Regime Shift in Antitrust

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Abstract

This paper empirically models the longer-run deep-seated shift in intellectual thinking that followed the Chicago School's criticism of the older antitrust doctrine, the shorter-run driving forces related to switches of the political party in power, merger waves, changes in economic activity and the level of funding and quantifies their impact on enforcement by the Antitrust Division of the U.S. Department of Justice over the period 1958-2002. The key findings are: (1) a distinct *regime-shift* in antitrust enforcement during the 1970s and, post-regime-shift, there has been a marked compositional change with a quantitatively large increase (decrease) in criminal (civil) antitrust court cases initiated; (2) post-regime-shift, there appears to be a change in the role played by politics with Republicans initiating more (less) criminal (civil) court cases than Democrats and the estimated quantitative effects are large; (3) disaggregating the total number of court cases into the main categories under which they are initiated (price-fixing, mergers, monopolization and restraints-of-trade) shows that individual types of cases have widely differing responses to changes in the driving forces; and (4) in a horse-race between the regime-shift and political effect on one side and the remaining variables on the other, the former forces win hands-down in explaining broad shifts in enforcement. Modeling the longer-run shift and disaggregating the court cases emerge as crucial to gaining insights into the intertemporal shifts in enforcement. The paper elaborates on the causes for the shift in enforcement and on the effectiveness of antitrust.
Keywords: Antitrust enforcement; regime-shift; politics; supreme court; effectiveness.
JEL: L40; K00.

1. Introduction

Dating back to 1890, when the first major antitrust law was enacted in the form of the Sherman Act, the U.S. has perhaps had the richest history of antitrust enforcement with a complex confluence of economic, legal and political forces producing ebb and flow in enforcement.[1] As described in Harrington (2006): *"Antitrust enforcement is the process by which a more competitive environment is created through the prohibition of certain practices deemed illegal by antitrust laws."* More broadly, the objectives of antitrust can be thought of as fostering competitive markets and promoting innovation, with implications for prices, welfare and economic growth. While historically the US has had active enforcement,[2] the recent past has seen competition policy gaining emphasis in other parts of the world. The European Union has seen important changes in the enforcement of collusion and mergers, and calls for greater vigilance to ensure competitiveness.[3] Japan and Australia have put new emphasis on competition policy and debated harmonizing laws with major trading partners (Cassidy (2001), Homma (2002), OECD (1999) and Richardson and Graham (1997)). Against the backdrop of the historical US perspective, the changing global emphasis and the potential role played by competition policy in shaping the competitive dynamism of economies, the primary objective of this paper is to examine how changes in intellectual thinking, shifts in the legal landscape and political forces have influenced the intertemporal path of antitrust enforcement by the Antitrust Division of

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[1] In spite of it being in large part a political response to undesirable business conduct and emergence of large corporations, scholars have noted the close match of the wording of Sherman Act section 1 (restraints of trade) and section 2 (monopolization) to those of Adam Smith on collusion and monopoly power (Blair and Kaserman, 1985). See Bork (1966) and Lande (1982) for debates on the intent of the Sherman Act. Motta (2004, 1-9) provides a succinct history of the U.S. antitrust enforcement.

[2] Oz Shy (1995, p.5) writes: *".in the US real prices of products tend to be the lowest in the world. The US also has the most restrictive antitrust regulation structure in the world. Hence, although it is commonly argued that market intervention in the form of regulation results in higher consumer prices, here we observe that antitrust regulation is probably the cause for low consumer prices in the US..."*

[3] Contributions in Eekhoff (2004) make a case for competition policy vigilance in the newly deregulated sectors in Europe to ensure competition and growth. Enforcement of cartels in Europe has seen a big change; Harding and Joshua (2004) detail the shifts. Duso, Neven and Röller (2003) and Bergman, Jakobsson and Razo (2003) present analysis of EU merger enforcement. Motta (2004, p.9-17) provides an overview of competition policy in selected European countries and the EU.

the U.S. Department of Justice.[4] Below we discuss these driving forces.

We begin by focusing on the more deep-seated changes that occur relatively infrequently. The influence of the Chicago School is well documented and the genesis of their law and economics movement is often recognized to be Aaron Director and Edward Levi.[5] Director and Levi (1956) criticized the state of antitrust, disagreed with a variety of business practices like tying and vertical restrictions being anti-competitive and abuse of monopoly power, downplayed the likelihood of predatory pricing, emphasized efficiencies and noted flaws in key antitrust decisions like Standard Oil (1911) and Alcoa (1947).[6] In similar tone, Director (1957) criticized the United Shoe Machinery (1968) decision. Many of these arguments went on to become guideposts for the Chicago School's law and economics thrust. The influential contributions by Stigler (1964) and Williamson (1968), along with Demsetz (1973, 1974), Bork (1966, 1978)[7] and Posner (1969, 1974, 1976) among others, solidified the law and economics framework. The shift in thinking was as follows. First, vertical and conglomerate mergers, resale price maintenance, vertical restrictions and other conduct that were often viewed as anti-competitive under the older antitrust regime were given pro-competitive and efficiency interpretations. Second, the focus shifted to areas of clearer harm to welfare such as price-fixing and horizontal mergers in concentrated markets. Baker (2002, 2003), Crandall and Winston (2003), Kovacic and Shapiro (2000) and Motta (2004, p.2-9) provide discussion of these shifts.

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[4] The U.S. enforcement is conducted by the Antitrust Division of the Department of Justice and the Federal Trade Commission. The Antitrust Division is solely responsible for criminal prosecutions. Merger control in contrast is shared by the two agencies. In this paper we only focus on the Antitrust Division in part because it represents the totality of criminal enforcement and in part because disaggregated data on enforcement under the various Antitrust Acts were available for the Division.

[5] Director joined the Chicago Law faculty in 1946, founded the *Journal of Law and Economics* in 1958, and his students included Robert Bork, Frank Easterbrook and Richard Posner – legal scholars and judges who greatly influenced antitrust. Bork notes: *"[Director's] teachings...made him the seminal figure in launching the law and economics movement, which transformed wide areas of legal scholarship."* (From: Aaron Director, Founder of the Field of Law and Economics, University of Chicago Press, 2004.)

[6] Influenced by Director's hypothesis that firms would prefer mergers and other practices to attain monopoly status as opposed to predatory pricing, McGee (1958) tested whether Standard Oil engaged in predatory pricing, a key issue in the 1911 antitrust decision. His results suggested this was not the case.

[7] It is noteworthy that while Bork's book was published in 1978, it was completed as early as 1968-69.

Given this new intellectual thinking, there is the likelihood of a *regime-shift* in antitrust enforcement. While much has been written on this, there has been no formal attempt to empirically model this longer-run shift and quantify the effects using antitrust enforcement data. In this paper we use econometric techniques to detect potential regime-shift in enforcement and control for it when examining the impact of the political and other shorter-run driving forces.

Turning to the legal arena, the US Supreme Court is the final arbitrator of antitrust court cases. Pate (2004) presents an overview of the key role played by the Supreme Court in shaping US antitrust. Firms being prosecuted can appeal to this highest court for relief. The Antitrust Division can appeal to the Supreme Court if they lose in lower courts. The Court's decisions become landmark one's and influence future enforcement. The Supreme Court justices are nominated by the President and confirmed by the Senate. Republican Presidents are expected to appoint more conservative justices. The Republican-nominated justices of the Court had a simple majority during 1958-62, were in the minority during 1963-69, regained a simple majority during 1970-71 and attained a commanding two-thirds majority starting 1972 which they have not relinquished. Given the shift to a more conservative court and very likely a more *laissez-faire* perspective on economic activity, it is plausible that the stance on antitrust may have shifted. We return to this discussion in section 8.

The political spectrum is the more *time-varying* element due to changes in the party in power. The literature has thought about political attitudes towards regulation or interference in markets in alternative ways: costs v. benefits of regulation; probability of bureaucrats making mistakes resulting in different degrees of confidence in bureaucracy; income redistribution; and paternalistic views. For our purposes we distinguish between a relatively conservative *hands-off* v. a more liberal *hands-on* approach to markets. This categorization is common, see Bailey and Chang (2001) and the references there. Over our sample period (1958-2002) there have been ten different Presidents and six switches of the party in power, allowing us to measure the impact of political changes on enforcement. Studying the linkage between politics and enforcement is interesting due to the Antitrust Division's institutional structure. The US President nominates (and the Senate confirms) the Assistant Attorney

General who heads the Antitrust Division: the relationship is akin to a *Principal-Agent* one and analogous to the *affiliated-regulator* setting described in Faure-Grimaud and Martimort (2003). This potentially sets the stage for shifts in enforcement with switches of the party in power.

Apart from the above, we focus on other broad shorter-run driving forces that may affect enforcement such as merger waves in the economy, funding for the Antitrust Division and the level of economic activity. As part of our additional results, we examine the role played by foreign competition and the political composition of the U.S. House of Representatives and the Senate.

This paper contains several features that distinguish it from previous studies. First, prior empirical studies did not model the longer-run effects and certainly not jointly modeled the longer-and-shorter run driving forces. Longer-run shifts could arise from deep-seated changes in intellectual thinking about business conduct. As we demonstrate in sections 4 and 5, the antitrust enforcement data show longer-run shift in their levels, symptomatic of structural-breaks. Shorter-run fluctuations may arise from merger waves, shifts of the political party in power, economic conditions, the level of funding available for investigations, among others. Our empirical results show that omitting the control for the structural-break significantly reduces our understanding of the intertemporal path of enforcement and generates erroneous inferences about the shorter-run effects. Second, in contrast to the previous literature, we analyze antitrust data at several levels of disaggregation. The grand total number of court cases filed are first disaggregated into total criminal and total civil cases as they represent very different types of violations. Criminal court cases relate to price-fixing, bid-rigging and market-allocation schemes; these are *per se* violations with no efficiency justification. In contrast, civil court cases relate to mergers, exclusive contracts, tying agreements, bundling, vertical restraints, among others, and have market power v. efficiency considerations and *rule of reason* prevails where the specific business conduct is judged by the party's intent and the effect the conduct is likely to have. Next, we disaggregate the total civil cases into the three main components: Clayton Act section 7 (mergers) and Sherman Act sections 1 (restraints of trade) and 2 (monopolization). There is weak justification for even aggregating across different types of civil cases as the nature of

violations can be quite different, such as mergers and tying agreements. Our estimates show that the longer-run and shorter-run driving forces often have divergent sign patterns and varying quantitative effects on these different types of cases, vividly demonstrating that aggregation masks underlying heterogeneity. Third, we control for the total number of mergers in the US as merger evaluation is one of the primary tasks of the Antitrust Division, using up significant resources. This allows us to control for the Division's workload, examine the link between merger waves and enforcement, and whether when merger investigations increase, is there reduction of enforcement in other categories. Fourth, in section 6 we test for endogeneity of funding for the Antitrust Division and the US merger wave to the prevailing enforcement stance: e.g., vigorous merger enforcement may dampen merging spirits. Fifth, we study enforcement data over a long 45-year period, 1958-2002. These aspects make this one of the most comprehensive econometric studies of US antitrust enforcement. Since the abstract summarizes the key results, we do not repeat them here.

In section 2 we review some theoretical contributions, provide details about the institutional setting of the Antitrust Division and summarize previous empirical findings. The empirical model, econometric issues related to identifying regime-shift in antitrust enforcement, data and endogeneity of some model variables are presented in sections 3, 4, 5 and 6. Estimation results appear in section 7 and the paper concludes with a discussion of results and final remarks in sections 8 and 9.

2. Theory and Prior Empirical Findings

2.1. Some Insights from Theory

We review two papers that provide insights for our empirical analysis. Faure-Grimaud and Martimort (2003) link the institutional structure of the regulatory agency with policy implementation across political principals. The political principal 'P' delegates the task to regulatory agent 'R' and one of their results relates to the extent to which R can be captured by the firms it is entrusted with regulating. R can be (1) *independent*, implying that R stays as P changes or (2) *affiliated*, implying that R changes as P changes. Principal P_i puts weight ψ_i on the firm's utility, with $\psi_i<1$ indicating

that all principals dislike giving up rents to firms. Let P_1 be more concerned with rent extraction and less by efficiency than P_2; $\psi_1 < \psi_2$. They (p.418) interpret the gap $\psi_1 - \psi_2$ as the *degree of polarization*. In general, the *independent-regulator* can span several political principals, is longer-lived and has greater scope for capture by firms; here the principal can change without significant changes in enforcement. In contrast, the *affiliated-regulator* is shorter-lived, has less scope for being captured by firms and can be dismissed by the principal if there is deviation from the desired policy stance. The Assistant Attorney General (**AAG**) who heads the Antitrust Division is appointed by the US President; the relationship is akin to a *Principal-Agent* one and fits the *affiliated-regulator* setting.[8] We assume that the AAG will implement the President's desired policy stance as doing so may lead to future benefits like political and other appointments; else there could be removal from office and denial of future rewards. Over 1958-2002 there have been 10 different Presidents and 21 AAGs (excluding interim AAGs), indicating variation in both the principal and the agent. We note two aspects for our analysis: (1) with the Antitrust Division's *affiliated-regulator* setting, we potentially expect a distinction between Republicans and Democrats in the intensity of enforcement; and (2) we could proxy the gap $\psi_1 - \psi_2$ by the difference in the level of enforcement between Republicans and Democrats. In our empirical analysis, we estimate this gap and whether this has changed over time.

