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Output Effect of Private Antitrust Enforcement

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OUTPUT EFFECT OF PRIVATE ANTITRUST ENFORCEMENT

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ABSTRACT

A growing body of literature evaluates the impact of antitrust laws on economic growth. Most of these empirical studies identify a positive impact; however, the existing literature only studies the effect of the existence of antitrust laws, but not their enforcement. To fill this gap in the literature, this Article uses private antitrust case filing numbers to examine the growth effect. Employing U.S. data and, after addressing endogeneity, using a two-stage least squares (2SLS) regression analysis, I identify a negative and robust association between private enforcement and output on a national level in the short run over the period from 1954 to 2019. However, I do not find a robust association between the two in the long run. In view of the results, I hypothesize a mechanism of an adverse effect of private antitrust enforcement on output in the short run.

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I. INTRODUCTION: MECHANISM OF POSITIVE IMPACT

A. ANTITRUST AND LONG-RUN ECONOMIC GROWTH

Governments around the globe are increasingly interested in assessing the impact of their competition policies and the effectiveness of their antitrust authorities.[1] Measuring the effects of competition law and its enforcement benefits societies in at least three ways. First, evaluations allow legislators and the public to hold antitrust authorities accountable for their work.[2] Second, such measurement can help antitrust authorities identify and learn from poor enforcement decisions, hence improving their enforcement quality.[3] Third, evaluation studies facilitate the overall design of competition regimes.[4] In light of these advantages, from time to time, governments require their antitrust authorities to show evidence on the links between competition, competition policy, and various macroeconomic outcomes.[5] Economic growth is one of the macro-outcomes governments are concerned about.[6]

In the long run, competition policies affect economic growth by altering total factor productivity (hereinafter productivity). In 1956, Robert Solow, an economist who later received the Nobel prize, introduced a long-run growth model.[7] The Solow model suggests that a rise in productivity drives the economy to grow.[8] Productivity growth drives economic growth and reflects a more efficient use of production inputs, such as labor and capital.[9] In the field of antitrust, the currently prevailing view is that competition enhances productivity, which in turn

1. *See generally* ORG. FOR ECON. COOP. & DEV., FACTSHEET ON HOW COMPETITION POLICY AFFECTS MACRO-ECONOMIC OUTCOMES (2014), https://www.oecd.org/daf/competition/2014-competition-factsheet-iv-en.pdf [https://perma.cc/MCF3-C4XR].

2. Henk Don et al., *Measuring the Economic Effects of Competition Law Enforcement*, 156 DE ECONOMIST 341, 343 (2008).

3. *Id.*

4. *Id.*

5. ORG. FOR ECON. COOP. & DEV., *supra* note 1, at 1.

6. *Id.*

7. *See generally* Robert M. Solow, *A Contribution to the Theory of Economic Growth*, 70 Q.J. ECON. 65 (1956).

8. *Id.*

9. *Id.*

promotes economic growth.[10] Theoretically, stronger competition leads to higher productivity through three mechanisms. First, firms can quickly enter and exit the market when it is competitive.[11] Under such a market condition, more productive firms enter the market to gain market share and eliminate less productive firms (across-firms effect).[12] Second, under intense competition, firms raise managerial efforts to minimize costs to avoid being driven out of the market (within-firms effect).[13] Third, and most importantly, competition drives higher productivity by encouraging firms to innovate.[14] Through innovations, firms can outperform their competitors by gaining a cost advantage, differentiating their products, or bringing new products to the market.[15] Multiple empirical studies have been conducted to show a positive relationship between antitrust and productivity.[16]

Other scholars have conducted empirical studies to explore the link between antitrust and economic growth directly.[17] Although different methods have been employed, a strong majority have found a positive

10. ORG. FOR ECON. COOP. & DEV., *supra* note 1, at 2.

11. *Id.*

12. *Id.*; COMPETITION & MKT. AUTH., PRODUCTIVITY AND COMPETITION 15-16 (2015), https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/443448/Productivity_and_competition_report.pdf [https://perma.cc/4M8V-HP9S].

