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Special Interest Theory Reconsidered

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Selected Paper prepared for presentation at the 2015 Agricultural & Applied Economics Association and Western Agricultural Economics Association Annual Meeting, San Francisco, CA, July 26-28.

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1. Introduction

What role is there for the enforcement of ostensibly pro-market laws and regulations when international economic integration extends the boundaries of the market far beyond the borders of any particular jurisdiction? Specifically, do governments reduce, intensify, or in other ways materially change the enforcement of their antitrust laws after they open their markets to foreign competition?[1]

The answer to the latter question has important implications for the normative assessment of antitrust law and enforcement in the context of economic globalization, with immediate implications for public policy. A finding that antitrust laws are (ab)used to protect special interests would support demands to reduce the scope or stringency of antitrust law and cut down resources for its enforcement (see, e.g., Armentano 1999). A finding that antitrust enforcement varies with the changing need for protecting market competition (in line with the pro-market objectives long espoused by most supporters of antitrust law and policy) would support a much more optimistic view of the feasibility and desirability of such regulatory policies in the face of economic globalization (see e.g., Baer and Balto 1999; Baker 2003).

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[1] Antitrust laws prohibit conduct that reduce the ability or incentive of firms to compete with each other or with one or more dominant firms. This includes price-fixing, market-sharing, and other forms of cartels, collusion such as bid-rigging, as well as the (ab)use of market power by a monopolist or the dominant firm(s) in an oligopolistic market, for instance to deter market entry or price wars. U.S. antitrust law also entails mandatory regulatory review of mergers, acquisitions, or joint ventures, authorizing the government to restrict or prohibit such mergers if they substantially increase the concentration of market power in a given industry. For an accessible, concise introduction see Baron (2010). For discussions of the evolution of U.S. antitrust thought and practice, see, e.g., Fear (2008), First, Fox, and Hemli (2013), Fligstein (1990), Freyer (1992), and Gerber (2010:esp. 121-158).

We examine this question focusing on the United States. We begin with a review and replication of the study "Antitrust Enforcement and Foreign Competition" by Shughart, Silverman, and Tollison (1995), hereafter SST. Their work, a prominent application of special interest theory, contains the only previous systematic empirical assessment of the effect of economic integration on U.S. antitrust enforcement. Based on the assumption that government regulators in general—and antitrust enforcement agencies in particular—tend to serve special interests, SST hypothesize that trade openness prompts governments to engage in more and selective enforcement in order to shield inefficient domestic firms from foreign competition.

U.S. antitrust enforcement should be an easy case to find confirmation for SST's claim: During the period analyzed in their (and our) paper, the United States was the most powerful country in the global political economy and therefore should have been most at liberty to enforce antitrust laws selectively for protectionist purposes, unconstrained by the threat of foreign retaliation that might counterbalance the temptation in less powerful countries (Bradford 2007). Consistent with expectations, their time series analyses of U.S. antitrust enforcement budgets seem to support their claim.

We find their analysis both theoretically and empirically deficient. SST portray their analysis as exemplary positive social science that is empirically grounded in rigorous statistical analysis and provides a much-needed corrective to previous normative work on antitrust. Their argument is well-known, and their (interpretation of their) empirical finding have been widely cited—often as fact—in general works on antitrust law and policy (e.g., Crampton and Boudreaux 2003; Dabbah 2001:214; 2010; Sokol 2010:1087), as well as articles and chapters on trade and antitrust (e.g., Aubert and Rabassa 2001; Belokurova 2004:41f; de León 1997; Rugman and Verbeke 2005:120) and on antitrust under economic globalization (e.g., Aubert 2003; Bitetti 2013:81; Eickhof and Isele 2005:19; Evenett, Lehmann, and Steil 2000; Holmes, Kempton, and McGowan 1996; Kronthaler 2007, 2010; Kronthaler and Stephan 2007:145); general works on international and comparative law (e.g., Guzman 2002:903), and broader works on public policy and markets (e.g., Baradaran-Robison, Scharffs, and Sewell 2005; López 2003).[2] Upon closer scrutiny, however, we find their analysis theoretically deficient and empirically based on questionable data, unsuitable statistical methods (with results that cannot be replicated), with a tendentious interpretation of the statistical findings. Our replication suggests that the authors let their prior normative commitments bias their conclusions.

To advance the understanding of antitrust enforcement in the context of increased foreign competition, we go well beyond critique and replication. Theoretically, we develop an alternative argument about the consequences of increased foreign competition for U.S. antitrust enforcement, grounded in the public interest approach to regulation. Specifically, we allow for the possibility that antitrust law enforcement indeed seeks to protect market competition—however imperfectly. This line of reasoning leads a very different understanding of the relationship between antitrust enforcement and foreign competition. Empirically, we conduct improved time series statistical analyses of the enforcement resources allocated to the Department of Justice's Antitrust Division, covering more than eighty years and differentiating between import penetration, net imports, and total trade (exports + imports). These analyses yield a far more nuanced picture of the changes in U.S. antitrust enforcement in response to economic globalization.

[2] We also have found citations to SST's 1995 paper in several foreign languages, which we could not assess.

Our analyses suggest that the U.S. government has generally been more concerned with maintaining the regulatory effectiveness of U.S. antitrust regulators in the context of the global integration of markets (broadly consistent with a sophisticated public interest perspective) than with selective enforcement of antitrust law for protectionist purposes (as expected by a special interest approach). Looking beyond the realm of U.S. antitrust enforcement, our research also speaks to a long-standing debate over the relative importance of market failure and government failure, and has implications for scholarly debates across the social sciences—which we discuss in the conclusion.

2. Theory: The Effect of Economic Openness on Antitrust Enforcement

The traditional major schools of U.S. antitrust thought all rely more or less explicitly on the assumption of a closed economy, where the boundaries of the market coincide with the law's jurisdictional reach. Scholars interested in a theoretical understanding of the effect of economic openness on antitrust therefore have to turn elsewhere.

2.1. Shughart, Silverman, and Tollison's Special Interest Theory of Antitrust Enforcement and Foreign Competition

So when SST sought to explain how U.S. antitrust enforcement responds to increased foreign competition in U.S. product markets, they turned to public choice theory—a broad school of thought that has been very influential, shaping scholarship in law, economics, and political science for decades—and more specifically the special interest theory of regulation.[3] This approach has its origins in the early work of George Stigler, especially his "Theory of Economic Regulation" (1971). Stigler showed that, given the possibility that regulators would be "captured" by special interests (which was likely if they were narrowly self-interested individuals, as commonly assumed of individual actors in neoclassical economic theory), it was highly unlikely that regulation would serve some general public interest and much more likely that it would serve the interests of a narrow subset. "[A]s a rule, regulation is acquired by the industry and is designed and operated primarily for its benefit," Stigler concludes (1971:3).

Peltzman (1976) developed Stigler's core ideas into a formal theory of regulation as a means through which political leaders redistribute the costs and benefits of economic activity to maximize their own likelihood of remaining in power. Assuming minimalist democratic institutions, Peltzman focused on vote shares; later literature has focused on campaign contributions, which may make it politically optimal to serve the interests of an even narrower subset of the public.