Second, Peltzman's (1976) *vote-maximizing* regulator faces a trade-off between producer and consumer interests. The group interests are captured by commodity price 'p' for consumers and profits 'π' for producers. The regulator's objective function is $M = M(p, \pi)$, with $M_p < 0$, $M_{pp} < 0$, $M_\pi > 0$, $M_{\pi\pi} < 0$ and $M_{p\pi} = 0$; the negative second-order conditions imply diminishing political returns to higher π or lower p. The constraint is given by $\pi = f(p, c)$, where c=production costs and c=c(q) with q=output. This yields an equilibrium condition $-(M_p/f_p) = M_\pi = -\lambda$ which states that the marginal political product of a dollar of profits equals the marginal political product of a price cut that also costs a dollar of profits. Thus, equilibrium will *not* result in protection for only one group. We focus

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[8] This contrasts with EU's competition commissioner who is appointed for a four-year term and the US Federal Trade Commission with a panel of commissioners. With agent(s) spanning different political principals, these fit the *independent-regulator* setting in Faure-Grimaud and Martimort.

on two aspects for our empirical analysis: (1) re-write the regulator's (specific) objection function as $M=\log[\pi^{\kappa}(A-p)^{\varphi}]$, or $M=\kappa\log\pi+\varphi\log(A-p)$, where κ and φ are the weights attached to the relevant group's interests and A an arbitrary constant with A>p, satisfying the optimality conditions in Peltzman's model. Change in political regime could be conceptualized by change in κ and φ, with κ higher under Republicans who we assume are more pro-business (or *laissez faire*) with less vigorous enforcement; and (2) Peltzman's (p. 225-227) results show that given the positive marginal consumer (producer) opposition to higher (lower) prices (profits), the regulator will not force the entire adjustment onto one group. Thus, in depressed profits during weak economic conditions, the political wealth effect implies that the regulator's incentive to let prices fall will be attenuated so prices would not fall as far as they would in an unregulated setting; consumers will buffer some of the producer losses. Thus antitrust enforcement will be relatively slack (vigorous) when economic conditions are weaker (stronger), suggesting a <u>direct</u> relationship between economic conditions and enforcement.

2.2. Prior Empirical Findings

Several studies have analyzed the total number of antitrust court cases as they represent the most visible action in enforcement; Ghosal and Gallo (2001) and Harrington (2006) briefly review these studies. Using the Antitrust Division data, Posner (1970) did not find a relationship between the party of the President and enforcement and he noted that enforcement appeared procyclical. Lewis-Beck (1979) did not find evidence of Republicans, as measured by the Party of the President and the composition of the House and Senate, pursuing less vigorous enforcement over 1890-1974; he found that neither GDP growth nor unemployment rate explained variations in the number of cases initiated. Siegfried (1975) found that economic factors do not influence enforcement. Cartwright and Kamerschen (1985) did not find significant effect of the party of the President on the number of cases. Pittman (1992) did not find evidence that political factors systematically affected antitrust enforcement. Results in Ghosal and Gallo (2001) showed no effect of the party of the President, and that case activity appeared counter-cyclical. For Federal Trade Commission data, Faith, Leavens and

Tollison (1982) examined the link between political influence and case-activity and found support for the private-interest theory of FTC behavior. Amacher, Higgins and Tollison (1985) found weak evidence that Democratic dominated commissions pursued enforcement more vigorously; they found no consistent link between economic activity and cases initiated. Coate, Higgins and McChesney (1990) found some evidence in support of the interest-group hypotheses. Collectively, previous empirical studies show that: (1) there is little systematic evidence that the party of the President matters for antitrust enforcement; and (2) the relationship between economic activity and case-activity is ambiguous – the typical finding being a weak positive, or no, link.

3. Empirical Specification

The behavior of economic agents often involves gradual adjustment of the relevant decision variable to the desired (or equilibrium) levels. One modeling strategy is to consider a decision-maker's objective to minimize the expected present value of a quadratic loss function subject to *adjustment* and *disequilibrium* costs. Since the theoretical and empirical underpinnings of this framework are laid out in Gould (1968), Kennan (1979) and Treadway (1971), we do not repeat them here. We assume that the Antitrust Division pursues court case activity subject to minimizing these two costs: (1) *adjustment costs* arise for the Division due to resource constraints given by the number of attorneys, economists and funding, implying that actual change in the Division's case activity may be less than the desired change in order to minimize adjustment costs; and (2) *disequilibrium costs* arise as the Division is subject to pressures from the political arena and producer and consumer groups. If the Division's enforcement stance is above or below the desired level, there may be pressure to rectify this, bringing the Division unwanted scrutiny and publicity.[9] Thus the above framework may

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[9] E.g., the American Antitrust Institute (a private organization) filed a lawsuit against Microsoft and the US Department of Justice that the government's settlement does not go far enough to tame Microsoft; lobbying by both producer and consumer groups on open-access decisions in telecommunications (GTE's 1999 lawsuit against TCI, Comcast and AtHome and the follow-up lobbying); lobbying by physicians, hospitals and health insurance providers on antitrust decisions on collective bargaining by physicians. These examples reveal lobbying by competing groups following action or inaction on antitrust.

be a useful way of modeling the intertemporal path of antitrust enforcement.[10]

We begin by defining our main variables: CASES=number of antitrust cases filed in court; VIOL=number of (typically unobserved) violations; PRES=political party with PRES=1 if Republican, else 0; REG=regime with REG=1 if new regime, else 0; FUND=funding allocated to the antitrust division; WAVE=total number of mergers in the US; and ECON=economic conditions. Formally, the Antitrust Division is expected to minimize the quadratic loss function:

$$(1)\ \text{Min CASES}_t\ \ E\sum_{t=0}^{\infty}\rho^t[\phi_D(\text{CASES}_t-\text{CASES}_t^*)^2+\phi_A(\text{CASES}_t-\text{CASES}_{t-1})^2],$$

where CASES_t is the actual number of court cases in year t, CASES_t^* is the desired number, ϕ_D and ϕ_A are the disequilibrium and adjustment cost parameters and ρ is the constant discount factor. The Antitrust Division makes a sequence of actual CASES_t decisions designed to meet the target CASES_t^* which is a function of relevant driving variables (described below). Since the derivations are well documented in Gould (1968), Kennan (1979) and Treadway (1971), we don't repeat them here. Solving the model results in the partial-adjustment equation: $\text{CASES}_t-\text{CASES}_{t-1}=\lambda(\text{CASES}_t^*-\text{CASES}_{t-1})$, where the actual change in CASES is a fraction λ ($0\#\lambda\#1$) of the desired change, with λ being a function of the adjustment and disequilibrium costs noted above. Rewrite the above as:

$$(2)\ \text{CASES}_t=\lambda\text{CASES}_t^*+(1-\lambda)\text{CASES}_{t-1}.$$

The desired CASES_t^* is modeled as a function of the relevant driving variables. Below we

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[10] An alternative would be to write a structural model by including preferences of politicians, producers and consumers, with these preferences changing over time to influence the antitrust regime. The structural parameters of interest would include the impact of the politicians on the antitrust decision process and the impact of the resulting antitrust decision on which cases gets filed. This would allow us to understand how alternate mechanisms for the decision process underlying antitrust policy affect which cases are contested. Alternatively, one can think of modeling the demand for and supply of antitrust and estimating the underlying parameters. Unfortunately, there is nothing in our data that would enable us to identify such structural parameters. This is why we do not pursue structural estimation.

first outline our benchmark empirical specification and then address additional issues related to the dynamics and lagged structure of the model variables. Let $CASES_t^*$ be modeled as:

$$(3) \; CASES_t^* = a_1 VIOL_{t-1} + a_2 PRES_{t-1} + a_3 REG_{t-1} + a_4 WAVE_{t-1} + a_5 FUND_{t-1} + a_6 ECON_{t-1} + \upsilon_t.$$

Since the relevant factors take time to impact the number of court cases, we use lagged values.[11] $CASES_t^*$ is driven by the number of (typically unobserved) violations, VIOL, assuming that it takes time for information about possible violations to flow into the Antitrust Division; a_1 is expected to be positive. The Division's willingness to initiate new cases in part depends on the prevailing political stance, PRES, and regime, REG. A relatively hands-off markets stance may lead to fewer cases; a_2 and a_3 are expected to be negative. The US merger wave, WAVE, embeds multiple effects: (a) increase in WAVE may imply an absolute increase in the number of mergers blocked, leading to an increase in cases; (b) with an increase in WAVE, resources may have to be diverted to evaluate them, taking resources away from other types of investigations; and (c) information discovered during merger evaluation may unearth other violations, leading to increased cases.[12] Thus, the sign of a_4 is ambiguous. The ability to initiate new cases depends in part on the level of funding, FUND. Since current court cases typically result from past investigations, we use past funding as the relevant variable which allows the Division to conduct investigations; a_5 is expected to be positive.[13] The

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[11] E.g., with a new political party in power, change in enforcement is not likely to be instantaneous. Also, potential violators get to observe the new regime and alter decisions with a lag. Merger and Sherman Act sections 1 and 2 investigations can be drawn out with information gathering, preliminary investigation and possible issuance of Civil Investigative Demands. The legislative process for funding starts two-years before the fiscal year in which they are needed and goes through various committees and subcommittees in the House and Senate, the Department of Justice and Office of Management and Budget.

[12] E.g., while evaluating the pending merger between First Data Inc. and Concord EFS Inc. the Antitrust Division discovered evidence on exclusivity contracts between Western Union and retail outlets, leading the Division to start an investigation of Western Union and issue Civil Investigative Demands. When investigating the UPM Kymmene-Bemis MACtac merger, price-fixing violations were uncovered.

[13] The link between the Antitrust Division's funding and cases and investigations may be a weak one due to variation in utilization of economists and attorneys. During peak work-periods an economist or attorney can be simultaneously working on multiple investigations; this falls in off-peak periods. The staff *continued...*

economic activity variable, ECON, is motivated by the results in Peltzman (1976) which suggests a positive relationship. We assume the error term is iid: $u_t - (0, \sigma_u^2)$.

Next we model the (typically unobserved) violations as:

$$(4) \ \ \mathrm{VIOL}_t = v_0 + b_1 \mathrm{PRES}_{t-1} + b_2 \mathrm{REG}_{t-1} + b_3 \mathrm{WAVE}_{t-1} + b_4 \mathrm{ECON}_{t-1} + b_5 \mathrm{CASES}_{t-1} + e_t.$$

In our empirical analysis we examine the total number of court cases filed as well as disaggregated by specific types of violations. For example, price-fixing conspiracies take place in smoke-filled rooms and are unobserved till information becomes available to the Antitrust Division. In the case of mergers, the economy often goes through thousands of mergers per year and the number of mergers that have serious market power implications are not ascertained without investigation. Similarly, other violations such as restrictive trade practices and abuse of monopoly power are typically unobserved till information becomes available. The way we visualize equation (4) is that there are some driving forces that may cause violations to increase or decrease and we spell these out. We assume that every period has some given number of (unobserved) violations v_0. Regarding the factors that influence the intertemporal variation in VIOL, we assume that these effects take time and use lagged values. The presence of a conservative political party, PRES=1, and regime, REG=1, may imply greater VIOL as firms may feel they can escape close scrutiny; b_1 and b_2 are expected to be positive. During higher WAVE, firms may have an incentive to merge beyond the potential higher profits motivation because the Division's resources may be limited and thus the chances of escaping an investigation may be higher. Overall, potential violators may be more inclined to engage in anti-competitive behavior with the view that the Division is pre-occupied with evaluating mergers; $b_3 > 0$. The economic activity variable, ECON, controls for the potential link between economic conditions and antitrust violations. In general, the direction of the relationship (sign of b_5) is ambiguous. For

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are not hired and fired over the short-to-medium term workload cycles. Since utilization of staff is greater (lower) during peak (off-peak) workloads, funding may not follow workload cycles. More generally, Basu (1996) and Shapiro (1996) spell out the cyclical utilization of quasi-fixed and fixed inputs.

example, a large literature examines the formation of cartels and the stability of collusive agreements (see Levenstein and Suslow, 2002). But the literature is not conclusive on whether this is more or less likely during economic downturns. If the Antitrust Division is vigorously pursuing court cases, CASES, then potential violations may be lower due to the greater likelihood of detection and prosecution; $b_5 < 0$. The error term is assumed to be iid: $u_t \sim (0, \sigma_e^2)$.

