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Remedy for Now but Prohibit for Tomorrow: The Deterrence Effects of Merger Policy Tools

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Discussion Paper No. 218

Remedy for Now but Prohibit for Tomorrow: The Deterrence Effects of Merger Policy Tools

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September 7, 2007

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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 – Prohibitions, Remedies, 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 merger prohibitions to lead to decreased merger notifications in subsequent periods, and remedies to weakly increase future merger notifications: in other words, prohibitions involve a deterrence effect but remedies do not.

JEL: L40, L49, K21

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I. INTRODUCTION

Antitrust policy involves more than just policing actual antitrust cases and violations; it also involves deterrence effects. 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, Owen (2003) points out how the lack of resources and the inability to create credible commitments has led to the absence of any deterrence effect for Latin American antitrust policies; while Stigler (1966) attributes the decline in the relative frequency of horizontal mergers in the 1950's 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).

While an important issue, research on the deterrence effects of antitrust policy is generally an underdeveloped area. The 'dearth of literature' charge holds less well for collusion and anti-cartel policies where a number of studies exist (Block, Nold & Sidak, 1981; Kobayashi, 2002; Spagnolo, 2007). Yet merger policy is an area where deterrence has been particularly under-researched. For instance, the influential U.S. Federal Trade Commission (1999) divestiture study does not factor deterrence implications. Instead the authors simply acknowledge that the total effect of the Commission's merger enforcement is presumably much greater than reflected by the actual number of remedies and prohibitions; hence, they observe that deterred merger undertakings must certainly exist. This lack of accounting for merger-policy deterrence effects is also exhibited by the U.S. Department of Justice (DOJ)

[1] By antitrust actions, we collectively refer to the use of various merger policy tools by authorities: monitored, remedied and prohibited mergers – all constitute antitrust actions (see sections' III & IV for more explanation).

which stated in its Fiscal Year 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 the lack of accounting for deterrence-effects suggests that the total-effect of merger policy is far more consequential than we currently acknowledge.

Antitrust authorities in recent years have shown a proclivity to employ remedies to ameliorate the anti-competitive elements of proposed mergers instead of engaging in out-and-out prohibitions. For instance, the European Commission (EC) has largely relied on structural and behavioral remedies by only blocking two mergers since 2001. In the US, remedies constituted only twenty-three percent of US merger policy actions in the late 1980s; but by the year 2000, remedies were employed in over sixty percent of US merger cases requiring antitrust action (Parker & Balto, 2000). The increased adoption of remedies spurred the U.S. Federal Trade Commission (FTC) into studying the success of divestitures as a remedy for anti-competitive concerns: that already-mentioned study (U.S. FTC, 1999) found divestitures to generally create viable competitors. Accordingly, the FTC issued guidelines for remedies in 1999, the EC followed suit by issuing guidelines in 2001, and the U.S. Department of Justice (DOJ) in 2004 (Duso, Gugler & Yurtoglu, 2007). The codification of remedies as an important merger policy tool in the three highly visible authorities would seemingly influence less-experienced authorities which look to established authorities for guidance and benchmarking in the development of antitrust practices. An example of an overt influence by established authorities on less-experienced authorities rests with the European Union's (EU) accession criteria mandating that candidate-nation antitrust policies conform to EU policies (Dutz & Vagliasindi, 2000). Figure 1 corroborates the above conjecture on the diffusion of remedies as a favored practice by illustrating that the average ratio of remedies to prohibitions

for OECD nations has substantially increased over the 1995-2005 period; thus, remedies have become the most popular merger-policy tool in the cross-national environment for antitrust.

Our main concern is that the above three points – i., the fact that merger policy involves important deterrence implications; ii., the deterrence effects of merger policy have, however, largely gone unmeasured; iii., the trend towards increased use of remedies as opposed to prohibitions as a merger policy tool – raise a potentially important issue that may have been neglected by antitrust authorities. In particular, the increased adoption of remedies 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. Remedies may deal with the immediate anti-competitive concerns due to a proposed transaction, but may not send the necessary clear-signal of 'probable detection and serious consequences' for effective deterrence. In short, for deterrence purposes merger remedies may be an ineffective policy instrument; hence, the trend toward increased deployment of remedies as a merger policy tool may come at the expense of decreased deterrence.

Accordingly, we propose to study the impact of different merger policy tools (prohibitions, remedies and monitorings) on the proclivity of firms to engage in future merger transactions. After all, antitrust policy is directly aimed at firms' conduct; hence, considering the effect of changes in merger policy on firms' 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 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 and remedies are helpful and others are not" (2003: 4). We begin that exercise of detecting the effectiveness of different merger policy instruments by considering whether prohibitions, remedies, and monitorings act as effective deterrents with regard to future merger frequencies.

The paper is structured as follows to support our analysis. Section II reviews the relevant existing literature in order to properly frame our contribution. Section III formally generates empirical expectations by setting out a model where prohibitions and remedies represent costs to proposed merger transactions; thus, spikes in antitrust actions potentially result in frequency-based deterrence. 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. Section VII discusses and concludes.

II. BACKGROUND ON RELATED LITERATURE

A number of studies mention the importance of deterrence with respect to merger policy (Joskow, 2002; Crandall & Winston, 2003; Baker, 2003); however, very few – with the exception of Davies and Majumdar's (2002) commissioned report for the UK Office of Fair Trading – 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: in other words, the proper penalties to discourage collusion. 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 above three literature strands (cartel deterrence, composition-based deterrence, and price-cost markup studies) in order to further motivate our analysis and nest our contribution in the relevant literature.