The Stigler-Peltzman framework has informed a large amount of research on government regulation. Their approach forms the core of many models that yield the theoretical conclusion (i.e., the hypothesis) that government intervention is unlikely or inherently unable to address market failures in the public interest—even or maybe especially in democracies. It also inspired, or at least provided the theoretical underpinning for, a line of research about U.S. antitrust law

[3] As briefly discussed in the conclusion, the public choice tradition is much broader than special interest theory, and even though the authors discussed here maintain that their work represents *the* public choice approach to antitrust law and policy, a range of assumptions about the intent and preferences of antitrust regulators and the elected officials who allocate them resources for antitrust enforcement can in principle be accommodated within a public choice approach.

and policy by a group of economists, including Shughart and Tollison, most of whom first came together as political appointees in the FTC under President Reagan, and who claimed for themselves the mantle of public choice theory. Viewing even the antitrust scholars of the Chicago School as naïve and "pious" for assuming that antitrust enforcement can advance public interests, such as consumer welfare, they criticized previous antitrust scholarship for its misguided normative pre-commitment to having and enforcing antitrust laws (e.g., McChesney and Larkin-Wong 2013:291). Disclaiming any normative motivations, they (cl)aimed to show through strictly positive and rigorous empirical research that antitrust generally fails to advance any kind of public interest and instead overwhelmingly tends to serve special interests. Their allegedly positive research of course started from the highly normative assumption that public officials—including antitrust regulators and the politicians who oversee them (indirectly) through appointment and the power of the purse—should be theoretically modeled to be only narrowly self-interested (Shughart 1990).

SST's operationalization of special interest theory focuses on the allocation of budgetary resources for the two U.S. antitrust enforcement agencies that share the antitrust enforcement function in the United States: the Department of Justice's Antitrust Divisions (DoJ AtD) and the Federal Trade Commission (FTC). The U.S. Congress ultimately decides the budget allocation for these enforcement agencies each year, based on a budget request by the President. SST argue that, if policymakers intended antitrust to serve the public interest, then an increase in the exposure of U.S. manufacturers to foreign competition should reduce antitrust budgets, because foreign competition should under these conditions already constrain domestic anti-competitive activity.[4] Conversely, antitrust budgets should grow in the periods of increased trade protectionism as public interest minded governments seek to offset the welfare-reducing effects of tariffs and various non-tariff barriers. In other words, a public interest perspective suggests a negative correlation between the exposure of U.S. firms to foreign competition and the budgets allocated to the antitrust enforcement agencies. In contrast, if antitrust "is not intendedly pro-competitive, but instead serves as means by which some firms […] can obtain protection from the forces of effective competition," as SST assume (Shughart 1995:180), the correlation between the degree of foreign competition and the antitrust budget allocation should be positive.

The logic of this argument starts from the assumption that small firms are generally less competitive than large firms in the same industry, which results in the expectation that "import competition tends to come at the expense of small domestic firms" (Shughart, Silverman, and Tollison 1995:180). SST do not model the political process, but it is apparent that they expect those small firms to respond to the threat from increased foreign competition by clamoring for the government to protect them. AndGovernment are expect to do so through "more active antitrust enforcement" vis-à-vis "larger domestic firms in order to shift some of the costs associated with import competition away from their smaller rivals" (1995:181), consistent with the Peltzman model of regulation as a mechanism for redistribution. The antitrust regulator might for instance issue "complaints of unlawful predatory pricing" against "large firms that attempt to hold on to their market shares by cutting price." Or it might unduly challenge "attempts by large firms to consolidate their assets and become more efficient through merger" (Shughart, Silverman, and Tollison 1995:181).

[4] Based on a long tradition in trade economics, which is beyond the scope of this paper, SST explicitly assume that low "trade barriers …provides an effective margin of market discipline for domestic monopolists" (Shughart, Silverman, and Tollison 1995:180). For a critique of this reasoning, see Büthe (2014).

SST focus on import penetration, operationalized as the total value of the imports of durable and nondurable goods as a percentage of gross national product (GNP), as the key measure of the effective level of foreign competition experienced by domestic firms. This leads them to their main operationalized hypothesis:

H_1: The greater the level of imports as share of the national economy, the greater the size of the budget of the enforcement agencies.

SST focus solely on imports. But if the argument is correct for firms and industries experiencing increased foreign competition through imports, then firms experiencing increased foreign opportunities and profits through exports should have the opposite policy preference and might be expected to oppose such a protectionist use of antitrust, as Milner (1988) has shown for trade protectionism generally.[5] An alternative, better version of SST's main hypothesis might therefore be:

H_{1a}: The greater the level of net imports as share of the national economy, the greater the size of the budget of the enforcement agencies.

Critique

The theory offered by SST is surprisingly informal and in some key places lacks tight logical derivation. For example, the argument that import competition comes at the expense of less efficient domestic firms (surely correct and well grounded in standard economic theory) morphs into a claim that import competition comes at the expense of smaller firms. No theoretical rationale is provided for equating small firms with inefficient firms. It also is unclear why the presumed-to-be-efficient larger firms have not already eliminated their presumed-to-be-inefficient small domestic counterparts already before trade liberalization. The paper ignores these theoretical problems and simply references two empirical studies that found small firms to suffer more than large firms from an increase in trade openness. Similarly, it seems as a deductive matter puzzling why SST do not hypothesize that antitrust enforcement will target the foreign firms that are responsible for the increased imports into the United States. Given their assumption that antitrust can and will be selectively enforced for political gain of the elected politicians who can influence the enforcement agencies' budget, such an enforcement strategy would seem much better suited to the objective of protecting less efficient domestic firms.[6] Even if such enforcement actions were ultimately found to be without any merit, defending themselves against accusations of predatory pricing etc. would raise foreign firms' cost of doing business in the United States, reducing their incentive to lower prices below previous domestic equilibrium levels.[7]

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[5] The most immediate incentive for exporters to oppose protectionist policies is to want to avoid retaliation abroad. They may also want to establish a general commitment to free trade and therefore oppose protectionist policies categorically.

[6] Such a use of antitrust would seem more directly consistent with SST's core assumptions about how antitrust works, especially since they explicitly suggest that antitrust obfuscates protectionism sufficiently that retaliation from U.S. trading partners is unlikely (the threat of which might otherwise keep such a protectionist abuse of antitrust in check). For an excellent discussion of why democratic governments might generally find such hidden trade protectionism attractive, see Kono (2006), who, however, did not consider whether governments might do so through the selective enforcement of antitrust law.

[7] A proper test of such a hypothesis would suggest an analysis at the level of firms (conditional on the degree of import penetration or net imports at the industry level) and hence would require data about the nationality of the

In addition to these weaknesses, the theory suffers from two major flaws. First, it fails to generate unique empirically observable implications. SST assert that a positive correlation between the level of imports and enforcement budget allocations "cannot be explained by the public-interest model which asserts that antitrust is driven by the goal of protecting the interests of domestic consumers" (1995:187). But this claim does not hold: Even if U.S. antitrust enforcement were only driven by the goal of protecting the interests of domestic consumers, economic integration complicates this task. When those consumers increasingly consume imported goods, protecting their interests may require more resources because antitrust regulators now has to monitor and analyze foreign and international markets in addition to the domestic market, requiring more staff, ceteris paribus.[8] We thus are left with the old problem of observational equivalence: Both SST's special interest theory and the classic public interest theory that they seek to expose as empirically wrong in fact can explain the same empirical pattern.

The second and most crucial flaw of the theoretical argument concerns SST's prediction regarding the enforcement vis-à-vis large domestic firms. SST claim that the government uses antitrust enforcement to shift some of the costs of adjusting to increased import competition from small firms to large firms. In doing so, however, the government would undercut the ability of otherwise competitive domestic firms to maintain their market share in the face of foreign competition. Foreign entrants would simply be able to capture an even larger share of the U.S. market, driving large firms out of business along with small ones. The politically consequential losses of such a policy (fewer domestic corporate campaign contributors, increased unemployment, etc.) should be even higher than in the absence of the hypothesized protectionist antitrust enforcement. Moreover, these losses should materialize without significant delay, so the standard assumption that political leaders are myopic and thus willing to incur substantial long-term costs for a short-term gain, does not help restore the theoretical logic of the claim.

