j\in i}(EBITDA_{jt} / Sales_{it})$ for $Sales_{jt} > 0$.

Table 2. Preliminary Statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Horizontal	690	25.02	43.61	0	342
Non-Horizontal	690	13.08	16.52	0	173
Total	690	38.10	55.28	0	515
Rel-Horizontal	690	0.52	0.27	0	1
Detect	690	0.07	0.13	0	1
Punish	681	0.10	0.29	0	1
Degree	690	0.10	0.28	0	1
HHI	689	-2.41	0.88	-4.88	-0.10
Growth	689	0.19	0.15	-0.24	1.17
Cash	689	0.12	0.10	-0.31	0.58

TABLE 3: REGRESSIONS OF RELATIVE HORIZONTAL MERGER CONDUCT ON ANTITRUST PROBABILITIES

Variable	Fixed Panel & Period Effects (1)	Tobit Panel & Period Effects (2)	GMM1 Instrumenting for Mergers & Antitrust Probabilities (3)	GMM2 Instrumenting for Full set (4)
RelHoriz $_{t-1}$	0.182***	0.186***	0.146*	0.164**
	(0.0469)	(0.0510)	(0.0820)	(0.0793)
RelHoriz $_{t-2}$	0.0623	0.0619	0.187**	0.203***
	(0.0670)	(0.0753)	(0.0753)	(0.0641)
$_{k=1}\sum^{2}$ Detect $_{t-k}$ /2	-0.123*	-0.125*	-0.153	-0.221**
	(0.0681)	(0.0707)	(0.104)	(0.0974)
$_{k=1}\sum^{2}$ Punish $_{t-k}$ /2	-0.0441*	-0.0470**	-0.0529**	-0.0729**
	(0.0228)	(0.0235)	(0.0240	(0.0288)
$_{k=1}\sum^{2}$ Degree $_{t-k}$ /2	-0.0178	-0.0150	-0.0682*	-0.0526
	(0.0325)	(0.0345)	(0.0413)	(0.0385)
Cash $_{t-1}$	0.0531	0.0905	-0.194	0.254
	(0.234)	(0.306)	(0.648)	(0.226)
Growth $_{t-1}$	0.152*	0.167*	0.446*	0.186
	(0.0913)	(0.0979)	(0.261)	(0.131)
HHI $_{t-1}$	-0.0515	-0.0601	-0.134***	-0.155***
	(0.0345)	(0.0427)	(0.0377)	(0.0411)
Constant	0.269***	0.0881	-0.0111	-0.0768
	(0.0900)	(0.0629)	(0.111)	(0.0880)
Arellano-Bond test that aver. auto covariance in residuals of order 2 is 0			z = -.43422 Pr > z = (0.6641)	z = -.40037 Pr > z = 0.6889
Hansen Test of over-identifying restrictions			chi2(92)=42.97334 Prob > chi2=1.0	chi2(123)=51.20254 Prob > chi2=1.0
sigma Constant		0.207*** (0.0160)		
R^2	0.410			

NOTE.—The dependent variable is the relative number of horizontal over total notified mergers. All four estimations involve fixed period-specific effects (year dummies) and 690 observations. Robust standard errors are in brackets. Furthermore, *** = 1%, ** = 5%, and * = 10% Significance.