First, the most extensive literature on antitrust deterrence resides in the realm of anti-cartel and collusion policies. Building on Becker's (1968) and Ehrlich's (1973) early work on the economics of illicit behavior, a great deal of scholarship has considered how collusive-

pricing and market-sharing behavior might be understood, detected and discouraged (see Baker, 2001; Van Eden, 2002; Spagnolo, 2007 for reviews). The following works in this tradition appear most germane to our topic, as they empirically support the existence of a deterrence effect when it comes to anti-cartel and collusion policy. Feinberg (1980) finds firms incurring Sherman act enforcements to subsequently have lower markups. Block, Nold and Sidak (1981) observe cartel prosecutions in one market to decrease markups in both that market and in similar regional markets. Block and Feinstein (1986) find antitrust enforcement in a market to robustly spillover into other sub-markets. Furthermore, Clarke and Evenett (2003) show that international cartel members tend to reduce their overcharges in national jurisdictions with strong cartel enforcement regimes. Accordingly, the empirical literature appears to have substantiated the existence of deterrence effects for anti-cartel policy.

Second, a small literature exists on the deterrence effects of merger policy with respect to the composition of proposed mergers. As Aaronson (1992) notes, merger policy deterrence effects conceivably manifest in merger plans being forsaken (namely, a frequency-based deterrence effect), but also in mergers being shaped differently (namely, a composition-based deterrence effect). George Stigler (1966) first considered composition-based deterrence effects when he considered the impact of the 1950 anti-merger amendment to the Clayton Act. He found that statute – and the enforcement of that statute – to substantially discourage horizontal mergers, and encourage vertical and conglomerate mergers; hence, the composition of mergers tilted toward more vertical and conglomerate types of pairings. Following in this vein, B. Espen Eckbo employed stock-price data to measure the composition of mergers and gain insights in to both merger welfare implications and potential deterrence effects. 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 the HSR act deterred a number of anti-

competitive mergers.[2] Note, however, that Eckbo and Wier go on to question the optimality of the HSR act as they find antitrust actions to generally be anti-competitive: in that, rival firms surprisingly experience positive abnormal returns when authorities take action against merger proposals. Eckbo (1989) attempts to clarify the above contribution when he agrees that mergers are certainly deterred by more aggressive antitrust policy; though, he is concerned that efficient mergers are generally being deterred. In later work, Eckbo (1992) makes use of the absence of Canadian antitrust enforcement prior to 1985 to indirectly test whether US policy deters anti-competitive mergers. He conjectures that there should be more anti-competitive horizontal mergers in Canada due to effective US deterrence; yet over the 1961-1982 period, he finds no evidence of the composition of proposed Canadian mergers being more anti-competitive than the composition of proposed US mergers. Although Eckbo (1992) considers how merger policy influences the type of proposed mergers (at least with respect to stock market evaluation), he does not directly identify whether a change in policy impacts (namely, deters) future merger behavior.

Third, a number of studies look at how antitrust policy indirectly influences firm behavior by studying its impact on industries 'market power and price-cost margins'. This literature, however, tends to be somewhat 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 increase price-cost margins in US manufacturing industries; Konings, Van Cayseele and Warzinsky (2001) detecting no significant 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 antitrust budgets to decrease markups for bread in the US; Warzinsky (2001) detecting tougher US merger policy

[2] See Werden and Williams (1989) for a critique of Eckbo & Wier (1985) and the event-study methods' appropriateness for evaluating merger policy (Fridolfsson & Stennek, 2005 also perform the latter).

from 1945-1973 leading to lower firm markups as compared to the more lax period of 1973-1991; Hoekman and Kee's (2003) forty-two country study where the presence of antitrust legislation generates lower markups via improved entry conditions. The above studies conceivably capture both the direct regulatory effects and the indirect deterrence effects involved with the establishment, resourcing and use of antitrust policies, but, do not disentangle the regulatory from the deterrence effects.

The literature summarized above covers a great deal of ground and generates a number of important insights, but suffers from some limitations. First, most of the above 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 and Warzinsky 2001; Warzinsky, 2001; Hoekman and Kee, 2003), or the countries-having versus countries-not-having antitrust laws comparison (Eckbo, 1992; Konings, Van Cayseele and Warzinsky, 2001). Second, the price-cost markup literature finds it difficult – as already noted – to disentangle regulatory from 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 direct responses by firms to changing antitrust policies. Third, there appears to be a dearth of literature analyzing frequency-based manifestations of deterrence. Fourth, it is fair to say that the empirical literature on the deterrence effects of cartel policy is far more advanced (and consistently supportive) than the literature concerning the deterrence effects of merger policy.

We propose then to directly analyze the deterrence effects of different merger policy tools on future merger behavior, and by doing so, we hope to improve upon the existing literature in some respects. First, 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 and help us move beyond the previous before/after and having/not-having comparisons. Second, by considering future changes in the number of

notified mergers, we can both capture the proclivity to seek mergers (actual merger behavior) and separate out the deterrence effects of merger policy from the regulatory effects. Third, by fixing the number of notified mergers as the dependent variable of interest, we can provide some evidence with respect to the frequency-based deterrence effects of merger policy (namely, how many mergers are forsaken) as opposed to the literature's (admittedly a small literature) focus on the composition of proposed mergers. Fourth, 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 effects.

We should be clear here and note that investigating a change in the number of notified mergers does not necessarily equate to a change in the number of anti-competitive mergers—the main interest of antitrust authorities. For instance, Carlton (2007: 400) notes that “a decision to forbid a particular merger may dissuade other firms from merging despite the fact that the merger involving those firms may enhance efficiency”. Eckbo (1989, 1992) has made this point in expressing concern, backed by some evidence, as to socially-efficient mergers being the types that are primarily deterred. It seems, however, more probable that a change in antitrust actions would have a greater impact on potential anti-competitive mergers than on potential competitive mergers. Yet in our data, we are unable to detect whether deterred mergers are efficient or anti-competitive; instead, we can only elicit frequency-based and not composition-based deterrence. Deterrence effects for merger policy instruments should, nevertheless, manifest both as composition-based and frequency-based deterrence. For deterrence effects to only manifest in composition form, one must assume that there exists a substitute merger for every merger type that receives antitrust scrutiny. Put differently, when antitrust authorities curb certain types of merger activity this means that some firms will opt to not engage in future merger activity instead of proposing other types of mergers.