2.2. An Alternative Theory of Antitrust Enforcement and Foreign Competition

Before turning to the empirical analyses, we briefly articulate an alternative theoretical position, developed by one of us, originally partly in joint work (Büthe and Bradford 2012; Büthe 2014; Bradford and Büthe 2015). This approach takes firms seriously as (potential) political actors—and not just vis-à-vis their domestic governments but also transnationally (Büthe 2010; Büthe and Mattli 2011). Conceptualized in this way, firms do not need to clamor for government protection when they seek protection from competition. Instead, they can also pursue "private protection" by, for example, colluding with other firms (see Ludema 2001; Trebilcock 1996; Williams and Rodriguez 1995). The possibility of private protection through collusion maters because trade liberalization creates new incentives and opportunities for transnational collusion. Extending the boundaries of the market brings economic actors who previously had not stake in each other's actions into a relationship of interdependence. While this may undermine purely domestic collusive arrangements, it creates incentives to collude transnationally. And while it may increase the number of actors, it also increases the rents available, for instance when a larger market can be captured by the transnational cartel. At the

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firms and individuals targeted in each enforcement action and how the share of such foreign "respondents" of U.S. enforcement actions has changed over time—which is beyond the scope of this paper.

[8] Ironically, here, too, freer markets might require more regulation, as Steven Vogel observed at about the same time as SST published their paper (Vogel 1996).

same time, risk of detection is diminished because monitoring of global markets is more difficult and costly, and because evidence of transnational collusion can be more easily kept out of the reach of any given enforcement agency.[9] This implies that we should see an increase in transnational anticompetitive behavior as a function of the institutionalization of product market integration. This behavior as such is not the focus of this paper, but we stipulate that governments will understand the opportunities and incentives for increased, transnational anticompetitive conduct created by economic globalization.

We also treat governments and regulatory agencies as political actors. Here, we make two crucial assumptions: First, we assume that governments (and specifically antitrust regulators) generally understand the international political economy of competition to the point where they recognize the opportunities and incentives for increased, transnational anticompetitive conduct created by economic globalization. Second, we assume, as a starting hypothesis, that antitrust regulators take a public interest approach (Christensen 2011).[10] Specifically, we assume that antitrust regulators—at least where the agency enjoys significant independence—see safeguarding market competition as their primary objective. Responsive to the expected increase in transnational anticompetitive activity in the presence of open trade, governments thus should increase these agencies' resources for antitrust enforcement. It follows that governments should see trade openness and the need for vigorous competition policy as complements.

Empirically, this implies a positive relationship between economic openness and antitrust enforcement resources. However, we suggest a different metric than the one used by SST, namely *trade openness*—rather than import penetration or net imports. Our ability to observe this relationship in time series of the budget of the DoJ and the FTC, i.e., the public resources devoted to U.S. antitrust enforcement, depends upon the willingness of the President and the Congress to support the approach that our theory attributes to the antitrust enforcement agencies rather than governments at large. Such support is by no means guaranteed, as Presidents have for ideological reasons varied in their support for stringent antitrust enforcement (an issue we will examine separately),[11] and the members of the Congress have at various times severely

[9] Furthermore, insofar as *institutionalized* trade liberalization succeeds in constraining governments' urge to protect domestic firms, it creates further incentives for firms to turn to private protection.

[10] We recognize that special interests might, under identifiable conditions, capture regulatory agencies and elected officials, but we reserve a more marginal role for agency capture.

[11] A long line of research has shown Republicans to be generally much more skeptical about, and often directly hostile to, business regulation than Democrats. Whether this partisan difference regarding regulation in general should also apply to antitrust, which many see as pro-market and in the material interest of most businesses (as well as consumers), is unclear. Republicans greater proximity to large, often dominant firms ("big business") would suggest so, but it could also be argued that their greater ideological commitment to the free enterprise systems should make Republicans more pre-disposed toward stringent antitrust enforcement (Green, Moore, and Wasserstein 1972:113). The empirical literature tends to emphasize the Presidency of Ronald Reagan (which elevated many proponents of the special interest theory to senior antitrust policy positions) rather than partisanship in general (see Handler 1990; Shughart 1989). Indeed, Eisner and Meier (1990) and Kovacic (2003) for different reasons question the partisan affiliation of the President as too blunt and remote of an explanatory factor for variation in U.S. antitrust enforcement practices. Lewis-Beck (1979:178-180) finds no empirical support for Presidential partisanship affecting antitrust enforcement in statistical analyses covering 1929-1973, whereas Yandle (1988) does in analyses covering 1951-1979. Wood and Anderson (1993:8f, 11f) point out that Presidential support for antitrust enforcement has varied within as well as across Parties, prompting them to use indicators for individual Presidents rather than their partisanship in analyses that cover 1970-1989. They find a partisan difference on average but primarily differences across individual Presidents.

criticized the enforcement agencies practices (e.g., Kovacic 1982). But to the extent that President and Congress share this perspective, increased trade integration should prompt them to devote more public resources to U.S. antitrust enforcement. These increased resources would allow antitrust agencies to better monitor domestic and international markets, including carry out more complex analysis required to detect and assess allegations of transnational anticompetitive behavior. Enhanced resources would also enable greater transgovernmental regulatory cooperation that is a precondition for obtaining the evidence necessary to prosecute antitrust cases that span across multiple markets.

This theoretical logic leads to the following hypothesis:

H_2: The greater the level of trade openness (X+M/GDP), the greater the size of the budget of the enforcement agencies.

3. Empirical Analysis

3.1. Replication of the Statistical Analyses by Shughart et al.

To assess H_1 empirically, SST estimate regression equations of the annual budgets of the Federal Trade Commission (FTC, model 1), the Department of Justice's Antitrust Divisions (DoJ AtD, model 2), and the combined budgets of the two enforcement agencies (model 3). SST convert the time series of the DoJ AtD and the FTC budgets into constant 1982 dollars (1995:183), thus appropriately "cleaning" the time series of changes in the budget that may have been simply attributable to inflation. Then, they model these budgets as a function of the following nine regressors (Shughart, Silverman, and Tollison 1995:182-184):

IMPORTS AS A % OF GDP$_{t-1}$: The value of durable and nondurable imports as a percent of U.S. GNP in the prior year serves as their measure of import penetration. Since they expect "antitrust law enforcement activities [to be] designed in part to protect certain domestic interests against the rigors of international competition" (1995:184), SST expect to estimate a positive coefficient for imports.[12]

CASES$_{t-1}$: The number of cases brought by the respective agency (or both agencies combined for model 3) in the immediately prior year is included as a measure of the publicly visible workload and law enforcement "output produced" and in that sense agency "performance" (1995:183). SST therefore expect the estimated coefficient to have a positive sign.

DOCKET OF CASES: The sum of the cases launched by the respective agency in the 3 prior years (= CASES$_{t-1}$ + CASES$_{t-2}$ + CASES$_{t-3}$) is also included, as a proxy for the agency's total docket of cases. This measure is based on the assumption that antitrust investigations take, on average, three years from the case being brought to their resolution (which is how SST interpret Posner's (1970:374-381) analysis of case duration). It is included on the assumption that "a greater number of ongoing antitrust matters is viewed sympathetically by the appropriations committee,"

[12] Conversely, if Congressional budget allocation were public-interest-maximizing, SST expect a negative coefficient (1995:183).

resulting in the expectation of a positive coefficient (Shughart, Silverman, and Tollison 1995:183).[13]

$GNP_{real, t-1}$: Real (i.e., inflation-adjusted) Gross National Product as a measure of the total level of economic activity and thus "the potential supply of antitrust violations" (1995:183). This variable controls for the possibility that the budget for antitrust enforcement simply increases with the level of economic activity. A statistically significant positive coefficient would therefore be broadly consistent with a public interest approach to antitrust enforcement.

HART-SCOTT-RODINO: An indicator variable to capture the substantial increase in the antitrust enforcement agencies' merger review workload due to the Hart-Scott-Rodino Act of 1976. This legislation made advance notification mandatory for all U.S. mergers, acquisitions and joint ventures above certain size thresholds—and required the FTC and the DoJ AtD to review those pre-merger notifications in a timely manner. Based on the approach (and statistically significant findings) of Yandle (1988:270f), this variable is coded 1 for 1976 and all subsequent years; 0 otherwise.

