Settle for Now but Block for Tomorrow:

The Deterrence Effects of Merger Policy Tools

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Abstract: Antitrust policy involves not just the regulation of anti-competitive behavior, but also an important deterrence effect. Neither scholars nor policymakers have fully researched the deterrence effects of merger policy tools, as they have been unable to empirically measure these effects. We consider the ability of different antitrust actions – blocked-mergers, negotiated-settlements, and monitorings – to deter firms from engaging in mergers. We employ cross-jurisdiction/pan-time data on merger policy to empirically estimate the impact of antitrust actions on future merger frequencies. We find blocked-mergers to lead to decreased merger notifications in subsequent periods, and negotiated-settlements to weakly increase future merger notifications; in other words, blocked-mergers involve a deterrence effect but negotiated-settlements do not.

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I. INTRODUCTION

Antitrust policy involves more than just policing actual antitrust cases and violations; it also involves deterrence. Paul Joskow states "U.S. antitrust policy is primarily a 'deterrence' system not a regulatory system" (2002: 98). Antitrust simply cannot scrutinize and vet all market behaviors, but instead relies on the development of rules which businesses are expected to internalize into their decisions. That deterrence effect is best enabled by an antitrust law system with clear signals and repercussions. For instance, Stigler (1966) attributes the decline in the relative frequency of horizontal mergers in the 1950s to the deterrence effects of more vigorous enforcement of US antitrust laws. Accordingly, antitrust actions – when entwined with effective deterrence – may involve robust policy implications.[1] In essence, the number of actual cases involving antitrust actions is potentially just the 'tip of the iceberg' when it comes to merger policy effects, as additional anti-competitive mergers are considered by businesses but never proposed (Davies & Majumdar, 2002).

Despite being an important issue, merger policy deterrence has generally been under-researched due to measurement difficulties. For instance, the influential U.S. Federal Trade Commission (1999) divestiture study does not factor deterrence implications; but instead, notes that merger enforcement's total effect is much greater than reflected by actual antitrust actions (that is, deterred merger undertakings must certainly exist). This non-accounting for merger-policy deterrence is also exhibited by the U.S. Department of Justice which stated in its 2001 Congressional submission: "we have not attempted to value the deterrent effects … of our successful enforcement efforts. While we believe that these effects … are very large, we are unable to approach measuring them" [Davies & Majumdar, 2002: p. 72]. A number of scholars (Eckbo, 1989; Davies & Majumdar, 2002; Joskow, 2002; Crandall & Winston, 2003; Baker, 2003) agree that not accounting for deterrence effects suggests that the total effect of merger policy is far more consequential than we currently acknowledge.

[1] By antitrust actions, we collectively refer to the use of various merger policy tools by authorities: monitorings, negotiated-settlements and blocked-mergers all constitute antitrust actions.

Accordingly, we study the impact of different merger policy tools on the proclivity of firms to engage in future merger transactions. Since antitrust policy is directly aimed at firms' conduct, considering the effect of changes in merger policy on future merger behavior merits attention. The immediate benefits of such a study are twofold. First, we begin the process of factoring the deterrence effects of merger policy; thus, we start to quantify what has until now been unquantified. Second, by considering the deterrence effects benefits of different merger policy tools we are able to elicit which particular tools involve adequate deterrence. As Crandall and Winston argue, a need exists to "explain why some enforcement actions … are helpful and others are not" (2003: 4). We begin that exercise of detecting the effectiveness of different merger policy instruments by considering whether blocked-mergers (merger out-and-out rejected by the authority), negotiated-settlements (merger cleared but with behavioral and/or structural undertakings), and monitorings (merger cleared but subject to potential future oversight) act as effective deterrents with regard to future merger frequencies.

The paper is structured as follows. Section II reviews the relevant existing literature in order to properly frame our contribution. Section III generates empirical expectations by setting out a model where Antitrust Actions represent costs to proposed merger transactions; thus, spikes in antitrust actions potentially result in fewer future mergers. Section IV describes the unique cross-section/time-series data set (28 antitrust jurisdictions over 1992-2005) employed for the empirical estimations. Section V describes issues and techniques with regard to our dynamic panel data estimation. Section VI presents empirical results, and Section VII robustness tests. Section VIII discusses and concludes.

II. BACKGROUND ON RELATED LITERATURE

A number of studies mention the importance of deterrence for merger policy (Joskow, 2002; Crandall & Winston, 2003; Baker, 2003); however, very few go further to analyze in depth the deterrence effects involved with merger policy. Instead, the great majority of the substantive scholarship on antitrust deterrence focuses on anti-cartel and collusion policies. Nevertheless, two general strands of empirical literature exist that broadly consider the merger-policy/deterrence-effect relationship: first, a few studies consider the composition of mergers to elicit how merger policy law changes might alter the nature of proposed mergers; second, a series of studies employ price-cost margins to measure the net-impact of antitrust and merger policies. The following paragraphs quickly review the two literature strands in order to further motivate our analysis and nest our contribution.

First, a small literature exists on merger policy deterrence as measured by the composition of proposed mergers. As Aaronson (1992) notes, deterrence conceivably manifests in merger plans being forsaken (namely, abandoned), but also in the composition of mergers being shaped differently (namely, modified). Stigler (1966) first considered composition effects when he found the 1950 Clayton Act amendment to discourage horizontal mergers, and encourage vertical and conglomerate mergers; hence, the composition of mergers tilted toward non-horizontal types. Following in this vein, Eckbo and Wier (1985) present evidence that non-merging firms (rivals to the merger) generally earned higher stock market premiums prior to the onset of the U.S. Hart-Scott-Rodino (HSR) reform than they did post-HSR; thus, suggesting that HSR deterred a number of anti-competitive mergers.[2] In later work, Eckbo (1992) makes use of the absence of Canadian antitrust enforcement prior to 1985 to indirectly test whether U.S. policy deters anti-competitive mergers; he finds no evidence that Canadian mergers are more anti-competitive than U.S. mergers.

Second, a number of studies look at how antitrust indirectly influences firm behavior by studying its impact on industry market power and price-cost margins. This literature tends to be inconclusive as some studies generate evidence critical of antitrust policy while others generate supportive evidence. The critical studies include: Crandall and Winston's (2003) finding unsuccessful merger challenges to decrease and consent-decrees (divestitures) to

[2] Eckbo and Wier (1985) go on to question HSR's optimality as they find merger control to be anti-competitive in that rival firms surprisingly experience positive abnormal returns when authorities challenge mergers.

increase price-cost margins in U.S. manufacturing industries; Konings, Van Cayseele and Warzynski (2001) detecting no changes in Belgian firms' markups after the strengthening of Belgian antitrust laws in 1993, but lower price markups in the Netherlands where there was then no effective antitrust policy. The supportive studies include: Block, Nold and Sidak's (1981) finding larger U.S. antitrust budgets to decrease bread markups; Warzynski (2001) detecting tougher U.S. merger policy from 1945-1973 leading to lower firm markups as compared to the lax 1973-1991 period; Hoekman and Kee's (2003) forty-two country study where antitrust legislation generates lower markups via improved entry conditions.

The literature summarized above covers a great deal of ground and generates many important insights, but suffers from some limitations. First, while some literature considers how merger policy modifies merger types, there appears to be a dearth of literature analyzing merger frequencies. Second, the price-cost markup literature finds it difficult to disentangle direct regulatory effects from indirect deterrence effects: in essence, markups capture the 'total effect' of antitrust policies. Furthermore, by only looking at resulting market outcomes, the markup studies fail to consider merger behavior directly and fail to capture firms' direct responses to changing policies. Third, most of the studies in the merger policy realm use only one observation of policy change: the before versus after a policy change comparison (Stigler, 1966; Eckbo & Wier, 1985; Konings, Van Cayseele & Warzynski 2001; Warzynski, 2001; Hoekman & Kee, 2003), or the countries having versus not-having antitrust laws comparison (Eckbo, 1992; Konings, Van Cayseele & Warzynski, 2001).