13. ORG. FOR ECON. COOP. & DEV., *supra* note 1, at 2; COMPETITION & MKT. AUTH., *supra* note 12, at 14-15.

14. ORG. FOR ECON. COOP. & DEV., *supra* note 1, at 3; COMPETITION & MKT. AUTH., *supra* note 12, at 16-18.

15. COMP. & MKT. AUTH., *supra* note 12, at 16-18.

16. *See* ORG. FOR ECON. COOP. & DEV., *supra* note 1, at 22; COMP. & MKT. AUTH., *supra* note 12, at 10-13.

17. *See generally* Mark A. Dutz & Aydin Hayri, *Does More Intensive Competition Lead to Higher Growth?* (World Bank, Working Paper No. 2320, 2000), http://documents.worldbank.org/curated/en/419001468739337795/pdf/multi-page.pdf [https://perma.cc/LN3Z-SMDP]; AUSTL. GOV'T, PRODUCTIVITY COMM'N, REVIEW OF NATIONAL COMPETITION POLICY REFORMS (2005); Hiau Looi Kee & Bernard Hoekman, *Imports, Entry and Competition Law as Market Disciplines*, 51 EURO. ECON. REV. 831 (2007); Joseph A. Clougherty, *Competition Policy Trends and Economic Growth: Cross-National Empirical Evidence*, 17 INT'L J. ECON. BUS. 111 (2010); Tay-Cheng Ma, *The Effect of Competition Law Enforcement on Economic Growth*, 7 J. COMPETITION L. & ECON. 301 (2011); Niels Petersen, *Antitrust Law and the Promotion of Democracy and Economic Growth*, 9 J. COMPETITION L. & ECON. 593 (2013).

and robust relationship between the two.[18] Many of these studies utilized the typical research methodology of undertaking a regression analysis using cross-country data.[19] One study, in order to quantify the impact of antitrust in the long run, lagged its variables.[20] Alternatively, a few studies evaluated the long-term effect by examining the average annual growth rate of real output (sometimes per capita) over several years as their dependent variable.[21] An advantage of (or a need for) employing an average growth rate over the years is that it smooths out any business cycle effects.[22] Then, to model the existence or intensity of antitrust laws, these studies control for one of four variables of interest, each with its own limitations. For the first variable, most authors control for the effect of competition law by a dummy variable (i.e., a one if competition law exists and a zero otherwise).[23] The shortcoming of using such binary coding is that it focuses on the law but not its implementation.[24] As another variable option, Tay-Cheng Ma controlled for a SCOPE index,

18. *See* Dutz & Hayri, *supra* note 17; AUSTL. GOV'T, PRODUCTIVITY COMM'N, *supra* note 17; Kee & Hoekman, *supra* note 17; Clougherty, *supra* note 17; Ma, *supra* note 17; Petersen, *supra* note 17. Kee and Hoekman find no significant effect of competition laws on economic growth. However, this result should be read in caution because, due to data constraints, the authors used growth rate of value added per unit of capital instead of the growth rate of real output as the dependent variable. Kee & Hoekman, *supra* note 17, at 838.

19. In the field of antitrust, cross-country data is commonly used to measure not only economic growth, but also other macro aggregates, such as price-cost margins and productivity. *See, e.g.*, Mats A. Bergman, *Quis Custodiet Ipsos Custodes? Or Measuring and Evaluating the Effectiveness of Competition Enforcement*, 156 DE ECONOMIST 387, 399-401 (2008); Stephen W. Davies & Peter L. Ormosi, *A Comparative Assessment of Methodologies Used to Evaluate Competition Policy*, 782 J. COMP. L. & ECON. 769, 782 (2012).

20. *See* Petersen, *supra* note 17, at 611.

21. *See* Dutz & Hayri, *supra* note 17, at 6; Ma, *supra* note 17, at 312; Clougherty, *supra* note 17, at 122.

22. Clougherty, *supra* note 17, at 121.

23. In regression analysis, a dummy variable is a numerical variable used to represent categorical data, such as gender. A dummy variable takes on values of 0 or 1, indicating the presence or absence of an attribute (e.g., a 0 may indicate a male and 1 may indicate a female). DAMODAR N. GUJARATI & DAWN C. PORTER, ESSENTIALS OF ECONOMETRICS 178-80 (4th ed. 2010). *See also* Petersen, *supra* note 17, at 605-07.