LIAISON AGREEMENT: This variable is supposed to capture the effect of the 1948 liaison agreement between the FTC and the DoJ (dividing lead responsibility for antitrust enforcement between the agencies by industry), which according to Higgins, Shughart and Tollison substantially increased both antitrust enforcement agencies budgets. Coded 1 for 1948 and all subsequent years; 0 otherwise.

REPUBLICAN PRESIDENT: An indicator for years with a Republican President "to test the conventional wisdom that Republican administrations are less activist on the antitrust front" (Shughart, Silverman, and Tollison 1995:184). Coded 1 for years with a Republican incumbent of the White House; 0 otherwise.

GREAT DEPRESSION: The final regressor included in SST's empirical models is an indicator for 1932 and 1933, the two years of the Great Depression included in their analysis. It is intended to safeguard against the possibility that these two years might be influential points in the statistical analysis. This variable is also supposed to test the hypothesis—derived from Peltzman's "general" model of regulation as redistribution of the uneven costs and benefits from the business cycle (1976)—that antitrust enforcement is counter-cyclical, as Shughart and Tollison had found in previous research on antitrust enforcement with Amacher and Higgins (1985).[14]

Our attempt to obtain a replication dataset from Shughart, Silverman and Tollison failed,[15] but Shughart kindly provided a copy of a typed 4-page table that reported the budget and

[13] Posner cautions against mistaking the numbers he reports in that section of his 1970 article, especially Tables 7 and 9, as measures of the average duration of all DoJ and FTC antitrust investigations since he was only able to examine investigations that resulted in a case being brought and excluded from his analysis all cases that were concluded within 6 months, both of which should result in overestimating the duration of investigations. At the same time, subsequent literature has suggested that, by the time the DoJ files a case, it has on average investigated the anticompetitive practices for about a year, which suggests Posner's data (and ours) will underestimate the duration of investigations.

[14] More recent work by Ghosal and Gallo finds a substantively and statistically significant pro-cyclical pattern instead, more consistent with the public interest approach than with any variant of special interest theory.

[15] According to an 11/28/2011 email from William Shughart to Tim Büthe, the data had been compiled by Jon Silverman, whose current whereabouts were unknown, and neither Shughart nor Tollison had kept a copy of the dataset.

cases for the FTC and the DoJ Antitrust Division from 1890 to 1981 (for available years), which he suggested might contain the data used for the analysis in the 1995 paper. The cases reported for 1929-1981 indeed allowed us to compile times series of FTC and DoJ $CASES_t$ and DOCKET OF CASES (i.e., $CASES_{t-1} + CASES_{t-2} + CASES_{t-3}$) with descriptive statistics that—for the years 1932-1981, the 50 years covered by SST's analysis according to their footnote 14—exactly match the descriptive statistics reported for these variables in Table 1 of SST (1995:185). We reproduce their Table 1 and the corresponding descriptive statistics of our reconstructed dataset below.

TABLE 1:
Summary Statistics Table from SST 1995:185

Table 11.1 Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
RFTCB*	32,165.08	22,916.44	10,770.49	86,011.08
RDOJB*	17,623.66	12,115.24	1,262.29	50,809.80
FTC CASES	204.48	122.60	18.00	560.00
FTC DKT	624.14	305.22	169.00	1,251.00
DOJ CASES	41.24	25.23	4.00	116.00
DOJ DKT	113.58	63.23	15.00	273.00
RGNP†	1,702.09	816.86	498.50	3,248.80
MDNPCT	3.76	1.91	1.70	9.06

*Thousands of 1982 dollars.
†Billions of 1982 dollars.

Comparisons with data from the DoJ and FTC showed that the budget data in the table provided by Shughart were reported in constant dollars, and extensive trial and error allowed us to infer that, for their conversion into constant 1982 dollars, SST must have used the "Implicit Price Deflator for Gross National Product, 1929-1982" from the September 1986 print edition of the Bureau of Economic Analysis' *National Income and Product Accounts of the United States, 1929-1982*, table 7.4 (pp. 327f), line 1. We add time series data for imports and GNP from the same source.[16] This yields a dataset with the descriptive statistics shown in Table 2. With the exception of the DoJ budget in constant dollars, for which mean and standard deviation closely approximate but do not perfectly match the corresponding descriptive statistics reported by SST, the summary statistics of our dataset are identical (or virtually identical to all but the last decimal) to the ones shown in SST's Table 1.

[16] SST note the 1986 BEA *National Income and Product Accounts* publication in footnote 7 as the source of their data on imports and GNP; no source is given for the deflator.

TABLE 2:
Summary Statistics of the Non-Dichotomous Variables Used for Our Replication Analysis

Variable	Corresponding SST Variable	Mean	Standard Deviation	Minimum	Maximum
FTC budget (t)	RFTCB	32,165.14	22,916.41	10770.49	86,011.08
DoJ Antitrust Div budget (t)	RDOJB	17,772.24	12,360.00	1262.295	50,809.80
FTC Cases (t)	FTC CASES	204.48	122.60	18	560
FTC Docket = Cases(t-1) + Cases(t-2) + Cases(t-3)	FTC DKT	624.14	305.22	168	1251
DoJ Cases (t)	DOJ CASES	41.24	25.23	4	116
DoJ Docket = Cases(t-1) + Cases(t-2) + Cases(t-3)	DOJ DKT	113.58	63.23	15	273
GNP (real, t)	RGNP	1,702.09	816.86	498.50	3248.80
Merchandise Imports (t)	MDNPCT	3.76	1.91	1.70	9.06

Note: *N*=50 (1932-1981). FTC and DoJ budget in thousands of constant 1982 dollars; GNP in billions of constant 1982 dollars. Summary statistics rounded as in SST's Table 1, except where our figures do not exactly match theirs, in which case additional decimals are shown. Merchandise imports is the sum of durable and nondurable goods purchases from the 1986 print edition of the NIPA tables, Table 4.1 (p.208), line 9. We follow SST in displaying these summary statistics for the 50 years 1932-1981, even though the statistical analysis uses many of them lagged.

For the FTC and the combined budget (models 1 and 3), SST estimate an ARIMA model with a first-order moving average term.[17] For the DoJ budget, they appear to estimate a regular OLS (1995:185). As shown in Table 3, our replication agrees with SST on the sign, the order of magnitude, and the statistical significance of the coefficient estimated for lagged imports, which is what Shughart, Silverman, and Tollison had expected. However, we are otherwise unable to replicate the estimates reported by SST, even with the carefully reconstructed dataset and using the exact same methods reportedly used by those authors. Some of the estimated coefficients are close, but many are twice or half as large as those reported by SST. We also estimate statistically significant coefficients for some of the variables in some models where SST estimated none—and vice versa. Should our inability to replicate the estimates more fully and precisely be cause for concern?

[17] The discussion of their specific empirical methods is extremely scarce, but SST provide one reference to the discussion of first-order integrated moving average processes and ARIMA models in Granger and Newbold's *Forecasting Economic Time Series* (1977), and they explicitly note that "Preliminary empirical analysis indicated that the FTC and combined antitrust budget time series follow a first-order moving average process; such a term is therefore included on the right-hand side of these two specifications" (Shughart, Silverman, and Tollison 1995:185).