Furthermore, frequency-based deterrence is important once one realizes that antitrust authorities generally lack the resources and manpower to sufficiently vet all of the problematic merger proposals.

III. DETERRENCE EFFECTS

In order to ground the empirical analysis, it behooves us to analyze in a somewhat formal manner how merger policy tools may involve deterrence. An attempt to think rigorously about the effects of different merger policy instruments helps us move beyond casual justifications for empirical expectations. We concentrate here on the frequency-based deterrence effects of prohibitions and remedies; furthermore, we consider the implications of a shift in the use of one policy tool towards another, and how that may affect future merger notifications. The premise behind our theoretical foundations is that spikes in remedies and prohibitions signal to firms the future intentions and tendencies of antitrust authorities.

We know that effective deterrence for antitrust policy requires those potentially 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 serious consequences (Craswell & Calfee, 1986; Baker, 2001). Becker (1968) notes that if the aim is simple deterrence then raising the probability of conviction close to one and having punishments exceed the gains would virtually eliminate offences. That insight simply suggests that effective deterrence requires the probability of detection and fines 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 some efficiency-related profits for merging firms, yet some profits will also accrue from market-power gains.[3] It is, of course, the market-power profits that enhance anti-competitive concerns. Accordingly, antitrust authorities assess merger notifications for their anti-competitiveness levels and then employ antitrust actions in order to address such concerns. Moreover, the profit streams of merging firms depend on the antitrust action incurred. Let us focus here on the differences between prohibitions (merger out-and-out rejected), remedies (merger cleared but with behavioral or structural modifications), and clearances (merger cleared without conditions). If the merger is prohibited, then the transaction-specific profits are negated: merging firms reap no additional profits. If the merger is approved subject to remedies – which should ideally eliminate the anti-competitive elements – merging firms are allowed to consummate the transaction and hold on to pro-competitive profits based on efficiency gains, Π^R.[4] Finally, if the merger is cleared without conditions, then the merging firms reap all of the profits from the transaction, Π^C. Normalizing the profits of a prohibition to zero, we therefore assume that $\Pi^C > \Pi^R > 0$.

Second, we must also consider that different antitrust actions imply different deterrence effects. With prohibitions, merging firms reap no positive profits as the penalty involves a full rejection. Moreover, merging firms experience a penalty in that they might have reaped the profits from an alternative merger with fewer anti-competitive concerns that would not have incurred significant antitrust scrutiny. A prohibition's elimination of total merger profits brings about then the punitive measure that Posner (1970) acknowledges as crucial for deterrence. Remedies, on the other hand, by attempting to identify and eliminate only the transaction's anti-competitive elements, allow merging firms to reap some of the merger profits, Π^R. When an antitrust authority imposes remedies as opposed to prohibitions, the disincentive for firms to propose anti-competitive mergers is clearly lower. Furthermore,

[3] See Röller, Stennek and Verboven (2000) and Pautler (2001) for surveys of mergers and efficiency gains.
[4] If remedies functioned imperfectly (by not fully eliminating the anti-competitive part or by necessarily eliminating the pro-competitive part of a proposed merger), then our analysis would be qualitatively similar. We only assume that, all else equal, a merger with remedies should yield lower profits than a merger with clearances.

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, prohibitions seemingly involve more deterrence than do remedies (as prohibitions represent larger punishments); and both prohibitions and remedies involve more deterrence than clearances.

With the above as a backdrop, we now give a more formal structure and introduce some additional features to make our analysis realistic and suited for empirical testing. The features of our model include the following: firms decide on the type – restrictiveness – of mergers proposed; antitrust authorities either clear, remedy or prohibit a merger proposal; an antitrust authority has the option to alter the tendencies of its merger policy regime (for example, using more remedies at the expense of prohibitions); and uncertainty exists regarding an antitrust authority's stance.[5]

First, in addition to deciding on whether or not to propose a merger, firms also choose merger type: namely, the restrictiveness (η) of a merger proposal. In particular, firms may set a 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 the positive impact on anti-competitiveness, a merger proposal's restrictiveness naturally impacts merging firm 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

[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 behaviour might evolve over time.

to follow Barros (2003) in that firms face a profit incentive to propose mergers with high 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 the merging firms with its own judgement of the market—a judgement that defines an admissible level $\hat{\eta}$. As already noted, antitrust authorities have three possible courses: prohibition, remedy and clearance. 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 remedies. Note that parameter α – the remedy solution range that is taken as known to simplify exposition – denotes the extra restrictiveness level the authority is willing to accept as long as remedies are imposed. Finally, for $\eta > \hat{\eta} + \alpha$, the merger is prohibited.

The antitrust authority's judgement with respect to admissible levels of restrictiveness is ex-ante unclear when firms decide on whether and which type of merger to propose. Unlike cartels – where it is relatively easy to see where the lines are drawn – many firms will 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 antitrust authorities can give 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 or equal to η.