We propose to directly analyze the deterrence effects of different merger policy tools on future merger behavior; by doing so, we improve upon the existing literature in some respects. First, by fixing merger notifications as the dependent variable of interest, we provide evidence on how many mergers are forsaken as opposed to the literature's (admittedly a small literature) focus on the composition of proposed mergers. Second, by considering future changes in the number of notified mergers, we capture the proclivity to seek mergers (actual merger behavior) and separate out the deterrence effects of merger policy from the regulatory effects. Third, by considering the annual use of merger policy tools by different antitrust jurisdictions, we have numerous antitrust actions that provide multiple observations of antitrust conduct that help us move beyond the previous before/after and having/not-having comparisons. Last, we provide a much-needed methodological approach – employing dynamic panel data methods to explain merger waves and measuring deterrence as a departure from that wave – to advance the literature on the deterrence effects of merger policy. As already noted, antitrust authorities and scholars have been at an impasse as to how to proceed and measure merger-policy deterrence.

Using changes in the number of merger notifications to elicit deterrence raises two issues. First, some scholars (Eckbo 1989, 1992) have expressed concern that pro-competitive mergers are primarily deterred. It seems, however, more probable that altering antitrust would have a greater impact on anti-competitive mergers. The Deloitte and Touche (2007) study for the U.K. Office of Fair Trading provides corroborating evidence, as that survey indicates that U.K. merger policy rarely deters pro-competitive mergers. Yet with our data, we cannot detect the composition of deterred mergers. Second, deterrence effects may only manifest via the modification of merger proposals and not via the forsaking of merger proposals. For deterrence effects to only manifest in composition form, however, one must assume that a substitute merger exists for every merger type receiving antitrust scrutiny – a rather strong assumption. It seems more realistic that when antitrust authorities curb certain types of merger activity, this means that some firms will opt to not engage in future merger activity. With our data, we can detect deterrence as manifested by this later effect.

III. DETERRENCE EFFECTS

In order to ground the empirical analysis, we analyze in a somewhat formal manner how merger policy tools may involve deterrence. Thinking rigorously about the effects of different merger policy instruments helps us move beyond casual justifications for empirical expectations. We concentrate here on the ability of blockings and settlements to generate forsaken mergers; furthermore, we consider the implications of a shift in the use of one policy tool towards another, and how that may affect future mergers. Policy shifts are particularly relevant in our empirical context, as antitrust authorities have increasingly shown a preference to employ negotiated-settlements to ameliorate the anti-competitive elements of proposed mergers instead of blockings. Figure 1 corroborates the diffusion of settlements as a favored practice by illustrating that the average ratio of negotiated-settlements to blocked-mergers for OECD nations has substantially increased over the 1995-2005 period; thus, settlements have become the most popular merger-policy tool in the cross-national environment for antitrust.

We know that effective deterrence requires those tempted by anti-competitive actions to have a degree of certainty regarding the rules, and to believe that transgressing those rules involves a reasonable probability of being caught and suffering consequences (Becker, 1968; Craswell & Calfee, 1986; Baker, 2001). Hence, effective deterrence requires the probability of detection and penalties to be sufficiently high. To begin conceptualizing how deterrence can be operationalized in a merger context, we first consider the makeup of merger-related profits and then indicate how antitrust actions potentially address anti-competitive problems.

First, most proposed transactions will create for merging firms some efficiency-related profits and some market-power profits – the market-power profits, of course, raising anti-competitive concerns (Röller, Stennek & Verboven, 2000; Pautler, 2001). Accordingly, authorities assess merger notifications for their anti-competitiveness levels and then employ antitrust actions to address such concerns. Further, merging firms' profit streams naturally depend on the antitrust action incurred. Let us focus here on the differences between blocked-mergers, negotiated-settlements, and clearances (no conditions on the merger). If the merger is blocked, then merger profits are negated. If the merger is approved subject to a negotiated-settlement – ideally eliminating the anti-competitive elements – merging firms consummate the transaction and hold on to efficiency-based pro-competitive profits gains, Π^R.[3] Finally, if the merger is cleared without conditions, then merging firms reap all of the transaction profits, Π^C. Normalizing the profits of a blocking to zero, we therefore assume $\Pi^C > \Pi^R > 0$.

Second, we must also consider that different antitrust actions involve different penalties and thus imply different deterrence effects, since penalty size impacts deterrence magnitude. With blocked-mergers, merging firms reap no positive profits as the penalty involves a full rejection.[4] Negotiated-settlements, on the other hand, by attempting to identify and eliminate only the transaction's anti-competitive elements, allow merging firms to reap some profits, Π^R. When an antitrust authority imposes negotiated-settlements as opposed to blockings, the disincentive for firms to propose anti-competitive mergers is clearly lower. Furthermore, clearances involve the least amount of deterrence, as no punishment is involved and merging firms earn the full profits from the transaction, Π^C. In short, blocked-mergers seemingly involve more deterrence than do negotiated-settlements (as blocked-mergers represent larger punishments); and both blocked-mergers and negotiated-settlements involve more deterrence than clearances.

With the above as a backdrop, we now give a more formal structure and introduce additional features to make our analyses realistic. Our model includes the following: firms decide on the restrictiveness of proposed mergers; antitrust authorities either clear, settle or block a merger proposal; an authority can alter the tendencies of its merger policy regime; and uncertainty exists regarding an authority's stance.[5]

First, in addition to deciding on whether or not to propose a merger, firms choose a merger type: the restrictiveness (η) of a merger proposal. In particular, firms may set a

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[3] If negotiated-settlements functioned imperfectly (by not fully eliminating anti-competitive parts or partly eliminating pro-competitive parts of mergers), our analysis would be qualitatively similar. We only assume that a merger with negotiated-settlements yields lower profits than a cleared merger.

[4] Moreover, merging firms experience a penalty in that they might have reaped profits from an alternative merger with fewer anti-competitive concerns that would not have incurred antitrust scrutiny

[5] Sah (1991) models a process similar to our setup in the context of explaining crime patterns. Like us, he is interested in how individual choices are determined endogenously while incorporating available information. The resulting dynamic relationships are then studied to examine how behavior might evolve over time.

merger in ways that are more or less 'restrictive' in terms of future market competition. For example, mergers with low restrictiveness may involve pre-announced asset divestitures that minimize market power concerns, while high restrictiveness mergers may involve a merger-for-monopoly dynamic. Accordingly, we define restrictiveness broadly: decisions over merger targets, product lines, geographic markets, contracts with suppliers, and so on, are all subsumed in the variable. In addition to increasing anti-competitiveness, a merger proposal's restrictiveness naturally impacts a merging firm's profitability. For instance, a firm may acquire a direct competitor instead of a less-related target, thus increasing restrictiveness and thereby both merging firm profits and anti-competitive effects. It is accordingly intuitive to follow Barros (2003) in that firms face a profit incentive to propose mergers with higher restrictiveness levels, $\frac{\partial \Pi^C}{\partial \eta} > 0, \frac{\partial \Pi^R}{\partial \eta} > 0$.[6]

The antitrust authority then assesses the merger by confronting the restrictiveness level, η, proposed by merging firms with its own judgement of the market – a judgement that defines an admissible level $\hat{\eta}$. We model this decision process in a simple way: for $\eta \leq \hat{\eta}$, the merger is cleared without conditions; for $\hat{\eta} < \eta \leq \hat{\eta} + \alpha$, the merger is subjected to negotiated-settlements. Note that parameter α – the settlement solution range that is taken as known to simplify exposition – denotes the extra restrictiveness level the authority is willing to accept as long as settlements are imposed. Finally, for $\eta > \hat{\eta} + \alpha$, the merger is blocked.