Using equations (4), (3) and (2), we get:

$$(5) \quad CASES_t = c_0 + \sum_{j=1}^{2} [\alpha_j PRES_{t-j} + \beta_j REG_{t-j} + \delta_j WAVE_{t-j} + \gamma_j ECON_{t-j} + \zeta_j CASES_{t-j}]$$
$$+ \theta FUND_{t-1} + \varepsilon_t,$$

where $\varepsilon_t = (u_t + a_1 e_{t-1})$. Given the structure of equations (4), (3) and (2), apart from FUNDS which has one-lag, all the other variables enter with two-lags. The coefficients in equation (5) are typically combinations of the coefficients in equations (2), (3) and (4). Given our assumptions for u_t and e_t, and assuming zero covariance between u_t and e_t, ε_t is a linear combination of two iid errors: $\varepsilon_t \sim iid(0, \sigma_u^2 + a_1^2 \sigma_e^2)$. The error term ε_t is similar to a MA(1) process. Next, some additional comments regarding the specification: (1) As noted earlier, we do not include current period variables in either equations (3) or (4). This is because it is reasonable to assume that various effects take time (see footnote 11); (2) In equation (5), the variables typically contain two annual lags. If we consider deeper lags in equations (3) and (4), then equation (5) retains the same form but will include additional lags and the error term ε_t could be higher-order MA(.) process. Our experiments showed that deeper lags of the included variables were not significant. In section 7, we formally test for higher-order MA(.) process; and (3) As noted in the introduction, two variables in equation (5) may potentially be endogenous: the U.S. merger wave (WAVE) and the Antitrust Division's funding (FUNDS). In section 6, we conduct econometric tests to evaluate this.

4. The Data

Our data are annual over 1958-2002. Data on disaggregated court cases were available starting 1958. Data on antitrust court cases and funding are from the Antitrust Division workload statistics. Data on the total number of mergers in the US are from the Federal Trade Commission merger series (1958-1977) and Thompson's Financials M&As (1978-2002). Data on the Republican v. Democratic nominated Supreme Court justices are from the U.S. Supreme Court archives.

We examine antitrust enforcement data at different levels of disaggregation. A natural pyramid is as follows. First is the total number of antitrust court cases filed in **Figure 1(a)**; these data show intertemporal fluctuations but no obvious trend. Second, the total cases are disaggregated into the criminal and civil components in **Figure 1(b)** and **Figure 1(c)**. The criminal component includes *per se* violations such as price-fixing. The civil component includes mergers, monopolization and restraints of trade cases. Our motivation for disaggregating the total cases was outlined in the introduction. The data in figures 1(b) and 1(c) reveal longer-run drifts with criminal cases showing a marked increase around the late-1970s and early-1980s and civil cases showing a decline after the mid-1970s. In the latter part of the sample, the enforcement data reveal a stark compositional shift with a greater proportion of criminal cases. We return to this issue when we discuss our empirical results in section 8. Third, the total civil cases are next disaggregated into the main Acts via which they operate: Clayton Act section 7 (merger), Sherman Act section 1 (restraints of trade) and Sherman Act section 2 (monopolization). Apart from data on the absolute number of merger court cases filed in **Figure 2(a)**, we also look at the <u>ratio</u> of merger court cases filed to the total number of mergers in the US in **Figure 2(c)**. Examining the ratio is meaningful as a merger wave in the US may naturally result in an absolute increase in the number of mergers challenged. To get an idea of, for example, whether the intensity of merger enforcement varies across political regimes, it is important to deflate the total number of mergers challenged by the total number of mergers in the US. The absolute number of mergers (figure 2a) show a decline in the early-1970s, remain low through the early-1990s and then show an upward movement later in the 1990s. In contrast, the ratio of merger

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cases filed to the total US mergers (figure 2c) shows a sharp decline around the mid-1970s with no meaningful subsequent increase. **Figures 3(a)** and **3(b)** plot the Sherman 1 and Sherman 2 cases filed; the former shows a decline starting early 1980s and the latter sometime in the 1970s. Even a cursory look at Figures 1-3 show significant differences in the intertemporal path of the different types of cases, highlighting the perils of aggregation. Further, the data reveal distinct shifts in levels, indicative of structural-breaks. **Figure 4** displays the Antitrust Division's real (deflated by the GDP deflator) dollar funding. The level of funding generally increases till the late-1970s, declines over the next decade before increasing starting the late-1980s. Funding (figure 4) declines during the very period when criminal prosecutions increase (figure 1b), whereas the civil cases (figure 1c) and funding show similar movements during the late-1970s and through the 1980s. Finally, we present the political variables. **Figure 5(a)** displays data on the party of the President. Over our sample period the US has had Republican Presidents for 24 years and Democrat for 20 years. In ancillary specifications we include the Republican v. Democrat composition of the House and the Senate due to their potential influence on the funding and other decisions.[14] The data in **Figure 5(b)** show that Republicans have had majority in the Senate for 13 years and in the House for only 8 years.

5. Detecting Regime Shift

In the introduction we discussed the potential reasons for expecting a regime-shift in enforcement and several of the antitrust court case variables in section 4 revealed discernable shift in their levels, indicative of structural-breaks. The precise break-date is unknown and we do not have an *a priori* dummy variable to separate the before and after periods. The literature shows that if the data contain a break-point and this is not known a priori, conventional hypothesis testing is not valid (Banerjee, Lumsdaine and Stock, 1992). Given this, we use econometric techniques on detecting structural-

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[14] The Antitrust Division regularly briefs the House and Senate on its activities. If the legislative bodies disagree with the Division's activities, they can reduce the level of funding allocated to the Division. For funding battles, see the debates during the Microsoft trial when Senator Gorton from the State of Washington, where Microsoft is headquartered, and other Microsoft supporters lobbied to cut the Division's funding ("Microsoft Targets Antitrust Division's Budget: Software Giant Urges Congress to Cut Millions from Clinton's Proposal," *Washington Post*, October 1999).

break at an unknown date; Andrews (1993), Hamilton (1994, Ch.22), Stock (1994) and Stock and Watson (1996, 2003) detail this methodology. Let τ the hypothesized break-date for the data on $CASES_t$. The dummy variable $D(\tau)$ is defined as $D_t(\tau)=0$ for $t\#\tau$ and $D_t(\tau)=1$ for $t>\tau$. An autoregressive specification with an unknown break point τ is given by (H_0: $\beta=0$):

$$(6) \quad CASES_t = c_0 + \sum_{k=1}^{n} \zeta_k CASES_{t-k} + \beta D_t(\tau) + \omega_t.$$

It is useful to clarify what an identified structural-break date means. Let data on $CASES_t$ be over $t=0,...,T$ and the statistical tests reveal a break-date in the year τ. In principle, nothing may have changed in the year τ itself, but the sequence of events over $t=\tau+1,...,T$ result in a sample mean for CASES that is different from $t=0,...,\tau$. Regarding estimation, since the break-date is unknown, we consider a series of dates between two potential dates τ_0 and τ_1. For each possible break-date, we estimate equation (6) and get a F-statistic from testing H_0: $\beta=0$. Since we test between τ_0 and τ_1, we get a sequence of F-statistics and use the Quandt Likelihood Ratio (QLR) statistic to detect the break point. The distribution of the QLR statistic depends on (1) the number of restrictions being tested **q** (q=1 in equation 6) and (2) the width of the end-points τ_0/T and τ_1/T, where T is the total sample size (Stock and Watson, 1996). For large sample approximations to the distribution of the QLR statistic to be a good one, τ_0 and τ_1 cannot be too close to the sample endpoints. We follow Stock and Watson (1996) and consider 15% trimming; i.e., $\tau_0=0.15T$ and $\tau_1=0.85T$. Given our sample period of 1958-2002, this implies that we test for a potential structural-break over the window 1965 to 1996.[15]

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[15] The literature on structural-breaks provides techniques for estimating multiple break points as well as identifying slowly evolving breaks using markov-switching techniques. We did not pursue these for two reasons. First, we do not have a compelling argument for a second structural-break in enforcement – apart from the Chicago-School inspired change. Some authors have argued about a game-theory driven revolution, but even if this has occurred it is likely to be at the tail-end of our sample period and not suitable for econometrically estimating another break point. Second, we do not use markov-switching techniques as our annual time series of 45-years is too short to employ sophisticated non-linear maximum-likelihood techniques which perform optimally with much longer time series data.

Next we augment equation (6) to include additional explanatory variables as omission of relevant variables may falsely generate a structural-break observation. The general form is given by:

$$(7) \ \text{CASES}_t = c_0 + \sum_{k=1}^{n} \zeta_k \text{CASES}_{t-k} + \beta D_t(\tau) + \mathbf{\Psi' X_t} + \mathbf{\Phi' X_t} D_t(\tau) + \upsilon_t,$$

where $\mathbf{X}$ is a vector of explanatory variables and $\mathbf{\Psi}$ and $\mathbf{\Phi}$ the coefficient vectors. Using equation (7), the null hypothesis is $\beta=\mathbf{\Phi}=0$. From equation (5), the components of $\mathbf{X}$ are PRES, FUND, WAVE and ECON. The regime-shift variable REG is replaced by $D(\tau)$. For the interaction term, we only consider one: $D(\tau)\text{PRES}_{t-1}$, the interaction between the regime-shift and the Presidential variable. Our motivation, in part following our discussion of Faure-Grimaud and Martimort (2003), is to examine possible changes in the enforcement gap over time between Republicans and Democrats. Finally, some econometric issues: (1) for PRES, since the current and lagged values are identical for many years, we only enter one-lag PRES_{t-1}; and (2) several variables appear non-stationary – funding (fig. 4), total number of mergers (fig. 2b) and real GDP. We tested for non-stationarity using the Augmented Dickey-Fuller and Phillips-Perron unit root tests (Enders, 1995, Ch.4). The tests could not reject the null that the variables are difference stationary. Given this, we entered FUND, WAVE and GDP in *first-differences*. The specification we estimate is:[16]

$$(8) \ \text{CASES}_t = c_0 + \alpha_1 \text{PRES}_{t-1} + \beta D_t(\tau) + \zeta D_t(\tau)\text{PRES}_{t-1}$$
$$+ \sum_{j=1}^{2} [\delta_j \Delta \text{WAVE}_{t-j} + \gamma_j \Delta \text{ECON}_{t-j} + \zeta_j \text{CASES}_{t-j}] + \theta \Delta \text{FUND}_{t-1} + \varepsilon_t.$$

We implemented the following sequence for estimation: (a) first we estimated equation (8). Then we dropped all statistically insignificant second-lags. Insignificant first-lags were not dropped;

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[16] The lag structure of WAVE, ECON, CASES and FUND is same as in equation (5). Our experiments with longer lags showed they were almost uniformly insignificant and did not alter our estimation results and inferences. We return to this when we present our empirical results in section 6.

and (b) re-estimated equation (8) with the optimal lag lengths. We estimate both equations (6) and (8) to detect structural-breaks. **Table 1** summarizes the results for each type of case using $D(\tau)$ ranging from 1965 to 1996. The estimated structural-break dates are basically the same across the specifications. For the Clayton 7 (merger) cases, equation (8) reveals a break-date in 1974 whereas equation (6) in 1973. Given the marginal difference and that (8) is a more complete specification, we use 1974 as the break-date. The solid vertical black lines in figures 1(b), 1(c), 2(a), 2(c), 3(a) and 3(b) show the estimated structural-break dates. In section 8, where we interpret and discuss our empirical results, we comment on the implications of the estimated structural-break dates.