According to the above setup (where the merging parties receive full profits under a clearance, the pro-competitive profits under a remedy, and zero profits under a prohibition), firms expect a profit given by:

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

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

Note that K measures the cost of building up the merger proposal—a fixed cost. Accordingly, a merger is proposed if firms have $V > 0$ after optimally choosing η (namely, maximize V with respect to η),. Clearly, a lower K leads to more mergers. Yet more interesting is the impact of a change in α, as this reflects a merger policy change (a movement of the boundary between remedies and prohibitions). Increasing α means that authorities are willing to use remedies in cases where previously they would outright reject a merger. Per our descriptive analysis, this policy substitution from prohibitions to remedies is empirically relevant. 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 prohibitions to remedies should induce a higher number of future merger proposals. The same model can be used to address two additional policy shifts of importance: increasing remedies at the expense of fewer clearances; and increasing prohibitions at the expense of fewer clearances. In both cases, we would expect more frequency-based deterrence – fewer merger notifications – as both remedies and prohibitions involve more punishment than do clearances.[7]

Before turning to empirical expectations, it is important to conceptually make the link between changes in merger policy, changes in antitrust actions, and changes in firm beliefs with regard to antitrust stances. We propose here how one might reason the above process while taking into account uncertainty regarding merger policy rules. First off – due to the lack of clarity for merger policy rules – firms must make inferences as to the actual stances of antitrust authorities: namely, what are the critical parameters for eliciting remedies and prohibitions. Firms make inferences then about $\hat{\eta}$ and α based both on the antitrust authorities' past antitrust actions and on the imperfect knowledge firms have of the η for the previously proposed mergers of other firms. The imperfect knowledge comes from

[7] We refer to the working paper version of this manuscript for a more detailed analysis of these policy shifts (Seldeslachts, Clougherty & Barros; CEPR Discussion Paper No. 6437; September 2007).

information publicly released – from the press and antitrust authorities – about the merger and from information firms have about the industry.[8] Changes in antitrust actions (prohibitions and remedies) 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 this updating is done in a Bayesian manner, then increases in a particular antitrust action lead to the positive updating of the probability of eliciting such an action. Indeed, 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 antitrust authority's real position; in other words, a change in antitrust actions potentially signals the future tenor of merger policy.[9] With the above in mind, we can generate some simple expectations for empirical testing.

First, we predict that a spike in prohibitions leads to positive updating of the perceived probability of a prohibition. Prohibition spikes may reflect that the boundary between remedies and prohibitions has been altered (a decrease in the perceived α), but may also reflect a mass transfer in probability from clearances to prohibitions. Since prohibitions have a significant deterrence effect (namely, they represent a higher cost to merging as compared to remedies and clearances), individual firms will be less inclined to propose a merger and future merger notifications should go down. The above can be summarized as follows:

> ***Hypothesis 1:*** *An increase in prohibitions leads to more frequency-based deterrence: a decrease in future merger notifications.*

Second, we predict that a spike in remedies leads to positive updating of the perceived probability of a remedy. Remedy spikes may reflect that the boundary between remedies and

[8] If other firms, for example, propose a merger that would create a monopoly or facilitate exclusive territories; then, the proposed η is high. If the proposed merger would not substantially increase concentration; then, the 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 the antitrust actions taken in these cases.

[9] For example, if a merger with high η is prohibited, then there may be little updating on α. But if firms see the prohibition of a merger with a relatively low η, then α is revised substantially downwards.

prohibitions has been altered (an increase in the perceived α), but may also reflect a mass probability transfer from clearances to remedies. Accordingly, an increase in remedies can have two possible effects on future merger frequencies: 1) a negative effect when remedies come at the expense of clearances; 2) a positive effect when remedies come at the expense of prohibitions.[10] Thus when firms positively update their perceptions about an authority using remedies, the net-effect depends on whether the spike in remedies lowers the perceived probability of prohibitions more than the perceived probability of clearances. If the former effect dominates, then an increase in remedies leads to the perception of a less-tough future merger policy and we should witness a higher proclivity for firms to seek mergers. Yet, if the probabilities of future clearances go down then an increase in remedies should result in a lower proclivity for firms to seek mergers. The above can be summarized as follows:

> ***Hypothesis 2:*** *An increase in remedies leads to indeterminate frequency-based deterrence: future merger notifications decrease when remedies substitute for clearances, but future merger notifications increase when remedies substitute for prohibitions.*

To keep things simple, we have until this point excluded the merger policy option of monitorings. In part, this was driven by the prior that the effects of monitorings are relatively suspect, as firms subjected to monitorings may not actually be incurring a real punishment. Monitorings may then send a noisy signal of an antitrust authority's true intentions; 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

[10] In our empirical setup, we will look at the coefficient estimates for both Remedies and Prohibitions; hence, the Remedies coefficient estimate could be considered to strictly represent the effect of increases in Remedies with respect to Clearances since we hold Prohibitions constant. But the prevailing relationship between Remedies and Prohibitions is positive, thus the secondary substitutive relationship between Remedies and Prohibitions is not held constant in our estimations. Accordingly, spikes in Remedies can represent more 'Remedies with respect to Clearances' but also more 'Remedies with respect to Prohibitions'.

consider the impact of monitorings – since 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 the 1992-2005 period; 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 national 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.[11]

The first construct of primary interest for our empirical analysis is merger behavior—namely, the number of mergers occurring. As a measure for the number of mergers, our dependent variable, we use 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 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.

[11] See Clougherty (2005) for a different application of this OECD-based data.

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, remedies, and prohibitions. Where 'Monitorings' are the number of transactions cleared but with commitments by the antitrust authority to monitor post-merger behavior, 'Remedies' are the number of transactions cleared but forced to undertake behavioral or structural remedies to ameliorate anti-competitive concerns, and 'Prohibitions' are the number of transactions that are out-right prevented by the antitrust authority.[12] 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 (not one year) lag in terms of benefits (Davies & Majumdar, 2002).[13] Accordingly, antitrust actions essentially represent a two-year count of the possible merger policy actions taken by a particular jurisdiction with respect to merger behavior: with monitorings, remedies and prohibitions representing the three sub-categories of actions. 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 1990's 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

[12] We have data on abandonments (transactions abandoned by merging parties due to anti-competitive concerns by the authority) for 26 jurisdictions (except Australia & Greece). Yet, this variable suffers from a great deal of measurement error, as jurisdictions differ significantly in the degree to which (and how) they report abandonments. Nevertheless, unreported estimations support that the empirical results reported later in the paper are robust to the inclusion of this additional Antitrust Action variable.