The antitrust authority's judgement with respect to admissible levels of restrictiveness ($\hat{\eta}$) is ex-ante unclear when firms decide on whether and which type of merger to propose. Unlike cartels – where it is easy to see where the lines are drawn – many firms would be uncertain as to whether a proposed merger would be too restrictive in the eyes of the antitrust authority. Merger policy regulations exhibit a degree of uncertainty as authorities can give

[6] To avoid corner solutions, we assume that firm gains to increasing restrictiveness occur at a decreasing rate: $\frac{\partial^2 \Pi^C}{\partial \eta^2} < 0, \frac{\partial^2 \Pi^R}{\partial \eta^2} < 0$.

guidelines but not very precise guidelines (Davies & Majumdar, 2002) – in essence, merger policy is simply too complex to generate 'per se' rules. Firms' uncertainty about the value of $\hat{\eta}$ is described by a probability distribution, $F(\hat{\eta} \leq \eta) = F(\eta)$, which denotes the probability that the critical threshold for the antitrust authority is smaller than η.

According to the above setup, firms expect a profit given by:

$$V = 0 \times F(\eta - \alpha) + \Pi^{R}(\eta) \times \left(F(\eta) - F(\eta - \alpha)\right) + \Pi^{C}(\eta) \times \left(1 - F(\eta)\right) - K \,.$$

Here, K measures the fixed cost of building up a merger proposal. A merger is proposed if firms have $V > 0$ after optimally choosing η. A lower K leads to more mergers, yet more interesting is the impact of a change in α, as this reflects a boundary movement between settlements and blockings. Increasing α means that authorities are willing to use settlements in cases where they would previously use blockings: an empirically relevant policy substitution. By the envelope theorem, the impact on merger proposals can be seen from

$$\frac{\partial V(\eta)}{\partial \alpha} = f(\eta - \alpha)\Pi^{R}(\eta) > 0 \,.$$

Hence, the policy substitution from blocked-mergers to negotiated-settlements should induce more future mergers. The same model can be used to address two additional policy shifts of importance: increasing negotiated-settlements at the expense of fewer clearances; and increasing blocked-mergers at the expense of fewer clearances. In both cases, we would expect fewer merger notifications, as both settlements and blockings involve more punishment than do clearances (for more detail see Seldeslachts, Clougherty & Barros, 2007).

Before turning to empirical expectations, we must make a conceptual link between changes in merger policy, changes in antitrust actions, and changes in firm beliefs with regard to antitrust stances. We outline here how one might reason the above process while taking into account uncertainty regarding antitrust rules. First off – due to the complexity of merger policy – firms must make inferences as to actual antitrust stances: namely, what are the critical parameters for eliciting negotiated-settlements and blocked-mergers. Firms make inferences about $\hat{\eta}$ and α based on the antitrust authorities' past actions and on the imperfect knowledge firms have of the η's for the previously proposed mergers of other firms.[7] Changes in antitrust actions represent then manifestations of actual merger policy changes; hence, firms update their beliefs on the antitrust authority's stance when they witness changes in antitrust actions. If updating is done in a Bayesian manner, then increases in a particular antitrust action lead to positive updating of the probability of eliciting such an action. Sah (1991) showed that if perceptions are described by Bayesian inference, then the above properties are satisfied. Accordingly, a spike in the application of a particular antitrust action can lead to firms updating their perception about an authority's real position; in other words, a change in antitrust actions potentially signals the future tenor of merger policy.[8] With the above in mind, we can generate some simple expectations for empirical testing.

First, we expect that a spike in blocked-mergers leads to positive updating of the perceived probability of eliciting a block. Spikes in blocked-mergers may reflect that the boundary between settlements and blockings has been altered (a decrease in the perceived α), but may also reflect a mass transfer in probability from clearances to blocked-mergers. Since blocked-mergers have a significant deterrence effect (namely, they contain a higher penalty than do negotiated-settlements and clearances), individual firms will be relatively less inclined to propose a merger and future merger notifications should go down.

Second, we expect that a spike in negotiated-settlements leads to positive updating of the perceived probability of a settlement. Spikes in settlements may reflect that the boundary between settlements and blockings has been altered (an increase in the perceived α), but may also reflect a mass probability transfer from clearances to settlements. Accordingly, an

[7] For example, if other firms propose a merger that creates a monopoly or facilitates exclusive territories; then, proposed η is high. If the proposed merger would not substantially increase concentration; then, proposed η is low. Authorities decide then on a particular antitrust action for each proposed merger with its attendant restrictiveness level. Accordingly, firms 'see' $\hat{\eta}$ and α through previous merger proposals and antitrust actions.
[8] For example, if a merger with high η is blocked, then there may be little updating on α. But if firms see the blocking of a merger with a relatively low η, then α is revised substantially downwards.

increase in negotiated-settlements can have two possible effects on future merger frequencies: 1) a negative effect when settlements come at the expense of clearances; 2) a positive effect when settlements come at the expense of blockings. Thus when firms positively update their perceptions about an authority using negotiated-settlements, the net-effect depends on whether the spike in settlements lowers the perceived probability of clearances more than the perceived probability of blockings. If the former (later) effect dominates, then an increase in negotiated-settlements leads to the perception of a more-tough (less-tough) merger policy and we should witness a reduced (enhanced) proclivity for firms to seek mergers.

To keep things simple, we have until this point excluded the policy option of monitorings. In part, this was driven by the prior that the effects of monitorings are relatively suspect since this tool may not involve a real punishment; hence, merging parties' perceptions may not change. In fact, nowhere have we seen any claims by antitrust authorities or experts that monitorings involve deterrence. Nevertheless, we will consider the impact of monitorings – as they are an antitrust action – on future merger frequencies in the empirical setup.

IV. THE DATA

The actual data consist of annual measures of merger policy for 28 antitrust jurisdictions over 1992-2005; hence, the unit of observation for the empirical tests is antitrust jurisdiction by year. The OECD directs members and associates to respond to specific questions in order to generate annual reports on the state of antitrust policy in those jurisdictions. Thus, the OECD's (2005) annual reports provide raw data on cross-national antitrust policies that can be compiled into empirical measures. While the OECD reports are the main data source, additional data were gathered via direct contact with – and specific reports from – actual antitrust authorities in order to fill any data holes and reconcile any incompatibilities. The overriding concern in data compilation was to create consistently accurate measures of antitrust policies. In doing so, the data are necessarily characterized as unbalanced panels, as a number of annual observations were missing or necessarily dropped in order to yield consistent cross-jurisdiction/pan-time measures.[9]

The first construct of primary interest for our empirical analysis is merger behavior – namely, the number of mergers occurring. We use for our dependent variable the annual number of transactions that are notified in the antitrust jurisdiction (hereafter referred to as Mergers). This measure has two main advantages. First, the construct itself has very little measurement error since antitrust authorities report accurately the number of notified mergers by year. Second, it is probable that those mergers that must be notified to merger authorities – of which the number of potentially anti-competitive mergers is a subset – change their behavior the most in response to a change in antitrust actions.

We also have measures that help capture the annual level of regulatory scrutiny given merger activity in a particular antitrust jurisdiction: our core explanatory variables. 'Antitrust Actions' refers to an antitrust jurisdictions two-year average of monitorings, negotiated-settlements, and blocked-mergers. We employ a two-year average for antitrust actions since Leary (2002) points out that annual numbers may be sensitive to temporary anomalies. 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).[10] Table 1 reports summary statistics – based on the observations employed in the empirical estimations – for the Mergers variable and the three types of antitrust actions broken down by the twenty-eight antitrust jurisdictions.

Antitrust authorities have steadily increased their actions during the 1990s both in absolute and relative terms. With regard to an absolute increase in antitrust actions: the average number of yearly antitrust actions has evolved from a little under six actions per jurisdiction at the beginning of our sample to a peak of roughly 10.4 actions per jurisdiction in 2000. Figure 2 illustrates the trend in yearly antitrust actions along with the average number

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[9] See Clougherty (2005) for a different application of this OECD-based data.
[10] An anonymous referee made this suggestion to de-sensitize Antitrust Actions to yearly variation. Accordingly, the value for a lagged Antitrust Action in one particular observation year is the following: ((Antitrust Action $_{t-1}$ + Antitrust Action $_{t-2}$) / 2). Further, the empirical results are robust to simply employing the annual measures.

of merger notifications per jurisdiction over the 1994-2004 period. Witness how the tendencies in antitrust actions coincide with the global merger wave: clearly, if more mergers are notified, then more mergers may be subject to some sort of antitrust scrutiny.