6. Potential Endogeneity

First, the Antitrust Division has to request funds, FUND, from the legislature. Requests for increase in FUND may follow increase in CASES, potentially making FUND endogenous to CASES. However, CASES is not the only factor influencing FUND; the complexity of cases, number of internal investigations, party of the President, composition of the House and the Senate, among other factors determine FUND. In addition, utilization of economists and attorneys (whose salaries are the most significant component of the Antitrust Division's annual budget) vary considerably over work cycles. As noted in footnote 13, this variation in utilization poses a problem of clearly linking investigations and CASES to FUNDS. Second, increase in CASES may dampen merging spirits, potentially making the merger wave, WAVE, endogenous to CASES (merger challenges in particular). Of course, CASES is not the only factor affecting WAVE. Shifts in technology, stock market movements, deregulation, among other factors determine WAVE; see Jovanovic and Rousseau (2001). Further, while only lagged values of FUND and WAVE enter equation (8), the problem may be exacerbated as the error term is MA(.).

We use the Granger (1969) and Geweke, Meese and Dent (1983) procedures to test for (econometric) causality (or joint-determination) by using information about the dynamic

relationships between pairs of variables.[17] Both test whether FUND and WAVE are exogenous (or pre-determined) in a bivariate relationship with CASES. The Granger test examines whether lagged-values of CASES affect current values of FUND and WAVE. The Geweke et al. test examines whether lead-values of FUND and WAVE affect current CASES. Hamilton (1994, p.307-308) provides a discussion of these tests. For FUND we conducted the following tests. For the Granger test we regressed FUND on its own two lags and two lags of CASES: we regressed FUND on the total number of cases as well as total civil and total criminal cases. For the Geweke et al. test, we regressed CASES on two lags of FUND, two leads of FUND and two lags of CASES; as before, we used data on total number of cases and total civil and total criminal. For the U.S. merger wave, we replaced FUND with WAVE in the above equations. For WAVE, we report results for total civil cases and total merger cases (Clayton 7) as these are the most relevant; criminal court cases are not expected to be a factor in affecting U.S. merger waves. **Table 2** presents the estimation details. Given the F-statistics and significance levels, both FUND and WAVE are best treated as pre-determined. It does not appear that, over our sample period, merger waves are endogenous to the Division's merger challenges. Factors such as technological change, stock market movements and deregulation are the likely primary drivers of mergers. Neither is the Division's level of funding jointly-determined with the number of court cases, implying that case complexity, partisan politics, among others are the key drivers. Given these results, we do not pursue instrumental variable estimation. OLS estimates corrected for the MA(.) error structure will provide us with unbiased and consistent parameter estimates and standard errors using the Newey-West (1987) procedure.

7. Estimation Results

Table 4 presents the main results and several checks for robustness appear in **Tables 5-7**. The

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[17] A Hausman test is an alternative strategy but finding instrumental variables, for the Division's funding and the U.S. merger wave, that would be highly correlated with the variables but orthogonal to the regression error is challenging. We experimented with federal government spending and lagged cases as potential instruments for funding, but the explanatory power of the first-stage regressions were very low indicating that these were poor instruments. Given this we did not pursue this option.

interpretation of results and implications are postponed till section 8. In the estimated equation the $D(\tau)$ dummies are replaced by the structural-break dates (table 1). As noted in section 5, only one-lag of PRES is included and statistically insignificant second-lags of the other variables were dropped. The error-structure is MA(1) (equation 5). To check whether a higher-order MA(.) representation is needed – as would result from a richer lagged structure of equations (3) and (4) – we conducted Lagrange-Multiplier tests. The test statistics in table 4 show that for total civil, Sherman 1 and Sherman 2 cases we require MA(2), and MA(1) for total criminal, Clayton 7 and Ratio:Clayton 7. Finally, except for the Constant, $PRES_{t-1}$, $D(\tau)$ and $D(\tau)PRES_{t-1}$, the reported numbers are the **coefficient estimates multiplied by one-standard-deviation** (one-s.d.) of the variable. Given the considerable variation in the size of the estimated coefficients and means and standard deviations of the variables, the estimates multiplied by one-s.d. gives us a ready look at the quantitative effect.

7.1. Main Results

First we look at the results in **Table 4** for total criminal cases (column A) and total civil cases (column B). For the <u>criminal cases prosecuted</u>, the regime-shift dummy indicates a sharp increase in the level from about 24 to 56 court cases per year. The PRES coefficient is insignificant indicating no pre-regime-shift difference between Republicans and Democrats in their propensity to initiate criminal cases. The $D(\tau)$PRES interaction term, however, shows that post-regime shift the Republicans, on average, have initiated about 17 more cases per year than Democrats. In terms of the other variables, the merger wave has no impact on criminal cases. A one-s.d. decrease in real GDP, with a lag of about two years, results in an increase of about 6 criminal cases. The estimate is highly significant and the quantitative effect is economically meaningful: criminal cases prosecuted appear to be countercyclical. Greater funding, has no effect on the number of criminal cases prosecuted. Next we look at the estimates for the total <u>civil cases initiated</u> in column B. The regime-shift dummy reveals a large drop in the level from about 13 to 4 cases per year. Similar to criminal cases, the quantitative effect here is large. The PRES coefficient indicates that pre-regime shift, Republicans

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initiated about 11 more cases per year than Democrats. The D(τ)PRES interaction term, however, is negative and large indicating that post-regime shift the Republicans have initiated about 10 fewer civil cases per year. There appears to be a sharp reversal of the political effect before and after the regime-shift. As we discuss below, the pre-regime-shift Republican effect is primarily being driven by an increase in civil cases for about two years during the Nixon Presidency. In terms of the other variables, total civil cases increase by about 2-3 following a one-s.d. increase in mergers in the US. A similar result is obtained for the Division's funding. Finally, there appears to be no significant relationship between economic conditions and civil cases. A key point to note from columns A and B is that, post-regime-shift, there is a clear *compositional change* in US antitrust enforcement: criminal cases increase, civil cases decrease, and within the post-regime-shift period, Republicans initiate more (less) criminal (civil) cases than the Democrats. We discuss this result in detail in section 8.

The total criminal cases and civil cases are next aggregated to get the grand total number of court cases. Starting with Posner (1970), many studies have analyzed the total number of court cases (see section 2). Since the total cases data do not show a structural-break (table 1), we do not include the D(τ) and D(τ)PRES variables. Equation (9) – similar in specification to table 4, col. A and B – presents the total cases regression (*p-values* are in parentheses).

$$(9)\ \text{CASES}_t = \underset{(0.001)}{34.02} + \underset{(0.112)}{8.23}\,\text{PRES}_{t-1} + \underset{(0.013)}{5.64}\,\Delta\text{WAVE}_{t-1} + \underset{(0.554)}{0.91}\,\Delta\text{FUND}_{t-1}$$

$$\underset{(0.351)}{-\,2.17}\,\Delta\text{GDP}_{t-1} - \underset{(0.182)}{2.74}\,\Delta\text{GDP}_{t-2} + \underset{(0.001)}{9.74}\,\text{CASES}_{t-1} \qquad \overline{R}^2 = 0.38$$

The PRES variable is positive and insignificant at conventional levels indicating that in the total cases data we don't find a clear political effect; this result is reminiscent of several previous studies. Total cases increase following an increase in mergers in the US; this is similar to the result for total civil cases (table 4, col. B). There is no meaningful relationship between funding and the total cases initiated; this result is similar to the total criminal cases (table 4, col. A). And there is a weak negative link between economic conditions and total cases; this result is in the same ballpark as the

total civil cases (table 4, col. B). Finally, the adjusted-R^2 is much lower than those in table 4. The message from comparing the estimates in columns A and B of table 4 with the total cases results in equation (9) above is that examining total cases data masks significant heterogeneity in the underlying components, making inferences less meaningful.

Next we turn to the disaggregation of the total civil cases results presented in columns C through F. For the total Clayton Act 7 (merger) court cases in column C, the regime-shift resulted in a drop from about 10 to 5 cases per year. The PRES variable is insignificant and so is the D(τ)PRES interaction term. These indicate no systematic political effect before or after the regime-shift in terms of the merger cases filed. Similar results were obtained using the ratio (of merger cases filed to total number of mergers in the US) variable (column D). The summary statistics in table 3 indicate that for the sample period as a whole this ratio is 0.0065, implying that less than 1% of the mergers are challenged. The regime-shift resulted in the intercept dropping from about 0.0113 to 0.0036, a large quantitative effect. As in column C, the PRES and D(τ)PRES coefficients are insignificant in column D. The Division's funding appears to have no meaningful impact on the number of merger cases filed. A one-s.d. increase in the number of mergers in the US, with a lag, results in about 2 additional mergers challenged in court. While an increase in GDP appears to lead to an increase in the number of mergers challenged (column C), the intensity of merger enforcement column D) falls with an increase in GDP. In both columns C and D the quantitative effects are very small.

Finally we look at columns E and F, the Sherman Act section 1 (restraints of trade) and Sherman Act section 2 (monopolization) court cases. The D(τ) estimates indicate a significantly lower number of court cases post-regime-shift. For the Sherman 1 cases, the level drops from 23 to 11 cases per year and for Sherman Act 2 cases the drop is from 2.8 to 0.6 cases per year. These represent large quantitative effects in terms of percentage declines. For both Sherman 1 and Sherman 2 cases, the signs on the PRES coefficient are positive indicating that the pre-regime-shift number of cases were higher under Republican Presidents. And the signs on the D(τ)PRES interaction terms are negative signaling that post-regime-shift the court cases are lower under Republican Presidents; the

estimates imply a drop of 4-5 cases per year. The pre-regime-shift effect is similar to the total civil cases results in column B. Pre-regime-shift, the Republicans were in power during the Nixon-Ford administrations (1969-76), and we capture part (1958-60) of the Eisenhower administration. Our examination of the data (figures 1c, 3a and 3b) indicate that the positive pre-regime-shift Republican effect is being driven by outcomes from about two-years during the Nixon administration which saw increases in Sherman Act (sections 1 and 2) cases; so this does not appear to be a pervasive Republican effect, but a more idiosyncratic one from a couple of years of data. Overall, in terms of the regime-shift and political effects, the total civil cases results in column B generally reflect the patterns from columns E and F. In terms of the other variables, a one-s.d. increase in the Division's funding leads to an increase of about one case per year for Sherman 1 and even less for Sherman 2; these are very small quantitative effects. The estimates of the WAVE coefficient show that a one-s.d. increase in mergers in the US show an increase of about 2 Sherman 1 cases, and no impact on Sherman 2 cases. Economic conditions, as measured by changes in GDP, have somewhat mixed and relatively small quantitative effects on the Sherman 1 and 2 cases. The overall picture that emerges is that changes in funding for the Antitrust Division, merger waves and fluctuations in economic conditions appear to have rather small quantitative effects on antitrust court case activity. The quantitative impacts are much larger for the regime-shift and Presidential effects.

Does Controlling for Regime-Shift Matter?

   Table 5 presents results comparable to those in table 4, but without the regime-shift dummy $D(\tau)$ and the $D(\tau)$PRES interaction term. The results are quite different: the adjusted R^2's are lower; not a single PRES coefficient is significant; only one GDP and one FUND coefficient is significant; the WAVE coefficients display somewhat different significance patterns; and many of the lagged CASES coefficients are larger – in part these may be picking up the effects of the omitted regime-shift variable. Overall, controlling for regime-shift appears quite important for detecting the influence of political and other forces on antitrust enforcement.

Does Disaggregation of Cases Matter?

First we disaggregated the grand total number of court cases into total criminal and total civil (columns A and B in table 4). The differences are stark: the regime-shift effects have opposite signs for criminal and civil cases; post-regime-shift, the political effect has opposite signs for criminal and civil cases; change in GDP enters with opposite signs in the criminal and civil cases results; finding is inconsequential for criminal cases but somewhat important for civil. Thus, it is not useful to lump criminal and civil cases into one category – as many previous studies have done. Second, consider the results in columns C through F. The estimated structural-break dates vary across the different types of civil cases. For the regime-shift and political effects, the directional and qualitative effects appear roughly similar for Sherman 1 and Sherman 2 cases but Clayton 7 merger cases follow a different pattern – e.g., the Presidential effects are dramatically different. And funding, which matters for the Sherman 1 and 2 cases, appears not important in merger cases. Thus it is not even useful to lump the different types of civil cases into a total civil category. Overall, aggregation masks heterogeneity and prevents us from getting a clearer look at the relationships.