[13] We thank an anonymous referee for this suggestion to de-sensitize the Antitrust Actions data to yearly variation. Accordingly, the value for a lagged Antitrust Action in one particular observation year will be the following: $((\text{Antitrust Action}_{t-1} + \text{Antitrust Action}_{t-2}) / 2)$. 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.[14]

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. The figure shows a steady rise in that rate from around 1.5% in the early nineties to around 5% by 2005. The tendencies in the absolute number of antitrust actions are surely a response to the global merger wave. If more mergers are notified, then more mergers may be subject to some sort of antitrust scrutiny. The relative increase of actions, however, tells us more. It may convey that antitrust authorities have become tougher and increasingly "punish" proposed mergers; thus, conforming to the received wisdom that antitrust in the cross-national context has experienced both greater adoption and strengthening over the last two decades. It may also indicate that authorities have raised notification threshold levels in an effective manner in order to not waste limited institutional resources on harmless mergers (De Loecker, Konings & Van Cayseele, 2007). This leads naturally to a higher percentage of antitrust actions, as the population of notified mergers will consist of 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 1990's 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 remedies; and less commonly use the practice of prohibitions.

[14] The average number of antitrust actions was around 9.2 in 1998, falling to 8.9 in 1999 and rising again to 10.4 in 2000. Further, the average number of merger notifications peaked at 598 in 1998, falling to 399 in 1999 and rising again to 437 in 2000. Thus – although not peaking in the same year – antitrust actions and merger notifications go together both up and down.

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 (Prohibitions, Remedies and Monitorings) 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, 2004). 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 any additional period-specific shocks.

Merger waves also typically coincide with economic booms and high stock markets (Gugler, Mueller & Yurtoglu, 2004; 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.[15] 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.

The period 1992-2005 was also a very active law-making period – in large part due to the expected entrance of ten new EU members. As previously noted, some scholarship considers how changes in antitrust laws impact competition (Stigler, 1966; Hoekman & Kee, 2003); hence, changes in antitrust statutes may also 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 should point out that we are unable to detect the direction of the law change: i.e., whether the change represents a more vigorous or more lenient antitrust 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

[15] 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'.

to antitrust legislation. Note also 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 antitrust authorities. The stated aim was for better coordination of enforcement efforts and the promotion of a common competition culture 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, 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 Prohibitions, Remedies 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.

We log-transform the Mergers and Antitrust Actions variables in order 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.[16] Hence, all the variables are in logs for our estimations—except for the dummy variables and the two economic conditions variables.[17]

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.[18] 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]". Furthermore, 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.

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 – if the original equations in levels were added to the system – additional moment conditions could be brought

[16] 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.
[17] 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.
[18] The OLS level estimator is inconsistent and biased upwards, since the lagged term of Mergers is positively correlated with the error term. It can also be shown that the standard Within Group estimator is inconsistent and biased downwards when including autoregressive dynamics of the dependent variable.

to bear to increase efficiency and to reduce finite sample bias; hence, we employ Roodman's Stata procedure for System GMM.[19] 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.[20]

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 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

[19] See http://econpapers.repec.org/software/bocbocode/s435901.htm for more information on the software.
[20] Additionally, the main assumption to use the augmented system estimator is that unobserved jurisdiction effects are uncorrelated with changes in the error term.

available observations.[21] 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 OLS, Random-effects and 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 six regression specifications that attempt to take the above issues into account. First, regressions' #1, #2 & #3 respectively report the OLS, Random-effects and Fixed-effects estimation results—where all three estimations include fixed period-specific effects. Regression #4 reports the results of a GMM estimation where only the clearly-endogenous lagged dependent variables are instrumented for. Regression #5 reports a GMM estimation where the potentially-endogenous variables (lagged dependent variables and the Stock-Market variable) are instrumented for. Regression #6 reports a GMM estimation where the potentially-endogenous variables (lagged dependent variables and the Stock-Market variable) and the potentially-predetermined variables (Prohibitions, Remedies, and the Threshold variables) are instrumented for.

VI. EMPIRICAL RESULTS

Table 2 reports the estimation results for the six regression specifications. Before we discuss the constructs of primary interest, we comment on the adequateness of the model in all six regressions. First, the Hansen test of overidentifying restrictions yields evidence in all three GMM estimations (regressions' #4, #5 & #6) indicating that one cannot reject the

[21] In cases where the number of instruments is large relative to the number of observations, system GMM results are biased toward those of OLS.

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 the error terms (the smallest of all three estimations reports Pr>z=0.561). Third, the R-squared terms in Regressions' #1 & #3 are relatively high at 0.96 & 0.91 respectively, though largely a function of the regression model's dynamic nature. In short, the regression model seems to pass the necessary diagnostics and be well specified in terms of statistical significance. Before discussing the coefficients of primary interest, we comment here on the control variables:

The two lags of Mergers seem to be important variables. The first lag is positive and highly significant for all six estimations. While the second lag is insignificant – and negative – throughout, 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 six specifications.[22]

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 #3 (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).

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 #3 (the fixed-effects estimation) involves positive statistical

[22] We performed simple t-tests. Bond et al. (2005) showed that in panel data with relatively short panels, t-tests have good size properties and high power—even when series are potentially close to unit root. These properties are particularly valid in simple OLS and system GMM specifications. Note that the main results are robust to only including one lag of the dependent variable.

significance. Further, the second law change in regression #4 involves negative statistical significance. Since these are dummy variables in a log-log regression, the same comment applies as above with the threshold changes. We should also reiterate our previous comment regarding the inability to detect the direction of the law change; thus, do not read too much into this measure's lack of significance. The EU 2004 Reform variable is significant and negative in five out of the six regression equations; thus, suggesting that the EU reforms generally led to fewer merger notifications in all the EU member antitrust jurisdictions.