24. Petersen, *supra* note 17, at 605. *See generally* Jerg Gutmann & Stefan Voigt, *Lending a Hand to the Invisible Hand? Assessing the Facts of Newly Enacted Competition Laws*, SSRN (Feb. 8, 2014), https://ssrn.com/abstract=2392780 [https://perma.cc/UE98-B6F2].

an index constructed by Keith Hylton and Fei Deng.[25] Hylton and Deng surveyed 102 countries between 2001 and 2004 and then summed scores for the various aspects of antitrust into an overall index for each country.[26] The downside of the SCOPE index is that it exists exclusively in a cross-sectional form, meaning that it can only be applied in panel data analysis.[27] Employing a third alternative, Mark A. Dutz and Aydin Hayri used independent variables, such as how businesses perceive the effectiveness of a nation's competition policy, as a proxy for the intensity of competition.[28] However, some scholars criticized this strategy, arguing that perceived effectiveness is subjective and could be influenced by the effectiveness of policy in other areas.[29] A fourth and final possibility was undertaken by Joseph A. Clougherty, controlling for budgets committed to competition policy.[30] The author merely showed that an increase in the antitrust budget matched workload increases related to mergers and acquisitions.[31] Therefore, it is unclear how such budget affects cartel and monopoly enforcement. I will discuss Clougherty more below.

B. PRIVATE ENFORCEMENT AND SHORT-RUN OUTPUT EFFECT

As seen, antitrust could promote economic growth by boosting productivity. However, it takes time for productivity to rise. This is because the entry and exit of firms, adjustment of managerial incentives, and innovation often do not occur overnight. Thus, economic growth driven by productivity growth is a rather long-run process.

Antitrust also has an immediate and positive impact on national output. Such a short-run output effect is driven by the desistance and

25. Ma, *supra* note 17, at 309.

26. *Id.*; Keith Hylton & Fei Deng, *Antitrust Around the World: An Empirical Analysis of the Scope of Competition-Enhancing Policies on Productivity*, 74 ANTITRUST L.J. 271 (2007).

27. Gutmann & Voigt, *supra* note 24, at 6.

28. Dutz & Hayri, *supra* note 17, at 2-5.

29. Stefan Voigt, *The Effects of Competition Policy on Development- Cross-Country Evidence Using Four New Indicators*, 45 J. DEV. STUD. 1225, 1230 (2009); Paolo Buccirossi et al., *Competition Policy and Productivity Growth: An Empirical Assessment*, 95 REV. ECON. STAT. 1324, 1325 (2013).

30. Clougherty, *supra* note 17, at 113.

31. *Id.* at 114, 118.

deterrence effects of antitrust enforcement.[32] The mechanism behind this is straightforward–a reversal of wrongdoings. Economics teach us that the primary harm of anti-competitive conduct (i.e. wrongdoings) is a decline in output and a rise in price.[33] Therefore, when a cartel or monopoly is being detected and sued, it probably wants to cease its illegal conduct, meaning that the wrongdoer will restore competition; this is the desistance effect.[34] As a result, output will rise, and prices will drop immediately.[35] Moreover, such a desistance effect extends to other wrongdoers that remain hidden.[36] After witnessing a wrongdoer being sued, offenders in other markets might worry about being sued themselves and cease their anti-competitive conduct.[37] This response also causes output to rise and prices to drop.[38] Similarly, suing a wrongdoer warns would-be lawbreakers and discourages some of them from starting illegal conduct, producing a deterrence effect.[39] If we believe there is natural growth in the number of cartels over time, then the deterrence effect of antitrust enforcement reduces the decrease in output and increases the prices across the nation.[40]

Currently, there is only one empirical study that reveals the short-run output effect of antitrust enforcement. Clougherty conducted a cross-country study of 32 antitrust jurisdictions (mainly Organisation for Economic Co-operation and Development (OECD) members) from

32. *See* Yannis Katsoulacos & David Ulph, *Antitrust Penalties and the Implications of Empirical Evidence on Cartel Overcharges*, 123 ECON. J. 558, 559 (2013) (pointing out that enforcement could stop anticompetition conduct through deterrence or desistance).

33. The harm of anticompetitive conduct includes, but is not limited to, increasing prices, reducing outputs, hindering innovation, and slowing down economic growth. Jonathan B. Baker, *The Case for Antitrust Enforcement*, 17 J. ECON. PERSPECTIVES 27, 27 (2003).