7.2. Additional Results

Including House and Senate Effects

Appointments to the House and Senate judiciary committees depend on which political party has majority and these may be important influences exerted on the Antitrust Division via the budgetary and related processes. Also, the Assistant Attorney General has to brief the House and Senate on its activities. To examine if this matters, we re-estimated the specification in table 4 after including the percentage of Republicans in the House and in the Senate as additional controls. To conserve space, in **Table 6(a)** we only present results for the total criminal and civil cases. The House and Senate coefficients are all insignificant implying no meaningful effect. (While we do not report these, the adjusted-R^2s drop when we include these variables.) There are marginal differences including the borderline significance of the $D(\tau)PRES$ coefficient in the criminal cases equation, but

given that the House and Senate effects are insignificant it is not clear what this reduced significance means. Overall, the inclusion of the House and Senate variables do not materially affect our broad inferences on regime-shift, the party of the President or the other explanatory variables. This result may not be terribly unexpected given that over our 45-year sample period, the Republicans have been in the majority in the Senate for 13 years and in the House for only 8 years.

Including a Trend

One could argue that the merger cases data (figures 2a and 2c), for example, start high and end up low, implying a negative trend. Similarly, the criminal cases (figure 1b) start low and end high, implying a positive trend. Might controlling for a trend eliminate the regime-shift and political effects we found in table 4? To investigate this, we re-estimated the specifications in table 4 by adding a linear trend. To conserve space, in **Table 6(b)** we only report the total criminal and total civil cases. The trend variable is (hopelessly) insignificant in both specifications. The results for the total criminal cases remains intact. For total civil cases, the signs and the quantitative effects remain intact but the significance levels of the $D(\tau)$ and $D(\tau)$ PRES coefficients drop. However, given the very low significance levels of the trend coefficient – essentially implying a nuisance variable which should be dropped form the regression – the crux of our inferences from table 4 remain intact.

Foreign Competition

Increase in foreign competition is likely to affect at least some areas of enforcement. Merger control is an obvious one where the impact is expected via the product and geographic market definitions. Foreign suppliers will provide competition in cases where products are relatively homogenous. Even when products are differentiated, close substitutes may pose a competitive threat to domestic firms. Ghosal (2002) provides an analysis of actual and potential foreign competition in antitrust. In short, more foreign competition may expand the product and geographic markets reducing the likelihood that merged domestic firms have significant market power and consequently reduced merger

challenges by the Antitrust Division. For price-fixing, over the last 100 years the US has prosecuted a large number of international and domestic cases. An increased number of firms in the market may mean less ability to initiate and sustain collusive agreements. However, the Archer-Daniels Midlands case showed that the entry of foreign suppliers in the market in part lead to the collusive agreement. Thus, for price-fixing violations, the net effect seems uncertain. For restraints-of-trade (Sherman 1) and monopolization (Sherman 2) categories it is not obvious whether increased foreign competition is likely to increase or decrease such violations and the impact on the number of antitrust court cases in these areas. Our only objective here is the check whether our main results from table 4 are affected by including controls for foreign competition. The specific variable we considered is the ratio of US merchandise imports to GDP (since services imports are a non-factor till the tail-end of our sample period, we do not include this). The merchandise import-intensity averaged 3.05% during the 1960s, 6.06% during the 1970s, 8.35% during the 1980s, 9.61% during the 1990s, and just over 10% thereafter. These numbers are not large and it is only in the 1990s does it get above the 10% mark. We re-estimated equation (8) after including the import competition variable. Since our tests revealed that the variable is clearly non-stationary in levels and stationary in first-differences, we entered it in change form like GDP, WAVE and FUNDS. For total civil cases, Sherman 1 and Sherman 2 cases, and the ratio of merger challenges to total US mergers, the import-intensity variable was insignificant. For criminal cases, the second-lag is negative and significant and the quantitative effect shows that a one-s.d. increase in ΔIMS leads to a decline of about 4 criminal cases with a two-year lag. At face value, this suggests that more open markets with larger number of firms lead to less price-fixing and less cases prosecuted. For total merger challenges, the first-lag is negative and significant and implies that a one-s.d. increase in ΔIMS leads to one less merger challenge, a small quantitative effect. **Table 6(c)** reports some of the results. The main point we note is that our central inferences from table 4 regarding regime-shift and political effects remain similar.

Including Longer Lags

One could argue that including longer lags in equations (3) and (4) would result in a richer specification in our estimating equation (8) and might capture instances where information flows occur slowly and investigations are prolonged. To examine this we re-estimated the specifications in table 4 with one additional lag; since our data are annual, this allows for a fairly deep lag structure (including additional lags is tricky as it quickly erodes precious degrees of freedom). None of the additional lags were significant. This is not surprising given that many of the second-lags in table 4 are insignificant. One exception was that the second-lag of ΔFUND appeared with a negative and significant coefficient for criminal cases; but all other coefficients and inferences from table 4 remained intact. This negative coefficient at first glance is a bit puzzling. One likely explanation might be that during the late-1970s and early-1980s when the number of criminal cases increased considerably, the funding for the Antitrust Division dropped sharply (compare figure 1b with figure 4). Given that the other inferences did not change, we did not pursue this further.

Joint Estimation of Total Criminal and Total Civil Cases

Our main results presented in table 4 were from single-equation estimation, implying that the errors are uncorrelated across the different types of antitrust cases. Alternatively, one could think of the problem as one of a "common manager" – the AAG for the Antitrust Division – for the different types of cases and the decisions regarding one type of case are made in conjunction with the other types. This interdependence could arise due to resource constraints (e.g., budget, attorneys and economists) which may force substitution across cases (e.g., between criminal and civil). In this scenario, the errors across the different types of cases may be correlated leading to potentially biased inferences from the single-equation estimates in table 4. Since we do not have a structural model from which we can get cross-equation restrictions to estimate a simultaneous equations system, we instead estimated a Seemingly Unrelated Regressions (SUR) model which allows for error correlation across equations. We estimated a two-equation SUR comprising of the specifications in

columns A and B of table 4. The results in **Table 7** show relatively minor differences in the quantitative effects as compared to single-equation estimates presented in table 4; the qualitative conclusions are the same. Our key inferences remain intact.

8. Discussion of Results

In this section we discuss our empirical findings. Since the regime-shift and political effects are our main focus, we discuss these in greater detail and briefly comment on some of the other findings.

Regime-Shift

U.S. antitrust enforcement is a complex confluence of economic, legal and political forces and significant shifts in enforcement will very likely have one or more of these factors as the key drivers. We discuss these in turn for the civil and criminal cases and reiterate what we noted in section 5 regarding the estimated structural-break date τ: in principle, nothing may change in the year τ itself, but the sequence of events in the years $t=\tau+1,...,T$ result in a sample mean for antitrust court cases that is different from the previous period $t=0,...,\tau$.

For the civil cases (mergers, monopolization and restraints of trade), we noted the role played by Stigler, Williamson, and others, in emphasizing the pro-competitive effects and efficiencies of various types of business conduct. However, given the nature of US enforcement – e.g., to block a merger, the Antitrust Division has to directly challenge the merger in court – unless the lawyers and judges imbibe the new thinking, not much may change in terms of actual shifts in enforcement. That the Chicago School legal scholars launched a powerful assault on the traditional antitrust mindset is evident in the writings of Director, Bork, Posner, Landes, Easterbrook, among others. It is important to note that historically the U.S. Supreme Court's rulings have become landmark judgments (Pate, 2004). Past court decisions can constrain the type of cases the Antitrust Division can threaten to take

to court and also act as a signal to firms as to what might be a legitimate challenge by the Division.[18]

A plausible premise is that a greater fraction of Republican-nominated conservative justices resulted in a more *laissez faire* attitude towards economic activity.[19] To examine changes in the Supreme Court, we collected data on the justices: if they were appointed by a Republican President we assigned a value of '1', else '0'. For a given year we tally up the scores and divide by nine (total for Chief Justice and eight Associate Justices), giving us the fraction of Republican-nominated justices. **Figure 6** shows that the Supreme Court attained a commanding two-thirds Republican-nominated majority in 1972 and have not lost it since then. Overall, one way to interpret the estimated regime-shift dates for the civil cases is that they capture the turning points when the Chicago School's law and economics influences finally began turning the tide.

This, however, does not automatically explain why our estimated regime-shift dates vary across different types of civil cases: 1974 for mergers, 1972 for Sherman 1 and 1981 for Sherman 2 cases. One plausible explanation may relate to the nature of specific cases that came up before the court. We present two such cases that can arguably be identified as turning points:

(1) *U.S. v. General Dynamics Co. (1974)* where Justice Stewart delivered the opinion of the Supreme Court that went against the antitrust mindset of the 1950s and 1960s and did not find a violation even thought the existing market shares were high. The Antitrust Division had defined the product market as "coal". The Court disagreed with this definition and considered the market to be the more

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[18] E.g., courts have sided with the Antitrust Division that physicians cannot collectively bargain to set the fees they get from the insurers: Healthcare Partners Inc., 1995; Health Choice, St. Josephs Physicians Inc. and Heartland Health Systems, 1996. In equilibrium, physicians would have lower propensity to form such coalitions (at least explicit ones). In contrast, the Division lost several hospital merger cases: Mercy Health Services and Finley Tri-States Health Group, 1994; Long Island Jewish Medical Center, 1997. This may embolden hospitals to push more mergers. Legal precedence becomes an important factor.

[19] Referring to the earlier enforcement, Bork (1978, p.4) wrote: *"the Supreme Court, without compulsion by statute, and certainly without adequate explanation, has inhibited or destroyed a broad spectrum of useful business structures and practices."* Later (Bork, 1993, p.x-xi; reprint of the 1978 book) he wrote: *"The decisive cause [of the revolution]...was a change in the composition of the Supreme Court. The [new] justices were not liberal ideologues and they had a better and more sympathetic understanding of the business world...they also had a new...body of antitrust scholarship that made it easier to change the course of the law...the new scholarship is referred to as the Chicago School."*

overarching "energy" which included oil, gas, nuclear and geothermal power. The Antitrust Division had defined the geographic market narrowly. The Court disagreed with the geographic market definition and broadened it considerably arguing that the market area should be defined in terms of the transportation networks and freight charges that determine the cost of delivering coal and other energy. In addition, the Court examined in detail the actual and potential competition and entry conditions in the markets under consideration. This wide ranging evaluation of market conditions was a radical departure from the narrow concentration based mindset of the earlier decades.[20]

(2) *Continental TV v. GTE Sylvania (1977)* where the Supreme Court emphasized concepts related to competition in the market and argued that vertical restrictions are likely to promote interbrand competition by allowing producers to achieve efficiencies in distribution. This is the first time that the Court explicitly noted *efficiencies* to argue in favor of the pro-competitive effects.

These landmark cases roughly correspond to our estimated structural-break dates for the civil cases. A more detailed examination of merger and other civil cases during this transition period along with the specific rulings by the courts would shed greater light on the turning points for the specific types of violations. Finally, some key administrative milestones for merger evaluation are (a) the introduction of the Merger Guidelines in 1968 to streamline the procedures and (b) the 1976 Hart-Scott-Rodino act subsequent to which mergers above a certain valuation threshold had to be filed for clearance. While our estimated 1974 structural-break date for mergers roughly falls in this window, it seems that the shift in intellectual mindset about mergers and other business conduct and landmark court decisions may be the more compelling forces explaining the regime-shift. It does not seem persuasive that these administrative changes were, for example, more crucial than the landmark 1974 General Dynamics ruling by the Supreme Court.

For criminal cases, the explanation is potentially more complex. The estimated structural-

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[20] Von's Grocery (1966) and Brown Shoe (1962) are well known cases of blocked mergers based on a narrow concentration mindset. Another interesting example is Proctor and Gamble (1967) were P&G was prevented from acquiring Clorox due to concerns about competition in the household liquid bleach market. In Ford Motor Co. (1972), Ford's proposed acquisition of Autolyte, a spark plus manufacturer, was given a negative verdict. These serve as examples of the previous regime which was relentlessly attacked by the Chicago School law and economics scholars.

break roughly coincides with violations becoming felonies in the mid-1970s and the 1978 Corporate Leniency Program – which made it more attractive for companies and individuals to co-operate with the Antitrust Division in criminal investigations.[21] But as acknowledged by the Antitrust Division itself (Griffin, 2003, and Kolaski, 2002), the leniency program was not effective till changes were made in the 1990s. Further, since price-fixing is a *per se* violation, there is less of a role to be played by the courts, noting that the precedent was set early when the Supreme Court ruled price-fixing to be illegal.[22] One way to look at the structural-break is as follows. First, even though the corporate leniency program was not very effective when it was first introduced, it's initiation along with the violations becoming felonies reflected a significant policy shift against price-fixing. Second, the late 1970s onwards saw deregulation of major industries like airlines, trucking, railroads, oil, gas, among others. As Viscusi, Vernon and Harrington (1995, p.598-99) note, in an industry with price and entry regulation, there is reduced need for antitrust. Since the expected benefits of deregulation are dependent on firms not conspiring to raise price, there is potentially a greater role for antitrust in a deregulated industry. Overall, the greater emphasis towards criminal enforcement which is consistent with the change in intellectual thinking about the role of antitrust and criminal prosecutions along the lines postulated by Stigler, Bork, Posner, and others, and the deregulation of major industries, offers a probable explanation for our estimated structural-break date.