The macroeconomic conditions (Growth) variable is significant and positive in regressions' #1 & #2; thus, yielding some support that economic growth generates merger waves. This result is in line with neoclassical theories on mergers that assume managers to maximize profits and mergers to become more profitable during economic booms (Lambrecht, 2004). Stock market conditions have a negative and significant impact on Mergers in the three non-instrumented regressions (#1, #2 & #3). While this result is not in line with the behavioral literature on merger waves (where a higher stock market should lead to more mergers – see Harford, 2005, for an overview), the negative 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: Prohibitions has a statistically-significant negative impact on future merger behavior in all six regression equations. The consistent significance and strong impact of this variable suggests that spikes in the use of Prohibitions send a clear signal of toughness by antitrust authorities—a signal that significantly reduces future merger proclivities.

Remedies, on the other hand, seem to positively influence future Mergers; though, the coefficient estimate is only significant in three regression equations (#1, #2 & #4). Accordingly, we can interpret these results as suggesting that the effect of remedies coming at the expense of prohibitions (a lowering of antitrust toughness) is stronger than the effect of

remedies coming at the expense of clearances (an increase in antitrust toughness). In other words, we have some evidence that firms view spikes in remedies as indicating softer behavior on the part of antitrust authorities. Such an interpretation should be cautioned by the fact that the remedies coefficient estimate is not significant in the fixed-effects estimation (regression #3); thus, suggesting that the remedies effect may only be capturing cross-jurisdictional variation. Nevertheless, the important point here is that the application of Remedies 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 (while mostly negative) is highly insignificant throughout our estimations. Accordingly, firms seem to not give any importance to Monitorings as an indication of the future tendencies of authorities.

In sum, the empirical results support the robustness of Prohibitions – but not Remedies and Monitorings – in terms of deterrence effects.[23] Yet we must move beyond statistical significance and consider the economic significance of prohibitions on deterring future mergers. First off, the coefficient estimates for Prohibitions in Table 2 suggest that a 10% spike in Prohibitions leads to a percentage drop in Mergers in the following year ranging from 0.72% (Regression #1) to 1.84% (Regression #3).[24] Consider a comparison between the US and the EU for illustrative purposes: where the US stepped up prohibitions in 2003 (the two-year average going from 11 in 2002 to 15 in 2003) and the EU stopped prohibitions (the two-year average going from 2.5 in 2002 to 0 in 2003). Taking the conservative coefficient estimate from Regression #1 suggests that the US faced 2.6 percent fewer mergers in 2004 due to the 2003 spike in Prohibitions, while the EU faced some 7.2 percent more mergers in 2004 due to the 2003 drop in Prohibitions. In short, the empirical results generally support significant-and-robust deterrence effects for the application of Prohibitions.

[23] The main results are robust to different forms of clustering in standard errors and to other specifications: a linear-linear model, and a model with additional lags for the Growth and Stock-Market variables.
[24] The results must be interpreted as an upper bound since we transform zeros to ones with the log specification.

VII. DISCUSSION AND CONCLUSION

The deterrence effects of merger policy are generally an under-researched topic. The few studies in this area tend to focus on deterrence effects as manifested by mergers being shaped differently (composition-based deterrence), or as manifested by future price-cost margins (where it is difficult to disentangle regulatory from deterrence effects). Furthermore, few studies consider whether all the tools available for merger policy involve adequate deterrence. Accordingly, we focus on the impact of different merger-policy tools on frequency-based deterrence effects; we consider how different antitrust actions – prohibitions, remedies, and monitorings – influence the future proclivity of firms to engage in mergers. We bring empirical evidence to bear on this issue by employing a cross-jurisdictional data set for merger policies over the 1992-2005 period. The broad scope of our data allows consideration of whether changes in merger policy enforcement impact firms' merger behavior.

Our empirical results suggest that antitrust actions can impact future merger frequencies; however, not all merger policy tools are effective deterrents. Prohibitions involve deterrence implications with respect to future merger frequencies, but both remedies – the most popular merger policy tool – and monitorings seem to involve no deterrence implications. 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 prohibitions and remedies, however, generate more serious implications.

First, the positive findings for prohibitions (where merger prohibition spikes lead to firms forsaking future merger proposals) suggest that antitrust actions can involve deterrence implications. While a small literature supports the existence of composition-based deterrence effects for merger policy, 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 effects. Our empirical results suggest that merger policy involves frequency-based deterrence. Accordingly, antitrust authorities should appreciate that prohibitions carry an ability to deter the future proclivity of firms to seek mergers.

Second, the empirical findings for remedies suggest that this particular merger policy tool may not involve adequate deterrence. One should temper the implications from this finding, as we only consider frequency-based deterrence; yet, frequency-based deterrence should go hand-in-hand with composition-based deterrence. Plus, frequency-based deterrence 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 remedies: remedies may indeed ameliorate the anti-competitive effects of proposed mergers, but such actions – unlike prohibitions – carry no robust negative effect on the future proclivity to merge.

We are not the first to suggest that remedies 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 remedies. Joskow (2002) argues that structural remedies are neither easy for authorities to apply nor easy for firms to adopt in light of the transaction costs involved. Joskow points out that 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 remedies in the European context have not been effective; in particular, they suggest that remedies may act to enhance collusion. Furthermore, some empirical work finds remedies to generally be ineffective: Crandall and

Winston (2003) find US consent decrees (namely, remedies) in two-digit SIC manufacturing industries to increase – not decrease – future price-cost margins; Duso, Gugler and Yurtoglu (2006, 2007) generally find second-phase European Commission remedies to not ameliorate anti-competitive effects. Hence, we proffer one additional criticism – weak deterrence – to the list of potential concerns regarding remedies.