TABLE 3
SST Regression Results and Replication

	model 1 (SST)	model 1' (Replication)	model 2 (SST)	model 2' (Replication)	model 3 (SST)	model 3' (Replication)
$Imports_{t-1}$	4,589.75***	2,617.20***	1,214.09**	1,095.14**	6,451.80***	3,801.61***
	(5.75)	(4.20)	(2.50)	(2.72)	(6.47)	(5.65)
$Cases_{t-1}$	2.17	5.40	42.11	38.07	–2.51	–2.07
	(0.24)	(0.52)	(1.53)	(1.66)	(-0.22)	(-0.19)
Docket of Cases	7.99**	6.15	20.60	21.25*	9.47**	7.28
(=$Cases_{t-1}$ + … $Cases_{t-3}$)	(2.14)	(0.86)	(1.58)	(1.96)	(2.10)	(1.02)
GNP_{t-1}	18.69***	22.14***	7.78***	7.86***	27.09***	31.73***
	(11.13)	(7.78)	(7.47)	(9.09)	(12.71)	(12.17)
HSR	7,460.62**	12,377.32	6,949.40***	8,639.03***	13,606.27***	20,899.59***
	(2.14)	(5.51)	(3.34)	(5.00)	(3.15)	(6.45)
Liaison Agreement	–3,200.38	–5,836.93*	1,531.11	1,705.56	–1,332.53	–4,554.41
	(-1.74)	(-1.92)	(1.42)	(1.91)	(-0.53)	(-1.56)
Republican President	–2,164.74	–1,682.06	–2,479.75***	–2,690.64***	–4,745.09***	-4,387.99
	(-1.56)	(-0.63)	(-3.05)	(-3.98)	(-2.73)	(-1.20)
Great Depression	9,476.64***	10,951.15**	–386.24	–311.22	7,475.63	10,336.15
	(3.09)	(2.08)	(–0.21)	(-0.20)	(1.95)	(1.61)
constant	–19,178.42	–16,262.64	–4,612.41	–4,325.34	–24,138.89	–19,451.51
		(-5.31)		(-4.26)		(-5.25)
MA(1)	0.73***	0.999			0.53***	0.999
	(6.50)	(0.00)			(3.78)	(0.01)
R^2	0.97		0.97	0. 9805	0.98	
Durbin-Watson	1.37		1.47	1.28	1.72	
N	50	50	50	50	50	50

Notes: Estimated coefficients based on ARIMA models with first-order moving average term (MA(1)) for models 1 and 3; OLS regression for model 2. T-statistic (*z*-stat for ARIMA models) in parentheses, following SST. Years covered: 1932-1981 (all models). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$; two-tailed tests. SST report significance at the .01 level for Imports in model 2, but the reported t-stat suggests only significance at the .05 level.

Unfortunately, the answer is yes. Our attempts to find a way to replicate the results of SST more fully led us to discover numerous other, sometimes disturbing problems, which call most of the analysis and all of the conclusions into question. One set of problems concerns the estimation strategies employed in the original paper. The most obvious problem here is the lack of dealing with time dependence in the DoJ model. Whenever we employ OLS to analyze time series (as when we employ ARIMA estimation for the FTC and combined series), we make the assumption that all of the time series included in the model are stationary. SST test for first-order serial correlation in the errors (and appear to have included a moving average term in their ARIMA models to deal with the evidence of such autocorrelation in the errors when they found it there), but they appear to have paid no attention to the variables themselves. This may have been merely a careless oversight, but it is a consequential one—and stunning since they reference and quote from a section of Granger and Newbold's textbook on forecasting time series

(Shughart, Silverman, and Tollison 1995:185, footnote 15), which is specifically devoted to "danger[s] of spurious regression in econometric models" (Granger and Newbold 1977:202-214). Even a cursory look at the data suggests that both budget time series and imports as a percentage of GNP exhibit strong upward trend, as do the time series of DoJ and FTC cases and of real GNP. Thus, we have reason to believe that the original SST results suffer from a problem of "spurious correlation," where OLS estimates produce highly significant results between time series that contain a trend and are otherwise unrelated to each other. And while first differencing could fix this problem, SST explicitly note that all variables are entered in their levels.[18]

We also find the empirical model to be problematic. A key problem is the inclusion of the agencies' CASES as an independent variable in the model. Given that our theoretical variable of interest is antitrust enforcement, it would appear that we have to choose between two possible measures as possible dependent variables in time series statistical analyses: the budget of the relevant enforcement authorities or the number of cases filed by those authorities (Posner 1970). Including one on the left side of the equation and two versions of the other on the right results in an empirical model with circular logic and a very high probability of bias. And while it is possible that there is a causal relationship between cases and budgets, it seems likely to run in the other direction—which is what Ghosal and Gallo (2001:42) find when they explore this exact issue using Granger causality tests. We also are skeptical about two of the dummy variables. The 1987 paper by Higgins *et al*, which asserts that "interagency collusion" between the FTC and DoJ since 1948 has put the United States "effectively [under] a single antitrust-law-enforcement institution" (1987:178), provides the only motivation for the inclusion of the LIAISON AGREEMENT dummy variable. But that paper is based on a questionable depiction of the liaison agreement, problematic data, unclear econometrics, and a standard of inference that declares "broad support" for the authors' hypotheses as long as the data "do not refute the models' main predictions" (1987: 175, 177).[19] The DEPRESSION dummy is similarly questionable. Coded

[18] Moreover, when we transformed the data to allow for the possibility the might have accidentally failed to note that they had first-differenced their data, the estimated coefficients still bore no resemblance to those reported in SST's Table 2.

[19] The DoJ-FTC Liaison Agreement of 1948 and the various subsequent formal and informal cooperation agreements that were supposed to extend or reinvigorate the inter-agency cooperation, have been frequently noted (e.g., Denger *et al.* 1994:293; Kay 2012; Roll 1976; Winslow 1985:980), but only in recent years has it been critically examined in some detail (e.g., Kovacic 2009:esp. 63-65; Peay 2007). Published accounts (e.g., ABA Section of Antitrust Law 2004:13-16; Muris 2005) as well as interviews with current and former FTC officials indicate that the Liaison Agreement and successor agreements never quite worked as written, and that the allocation of cases between the agencies ("clearance") has remained a source of frequent tension between the agencies. The data used by Higgins *et al* are problematic inter alia because they combine Posner's time series of DoJ cases for 1890-1969 (which adjusts the case count downward by about 25% to account for practices such as filing separate civil complaints and criminal indictments for the same charges, or charging each respondent in a multi-respondent investigation separately) with the raw case count from the Commerce Clearing House's *Trade Regulation* Reporter for 1970 - 1981. Higgins *et al* also appear to count all FTC complaints and consent orders as FTC antitrust cases (1987:171), whereas the FTC also undertakes a broad range of consumer protection enforcement actions (to punish or curb false advertising, predatory lending, etc.), which in proportion to their antitrust activities vary substantially from year to year (see also Davis 1962; Kovacic and Shapiro 2000). The econometric methods used are incompletely described, for instance, when Higgins et al simply note that they used GLS "to adjust for the autocorrelation that was present in ordinary least squares estimates" (1987:174f) with no further details provided. The interpretation of the findings is correspondingly suspect: After the substantial increase in the total number of cases pre- versus post-1914, for instance, is attributed to the introduction of regulatory competition through the creation of the FTC, the slight further increase after 1948 is then presented as evidence of the pernicious effect of eliminating regulatory competition through the "collusion" between the regulatory agencies (FTC and DoJ), i.e., the

1 for only two years, 1932 and 1933, it effectively mostly removes those two years from the analysis. The rationale provided for this variable is that it captures the possibility of antitrust enforcement being pro- or anti-cyclical, as some previous studies had suggested. But in that case, a proper measure of the business cycle—such as the GNP or GDP growth rate or the unemployment rate—would seem much more appropriate.

There are also some major data issues in the original SST study, the first of which is the use of flawed FTC case data. SST make use of FTC case and budget data that represent total levels for the organization. That is, they include cases and budgets for issues which are not related to antitrust. A comparison of SST's time series of FTC cases with the time series of FTC restraint-of-trade cases reported by Posner (1970:369) shows that the divergence is substantial, with the total number of FTC cases in some years ten times as large as the number of restraint-of-trade cases recorded by Posner, and as Stewart and Cromartie note: "focusing on overall FTC activity as opposed to focusing on patterns within its widely varied areas of responsibility may obscure as much as it reveals about the political economy of the FTC" (Stewart and Cromartie 1989:389). In short, we simply can draw no meaningful inferences from the use of the FTC data, as it is invalid as a measure of antitrust enforcement. The DoJ budget and case data are in principle appropriate, though the case data suffer from strange inconsistencies when compared to their ostensible sources.