34. Paolo Buccirossi et al., *Measuring the Deterrence Properties of Competition Policy: The Competition Policy Indexes*, 7 J. COMP. L. & ECON. 165, 168 n.8 (2011).

35. *See* Gregory J. Werden, *Assessing the Effects of Antitrust Enforcement in the United States*, 156 DE ECONOMIST 433, 434-37 (2008) (discussing price effects of cartel and cartel enforcement).

36. Buccirossi et al., *supra* note 34.

37. *Id.*

38. Reversing the harm of anticompetitive conduct. *See* Werden, *supra* note 35 (explaining the harm of anticompetitive conduct).

39. Katsoulacos & Ulph, *supra* note 32.

40. Reversing the harm of anticompetitive conduct. *See* Werden, *supra* note 35.

1992 to 2007.[41] This study explores the impact of the antitrust budget on economic growth.[42] Here, antitrust funding is used as a proxy for antitrust workloads.[43] Clougherty uses not only the average annual growth rate of output over five years as a dependent variable, but also yearly observations.[44] The former essentially investigates the long-run growth effect, while the latter investigates the short-run output effect.[45] Clougherty found that raising competition policy funding increases the percentage change in real per-capita gross domestic product (GDP) in both the short and long runs.[46] However, this study has two shortcomings. First, as mentioned, the author shows a positive relationship between antitrust funding and M&A workloads[47] but does not mention the impact on other critical antitrust workloads such as cartel and monopoly enforcement. Second, endogeneity issues are not addressed in this study.[48] For instance, while Clougherty wants to claim that a rise in antitrust workload has caused a higher percentage change in real per-capita GDP,[49] the causal relationship between the two variables, if it actually exists, could have worked in the opposite direction (i.e., a rise in real pre-capita GDP caused a higher antitrust workload).

Another limitation of Clougherty's work is that antitrust law could be enforced not only by the government (public enforcement) but also victims (private enforcement). Since Clougherty adopts the antitrust budget as the variable of interest, his work only reveals the impact of

41. Clougherty, *supra* note 17, at 112-13.

42. *Id.* at 113-14.

43. *Id.*

44. *Id.* at 122.

45. Clougherty uses yearly growth as dependent variable in order to smooth out business cycle effects. *Id.* at 121-22. In addition to Clougherty, I also found that Taylor, and Gutman and Voigt use yearly output as the dependent variable when exploring the impact of antitrust on output (i.e. showing a short-run effect). Taylor and Gutman and Voigt are different from Clougherty because the former study the impact of the existence or suspension of a competition law, while the latter study the impact of the enforcement of a competition law. *See* Jason E. Taylor*, Output Effects of Government Sponsored Cartels During the New Deal*, 50 J. INDUS. ECON. 1, 4-8 (2002); Gutmann & Voigt, *supra* note 24, at 9.

46. Clougherty, *supra* note 17, at 123.

47. *Id.* at 114, 118.

48. Gutmann & Voigt, *supra* note 24, at 4.

49. Clougherty, *supra* note 17, at 114, 118.

public enforcement on output.[50] This means that there has been no work done to quantify private enforcement's output effect. Considering the growing importance of private enforcement around the globe, this gap in the literature needs to be filled. To do so, this Article employs private antitrust case filing data in the United States from 1954 to 2019 and evaluates the impact of private enforcement on domestic output. The United States is an ideal target for this empirical study because it has more private cases than most, if not all, other countries. For example, between 2009 and 2018, on average, private plaintiffs brought 658 antitrust cases to the U.S. federal district courts each year.[51] This amounts to approximately 97 percent of total antitrust cases filed in the country over the decade.[52] Countries with only a few private cases recorded are likely to not be suitable for a nationwide study. This is because any effects that private enforcement may have might not be substantial enough to cause a notable change to output on a national level but might be observable on the industry level. Note that the dataset created for this project does not cover the years before 1954 because I am constrained by the availability of data.[53] In addition, I use the number of cases *filed* rather than the number of cases *decided* as a measure of private antitrust enforcement, because there is not a robust dataset of private antitrust decisions ready for use. And, as I will explain later, private antitrust enforcement could have an impact on output as early as in the filing stage.