In sum, we find prohibitions to be effective and remedies to be ineffective in the deterrence of future merger frequencies. The weak-deterrence implications of remedies may be a concern in light of the trend toward increased use of remedies as a merger policy tool. Justice William J. Brennan Jr. captured the underlining faith that has led to increased adoption of remedies when he states "Divestiture … the most important of antitrust remedies. It is simple, relatively easy to administer, and sure".[25] To the degree that antitrust authorities are concerned about the deterrence effects of merger policy, our results suggest that they may want to re-evaluate their penchant to increasingly employ remedies – instead of prohibitions – to deal with anti-competitive merger proposals.

[25] 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 'Remedies to Prohibitions'

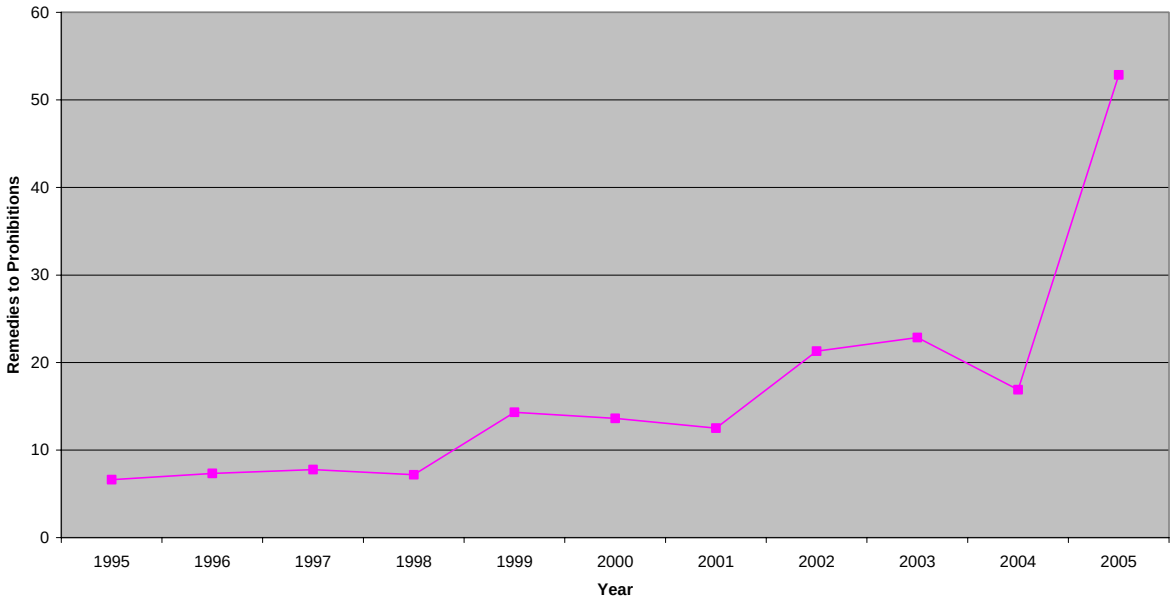

Figure 2: The Average Across All OECD Antitrust Jurisdictions for the Number of 'Antitrust Actions' and 'Mergers'

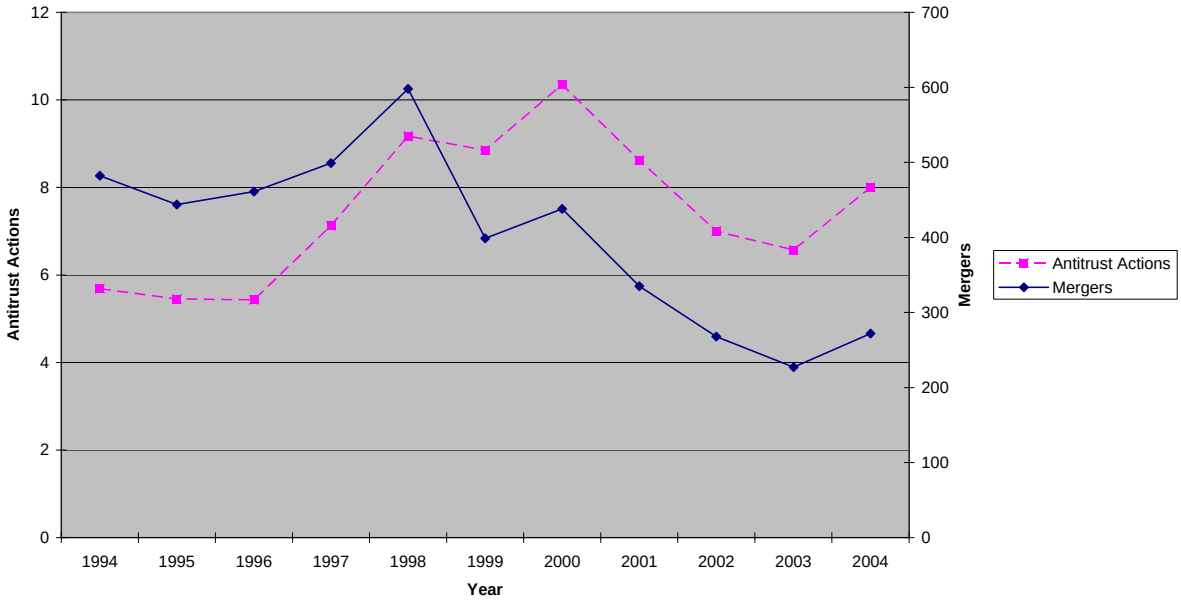

Figure 3: The Average Across All OECD Antitrust Jurisdictions for the Percentage of 'Notified Mergers Eliciting Antitrust Actions'