With regard to a relative increase in antitrust actions, figure 3 illustrates the average across all jurisdictions in our data set of the number of antitrust actions relative to the number of notified mergers: with a steady rise in that rate from around 1.5% in the early Nineties to around 5% by 2005. The relative increase in antitrust actions may convey that authorities have become tougher and increasingly "punish" proposed mergers; thus, conforming to the received wisdom that antitrust in the cross-national context has experienced greater adoption and strengthening over the last two decades. It may also indicate that authorities have raised notification threshold levels to not waste limited institutional resources on harmless mergers (De Loecker, Konings & Van Cayseele, 2008). This leads naturally to a higher percentage of antitrust actions, as the population of notified mergers will consist of relatively more anti-competitive mergers. We will control for this possibility in the empirical specification.

In sum, antitrust authorities responding to the need to vet many mergers during the unprecedented 1990s merger wave have tended toward the following practices. First, we see an increase in the number of antitrust actions: measured both in absolute (figure 2) and relative (figure 3) terms. The trend manifested in figure 3 toward higher levels of antitrust actions relative to merger proposals is potentially driven by beefed-up antitrust policies and/or by raising notification thresholds. Second, we see an important trend with respect to the employment of merger policy tools: figure 1 illustrates that antitrust authorities principally – and increasingly – rely on negotiated-settlements; and less commonly block mergers.

V. EMPIRICAL ESTIMATION STRATEGY

Our empirical analysis of the deterrence effects of merger policy tools relies on Mergers as the dependent variable and the various antitrust actions that an authority takes as the core explanatory variables. Yet, the number of antitrust actions undertaken is a function of the number of notified mergers. Thus as a first step in attempting to control for the endogeneity of antitrust actions, we use the various lagged antitrust actions as explanatory variables. Any study on merger behavior should also take into account that mergers often occur in waves (Andrade, Mitchell & Stafford, 2001; Harford, 2005). This is very much the case for our data: covering the 1992-2005 period and coinciding with the highest merger wave in history (Gugler, Mueller & Yurtoglu, 2006). To take the wave behavior into account, we include as right-hand side variables lagged terms of Mergers; hence, current merger behavior is partly explained by past merger behavior. We also include year dummies to capture additional period-specific shocks.

Merger waves also typically coincide with economic booms and high stock markets (Gugler, Mueller & Yurtoglu, 2006; Harford, 2005). Accordingly, we add two control variables for economic conditions to help capture merger waves. First, we add economic growth (percentage change in GDP from the previous year) – hereafter referred to as 'Growth'. Second, for stock market conditions, we add capitalization of listed companies as a percentage of GDP—hereafter referred to as Stock-Market. Note that the addition of Growth and Stock-Market represents two additional means (beyond the lagged Mergers and period-specific effects) to control for merger waves.

We may also need to control for changes in notification thresholds since we use notified mergers as the dependent variable. Merger thresholds tend to be composed of three different elements (worldwide sales, domestic sales, and market shares); furthermore, different antitrust authorities mix-and-match their use of these three elements with some authorities employing all three elements to elicit notifications and others employing only one or two elements. Moreover, these elements can sometimes be based on individual firm measures or on combined merger entity measures. Given the share complexity and variation in the different types of threshold regulations manifested in the cross-national environment for antitrust, we decided to use dummies for threshold changes. All jurisdictions in our sample experienced a maximum of three threshold changes in our sample period 1992-2005. Twenty-three jurisdictions experienced at least one change, twelve experienced two changes, and one experienced three changes.[11] Accordingly, we created three dummy variables: 'Threshold1', 'Threshold2' and 'Threshold3' were respectively set to one for the year (and subsequent years) when jurisdictions experience a first, second and third threshold change.

In large part due to the expected entrance of ten new EU members, 1992-2005 was a very active law-making period. Some scholars (Stigler, 1966; Hoekman & Kee, 2003) consider how changes in antitrust laws impact competition; hence, altered antitrust statutes may directly impact merger behavior. We constructed dummies for important changes in antitrust laws based on the OECD reports. It is arguably a subjective exercise to identify 'important' changes, so we specifically looked for the words 'important', 'substantial' and 'major' in the reports. We found twenty-three jurisdictions to have had at least one important legislative change and nine additional jurisdictions to experience a second important legislative change over the period of study. We are, however, unable to detect the direction of the law change: whether the change represents a more vigorous or lenient policy. Nevertheless, we again created two dummy variables: 'Law1' and 'Law2' were respectively set to one for the year (and subsequent years) when jurisdictions experienced a first and second substantive change to antitrust legislation. Note that we will control for panel specific effects; thus, helping to control for institutional differences across antitrust jurisdictions.

The European Union began applying a whole new set of rules in 2004 that created inter alia a new antitrust enforcement system: a system based on close cooperation between the European Commission and the national authorities. The stated aim was for better coordination of enforcement efforts and the promotion of a common competition culture

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[11] Data were collected mainly from Global Competition Review yearbooks and complemented with the OECD reports. The remaining countries that experienced 'no changes' in thresholds (Australia, Italy, New Zealand and Norway) officially do not have merger notification threshold levels. Further, all of the changes we observed in thresholds involved increases in the notification levels, hence these were all raisings of the 'bar'.

between the EU and its member states. Given its potential impact, we included an additional dummy 'EU 2004 Reform': set to one in 2004 and 2005 for all EU member jurisdictions.

Summarizing the above, we estimate how Mergers depend on past Mergers (the previous two years), Antitrust Actions and Controls:

$$Mergers_{i,t} = \alpha + \sum_{k=1}^{2} \beta_k Mergers_{i,t-k} + \gamma(\sum_{k=1}^{2} AntitrustActions_{i,t-k} / 2) + \delta Controls_{i,t} + \omega_i + \lambda_t + \varepsilon_{i,t},$$

where *i* indexes the twenty-eight antitrust jurisdictions, *t* indexes time (year), and *k* allows for convenient expressions. The vector of Antitrust Actions consists of two-year averages of Blocked-Mergers, Negotiated-Settlements and Monitorings – all lagged. Controls represents the vector of control variables: dummies for the first and second important changes in law (Law1 and Law2), the 2004 reform in the EU (EU 2004 Reform), dummies for the first, second and third threshold changes (Threshold1, Threshold2 and Threshold3), and economic and stock market variables (Growth and Stock-Market). Finally, ω_i represents the unobserved jurisdiction-specific effect, λ_t are the year dummies and $\varepsilon_{i,t}$ the disturbances.

By employing the absolute number of Antitrust Actions, the econometric specification departs slightly from the conditional probabilities setup for many empirical deterrence studies following Becker (1968) and Ehrlich (1973). This owes in part to our not having reliable cross-jurisdictional data on investigations (the detection probability) and all possible Antitrust Actions (the conditional probability of eliciting an Antitrust Action). Dezhbakhsh and Rubin (2007) argue that estimating deterrence probabilities requires the presence of all conditional probabilities. Further, we do not employ the probability of eliciting a particular Antitrust Action (for instance, blocked-mergers divided by merger notifications) as Mergers would represent a linked variable in the construction of the left and right-hand side variables: where measurement error could potentially lead to biased coefficient estimates that favor finding deterrence effects (Klein, Forst & Filatov, 1978; Avio, 1988; Donohue & Wolfers, 2005). In addition, the already mentioned changes in notification thresholds could lead to a different type of measurement error that affects the probabilistic Antitrust Action constructs and should be avoided (Berk, 2005). Nevertheless, we do hold constant the number of notified mergers via the lagged dependent variables, and we will perform a robustness check of the probability specification in the 'robustness of results' section.