Finally there is the issue of the tendentious interpretation of the data. In both the original SST models and our replication, we see a highly significant and positive coefficient estimate for GNP, which according to SST's own account would seem to signal support for the public interest account of antitrust enforcement. Increased levels of economic activity resulting in increased enforcement of antitrust policy are a signal that public actors are acting in a pro-competitive manner to protect market competition in times of robust economic activity. Rather than address this troubling strike against the special interest framework proposed in their paper, SST ignore the contrary evidence in the discussion.[20]

3.2. Original Statistical Analysis: DoJ Budget 1930 – 2011

To improve upon SST's empirical analysis and assess their argument in comparison with appropriate alternatives, we conduct an aggregate-level analysis of the effect of trade on the enforcement resources allocated to the DoJ Antitrust Division over an extended time period (1930-2011). To estimate the effect of economic openness on antitrust enforcement (budgets), we consider not only the equivalent of SST's measure of import penetration in the regression analysis, but also net imports and the standard measure of overall trade openness (exports + imports) as a direct test of H_2.

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reversal to the pre-1914 conditions. Major changes, such as increases in the complexity of the economy and hence in the necessary antitrust analysis over the better part of a century and increases in the demand for thorough economic analysis demanded by the courts (a change already explicitly noted by Posner 1970:370), are ignored.

[20] In introducing the variable, they note that a positive correlation between the size of the economy and the size of the antitrust enforcement budget could also be interpreted as support for Adolph Wagner's "law," according to which there is a long-run tendency, as a function of increases in a country's the level of economic development, for the public sector to grow, not just along with but as a share of the national economy (Wagner 1892). SST's research design, however, does not allow for a meaningful test of Wagner's Law (see Henrekson 1993), and SST simply remain silent on their statistical finding for this variable.

We use the DoJ AtD budget converted to constant 2009 dollars as our dependent variable.[21] It should be noted that, beginning in 1990, the DoJ AtD also began to collect revenue from merger filing fees, reported separately in the dataset posted by the DoJ. Because Congressional appropriations for the DoJ were adjusted downwards to account for this new source of revenue, we use the total DoJ AtD budget, which is the sum of filing fees plus appropriations for each year (adjusted to constant 2009 dollars). The independent variables included in the analysis are fully described below.

IMPORTS AS A % OF GDP: The value of total imports as a percent of U.S. GDP serves as our main measure of import penetration. Data on U.S. imports are drawn from the World Bank's World Development Indicators (WDI) dataset and are measured in billions of U.S. dollars. This is the key variable of interest in the SST analysis.

NET IMPORTS AS A % OF GDP: We consider this measure, which over time captures the direction of changes in international trade volumes, the more salient test of SST's public choice perspective on antitrust enforcement than just imports, because net imports—calculated as imports minus exports—in effect control for the possibly countervailing political influence exerted by exporting firms. If the special interest critique of antitrust enforcement is warranted, we should see a positive coefficient because when net imports increase, domestic producers should demand more (selective) antitrust enforcement from politicians. Data on U.S. exports and imports are again drawn from the WDI dataset and are measured in billions of U.S. dollars.

TOTAL TRADE AS A % OF GDP: The sum of exports plus imports as a percentage of GDP is the most widely used measure of trade openness. Data on U.S. exports and imports are drawn from the WDI dataset and measured in billions of U.S. dollars. Since we view the enforcement of antitrust law as a means of safeguarding market competition, as articulated in H_2, we would expect this variable to have a positive effect on the size of DoJ budgets.

REAL GDP: Real GDP, adjusted to constant 2009 dollars, is included as a measure of the overall size of the economy, and a possible measure of the "supply" of anticompetitive conduct. Unadjusted current GDP figures and the corresponding 2009 GDP deflator were drawn from the U.S. Bureau of Economic Analysis (BEA) and are measured in billions of U.S. dollars.

UNEMPLOYMENT RATE: We include the unemployment rate as a control variable for overall economic performance and the position of the U.S. economy in the business cycle. For 1947-2011, we use the unemployment rate as calculated by the U.S. Bureau of Labor Statistics (BLS). It reflects the currently unemployed percentage of the active labor force. Pre-1947 data are drawn from Lebergott (1957:215f, Table 1) and include corrections for changes to the definition of what constitutes the labor force.

HSR MERGER AUTHORIZATION: We also include a binary indicator for the Hart-Scott-Rodino (HSR) pre-merger notification requirement that was signed into law by President Ford in September 1976 with implementation slated for mid-1977. Since the 1978 fiscal year

[21] Data is drawn from the DoJ Antitrust Division website (http://www.justice.gov/atr/public/atr-appropriation-figures.html). Appropriations data are maintained in thousands of current U.S. dollars beginning in 1903 and continuing through 2014. The dataset posted on the DoJ website also maintains detailed notes on any revisions applied against original appropriations that have been incorporated in the time series. Note that from 1903 to 1976, the U.S. fiscal year started July 1 of the prior year and ended June 30 of the year for which the data are recorded. Beginning in 1977, the fiscal year was changed to run from October 1 through September 30.

was therefore the first full fiscal year of HSR implementation, it is coded as a 1 beginning in 1978; 0 otherwise.

PRESIDENTIAL PARTY: This is a binary indicator for the party of the current U.S. President. In keeping with the coding employed by SST, we code Republican administrations with a 1 and Democratic administrations with a 0.

Estimation Strategy:

Changes in the level of trade (and possibly some of the other variables that we treat as exogenous) may have persistent effects on the DoJ antitrust budget over time. To safeguard against the possibility that co-integration and the emergence of long-term dynamic equilibria might result in spurious correlation, we implement error correction models (ECMs) to estimate the effect of our possible explanatory variables on the antitrust budget. The ECMs estimated here are equivalent to an autoregressive distributed lag (ADL) model after straightforward mathematical transformation and safeguard against spurious correlation when dealing with non-stationary variables that are trending together (De Boef and Keele 2008).

We estimate the ECM using the first difference of the variable of interest as the dependent variable, i.e., in our model, the change in the DoJ AtD budget from year t-1 to year t is on the left-hand side of the model. The right-hand side of the model then includes the lagged value of the untransformed DoJ AtD budget as well as the lagged level and the first difference for each of the independent variables described above.

Another benefit of the ECM framework is the ease of interpretation. The coefficients on the lagged levels of the exogenous variables provide an estimate of their short-run effect on the DoJ AtD budget. These lagged levels will be the main focus of our discussion as they represent the most direct test of the theories linking trade policy to subsequent DoJ AtD budget allocations. The coefficient for the lagged endogenous variable (entered at its level) provides an estimate for the persistence over time of these short-run effects of the exogenous variables. Finally, the coefficients estimated for the first differences of the exogenous variables reflect the concurrent effect of a change in each variable on the DoJ AtD budget.

Results:

The results of our analyses on changes in DoJ AtD budgets for the period 1930 to 2011 are displayed in Table 4. Each model reflects the inclusion of a different measure of trade policy (separately) as the key independent variable. Table 5 presents the results of the same models for the restricted time period of 1932 to 1980—the time period considered by SST.

The models included in Table 4 provide the greatest insights into the effect of trade openness on antitrust budgets. In Model 4, we estimate for imports as a percentage of GDP—the key variable in the SST analysis—a positive, but not statistically significant effect on DoJ AtD budgets as our measure of antitrust enforcement. In the alternative specification in Model 5, we estimate a negative and statistically significant coefficient for net imports. This suggests that increases in the level of imports, over and above simultaneous increases in exports, actually result in a decline in antitrust budgets—directly contrary to what the special interest perspective would lead us to expect. When domestic producers are being negatively impacted by foreign imports, with no countervailing pressure from domestic exporters, they appear to have generally not been successful in exerting political influence to win increased enforcement of antitrust policy.