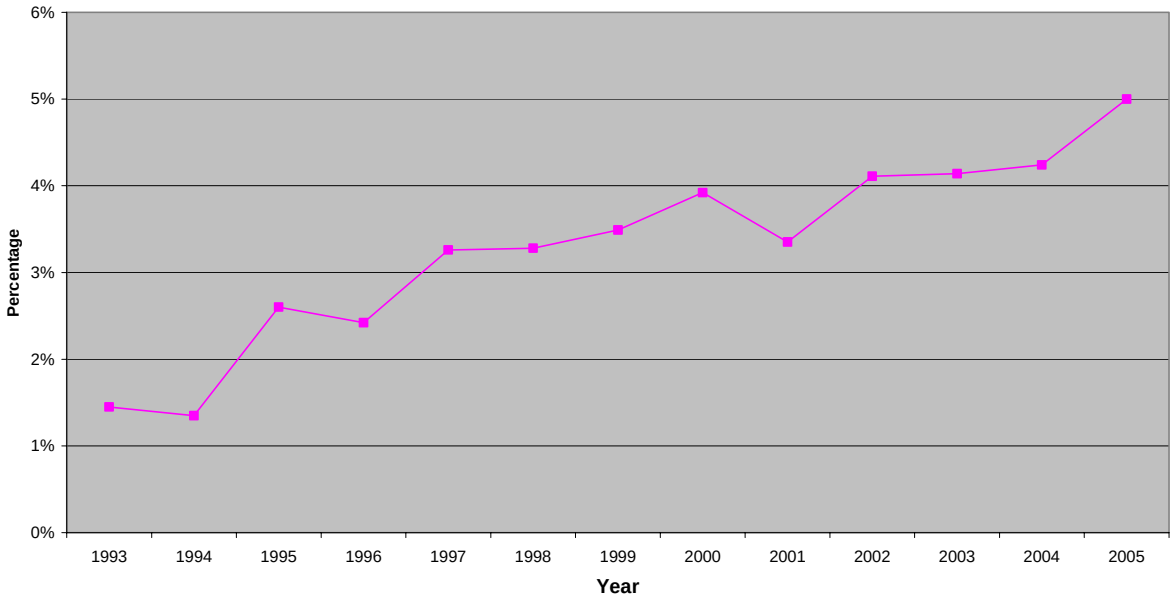

Table 1: Descriptive Statistics

- **Number of Observations and Variable Means by Antitrust Jurisdiction**

		Variable Means			
Antitrust Jurisdiction	*Observation Numbers*	*Mergers*	*Prohibitions*	*Remedies*	*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 Regression Results for Merger Conduct in Response to Antitrust Actions

- All Estimations involve Robust Standard Errors; fixed period-specific effects; and 198 observations
- Dependent Variable: Number of Notified Mergers

	Estimations Without Instrumenting			GMM Instrumenting Estimations		
	OLS	Random Effects	Fixed Effects	Instrument for Mergers	Instrument for Mergers & Stock-Market	Instrument for Full set
Regression #	(1)	(2)	(3)	(4)	(5)	(6)
Mergers $_{t-1}$	1.013***	1.041***	0.781***	1.001***	1.065***	1.019***
	(0.0796)	(0.0752)	(0.0864)	(0.103)	(0.0947)	(0.0767)
Mergers $_{t-2}$	-0.0521	-0.0729	-0.0294	-0.0276	-0.0878	-0.0637
	(0.0801)	(0.0807)	(0.0850)	(0.0762)	(0.0833)	(0.0784)
$_{k=1}\Sigma^{2}$Prohibitions $_{t-k}$ /2	-0.0722*	-0.0772**	-0.184**	-0.169*	-0.147*	-0.130**
	(0.0422)	(0.0363)	(0.0749)	(0.102)	(0.0803)	(0.0627)
$_{k=1}\Sigma^{2}$Remedies $_{t-k}$ /2	0.0601*	0.0655***	0.0115	0.119**	0.0539	0.0565
	(0.0315)	(0.0247)	(0.0475)	(0.0604)	(0.0526)	(0.0381)
$_{k=1}\Sigma^{2}$Monitorings $_{t-k}$ /2	-0.0536	-0.0653	0.0421	-0.0479	0.0116	-0.0939
	(0.117)	(0.0819)	(0.157)	(0.227)	(0.177)	(0.131)
Threshold1 $_{t}$	-0.00297	0.00485	-0.264***	0.0450	-0.0245	-0.0575
	(0.0585)	(0.0577)	(0.0908)	(0.130)	(0.121)	(0.0752)
Threshold2 $_{t}$	-0.0407	-0.0272	-0.102	0.198	-0.0554	-0.0252
	(0.0683)	(0.0739)	(0.0912)	(0.255)	(0.163)	(0.0890)
Threshold3 $_{t}$	0.223*	0.253	0.279**	-0.877	0.447	0.214
	(0.134)	(0.165)	(0.129)	(1.691)	(0.770)	(0.151)
Law1 $_{t}$	0.0282	0.0143	0.269**	-0.111	-0.111	-0.0975
	(0.0620)	(0.0595)	(0.111)	(0.136)	(0.152)	(0.114)
Law2 $_{t}$	0.000670	0.0112	-0.00172	-0.360*	-0.0333	-0.00307
	(0.0826)	(0.0716)	(0.103)	(0.204)	(0.128)	(0.0808)
EU 2004 Reform $_{t}$	-0.199**	-0.181	-0.192**	-0.713*	-0.442**	-0.243*
	(0.0993)	(0.111)	(0.0944)	(0.406)	(0.201)	(0.147)
Growth $_{t}$	0.0151*	0.0159***	0.000378	0.00319	0.00515	0.0192
	(0.00845)	(0.00561)	(0.0131)	(0.0151)	(0.0122)	(0.0139)
Stock-Market $_{t}$	-0.00073**	-0.00075***	0.00247*	-0.00066	0.000091	-0.00052
	(0.000369)	(0.000229)	(0.00149)	(0.000862)	(0.000588)	(0.000610)
Constant	0.165	0.144	0.932**	0.0798	0.170	0.255
	(0.272)	(0.209)	(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	0.96		0.91			

( ) = standard error; *** = 1%, ** = 5%, and * = 10% Significance.