Donohue and Wolfers (2005) further point out that deterrence variables require the consideration of scaling issues; hence, we log-transform the Mergers and Antitrust Actions variables to yield some additional estimation advantages. First, log-transforming helps moderate – or cancel out – the significant size differences between the different antitrust jurisdictions by generating elasticities for coefficient estimates. Second, log-transforming also addresses to some extent the count nature of the data for Mergers and Antitrust Actions by making the data more continuous.[12] Thus, all the variables are in logs for our estimations—except for the dummy variables and the two economic conditions variables.[13]

It also 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 (Mergers) on the right-hand side. The serial correlation in the 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 lagged Mergers – 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 [Antitrust Actions]". We appear to be the first to use this methodology in a merger-wave context; thus, employing the appropriate dynamic panel method represents a merit of the paper.

[12] To be precise, since many of our antitrust actions variables report zeros, we use the log-transformation after having added a 1 to all the Antitrust Actions variables.

[13] The economic conditions variables are non-count variables and already expressed in percentage terms: percentage change in economic growth, and stock market valuation as a percentage of overall GDP.

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 Roodman's Stata procedure for System GMM.[14] 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 overidentifying restrictions.

Accordingly, it behooves us to instrument for all potentially endogenous and predetermined variables. In particular, we treat the lagged merger variables as endogenous (as the methodology of dynamic panel data prescribes), the stock market variable as endogenous, and the lagged antitrust actions and threshold variables as predetermined. First, event studies show that merger announcements – which are customarily followed by a merger notification – may have a direct impact on stock markets (Andrade, Mitchell & Stafford, 2001); thus, the stock market variable may also be endogenous. Second, recall that our employment of lagged antitrust actions as explanatory variables mitigates the endogeneity problems with any contemporaneous relationship between merger notifications and antitrust actions. Nevertheless, lagged antitrust actions may sometimes be correlated with past merger

[14] See http://econpapers.repec.org/software/bocbocode/s435901.htm for more information on the software.

notification shocks when an antitrust authority does not come to a definite decision in the same year as the merger notification. Third, the OECD reports clearly state that merger thresholds have been changed in many countries in response to a rise in past merger notifications. Accordingly, we instrument – using system GMM – for the above 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 while only instrumenting for the clearly endogenous variables and while treating all other explanatory variables as exogenous. By doing so, we can keep the number of instruments relatively low and mitigate the over-fitting bias. Still, since our panel is relatively small, 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 these results.

Our main empirical results consist of four regression specifications that attempt to take the above issues into account. All four regressions involve fixed period-specific effects and robust standard errors. Regression #1 reports the results of the fixed panel-and-period specific effects procedure. Regression #2 reports the results of a GMM estimation where only the clearly-endogenous lagged dependent variables are instrumented for. Regression #3 reports a GMM estimation where the potentially-endogenous variables (lagged dependent variables and the Stock-Market variable) are instrumented for. Regression #4 reports a GMM estimation where the potentially-endogenous variables (lagged dependent variables and the Stock-Market variable) and the potentially-predetermined variables (Blocked-Mergers, Negotiated-Settlements, and the Threshold variables) are instrumented for.

VI. EMPIRICAL RESULTS

Table 2 reports the estimation results for the four regression specifications. Before discussing the constructs of primary interest, we comment on model adequateness. First, the Hansen test of overidentifying restrictions yields evidence in all three GMM estimations (regressions' #2, #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 cannot be rejected, suggesting that serial autocorrelation does not exist in error levels (the smallest of all three estimations reports Pr>z=0.561). Third, the R-squared term in Regression #1 is relatively high at 0.91, though largely a function of the regression model's dynamic nature. In short, the regression model passes the necessary diagnostics and is well-specified. We comment now on the control variables:

The two lags of Mergers seem to be important variables: the first is positive and highly significant in all four estimations; the second is negative and insignificant throughout, but its inclusion appears appropriate as the negative sign allows for the eventual downturn in a merger wave. Further, we tested for and rejected the presence of a coefficient equal or higher than one concerning the sum of the two lagged Merger terms in all four specifications.[15]

First, second, and third changes in thresholds ('Threshold1', 'Threshold2' and 'Threshold3') do not generally have a statistically robust effect on notified mergers in the regression equations. However, Threshold1 in regression #1 (the fixed-effects estimation) does indicate the expected 'fewer merger notifications' in the years subsequent to the first change in a jurisdiction's threshold level. The overall insignificance might owe to the fact that dummies in a log-log specification measure a change in slope and not a change in intercepts (we would, of course, expect threshold changes to largely impact intercept terms).

[15] We performed simple t-tests. Bond et al. (2005) show that when panels are relatively short, t-tests have good size properties and high power—even when series are potentially close to unit root. These properties are particularly valid in system GMM specifications.

Important changes in antitrust laws ('Law1' and 'Law2') do not generally indicate a statistically significant impact over our period of study. Yet akin to the threshold changes, the first law change in regression #1 involves positive statistical significance. Further, the second law change in regression #2 involves negative statistical significance. Since these are dummy variables in a log-log regression, the same comment applies as above with the threshold changes. Our previous comment regarding the inability to detect law change direction suggests that we not read too much into the measure's lack of significance. The EU 2004 Reform variable is significant and negative in all four regressions; thus, suggesting that the EU reforms generally led to fewer merger notifications in the EU antitrust jurisdictions.

The macroeconomic conditions (Growth) variable is positive in all four estimations per expectation, but insignificant throughout. Stock-Market conditions have a positive and significant impact on Mergers in the fixed-effects regression (#1), thus in line with the behavioral literature on merger waves (where a higher stock market leads to more mergers – see Harford, 2005). However, this effect is not statistically robust in the GMM specifications.

We can now look at the causal relations between the variables of primary interest: the relationship between antitrust actions and merger frequencies: Blocked-mergers has a statistically significant and negative impact on future merger behavior in all four regression equations. The consistent strong impact of this variable suggests that spikes in the use of Blocked-mergers send a clear signal of toughness by antitrust authorities – a signal that significantly reduces future merger proclivities.

Negotiated-settlements, on the other hand, appear at best to positively influence future Mergers; though, the coefficient estimate is only significant in regression equation #2. Accordingly, we can interpret these results as suggesting that the effect of negotiated-settlements coming at the expense of blocked-mergers (a lowering of antitrust toughness) is stronger than the effect of negotiated-settlements coming at the expense of clearances (an increase in antitrust toughness). Or, we can interpret the results as evidence that firms do not view spikes in negotiated-settlements as indicating increased toughness on the part of antitrust authorities. Hence, the important point here is that the application of Negotiated-Settlements does not seemingly involve a significant deterrence effect.

Permitting a merger with the promise to monitor closely the future behavior of merging parties appears to send no signal to firms – the coefficient estimate is highly insignificant throughout our estimations. Accordingly, firms seem to not give any consequence to Monitorings as an indication of the future tendencies of authorities.

In sum, the empirical results support the importance of Blocked-Mergers – but not Negotiated-Settlements and Monitorings – in terms of deterrence. Yet we must move beyond statistical significance and consider the economic significance of blocked-mergers on deterring future mergers. First off, the coefficient estimates for Blocked-Mergers in Table 2 suggest that a 10% spike in blockings leads to a percentage drop in Mergers in the following year ranging from 1.30% (Regression #4) to 1.84% (Regression #1).[16] Consider a comparison between the U.S. and the EU for illustrative purposes: where the U.S. stepped up blockings in 2003 (the two-year average going from 11 in 2002 to 15 in 2003) and the EU stopped blockings (the two-year average going from 2.5 in 2002 to 0 in 2003). The conservative coefficient estimate from Regression #4 suggests that the U.S. faced 4.7 percent fewer mergers in 2004 due to the 2003 spike in blockings, while the EU faced 13 percent more mergers in 2004 due to the 2003 drop in blockings. In short, the empirical results generally support significant-and-robust deterrence effects for the application of blocked-mergers.