Given the lack of support for the special interest theory on trade and antitrust policy, we now turn towards our test of the alternative theory of antitrust enforcement and foreign competition encapsulated in H_2. In Model 6, we observe a positive and significant coefficient on total trade as a percentage of GDP. This supports our argument that, as trade openness increases and corporations find more opportunities for transnational collusion, the U.S. government turns towards increasing antitrust enforcement as a means of safeguarding competitive markets under the changed conditions of economic globalization.

Some other interesting findings emerge in Table 4 for our control variables as well. Note, for instance, that across all models the coefficient on real GDP is positive and statistically significant confirming that increased economic activity within the country results in higher budgets allocated to the DoJ AtD for the following year. This finding is consistent with SST's finding for real GNP, but just like their finding supports a public interest view of antitrust enforcement. Note, however, that (at least on average over the whole time period of 1930 - 2011), the coefficient on the unemployment rate is statistically insignificant, i.e., indistinguishable from zero in all three models. This suggests that the business cycle has no clear effect on the subsequent level of the DoJ AtD budget; there is no clear procyclical (nor countercyclical) trend in U.S. antitrust enforcement.

The dummy variable for Presidential party is negative across all 3 models and statistically significant in models 4 and 6. It suggests that when a Republican administration is in office, budgets for antitrust enforcement decline—consistent with widely held expectations. The lack of statistical significance in model 5, however, suggests that this finding might warrant further research.

When comparing the fit of the three models presented in Table 4, we see that using the standard metrics of R^2 and BIC, Models 5 and 6 exhibit a better fit than Model 4. The findings do not, however, allow us to make a determination about which of these two models is superior. We can, however, treat the two models as empirical guidance to assess competing theories of antitrust enforcement and trade policy, as we have suggested above.

Turning our attention to Table 5, we observe that the same results generally hold. Indeed, when they are properly specified for the time period analyzed by SST, Models 7 or 8 provide no support for the special interest perspective on antitrust policy. Instead we again observe a positive and significant coefficient on our measure for total trade as a percentage of GDP in Model 9, confirming that even using this particular portion of the overall time series, our main finding regarding the effect of economic openness holds. In fact, the estimated effect of total trade is actually much larger when using the shortened time series. Here we also see that Model 9 also exhibits a clearly superior fit—in terms of both R^2 and BIC—to Models 7 and 8.

4. Conclusion

The public choice tradition has made important contributions to the study of law and policy-making across the social sciences (see, e.g., Farber and O'Connell 2010; Mueller 1996). It has been particularly influential in economics and political science, most prominently by developing systematic models of decision-making in groups, including the legislative, executive, and even judicial branches of government (see, e.g., Aldrich and Rohde 1982). Scholars in this tradition, beginning with Downs, Riker, and Olson, have made numerous important contributions to our understanding of non-market institutions for making choices in the absence of perfect

hierarchy, building on the foundational Arrow-Black-Condorcet "impossibility theorem" (Rowley 2013:14, 16f).[22] These studies of collective choice highlighted the difficulty of defining a "public interest" even when membership in the pertinent political community is well defined.

This important insight prompted public choice scholars to critically examine whose interest policymakers, bureaucrats, and even judges serve when they make decisions that are binding on others. Their work challenged social scientists not just to assume that public officials serve the public interest (or even faithfully pursue the interests of their "constituency" in the case of elected officials). Instead, they urged scholars to allow for the possibility that public officials might—individually or collectively—also or even primarily pursue their own, private interests. A key implication of allowing for such self-interested motivations is that public officials might cater to the interests of a narrow subset of stakeholders who are able to offer rewards or impose costs on the public official(s) and thus "capture" the policymakers.

One way to examine this alternative hypothesis—a hypothesis regarding the assumptions we should make about preferences—is to build theoretical models based on the assumption that public officials are only motivated by narrow self-interest (in line with the neoclassical *homo oeconomicus*). One could then examine what such models—in conjunction or maybe in competition with models built on different assumptions—might contribute to explaining the behavior of public officials or the resulting public policies. Stigler's 1971 article, which launched the special interest theory of regulation, is, at least in part, presented in this spirit; Buchanan and Tullock's foundational book (1962) explicitly adopt this position.

Over time, however, scholars of antitrust law and policy, who are place themselves in this public choice tradition, appear to have forgotten that the assumption about self-interested public officials who are prone to capture had been made for heuristic purposes, as part of a social-scientific project to interrogate the previously dominant public interest assumptions. For SST and some other scholars in the special interest tradition, what started out as a conditional hypothesis has become a core assumption that is taken to be a descriptively accurate, complete statement about the motivations of regulators and elected officials. It is accepted like an article of faith, without serious consideration of alternatives. This shift has serious implications. If public policymakers are solely motivated by self-interest and hence indirectly by concentrated private interests, then antitrust law and its enforcement becomes—by definition—an exercise in protectionism. More generally, public policy can under this assumption never meaningfully "correct" market failures, as government failure is necessarily at least as likely and worse than market failure.[23]

To be sure, turning a hypothesis into a core assumption that is not subject to further inquiry is epistemologically defensible if the theories built on it uniquely yield insights for which there is strong empirical support (Lakatos 1974). But is there much empirical support for the special interest theory of regulation? Leight (2010:esp. 233ff) suggests that many empirical studies that claim to provide such support suffer from methodological problems. And recent research on the capture of government regulators—one of the most prominent and widely accepted implications of the theory—suggests that there is surprisingly little capture, or at least little evidence of it (Carpenter and Moss 2014) (see also Dal Bó 2006).

[22] As Rowley and others have pointed out, much public choice scholarship built upon and helped rediscover pioneering work from the early stages of democratization.

[23] SST and others maintain this position even though it is unsustainable when taken to its logical conclusion, because property rights and market institutions do not exist in the "state of nature."

In this paper, we have re-examined the only prior statistical analysis of the effect of economic openness on antitrust enforcement. The paper by Shughart, Silverman and Tollison (1995) is a prominent application of special interest theory to U.S. antitrust enforcement, published in a volume of what is presented as exemplary positive research on the causes and consequences of antitrust, ostensibly providing strong, objective empirical support for the special interest view of antitrust. Having gone to great lengths to re-construct their dataset and having tried all possible estimation strategies that SST might have used, we conclude that the published empirical results cannot be replicated. Moreover, and maybe more importantly, we have shown that both the theory and the empirical strategy described in the paper are problematic and in part deeply flawed. A re-analysis of U.S. antitrust enforcement, focusing on the budget of the Department of Justice's Antitrust Division, SST's chosen (input) measure of U.S. antitrust enforcement, yields no support for special interest theory. Instead, it yields considerable support for an alternative analytical approach that takes politics—private and public politics—seriously but allows for the possibility that antitrust enforcement is, at least at the macro level and *inter alia*, driven by a desire of policymakers to protect market competition rather than uncompetitive firms.

These findings have important implications for antitrust law and policy. If SST were right that, from an economic perspective, trade openness were a superior substitute for antitrust enforcement, then the international integration of product markets would render antitrust laws increasingly superfluous. Spending resources on their implementation and enforcement would be wasteful and maybe even harmful. Antitrust enforcement should in that case be reduced, not only in the United States but also in other jurisdictions with antitrust laws on the books, many which are increasingly integrated into the global economy. Our analyses suggest instead that antitrust enforcement remains critically important despite the increase in foreign competition, in part precisely because market integration changes the nature and increases the cost of enforcement, which now entails monitoring foreign and global markets in addition to domestic ones and requires trans-governmental enforcement cooperation.