Political Effect

Post-regime-shift, we found that the number of criminal (civil) cases initiated by Republicans was generally higher (lower) than Democrats. One way to interpret this is that following the new beliefs regarding the central purpose of antitrust, the Republican administrations have potentially

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[21] Griffin (2003), Harrington (2006), Hunton and Williams (2003), Kobayashi (2001), Kolaski (2002), Motta (2004, p.192-194) and Paul (2000) note the various facets and effectiveness of the program.

[22] See the Trans-Missouri Freight Association (1897), Addyston Pipe (1898) and American Tobacco (1907) cases. Later, the Supreme Court ruling in Socony Vacuum Oil Co. (1940) was key as the rule against price-fixing became part of the law on criminal conspiracy. Presenting economic evidence on the impact became less important than the evidence of the actual agreement (see Posner, 1976).

taken this message to heart. Criminal enforcement was ratcheted up during the Reagan administration and the upswing continued into the Bush(Sr.) administration. Since then the number of prosecutions has tapered off (figure 1b), but there were dramatic increases in high profile cases and criminal fines during the Clinton administration. While prosecutions under the Bush(Jr.) administration have dropped off, it is not clear whether this is due to lack of enforcement or whether the deterrence effect is working.[23] President Bush commented (Financial Times, Feb. 17, 2000): *"My own personal view...is [that]...antitrust law needs to be applied where there are clear cases of price-fixing,"* and went on to suggest that antitrust should primarily focus on price-fixing. At least at face value, it does not appear that this administration is slack on price-fixing.

One interesting aspect of the political effect is that we see Democratic administrations being associated with a smaller number of non-merger civil cases (Sherman 1 and 2) but about the same merger court cases. One plausible explanation is that the range of potential violations under the non-merger civil category is much wider – spanning myriad aspects of vertical relations, restrictive contracts, attempts at monopolization, tying agreements, among others – than under mergers. Due to the strong precedence set by past court cases, it is potentially more difficult to challenge mergers. But the greater variety of non-merger civil cases may allow one to pursue more vigorous enforcement.

Our post-regime-shift result – broadly interpreted as an increase in policy gap between more conservative v. liberal administrations – is echoed in another strand of the literature. Bailey and Chang (2001) compute the conservative and liberal scores for U.S. Presidents. These scores are reported in **Table 8**; negative (positive) score is more liberal (conservative). Let us consider period-1 as covering the Eisenhower to Nixon administrations, and period-2 covering Ford to Bush(Jr.); this demarcation roughly follows our estimated regime-shift dates. A simple way to examine divergence from the Bailey-Chang data would be to: (1) compute the mean Republican and Democratic scores for periods 1 and 2; and (2) examine the difference between the mean Republican and Democratic

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[23] We conducted a quick search of the websites of several law firms regarding the issue of criminal antitrust violations. We found numerous prominent law firms and lawyers openly counseling their clients about the perils of price-fixing; Paul (2000) contains a flavor of this. This is an indication that the deterrence effect might be working. Also, colluders may have become smarter and are harder to detect.

scores for each period. This difference is 2.24 for period-1 and 2.89 for period-2, signaling that the gap between the conservative and liberal stances have increased. This is not a proper statistical test, but a quick look. Our increased divergence finding is similar in spirit to those in Bailey and Chang.

Other Results

First, the results on the Antitrust Division's funding are mixed. Civil enforcement in general is positively affected by funding, but the quantitative effects are very small. And there is no impact on criminal prosecutions. One explanation for the relative insignificance of funding might be that the utilization of economists and attorneys can vary over the workload cycle, alleviating the need to increase or decrease staff whose salaries constitute the major portion of the Antitrust Division's budget (also see footnote 13). For criminal cases, following the Socony Vacuum Oil (1940) Supreme Court decision the focus shifted to hard evidence on price-fixing conspiracies as opposed to evaluating economic effects and impact among other factors, it is plausible that criminal investigations in steady state are less dependent on funding and more on hardcore information revealed to the Division by informants and other sources. Second, effectively there is no link between economic activity and civil enforcement; the sign patterns and mixed and where the coefficients are statistically significant, the implied quantitative effects are very small. This result does not provide support for Peltzman's hypotheses where a direct relationship is expected (see section 2). In contrast, criminal prosecutions appear strongly country-cyclical, with the number of prosecutions increasing about two years after an economic downturn. Can this result shed light on collusive practices? Levenstein and Suslow (2004) note that while it is difficult to generalize, cheating and negative external shocks may be important contributors to cartel breakdowns. Baker (1989) concludes that cartels in the U.S. Steel industry were more likely to break down when demand is low. For cartels formed under the Webb-Pomerene Export Trade Act, Dick (1996) finds that stability was greater during periods of steady prices and growing demand. One plausible explanation of our counter-cyclical result is that collusive agreements may tend to break down during economic downturns and

subsequently information flows into the investigative offices of the Antitrust Division resulting in prosecutions. A detailed examination of this result, however, is beyond the scope of this paper. Third, increase in the total number of mergers in the US result in an increase in civil court cases filed, primarily emanating from more mergers challenged and additional Sherman 1 cases. For criminal prosecutions, while the coefficient is positive, it is statistically insignificant, implying that a greater number of mergers to be evaluated does not imply easing off on criminal prosecutions.

9. Final Remarks

We found that the criminal and civil components of US antitrust enforcement underwent a significant regime-shift typically in the mid-to-late-1970s, and the estimated quantitative effects were large. Controlling for this longer-run structural-break in the data emerged crucial in detecting interesting and quantitatively important political effects, post-regime-shift. Controlling for the regime-shift was also critical in sharpening the results on some of the shorter-run driving forces related to merger waves, economic activity and funding. Further, we demonstrated that lumping different categories of civil and criminal antitrust cases into aggregated totals – as many previous studies have done – produce misleading results and diminish our understanding of the broad forces that drive antitrust. For example, post-regime-shift, the political effect and the impact of economic fluctuations on criminal and civil enforcement were very different. Finally, in a *horse-race* between regime-shift and political forces on one hand and the rest of the broad driving forces on the other, the former emerged as clear winners in explaining the intertemporal changes in antitrust enforcement.

In section 2 we highlighted one result in Faure-Grimaud and Martimort (2003): political parties differ in their propensities to extract rent from producers, and with an affiliated-regulator structure (as is the case for the Antitrust Division) there is potential for noticeable shifts in policy. A complex scenario emerged, post-regime-shift. Republicans initiate (1) less monopolization and restraints-of-trade cases, (2) more criminal cases and (3) about the same number of merger cases. In a sense there is both more antitrust and less antitrust. This shift in the link between politics and

antitrust enforcement does not lend itself to easy interpretation in the Faure-Grimaud and Martimort sense of differential propensities to extract rent. If one takes the view that *per se* violations like price-fixing with their clear loss of welfare are to be set aside as a separate category and the Faure-Grimaud and Martimort analysis would apply to the civil enforcement component, then our empirical results appear supportive of their insights.[24] In a broad sense, one way to interpret the regime-shift, the dramatic compositional shift between criminal and civil cases and the post-regime-shift political effect is that the objective may have been to reduce noise in enforcement and increase the *effectiveness* of antitrust by focusing on areas with clearer harm to welfare. Given the dramatic compositional shift between criminal and civil cases, it would be useful to discern the overall impact on the effectiveness of enforcement and welfare. This would require a detailed cost-benefit analysis of enforcement with ambitious data requirements.[25]

We are pursuing a couple of extensions. First, in recent years, both the US and EU have seen emphasis on the prosecution of cartels. Given that criminal violations have occupied center stage, a detailed analysis of the information flows that lead to criminal prosecutions, the investigative process and link between economic activity and criminal violations and prosecutions would be useful. This is being investigated in Ghosal (2005). Second, the US and EU have had differences in competition policy regimes – such as criminal v. administrative law, differences in remedies, seriousness of enforcement efforts and social consequences. In addition, significant changes have taken place in EU competition policy over the last couple of decades. Using data on US and EU enforcement, Caree, Ghosal and Schinkel (2005) examine how the US and EU experiences and changes compare, and the differences and similarities in the underlying driving forces.

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[24] Our results did not show a meaningful difference between Republicans and Democrats pre-regime-shift. We noted in section 6 that the higher number of civil cases under Republicans pre-regime-shift was being driven by a greater number of cases for about two years under the Nixon administration, but aside from these years there was little difference between Republicans and Democrats pre-regime-shift.

[25] See Harrington (2006) for insights on the effectiveness of criminal enforcement.

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Table 1. Estimated structural-break dates		
Variable	Specification: equation (8) Largest F-statistic; τ=year.	Specification: equation (6) Largest F-statistic; τ=year.
1. Total Cases	04.10; τ=1979	06.36; τ=1979
2. Total Criminal Cases	13.22; τ=1979*	11.20; τ=1979*
3. Total Civil Cases	31.35; τ=1972*	16.17; τ=1972*
4. Clayton Act section 7 (merger) Cases	07.45; τ=1974*	08.13; τ=1973*
5. Ratio: (Clayton 7 Cases/Total US Mergers)	14.77; τ=1974*	26.09; τ=1974*
6. Sherman Act section 1 Cases	19.77; τ=1981*	21.81; τ=1981*
7. Sherman Act section 2 Cases	28.91; τ=1972*	16.77; τ=1972*

1. For equation (8) the number of restrictions q=2; and q=1 for equation (6). The F-statistics are compared with critical values of the QLR-statistic with 15% trimming:
(a) for q=2: 1%=7.78, 5%=5.86 and 10%=5.00.
(b) for q=1: 1%=12.16, 5%=8.68 and 10%=7.12.
2. A * indicates a statistically significant break date at least at the 10% level. The total cases data in row 1 do not show a statistically significant structural-break. The cases in rows 2-3 disaggregate the total cases in row 1. Rows 4-7 disaggregate the total civil cases, with rows 4-5 presenting the merger control cases; absolute numbers and the ratio. And rows 6-7 present the Sherman Act section 1 and 2 cases.

Table 2. Econometric causality tests

Panel A. Granger test.
Model: $FUND_t = a + \Sigma_m b_m FUND_{t-m} + \Sigma_n c_n CASES_{t-n} + u_t$, where 'a', 'b' and 'c' are parameters.
Null Hypothesis: $c_n = 0 \ \square \ n$.

1. Funding
Total Cases: F=0.2469 (0.7823)
Total Civil Cases: F=0.3719 (0.6917)
Total Criminal Cases: F=0.4788 (0.6229)

2. Merger Wave (FUND is replaced by WAVE in the above equation)
Total Civil Cases: F=0.0794 (0.9238)
Total Merger Cases: F=0.9345 (0.4018)

Panel B. Geweke, Meese and Dent test.
Model: $CASES_t = d + \Sigma_k p_k FUND_{t-k} + \Sigma_w q_w FUND_{t+w} + \Sigma_g r_g CASES_{t-g} + e_t$, where 'd', 'p', 'q' and 'r' are parameters. The test includes k-lags and w-leads of FUND.
Null Hypothesis: $q_w = 0 \ \square \ w$.

1. Funding
Total Cases: F=0.5603 (0.5760)
Total Civil Cases: F=0.2336 (0.7928)
Total Criminal Cases: F=0.7435 (0.4827)

2. Merger Wave (FUND is replaced by WAVE in the above equation)
Total Civil Cases: F=0.0128 (0.9872)
Total Merger Cases: F=0.3311 (0.7205)

1. The computed F-statistics and *p-values* (in parentheses) are reported above.
2. In Panel A, we estimated the model with lags m and n=1,2. In Panel B, the lags k, w and g=1,2. Including additional lags did not change our inferences.
3. We report the results for the most useful types of cases in each category. For funding we report results for total number of cases, total civil cases and total criminal cases. For mergers, we report the results for total civil cases and total merger (Clayton 7) cases. Similar results were obtained for the other types of cases.