The remainder of this article is organized as follows: Part II discusses my empirical model and the variables used; Part III presents

50. *Id.*

51. Figure based on data reported under table C-2 of the *Statistical Tables for the Federal Judiciary* published annually on 31st December by the Administrative Office of the United States Courts between 2009 and 2018. For example, in 2018, 526 out of 544 antitrust cases filed in federal district courts were private actions. STAT. TABLES FOR FED. JUDICIARY, TABLE C-2. U.S. DISTRICT COURTS—CIVIL CASES COMMENCED, BY BASIS OF JURISDICTION AND NATURE OF SUIT, DURING THE 12-MONTH PERIODS ENDING DECEMBER 31, 2017 AND 2018, https://www.uscourts.gov/file/25840/download [https://perma.cc/5MUN-MVAJ]. In comparison, among 30 European countries, only 53, 56 and 45 damages actions were judged for the first time in 2018, 2019 and 2020 respectively. Jean-François Laborde, *Cartel Damages Actions in Europe: How Courts Have Assessed Cartel Overcharges*, CONCURRENCES 232, 234-35 (2021).

52. STAT. TABLES FOR FED. JUDICIARY, *supra* note 51.

53. Penn World Table version 10.0 only provides total factor productivity data starting from 1954. *See infra* Table 1.

my results and performs a robustness check; Part IV discusses the results; and part V briefly concludes.

II. METHODOLOGY

A. EMPIRICAL SPECIFICATIONS

This study applies regression analysis[54] to evaluate the impact of private antitrust enforcement on GDP in the short run and long run. The models I use for the two evaluations are different. Below, I introduce the models and explain the choice of model.

1. Short Run Model

First, I address the impact of private antitrust enforcement on GDP in the short run. To obtain an unbiased estimate, the major challenge is endogeneity. On the one hand, there could be unobservable factors that lead to private enforcement and that also affect GDP. On the other hand, causation might not only occur in one direction. One may argue that victims are more willing to file antitrust suits against wrongdoers in years with a higher or lower GDP. This would be because victims and lawyers have more income to spend on litigation in years with high GDP (i.e., a high GDP causing more suits). Conversely, others could argue that the economic condition is poor in years with low GDP. Then, in these years, cartel members may be more likely to enter a dispute and end up suing each other (i.e., a low GDP causing more suits). These conflicting hypotheses suggest that private parties' decisions to enforce antitrust law could be endogenous.[55] There are two methods by which this problematic endogeneity can be corrected. First, researchers could lag the suspected variables by one or more periods.[56] This solution is

54. Regression analysis is a statistical method to study the relationship between the dependent variable and one or more independent variables. Regression analysis produces a regression equation where the coefficients represent the relationship between the dependent variable and each independent variable. GUJARATI & PORTER, *supra* note 23, at 21-22 & 25.

55. Similarly, some scholars suggest that public antitrust enforcement is endogenous. Joan-Ramon Borrell & Mara Tolosa, *Endogenous Antitrust: Cross-Country Evidence on the Impact of Competition-Enhancing Policies*, 15 APPLIED ECON. LETTERS 827, 827 (2008).

56. *See, e.g.*, Petersen, *supra* note 17, at 607-11.

based on the argument that even if current values of a variable X might be endogenous to current values of variable Y, it is unlikely that past values of X are subject to the same problem. However, as discussed, private enforcement (which is X in this study) could not only have a short-run but also a long-run impact on GDP (which is Y in this study).[57] If so, lagging the variable of private enforcement merely shows the long-run effect, if any, but not the short-run effect. Therefore, the lagging approach is not suitable for studying the short-run output effect.

The second group of solutions to the endogeneity problem is to use an instrumental variable.[58] This is the method this Article applies to deal with the endogeneity of private antitrust enforcement. More precisely, I use a dummy variable for two landmark antitrust decisions, both decided in 1977, as an instrumental variable and run a two-stage least squares regression ("2SLS regression").[59] The equation of the first-stage regression is in the form

$$Private = \gamma_0 + \gamma_1 Decisions + \gamma_x X + \varepsilon_1$$

where *Private* is the number of private antitrust case filings[60] and *Decisions* is the instrumental variable–a dummy variable for the two 1977 decisions–which is always included in the regression;[61] and X is a subset of variables chosen from a pool of explanatory variables of GDP.[62] From this first-stage regression, I obtain $\widehat{Private}$, the estimated

57. *See* discussions *supra* Part I.