TABLE 4: REGRESSIONS OF HORIZONTAL MERGER CONDUCT ON ANTITRUST PROBABILITIES

Variable	Fixed Panel & Period Effects (1)	Tobit Panel & Period Effects (2)	GMM1 Instrumenting for Mergers & Antitrust Probabilities (3)	GMM2 Instrumenting for Full set (4)
Horiz $_{t-1}$	0.281***	0.431***	0.541***	0.226***
	(0.0603)	(0.0496)	(0.0850)	(0.0642)
Horiz $_{t-2}$	0.153***	0.143***	0.222***	0.282***
	(0.0431)	(0.0439)	(0.0797)	(0.0704)
$_{k=1}\Sigma^{2}$ Detect $_{t-k}$ /2	-0.0962	-0.227	-0.719	-0.292
	(0.153)	(0.216)	(0.472)	(0.548)
$_{k=1}\Sigma^{2}$ Punish $_{t-k}$ /2	-0.132**	-0.150**	-0.220*	-0.296**
	(0.0641)	(0.0734)	(0.128)	(0.138)
$_{k=1}\Sigma^{2}$ Degree $_{t-k}$ /2	-0.0703	-0.0987	-0.0484	-0.325*
	(0.0892)	(0.0950)	(0.179)	(0.197)
Cash $_{t-1}$	-0.436	0.0998	-1.443	-2.031
	(0.477)	(0.469)	(2.419)	(1.407)
Growth $_{t-1}$	0.588***	0.610***	1.880*	1.399**
	(0.219)	(0.211)	(0.959)	(0.599)
HHI $_{t-1}$	-0.198	-0.373***	-0.957*	-0.844***
	(0.124)	(0.119)	(0.509)	(0.317)
Constant	1.141***	-0.294	-1.969**	-0.960
	(0.398)	(0.371)	(0.993)	(0.619)
Arellano-Bond test that aver. auto covariance in residuals of order 2 is 0			z = -.68925 Pr > z = 0.4907	z = -2.1572 Pr > z = 0.0310
Hansen Test of over-identifying restrictions			chi2(61)=53.55637 Prob > chi2=0.7397	chi2(133) = 61.92712 Prob > chi2 = 1.0
sigma Constant		0.555*** (0.0202)		
R^2	0.671			

NOTE.—The dependent variable is the number of horizontal notified mergers (in logs). All four estimations involve fixed period-specific effects (year dummies) and 690 observations. Robust standard errors are in brackets. Furthermore, *** = 1%, ** = 5%, and * = 10% Significance.

TABLE 5: REGRESSIONS OF NON-HORIZONTAL MERGER CONDUCT ON ANTITRUST PROBABILITIES

Variable	Fixed Panel & Period Effects (1)	Tobit Panel & Period Effects (2)	GMM1 Instrumenting for Mergers & Antitrust Probabilities (3)	GMM2 Instrumenting for Full set (4)
NonHoriz $_{t-1}$	0.145***	0.132***	-0.349**	-0.294***
	(0.0536)	(0.0437)	(0.139)	(0.0812)
NonHoriz $_{t-2}$	0.214***	0.207***	-0.0796	0.0562
	(0.0516)	(0.0421)	(0.0860)	(0.0962)
$_{k=1}\Sigma^2$ Detect $_{t-k}$ /2	0.0386	0.00308	-0.177	-0.00771
	(0.223)	(0.154)	(0.501)	(0.395)
$_{k=1}\Sigma^2$ Punish $_{t-k}$ /2	-0.0246	-0.0332	-0.000975	-0.126
	(0.0670)	(0.0613)	(0.0903)	(0.151)
$_{k=1}\Sigma^2$ Degree $_{t-k}$ /2	-0.0196	-0.0261	0.0387	-0.0223
	(0.0664)	(0.0727)	(0.120)	(0.272)
Cash $_{t-1}$	-0.0694	-0.0769	-1.032	-1.770
	(0.331)	(0.340)	(3.188)	(1.455)
Growth $_{t-1}$	0.266	0.229	-0.172	-0.0437
	(0.252)	(0.217)	(0.732)	(0.703)
HHI $_{t-1}$	-0.145	-0.155	-0.144	-0.436
	(0.111)	(0.0971)	(0.407)	(0.273)
Constant	0.928***	0.367**	2.989***	1.947**
	(0.345)	(0.185)	(1.122)	(0.775)
Arellano-Bond test that aver. auto covariance in residuals of order 2 is 0			z = .62806 Pr > z = 0.5300	z = -.66892 Pr > z = 0.5035
Hansen Test of over-identifying restrictions			chi2(61) = 54.09199 Prob > chi2 = 0.7223	chi2(133) = 52.53489 Prob > chi2 = 1.000
sigma Constant		0.445*** (0.0158)		
R^2	0.263			

NOTE.—The dependent variable is the number of notified non-horizontal mergers (in logs). All four estimations involve fixed period-specific effects (year dummies) and 690 observations. Robust standard errors are in brackets. Furthermore, *** = 1%, ** = 5%, and * = 10% Significance.