VII. ROBUSTNESS OF RESULTS

While we believe that our econometric specification is sound and generates unbiased and consistent estimates for the impact of merger policy tools on future merger frequencies, it is relevant to demonstrate the non-fragility of these results. Such a task is particularly

[16] The results must be interpreted as an upper bound since we transform zeros to ones with the log specification.

important as deterrence has proved to be quite fragile in other areas of research; for example, Donohue and Wolfers (2005) question the robustness of the literature supporting a deterrence effect for capital punishment. In order to illustrate the non-fragility of our results, we evaluate our mainline findings to changes in the mode of measuring the deterrence variables, in the scaling of core variables, in functional form, and in the clustering of error terms.

First, and as previously noted, many crime-and-punishment studies employ conditional probabilities in order to empirically capture deterrence effects, yet our mainline regressions employ log-transformed counts of Antitrust Actions. We take this approach because we lack data on the full set of conditional probabilities regarding merger reviews, and because we desire to minimize measurement error – measurement error which may manifest via a linked variable (Mergers) and/or via changes in merger notification thresholds. Nevertheless, we can divide through our three Antitrust Action variables by the relevant merger notifications to create a probability of eliciting a particular policy instrument. The first robustness specification in table 3 (the 'probabilities' specification) presents the coefficient estimates and standard errors for Blocked-Mergers and Negotiated-Settlements (the Antitrust Actions of principal concern) measured as probabilities, while conforming to the other properties of our four mainline regressions. These results conform to our principal results as Blocked-Mergers indicates a significant deterrence effect in all four regression equations, while Negotiated-Settlements is positive but only significant in Regression #2.

Second, scaling issues are also important in deterrence studies, as the researcher wants to ensure that neither relatively large nor small jurisdictions drive the empirical results. Our log transformation of the critical dependent and independent variables in the mainline regressions helps mitigate the significant size differences between different antitrust jurisdictions; further, unreported estimations – available on request – where the U.S. and other large antitrust jurisdictions are deleted also support our findings. Nevertheless, we can report a different means of rescaling by dividing the Mergers and Antitrust Actions variables through by the jurisdictions GDP. The second robustness specification in table 3 (the 'weighting by GDP' specification) again presents the coefficient estimates and standard errors for Blocked-Mergers and Negotiated-Settlements from an estimation that otherwise conforms to the properties of our four mainline regressions. Again, the Blocked-Mergers coefficient estimates are consistently negative and significant, while the Negotiated-Settlements coefficient estimates are positive but only significant in two GMM estimations.

Third, previous empirical studies of deterrence have often been criticized for not being robust to simple changes in functional form. For instance, Passell and Taylor (1977) criticized Ehrlich's (1975) work for this reason. A linear functional form for the core variables in our mainline regressions raises certain issues (for instance, large antitrust jurisdictions may drive the results), but estimating such an equation does allow an additional robustness test. Furthermore, coefficient estimates from a linear-linear functional form yield more intuitive economic interpretations than do elasticities from a log-log functional form estimation. The third robustness specification in table 3 (the 'linear-linear functional form' specification) again presents the coefficient estimates and standard errors for Blocked-Mergers and Negotiated-Settlements from an estimation otherwise conforming to our mainline regressions. The Blocked-Mergers variable is again supportive of deterrence in all four estimations, while Negotiated-Settlements is positive but only significant in one GMM estimation. The most conservative coefficient estimate for Blocked-Mergers suggests that each additional blocking leads to forty-three fewer merger notifications in the subsequent year.

Fourth, a standard practice in the deterrence literature is to follow Bertrand, Duflo and Mullainathan (2004) by employing robust standard errors clustered on the panel. This practice owes to the concern that serial correlation in panel data may lead to understating the standard deviation for estimators. While we directly consider and test for the presence of serial correlation in our GMM specifications (see the Arellano-Bond tests in Table 2), we can corroborate our approach by estimating a fixed panel-and-period specific effects specification (regression 1 from Table 2) while allowing for robust and panel clustered standard errors. Furthermore, it may behoove us to report results where we allow error term correlation across antitrust jurisdictions: namely, where all EU jurisdictions are allowed to have clustered error terms, and then where multiple potential clusters (‘old’ EU, ‘new’ EU, NAFTA, and Asia/Oceania jurisdictions) are allowed. The fourth robustness specification in table 3 (the ‘clustering’ specification) again presents the coefficient estimates and standard errors for Blocked-Mergers and Negotiated-Settlements from the mainline fixed panel-and-period specific effects specification allowing for the three types of clustering referred to above. Note that this robustness specification clearly departs from the first three robustness specifications as it does not employ GMM estimations. The coefficient estimates for Blocked-Mergers again suggest a consistent and significant deterrence effect, while Negotiated-Settlements elicit a positive but insignificant coefficient estimate.

VIII. SUMMARY AND CONCLUSION

Unlike cartel policy, merger policy deterrence has generally gone under-studied by law and economics researchers. In particular, we have been unable to identify any studies that consider whether the particular tools available for merger policy involve adequate deterrence. Accordingly, we focus on the impact of different merger-policy tools – blocked-mergers, negotiated-settlements, and monitorings – on the proclivity of firms to engage in future mergers. We bring empirical evidence to bear on this issue by employing a cross-jurisdictional data set for merger policies over 1992-2005. The broad scope of our data allows consideration of whether changes in merger policy enforcement impact merger behavior.

Our empirical results suggest that antitrust actions can impact future merger frequencies; however, not all merger policy tools are effective deterrents. Blocked-mergers involve deterrence implications with respect to future merger frequencies, but both negotiated-settlements – the most popular merger policy tool – and monitorings seem to involve no deterrence. The insignificant finding for monitorings does not greatly surprise, as they have generally been considered a weak merger policy tool in terms of regulatory effects; hence, few deem this tool able to involve serious deterrence effects. The findings for blocked-mergers and negotiated-settlements, however, generate more serious implications.

First, the findings for blocked-mergers (where spikes in blockings lead to firms forsaking future merger proposals) suggest that antitrust actions can involve deterrence. Antitrust scholars and practitioners have generally needed to assume the existence of deterrence effects for merger policy, as it has been difficult to validate and quantify merger policy deterrence. Our empirical results indicate that blocked-mergers can reduce the proclivity of firms to seek future mergers. Accordingly, antitrust authorities should appreciate that blocked-mergers carry a unique ability to deter merger activity.

Second, the findings for negotiated-settlements suggest that this particular merger policy tool may not involve adequate deterrence. One should temper the implications of this finding, as we only factor merger frequencies; yet, the forsaking of merger proposals should go hand-in-hand with the modification of merger proposals. Plus, forsaking mergers is important in a world where antitrust authorities face budgetary and resource constraints. Therefore, our results imply that antitrust authorities should be cautious with regard to over-using negotiated-settlements: as they may indeed ameliorate the anti-competitive effects of proposed mergers, but such actions – unlike blocked-mergers – carry no robust effect on the future proclivity to merge. In short, the increased adoption of negotiated-settlements as a tool to ameliorate the anti-competitive concerns with proposed mergers may come at the opportunity cost of neglecting the proper deterrence role of merger policy.

We are not the first to suggest that negotiated-settlements represent a weak merger policy tool; though, we are unique in citing the deterrence implications as a particular concern. A number of authors from different standpoints have criticized the effectiveness of negotiated-settlements. Joskow (2002) argues that structural settlements are neither easy for authorities to apply nor for firms to adopt in light of the transaction costs involved; for example, many of the difficulties identified by the FTC (1999) study on divestitures – size not mattering, strategic behavior of divesting firms, information deficiencies of buying firms, and advisability of divesting on-going businesses versus assets – conform to a transaction cost economics perspective. Cabral (2003) claims that the interaction of divestitures with entry conditions may lead to unexpected outcomes: in particular, divestitures may make further entry unprofitable, thus leading to welfare-inferior market equilibria. Motta, Polo and Vasconcelos (2006) review a number of drawbacks that suggest negotiated-settlements in the European context have not been effective; in particular, negotiated-settlements may act to enhance collusion. Furthermore, some empirical work finds negotiated-settlements to generally be ineffective: Crandall and Winston (2003) find negotiated-settlements in manufacturing industries to increase – not decrease – future price-cost margins; Duso, Gugler and Yurtoglu (2006, 2007) generally find second-phase European Commission settlements to not ameliorate anti-competitive effects. Hence, we proffer one additional criticism – weak deterrence – to the list of potential concerns regarding negotiated-settlements.