Table 3. Summary statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
1. Total Cases	68.53	18.76	30.00	110.00
2. Total Criminal Cases	40.48	26.45	4.00	96.00
3. Total Civil Cases	28.04	15.13	6.00	72.00
4. Clayton 7 Cases	10.44	5.99	3.00	26.00
5. Ratio: (Clayton 7 Cases/Total US Mergers)	0.0065	0.0057	0.0008	0.0189
6. Sherman 1 Cases	11.42	9.02	0.00	36.00
7. Sherman 2 Cases	3.08	3.58	0.00	15.00
8. ΔWAVE	91.64	581.17	-2187.00	1562.00
9. ΔFUND	2.04	4.15	-6.07	16.02
10. ΔGDP	161.24	114.13	-101.70	372.90
11. President	0.53	0.50	0.00	1.00
12. House	0.41	0.06	0.32	0.53
13. Senate	0.44	0.07	0.35	0.55

A 'Δ' indicates first-difference. The cases in rows 2-3 disaggregate the total cases in row 1. Rows 4-5 present the merger control cases; absolute numbers and the ratio. Rows 6-7 present the Sherman Act section 1 and 2 cases. Row 8 is the change in the total number of mergers in the US and row 9 presents the change in the real dollar funding for the Antitrust Division. Row 10 is the change in real GDP, and rows 12 and 13 are the percentage of Republicans in the House and the Senate.

	Disaggregation of Total Cases		Disaggregation of Civil Cases			
			Merger		Non-merger	
	A. Total Criminal	B. Total Civil	C. Total: Clayton 7	D. Ratio: Clayton 7	E. Sherman 1	F. Sherman 2
Constant	23.260* (0.001)	13.054* (0.001)	10.019* (0.001)	0.0113* (0.001)	23.024* (0.001)	2.822* (0.001)
$PRES_{t-1}$	-1.958 (0.565)	11.422* (0.025)	0.376 (0.884)	0.00029 (0.825)	3.102* (0.031)	3.868* (0.045)
$D(\tau)$	33.034* (0.001)	-8.825* (0.001)	-4.916* (0.011)	-0.0077* (0.001)	-12.486* (0.001)	-2.237* (0.001)
$D(\tau)PRES_{t-1}$	16.894* (0.023)	-10.670* (0.089)	-2.807 (0.367)	-0.00127 (0.451)	-5.265* (0.023)	-3.883* (0.065)
$\Delta WAVE_{t-1}$	2.778 (0.289)	2.382* (0.008)	0.465 (0.360)	-	1.859* (0.001)	0.035 (0.856)
$\Delta WAVE_{t-2}$	-	-	1.859* (0.005)	-	-	-
$\Delta FUND_{t-1}$	0.294 (0.881)	2.623* (0.009)	-0.095 (0.835)	0.0003 (0.240)	1.456* (0.025)	0.269* (0.011)
ΔGDP_{t-1}	-1.186 (0.536)	-0.023 (0.987)	0.912* (0.092)	0.00007 (0.844)	-0.923 (0.336)	-0.079 (0.735)
ΔGDP_{t-2}	-5.821* (0.008)	-	-	-0.00007* (0.013)	-1.710* (0.057)	-
$CASES_{t-1}$	9.019* (0.015)	5.734* (0.001)	1.531* (0.010)	0.001* (0.097)	-2.511* (0.026)	1.443* (0.001)
$CASES_{t-2}$	-5.633* (0.004)	3.343* (0.001)	-	-	-	-
Adjusted-R^2	0.811	0.711	0.535	0.792	0.629	0.703
LM: $\chi^2(2)$	0.247	0.010	0.311	0.242	0.071	0.018

Table 4. Estimation results

Estimated specification: equation (8)

1. The numbers on $\Delta FUND$, $\Delta WAVE$, ΔGDP and lagged-CASES are the **coefficient estimates multiplied by one-standard-deviation** of the respective variable. Numbers for Constant, $PRES_{t-1}$, $D(\tau)$ and $D(\tau)PRES_{t-1}$ are the estimated coefficients.
2. *p-values* (two-tailed), using the Newey-West (1987) heteroscedasticity and serial correlation consistent standard errors, are in parentheses. A * indicates statistical significance at least at the 10% level.
3. The row 'LM: $\chi^2(2)$' presents the *p-values* from the LM-test for a MA(2) error structure. Given the results, the specifications in columns A, C and D were estimated with MA(1) error structure and those in columns B, E and F with a MA(2) structure.
4. Col. D omits WAVE as the dependent variable is the ratio of the total merger cases to the total number of mergers in the US.

Table 5. Estimation excluding regime-shift dummies
The specifications are same as those in table 4 but do not include the regime-shift dummy $D(\tau)$ and $D(\tau)PRES$.

	Disaggregation of Total Cases		Disaggregation of Total Civil Cases			
			Merger		Non-merger	
	A. Total Criminal	B. Total Civil	C. Total: Clayton 7	D. Ratio: Clayton 7	E. Sherman 1	F. Sherman 2
Constant	7.547 (0.278)	4.901 (0.410)	5.791* (0.005)	0.0036* (0.039)	15.255* (0.001)	1.402* (0.074)
$PRES_{t-1}$	3.762 (0.464)	1.261 (0.699)	-2.348 (0.151)	-0.0012 (0.261)	-1.819 (0.430)	0.025 (0.971)
$\spadesuit WAVE_{t-1}$	3.487 (0.106)	2.382* (0.031)	0.639 (0.107)	-	4.706 (0.262)	0.053 (0.793)
$\Delta WAVE_{t-2}$	-	-	1.046 (0.262)	-	-	-
$\Delta FUND_{t-1}$	-0.456 (0.977)	1.652 (0.121)	-0.631 (0.235)	-0.00008 (0.804)	2.125* (0.022)	0.095 (0.613)
ΔGDP_{t-1}	-2.282 (0.355)	-1.642 (0.309)	0.045 (0.952)	0.00015 (0.776)	-1.961 (0.130)	-0.479 (0.114)
ΔGDP_{t-2}	0.570 (0.767)	-	-	-0.0011* (0.027)	-2.918 (0.118)	-
$CASES_{t-1}$	20.075* (0.001)	8.400* (0.001)	3.324* (0.001)	0.004* (0.001)	1.962 (0.223)	2.542* (0.001)
$CASES_{t-2}$	2.301 (0.341)	3.810* (0.024)	-	-	-	-
Adjusted-R^2	0.683	0.558	0.377	0.649	0.263	0.503

p-values are in parentheses. See table 4 for details.

Table 6(a). Estimation including HOUSE and SENATE effects.
The specifications are same as in table 4, augmented by the HOUSE and SENATE variables.
To conserve space, the FUNDS, WAVE, GDP and lagged-CASES estimates are not reported.
Only the total criminal and total civil cases are presented.

	Constant	$PRES_{t-1}$	$D(\tau)$	$D(\tau)PRES_{t-1}$	$HOUSE_{t-1}$	$SENATE_{t-1}$
Total Criminal	26.239* (0.001)	0.068 (0.988)	43.733* (0.001)	8.917 (0.214)	-13.792 (0.176)	3.973 (0.556)
Total Civil	13.492* (0.001)	11.778* (0.022)	-6.726* (0.003)	-12.005* (0.055)	-0.587* (0.828)	-4.361 (0.157)

p-values are in parentheses. The HOUSE and SENATE are dummy variables with HOUSE (or SENATE) = 1 if Republicans are in majority; else zero.

Table 6(b). Estimation including a linear trend
The specification is the same as in table 4, augmented with a linear trend.
To conserve space, the FUNDS, WAVE, GDP and lagged-CASES estimates are not reported.
Only the total criminal and total civil cases are presented.

	Constant	$PRES_{t-1}$	$D(\tau)$	$D(\tau)PRES_{t-1}$	TREND
Total Criminal	25.884* (0.001)	-3.387 (0.556)	35.776* (0.001)	14.313 (0.063)	0.069 (0.860)
Total Civil	13.876* (0.075)	11.433* (0.028)	-8.252 (0.121)	-10.854* (0.100)	-0.028 (0.891)

p-values are in parentheses.

Table 6(c). Estimation including import-penetration effect
The specification is the same as in table 4, augmented with a linear trend.
To conserve space, the FUNDS, WAVE, GDP and lagged-CASES estimates are not reported.

	Constant	$PRES_{t-1}$	$D(\tau)$	$D(\tau)PRES_{t-1}$	ΔIMP_{t-1}	ΔIMP_{t-2}
Total Criminal	24.301* (0.001)	-0.512 (0.905)	34.285* (0.001)	16.233* (0.023)	2.189 (0.223)	-3.852* (0.037)
Total Civil	16.034* (0.001)	16.324* (0.001)	-8.865* (0.001)	-16.481* (0.001)	0.126 (0.910)	1.342 (0.375)
Total: Clayton 7	10.854* (0.001)	2.359 (0.518)	-4.219* (0.080)	-6.008 (0.201)	-1.044* (0.016)	-0.122 (0.827)

p-values are in parentheses. IMP is the ratio of U.S. merchandise imports to GDP.

Table 7. Two-equation seemingly unrelated regressions (SUR) for total civil and total criminal cases.
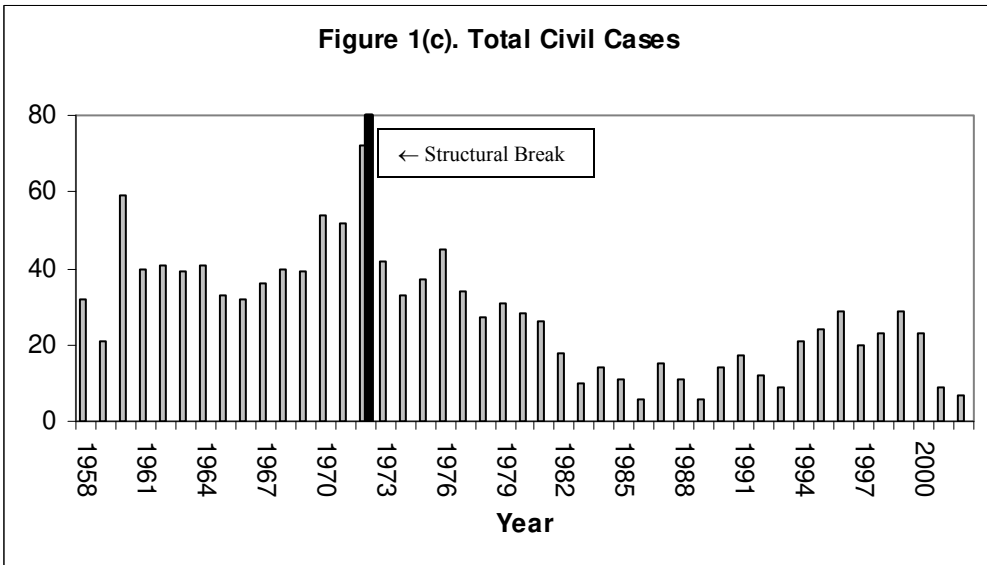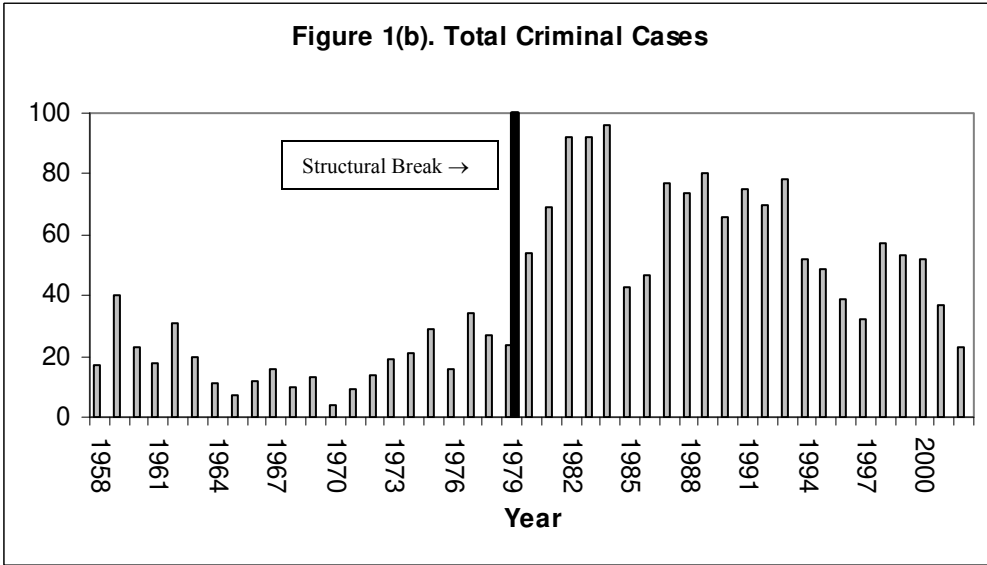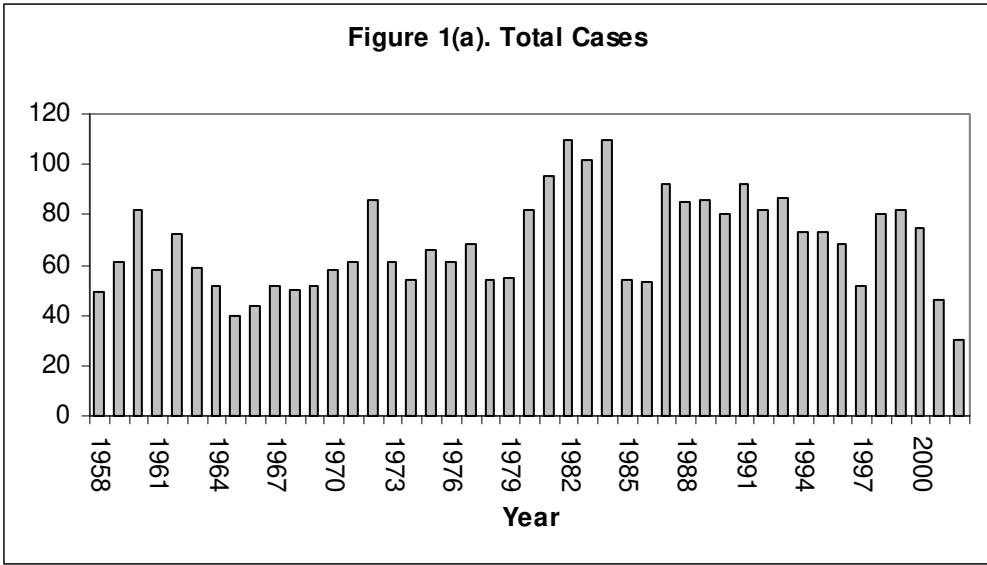Specifications are the same as in table 4, columns A and B.