Our research also has at least three broader implications for research on regulation across the social sciences. First, the lack of empirical support for SST's special interest view of U.S. antitrust enforcement underscores the value of research that focuses on market failures and on how to improve public policy interventions to address them so as to advance the public interest, as difficult as it may be to define at the margins (see Balleisen and Moss, 2010). Recognizing the possibility of government failure, which should be taken seriously as part of this research agenda (see, e.g., Mattli and Woods 2009; Stiglitz 2010), does not imply subscribing to the belief that government failure is so inevitable or pervasive that regulation, including antitrust law and enforcement, will inherently result in worse outcomes than "unmitigated" market failure.

Second, this research has implications for how we think about preferences and motivations of political and economic agents. Critiquing much work on state and market for assuming individuals to be selfish when they are economic agents while simultaneously assuming individuals to be selfless when they act as agents of the state, Buchanan called on social scientists to adopt a single common assumption about individuals' preferences and the logic of action that drives their behavior (Buchanan 1972). We reject this claim. Instead, social scientists should seek to more fully understand theoretically and more thorough examine empirically under which conditions decisionmakers are most likely to behave selfishly and under

which conditions they are more likely to define their "interests" broadly and act altruistically (Büthe, Major, and de Mello e Souza 2012).

Finally, our research yields insights into the relationship between positive analysis and normative concerns. Public choice has served as a useful corrective to the core assumption of the *Staatswissenschaft* tradition in law, which depicts the state as above the fray of political/distributional conflict and therefore, by definition, pursuing the public interest (e.g., Böhm 1961). It also has served as a corrective to related traditions in economics (where the literature on managerialism questioned similar assumptions about the senior management of firms) and even some parts of political science.[24] Critics of these earlier approaches in the special interest tradition, however, often label other scholarship as "normative." They advocate for strictly "positive" scientific research, characterized by value-free theory and rigorous empirical analysis. Yet, the call for strict separation of normative and positive analysis is partly misguided and, at least in the special interest literature on antitrust, appears to have been a rhetorical façade more than a description of practice by those who have preached it. In closing, it is appropriate therefore to reflect explicitly on the relationship between positive analysis and normative concerns.

Normative considerations can enter our analyses at three levels. At the most fundamental level, the selection of the questions we ask—which James Tobin called "one of the most important decisions a scholar makes" (2009)—often reflects our normative concerns and always has normative implications. This insight, frequently associated with the Frankfurt School or "critical theory," in fact was already articulated by de Tocqueville, as well as the early scholars of modern international relations. In sum, a truly value-free social science is impossible.

Quite unrelatedly, our assumptions carry normative force: Evidence from behavioral economics and beyond suggests that the assumptions we make tend to morph into (what we and our readers and students come to believe are) descriptively accurate general statements about human nature, preference formation, etc. (Bauman and Rose 2011; Marwell and Ames 1981).[25] This tendency holds even if these assumptions are originally made only for heuristic purposes and in full recognition of their inaccuracy. Even purely heuristically motivated analytical convenience thus can take on a prescriptive character. This raises the question for how long can we can tell our regulators and other public servants that all they do is pursue their narrow self-interest before we start to affect their behavior toward greater conformity with such assumptions? Social scientists are better served by thinking carefully about the normative consequences of their work instead of denying the existence of such consequences.

Finally, a scholar's normative commitments might enter into the data collection and coding, into the empirical analysis, or the interpretation of the results of those analyses. Here, the goal must clearly be to avoid letting normative commitment have any influence, so as to minimize bias and prejudice. But to achieve this, we need to be aware of our normative predispositions rather than deny them, as special interest scholars such as SST have been prone to do.

[24] In political science, public choice had important predecessors in early-20th century scholars of public and private bureaucracy (e.g., Michels 1962), and many advocates of the behavioral revolution

[25] See also the review of the literature "Does Studying Economics Inhibit Cooperation" by Frank, Gilovich, and Regan (http://www.gnu.org/philosophy/economics_frank/).

TABLE 4
DoJ Antitrust Division Budget as a Measure of Enforcement, 1930-2011

	Model 4	Model 5	Model 6
Lagged DoJ Budget	-0.1460**	-0.1323**	-0.1676***
	(0.060)	(0.058)	(0.057)
Imports (% GDP)$_{t-1}$	516.5		
	(932.9)		
Net Imports (% GDP)$_{t-1}$		-1283**	
		(598.4)	
Total Trade (% GDP)$_{t-1}$			779.8*
			(391.3)
Real GDP$_{t-1}$	1.602*	2.331***	1.290*
	(0.840)	(0.651)	(0.718)
Unemployment Rate$_{t-1}$	-0.3694	105.1	-27.44
	(134.8)	(121.8)	(118.7)
HSR	-2495	-1165	-4129*
	(2691)	(2146)	(2469)
Republican President	-3029**	-1955	-2811**
	(1231)	(1255)	(1164)
Δ Imports (% GDP)	-400.9		
	(938.3)		
Δ Net Imports (% GDP)		-84.06	
		(915.0)	
Δ Total Trade (% GDP)			-179.2
			(443.9)
Δ Real GDP	1.240	-1.705	1.414
	(4.557)	(4.633)	(4.009)
Δ Unemployment Rate	81.02	80.50	-35.95
	(340.8)	(325.4)	(332.4)
Δ HSR	7296	9737**	5916
	(5168)	(4714)	(4838)
Δ Republican President	-2322	-1446	-2095
	(1700)	(1669)	(1634)
Constant	-382.6	-2135	-3755
	(2206)	(2098)	(2632)
R-Squared	0.2166	0.2651	0.2634
BIC	1653.2691	1648.0225	1648.2191
N	82	82	82

Notes: Estimated coefficients, based on error correction models with Δ DoJ budget as the DV, have been rounded to 4 significant figures. Standard errors in parentheses. Years covered: 1930-2011 (all models). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$; two-tailed tests.

TABLE 5
DoJ Antitrust Division Budget as a Measure of Enforcement, 1932-1981

	Model 7	Model 8	Model 9
Lagged DoJ Budget	-0.2428	-0.0714	-0.2797*
	(0.188)	(0.140)	(0.158)
Imports (% GDP)$_{t-1}$	2098		
	(1292)		
Net Imports (% GDP) $_{t-1}$		-922.1	
		(636.2)	
Total Trade (% GDP) $_{t-1}$			1003**
			(462.1)
Real GDP $_{t-1}$	2.147	2.286	2.885**
	(1.433)	(1.466)	(1.378)
Unemployment Rate $_{t-1}$	-22.16	184.7*	5.067
	(156.2)	(103.0)	(119.1)
HSR $_{t-1}$	-1257	-331.3	1038
	(5268)	(5295)	(4979)
Republican President $_{t-1}$	-1758	-540.1	-1375
	(1432)	(1369)	(1224)
Δ Imports (% GDP)	-481.4		
	(1323)		
Δ Net Imports (% GDP)		258.6	
		(829.1)	
Δ Total Trade (% GDP)			-661.0
			(667.7)
Δ Real GDP	-1.199	-7.277	-5.118
	(7.900)	(7.740)	(8.704)
Δ Unemployment Rate	-362.6	-267.8	-503.2
	(361.6)	(337.3)	(360.1)
Δ HSR	4047	7844*	6745*
	(4271)	(4365)	(3900)
Δ Republican President	-3813**	-2727*	-3861**
	(1600)	(1540)	(1449)
Constant	-6307**	-4059**	-7470***
	(2547)	(1993)	(2442)
R^2	0.3967	0.3916	0.4834
BIC	982.1395	982.5550	974.3819
N	50	50	50

Notes: Estimated coefficients, based on error correction models with Δ DoJ budget as the DV, have been rounded to 4 significant figures. Standard errors in parentheses. Years covered: 1932-1980, matching the years of the SST analysis (all models). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$; two-tailed tests.

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