In sum, we find blocked-mergers to be effective and negotiated-settlements to be ineffective in the deterrence of future merger frequencies. The weak-deterrence implications of settlements may be a concern in light of the trend toward their increased use as a merger policy tool. Justice William J. Brennan Jr. captured the underlining faith that has led to increased adoption of negotiated-settlements when he states "Divestiture … the most important of antitrust remedies. It is simple, relatively easy to administer, and sure".[17] To the degree that antitrust authorities are concerned about the deterrence implications of merger policy, our results suggest that they may want to re-evaluate their penchant to increasingly employ negotiated-settlements – instead of blocked-mergers – to deal with anti-competitive merger proposals.

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[17] See Parker and Balto (2000: p.5) for this quote.

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Figure 1: The Average Across All OECD Antitrust Jurisdictions for the Ratio of 'Negotiated-Settlements to Blocked-Mergers'

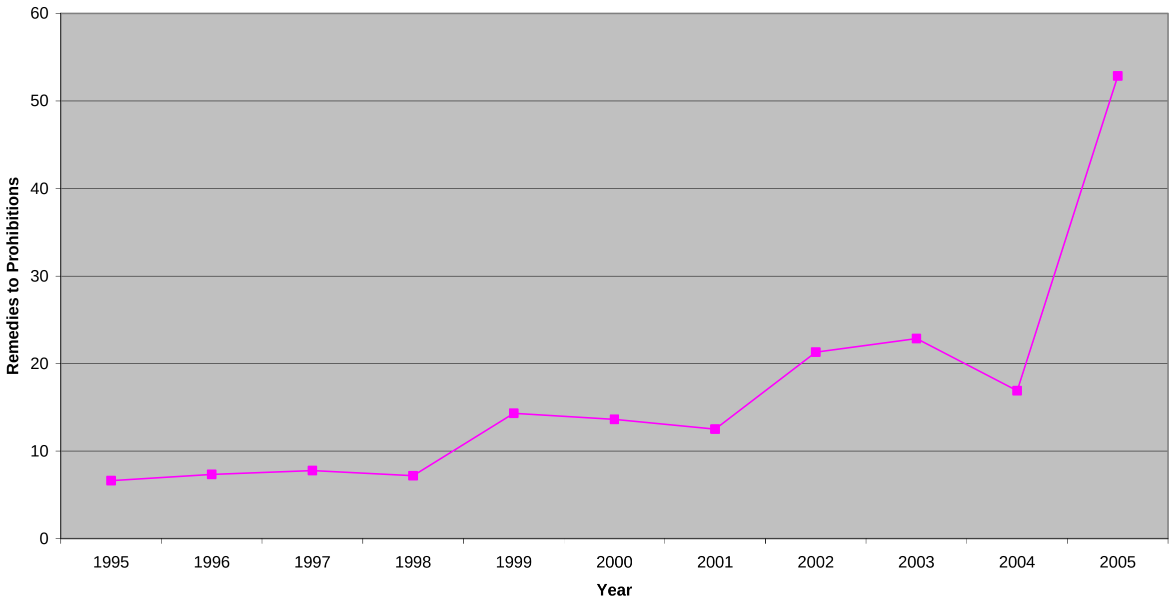

Figure 2: The Average Across All OECD Antitrust Jurisdictions for the Number of 'Antitrust Actions' and 'Mergers'

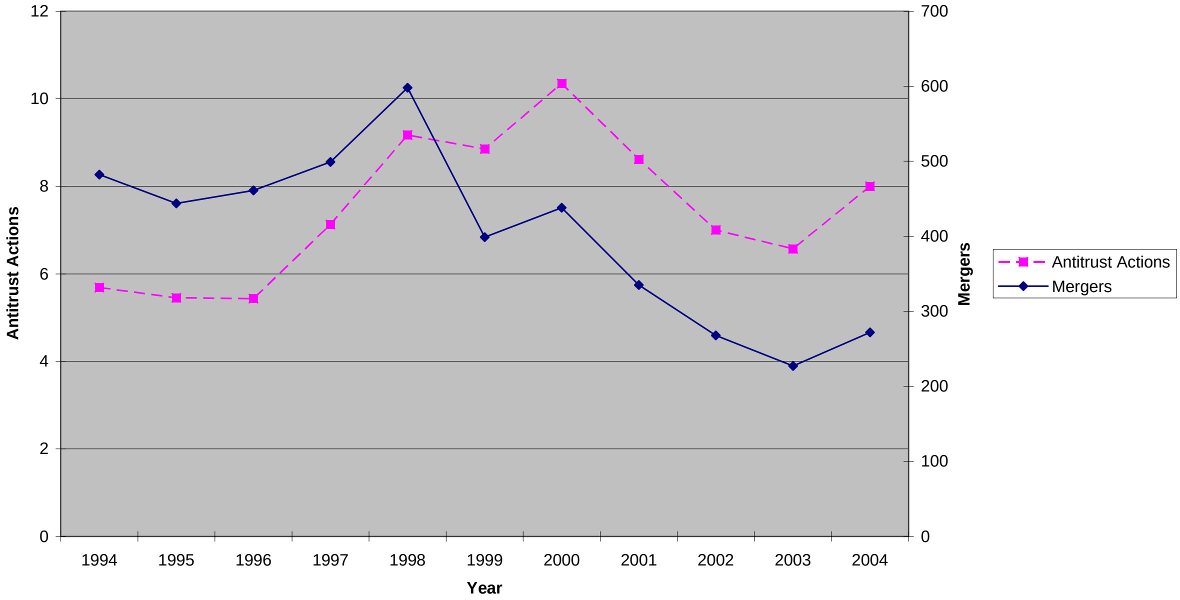

Figure 3: The Average Across All OECD Antitrust Jurisdictions for the Percentage of 'Notified Mergers Eliciting Antitrust Actions'