	A. Total Criminal	B. Total Civil
Constant	23.412* (0.001)	16.683* (0.001)
$PRES_{t-1}$	-2.217 (0.644)	20.412* (0.001)
$D(\tau)$	33.240* (0.001)	-8.215* (0.007)
$D(\tau)PRES_{t-1}$	17.576* (0.028)	-21.041* (0.001)
$\Delta WAVE_{t-1}$	2.789 (0.139)	2.633* (0.006)
$\Delta WAVE_{t-2}$	-	-
$\Delta FUND_{t-1}$	0.342 (0.853)	2.220* (0.029)
ΔGDP_{t-1}	-1.255 (0.531)	-1.186 (0.236)
ΔGDP_{t-2}	-5.820* (0.005)	-
$CASES_{t-1}$	8.745* (0.014)	5.310* (0.001)
$CASES_{t-2}$	-5.581 (0.105)	2.905* (0.025)

p-values are in parentheses.

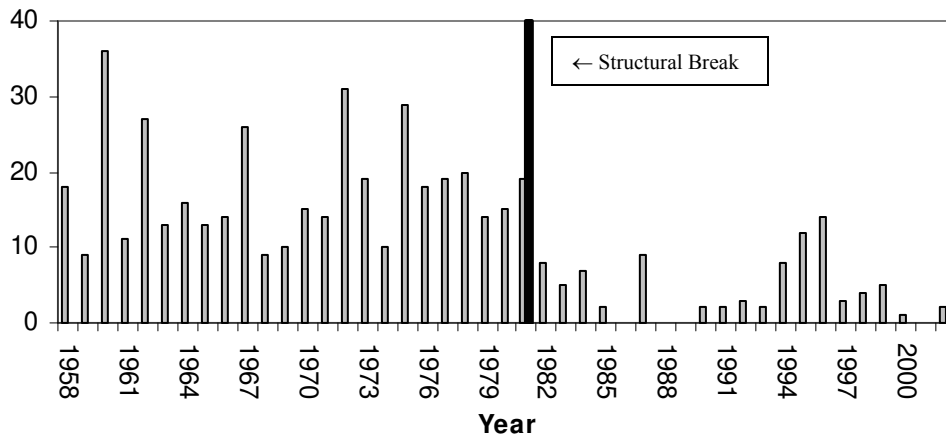| Table 8. Ideological scores	
President	Score
Eisenhower	-0.59
Kennedy	-2.97
Johnson	-2.59
Nixon	-0.49
Ford	-0.32
Carter	-0.99
Reagan	+2.76
Bush(Sr.)	+2.20
Clinton	-1.21
Bush(Jr.)*	+2.48*

1. These data are from Bailey and Chang (2001, Table 2, Column 1).

2. There was no data for Bush (Jr.) and the reported number above is the mean of Reagan and Bush (Sr.). (We believe that the Bush(Jr.) score is not an unrealistic one as much of the political commentary, based on economic and social policies, seems to indicate that Bush(Jr.) may be the most conservative President ever.)

Figure 1(a). Total Cases

Figure 1(b). Total Criminal Cases

Structural Break →

Figure 1(c). Total Civil Cases

← Structural Break

Figure 2(a). Total Merger Cases

Figure 2(b). Total US Mergers

Figure 2(c). Ratio: (Total Merger Cases / Total US Mergers)
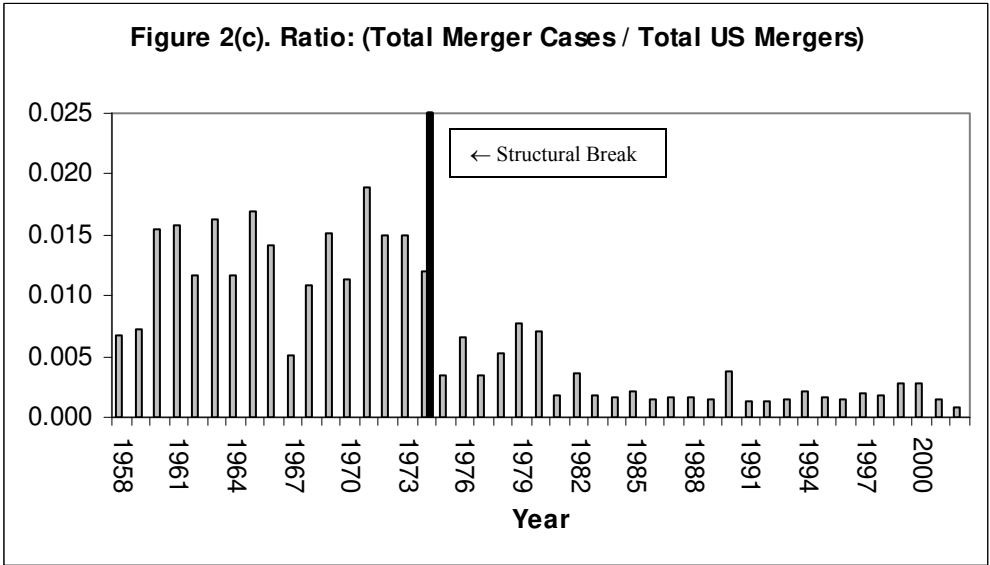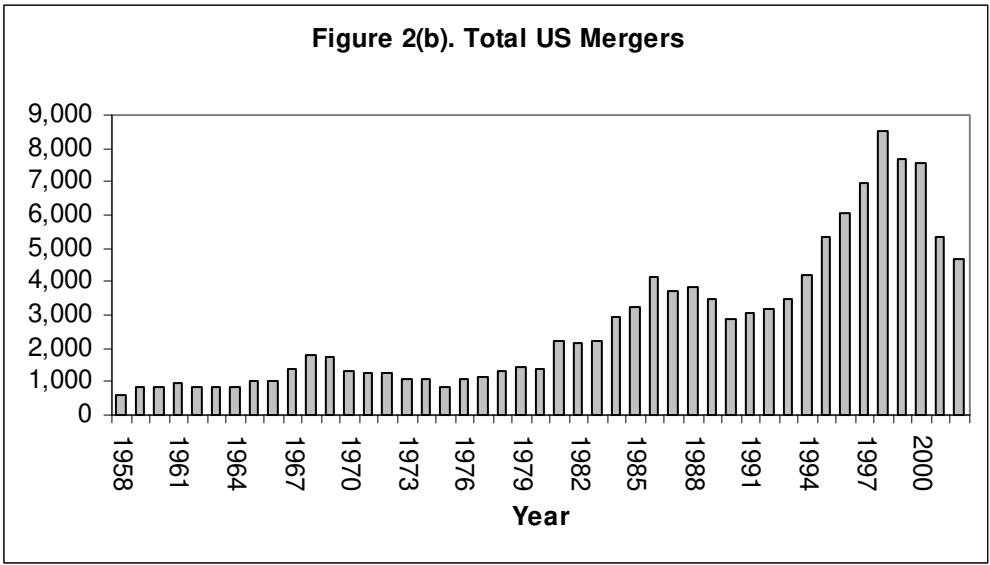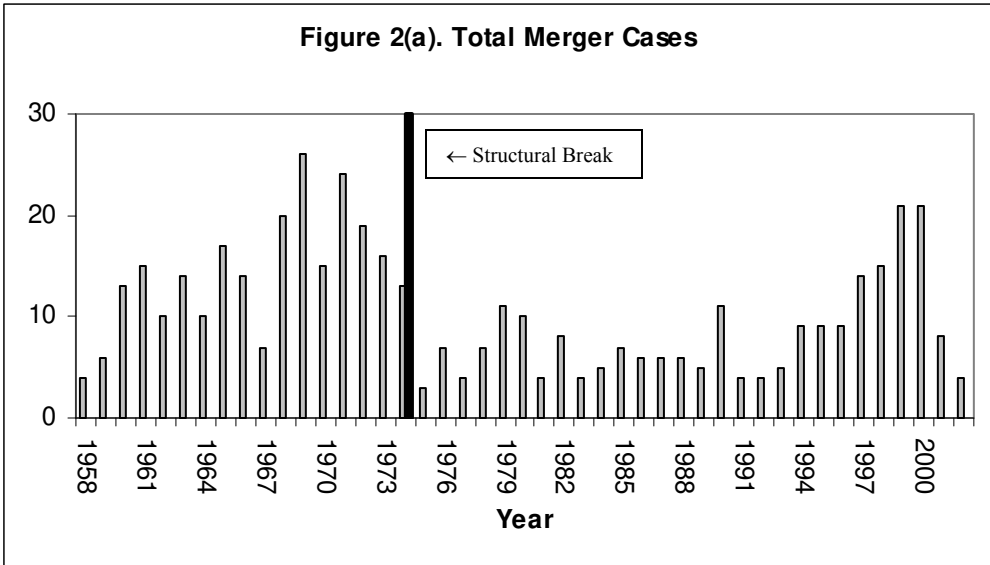
Figure 3(a). Sherman Act 1 (Restraints of Trade) Cases

← Structural Break

Year

Figure 3(b). Sherman Act 2 (Monopolization) Cases

← Structural Break

Year
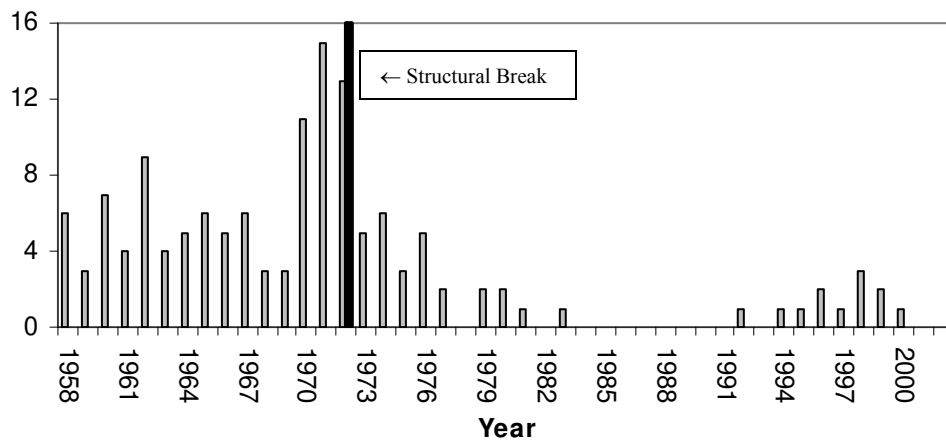
Figure 4. Antitrust Division's Funding

Real Dollars (millions)

Year

49

Figure 5(a). Party of The President

Figure 5(b). Percentage of Republicans in House and Senate

Figure 6. Percentage of Republican-Nominated Supreme Court Justices

50