58. An instrumental variable is a third variable (e.g., Z) used in regression analysis to estimate the causal relationship between the dependent variable (e.g., Y) and independent variable (e.g., X). *See* GUJARATI & PORTER, *supra* note 23, at 230 (explaining instrumental variables); Ma, *supra* note 17, at 315-17 (using an instrumental variable to address endogeneity).

59. Two-stage least squares (2SLS) is a specific type of regression analysis that uses instrumental variables to handle models with endogeneity. As the name suggests, 2SLS regression consists of two stages. In the first stage, the variable of interest is regressed on the instrumental variable and a set of independent variables. Then, the predicted values (i.e., fitted values) of the variable of interest could be obtained. In the second stage, the dependent variable is regressed on the predicted values of the variable of interest, and the same set of independent variables used in the first stage. GUJARATI & PORTER, *supra* note 23, at 362-66.

60. *See* discussion *infra* Section II.D.

61. *Id.*

62. *See* discussion *infra* Section II.C.

values of $Private$. Then, I use fitted values of $\widehat{Private}$ to run second-stage regression in the form

$$GDP = \beta_0 + \beta_1 \widehat{Private} + \beta_x X + \varepsilon_2$$

where GDP is per capita real GDP,[63] $\widehat{Private}$ is the variable of interest and is always included in the regression, and X is the same subset of explanatory variables of GDP mentioned above. Although I describe the two stages of regression separately, I regress the two stages simultaneously with a statistical tool to avoid getting incorrectly small standard errors. In the following parts, I will explain all the variables and data used for my 2SLS regression in more detail. It is important to note that I have transformed the dependent variable, the variable of interest, and most explanatory variables into (natural) logarithmic form.[64] This means that I am adopting a log-log model. The advantage of doing so is that the interpretation of the regression coefficients is straightforward. For instance, the β_1 coefficient would be interpreted as "a one percent increase in $\widehat{Private}$ is associated with a β_1 percent increase in GDP."[65]

2. Long Run Model

In contrast, there is no need for concern about the endogeneity problem when studying the impact of private antitrust enforcement on GDP in the long run. This is because, conceptually, there is no reason for us to believe that the current GDP affects the intensity of private enforcement in the past. Thus, a 2SLS regression is not required to study the long-run effect. Instead, I apply ordinary least squares (OLS) regression.[66] The long run model here uses the equation of the form

63. *See* discussion *infra* Section II.B.

64. The two commonly used bases for logarithmic transformation are 10 (i.e., common logarithms) and the mathematical number e (i.e. natural logarithms). There is a fixed relationship between the common log and natural log, so it does not matter which one is used. In economics, natural logs are usually preferred. Thus, I use the base e for all log transformation in this article. GUJARATI & PORTER, *supra* note 23, at 175-76.

65. *Id.* at 133-34.

66. OLS regression is a commonly used method to estimate the relationship between a dependent variable and one or more independent variables by minimizing the sum of the squared residuals. GUJARATI & PORTER, *supra* note 23, at 33-36.

$$GDP = \alpha_0 + \alpha_1(Private\ Lagged) + \alpha_x X + \varepsilon_3$$

where GDP is still per capita real GDP of the current year, X is the same subset of explanatory variables of GDP mentioned before, also of the current year, and $Private\ Lagged$ is a subset of three variables of interest. The three variables refer to the number of private antitrust case filings in the past. One of the three variables of interest is always included in the regression.[67] Like in my short run model, I use logs for variables on both sides of the econometric specification of my long run model. Therefore, the coefficient α_1 is the estimated percent change in GDP for a 1 percent change in $Private\ Lagged$.[68]

B. DEPENDENT VARIABLE

Previous studies of the effect of antitrust on growth have generally used the level[69] or growth rate of per capita real GDP as the dependent variable.[70] For both the short run and long run models, I use level per capita real GDP from 1954 to 2019 (in log form) as the outcome variable to evaluate the output effect of private antitrust cases filed in the United States. Data on real GDP and the population of the United States for the period were extracted from the U.S. Bureau of Economic Analysis[71] and the Federal Reserve Bank of St. Louis,[72] respectively.

67. *See* discussion *infra* Section II.D.