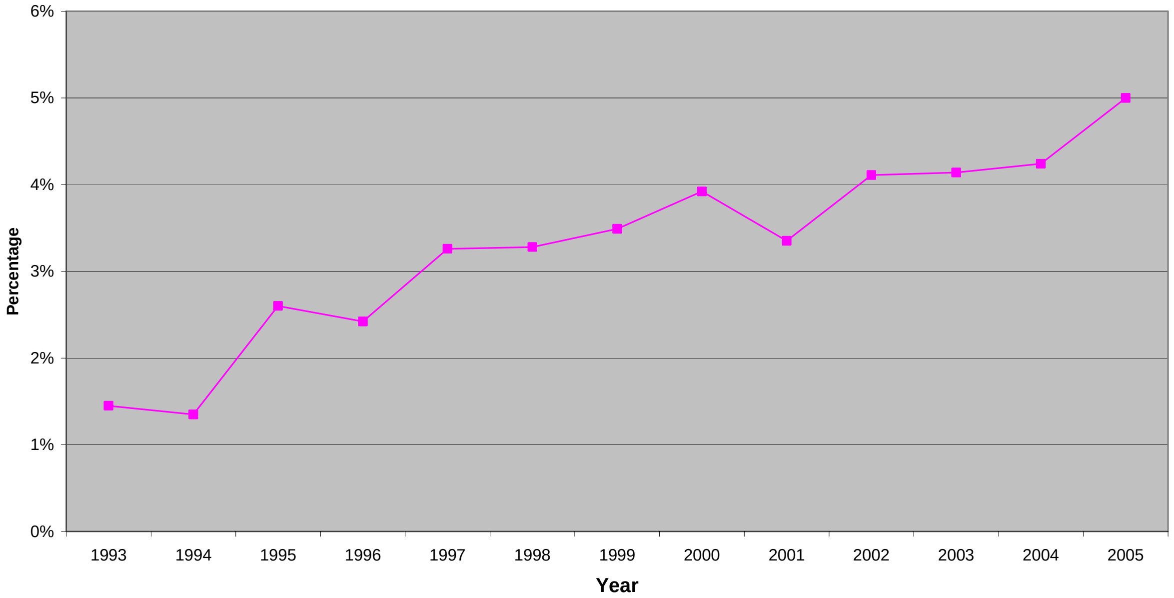

TABLE 1

DESCRIPTIVE STATISTICS OF THE DATA BY ANTITRUST JURISDICTION

		Variable Means			
Antitrust Jurisdiction	*Observation Numbers*	*Mergers*	*Blocked-Mergers*	*Negotiated-Settlements*	*Monitorings*
Australia	8	207.1	3.7	5.1	0.3
Austria	5	241.2	0.1	2.3	0
Belgium	5	47.6	0.8	0.1	0.1
Brazil	5	555.2	0.3	8.1	0
Canada	11	288.3	0.4	3.7	0.8
Czech Republic	7	128.6	1.2	5.2	0
Denmark	4	11.8	0	1	0
European Union	12	230.7	1.5	14.9	0
Finland	5	81.6	0.2	3.2	0
France	3	485	0	2.7	0
Germany	10	1401	3.75	6	0
Greece	1	105	1	0	0
Hungary	9	62.2	0.4	1.1	0
Ireland	7	229.3	0.2	0.1	0
Italy	10	499.3	0.6	2.4	0
Mexico	7	244.3	2.1	7	0
Netherlands	5	112.2	0.3	1.6	0
New Zealand	8	67.8	3.7	2.2	0
Norway	7	35.9	0.2	1.8	0.2
Poland	2	292.5	0.3	1.5	0
Portugal	6	42.5	0	0.3	0.3
Slovenia	12	60.3	0.4	0.9	0
Spain	8	67.9	0.4	3.3	0.1
Sweden	10	135.6	0.6	1.7	0
Switzerland	6	37.8	0	1	0
Turkey	5	117.4	0.4	1.6	0
United Kingdom	9	369.1	2.4	6	0.2
United States	11	2766.4	14.5	43	0
All Jurisdictions	*198*	*388.8*	*1.8*	*5.9*	*0.1*

TABLE 2

REGRESSIONS OF MERGER CONDUCT ON ANTITRUST ACTIONS

Variable	Fixed Panel & Period Effects (1)	GMM Instrumenting for Mergers (2)	GMM Instrumenting for Mergers & Stock-Market (3)	GMM Instrumenting for Full set (4)
Mergers $_{t-1}$	0.781***	1.001***	1.065***	1.019***
	(0.0864)	(0.103)	(0.0947)	(0.0767)
Mergers $_{t-2}$	-0.0294	-0.0276	-0.0878	-0.0637
	(0.0850)	(0.0762)	(0.0833)	(0.0784)
$_{k=1}\Sigma^{2}$Blocked-Mergers $_{t-k}$ /2	-0.184**	-0.169*	-0.147*	-0.130**
	(0.0749)	(0.102)	(0.0803)	(0.0627)
$_{k=1}\Sigma^{2}$Negotiated-Settlements $_{t-k}$ /2	0.0115	0.119**	0.0539	0.0565
	(0.0475)	(0.0604)	(0.0526)	(0.0381)
$_{k=1}\Sigma^{2}$Monitorings $_{t-k}$ /2	0.0421	-0.0479	0.0116	-0.0939
	(0.157)	(0.227)	(0.177)	(0.131)
Threshold1 $_{t}$	-0.264***	0.0450	-0.0245	-0.0575
	(0.0908)	(0.130)	(0.121)	(0.0752)
Threshold2 $_{t}$	-0.102	0.198	-0.0554	-0.0252
	(0.0912)	(0.255)	(0.163)	(0.0890)
Threshold3 $_{t}$	0.279**	-0.877	0.447	0.214
	(0.129)	(1.691)	(0.770)	(0.151)
Law1 $_{t}$	0.269**	-0.111	-0.111	-0.0975
	(0.111)	(0.136)	(0.152)	(0.114)
Law2 $_{t}$	-0.00172	-0.360*	-0.0333	-0.00307
	(0.103)	(0.204)	(0.128)	(0.0808)
EU 2004 Reform $_{t}$	-0.192**	-0.713*	-0.442**	-0.243*
	(0.0944)	(0.406)	(0.201)	(0.147)
Growth $_{t}$	0.000378	0.00319	0.00515	0.0192
	(0.0131)	(0.0151)	(0.0122)	(0.0139)
Stock-Market $_{t}$	0.00247*	-0.00066	0.000091	-0.00052
	(0.00149)	(0.000862)	(0.000588)	(0.000610)
Constant	0.932**	0.0798	0.170	0.255
	(0.365)	(0.306)	(0.204)	(0.210)
Arellano-Bond test that aver. auto covariance in residuals of order 2 is 0		z = 0.19 Pr > z = 0.852	z = 0.58 Pr > z = 0.561	z = 0.597 Pr > z = 0.568
Hansen Test of over-identifying restrictions		chi2(45)=1.36 Pr > chi2=1.0	chi2(61)=4.60 Pr > chi2=1.0	chi2(93)=0.43 Pr > chi2=1.0
R^2	0.91			

NOTE.—The dependent variable is the number of notified mergers. All four estimations involve fixed period-specific effects (year dummies) and 198 observations. Robust standard errors are in brackets. Furthermore, *** = 1%, ** = 5%, and * = 10% Significance.

TABLE 3

ESTIMATES FOR BLOCKED-MERGERS AND NEGOTIATED-SETTLEMENTS UNDER DIFFERENT ROBUSTNESS SPECIFICATIONS

	Fixed Panel & Period Effects (1)	GMM Instrumenting for Mergers (2)	GMM Instrumenting for Mergers & Stock-Market (3)	GMM Instrumenting for Full set (4)
Probability Specification				
${}_{k=1}\Sigma^{2}$ (Blocked-Mergers/Mergers) $_{t-k}$ /2	-0.677*	-1.085**	-1.126**	-0.726**
	(0.381)	(0.541)	(0.480)	(0.339)
${}_{k=1}\Sigma^{2}$(Negotiated-Settlements/Mergers) $_{t-k}$ /2	0.204	0.509*	0.0699	0.262
	(0.290)	(0.290)	(0.299)	(0.205)
Weighting By GDP Specification				
${}_{k=1}\Sigma^{2}$ (Blocked-Mergers/GDP) $_{t-k}$ /2	-7.870*	-8.864**	-8.927*	-6.247**
	(4.754)	(3.992)	(5.345)	(3.151)
${}_{k=1}\Sigma^{2}$(Negotiated-Settlements/GDP) $_{t-k}$ /2	0.708	3.046	2.248**	1.538*
	(3.781)	(2.473)	(0.899)	(0.861)
Linear-Linear Functional Form Specification				
${}_{k=1}\Sigma^{2}$ Blocked-Mergers $_{t-k}$ /2	-44.94*	-43.42*	-59.15***	-42.70***
	(26.89)	(22.16)	(10.55)	(13.86)
${}_{k=1}\Sigma^{2}$Negotiated-Settlements $_{t-k}$ /2	6.216	4.310	12.86*	7.983
	(6.233)	(5.167)	(7.140)	(5.368)

	Robust & Panel Clustered Errors (1)	Robust & EU Clustered Errors (2)	Robust & Multiple Clustered Errors (3)
${}_{k=1}\Sigma^{2}$ Blocked-Mergers $_{t-k}$ /2	-0.184**	-0.184***	-0.184*
	(0.0896)	(0.0538)	(0.0841)
${}_{k=1}\Sigma^{2}$Negotiated-Settlements $_{t-k}$ /2	0.0115	0.0115	0.0115
	(0.0519)	(0.0273)	(0.0325)

NOTE.— All estimations involve fixed period-specific effects (year dummies) and 198 observations. Furthermore, *** = 1%, ** = 5%, and * = 10% Significance. Full estimation results are available upon request.