68. GUJARATI & PORTER, *supra* note 23, at 133-34.

69. *See* Taylor, *supra* note 45, at 6.

70. Gutmann & Voigt, *supra* note 24.

71. BUREAU ECON. ANALYSIS, TABLE 1.1.6. REAL GROSS DOMESTIC PRODUCT, CHAINED DOLLARS, https://apps.bea.gov/iTable/iTable.cfm?reqid=19&step=2&isuri=1&1921=survey#reqid=19&step=2&isuri=1&1921=survey [https://perma.cc/VJ9B-Y3YV].

72. *Population*, FED. RSRV. BANK ST. LOUIS, https://fred.stlouisfed.org/series/B230RC0A052NBEA [https://perma.cc/2QMR-R4B4] (last visited Apr. 12, 2022).

TABLE 1–VARIABLES, SOURCES AND DEFINITIONS

VARIABLE	SOURCE	DEFINITION
Population	Federal Reserve Bank of St. Louis	Total population (in log form).[73]
Human Capital Index	Penn World Table (version 10.0)	An index based on years of schooling and returns to education.[74]
Capital Stock	Penn World Table (version 10.0)	Capital stock in constant dollars (in log form).[75]
Total Factor Productivity	Penn World Table (version 10.0)	Total factor productivity in constant dollars.[76]
GDP/Capita (lagged 1 yr.)	U.S. Bureau of Economic Analysis Federal Reserve Bank of St. Louis	Gross domestic product per capita in constant dollars one year before (in log form).[77]
Openness Index	U.S. Bureau of Economic Analysis	Sum of exports and imports in chained dollars measured as a share of real GDP.[78]
Real Gov. Expenses	U.S. Bureau of Economic Analysis	Government consumption expenditures and gross investment in chained dollars (in log form).[79]
Real Unit Labor Cost	U.S. Bureau of Labor Statistics	Labor costs per unit of output of the business sector in constant dollars (in log form).[80]

73. FED. RSRV. BANK ST. LOUIS, *supra* note 72.

74. *Penn World Table Version 10.0*, UNIV. GRONINGEN, https://www.rug.nl/ggdc/productivity/pwt/?lang=en [https://perma.cc/9PCV-4GE4] (last updated June 18, 2021).

75. *Id.*

76. *Id.*

77. BUREAU ECON. ANALYSIS, *supra* note 71; FED. RSRV. BANK ST. LOUIS, *supra* note 72.

78. BUREAU ECON. ANALYSIS, *supra* note 71.

79. *Id.*

80. Calculated by

$$(Hourly\ Compensation) \times (Hours\ Worked) \times \frac{2012\ Price\ Deflator}{Current\ Price\ Deflator} \times \frac{1}{Output}$$

using annual level data. U.S. BUREAU LAB. STAT., LABOR PRODUCTIVITY AND COSTS MEASURES FOR BUSINESS AND NONFARM BUSINESS SECTORS, https://www.bls.gov/lpc/#tables [https://perma.cc/RD8G-V3QJ].

VARIABLE	SOURCE	DEFINITION
Real Unit Total Cost	U.S. Bureau of Labor Statistics	Labor and nonlabor costs per unit of output of the business sector in constant dollars (in log form).[81]
Public Enforcement	U.S. Courts	Number of antitrust cases filed by the government in district courts (in log form).[82]
Real Money Supply	Federal Reserve System U.S. Bureau of Labor Statistics	M2 money stock in constant dollars (in log form).[83]
Expected Inflation	University of Michigan	Consumers' expected changes in inflation rates next year.[84]

81. Calculated by

$$(Hourly\ Compensation) \times (Hours\ Worked) \times \frac{2012\ Price\ Deflator}{Current\ Price\ Deflator} \times \frac{1}{Output} \times \frac{1}{Labor\ Share}$$

using annual level data. *Id.*

82. Data from 1954 to 1992 were obtained from *Annual Report of the Director* published by the Administrative Office of the U.S. Courts on various issues between 1954 and 1992. Data from 1993 to 2008 were obtained from *Federal Judicial Caseload Statistics* published by the U.S. Courts on various issues between 1993 and 2008. Data from 2009 to 2017 were obtained from *Judicial Business of the United States Courts* published by the U.S. Courts on various issues between 2009 and 2017.

83. I use consumer price index for all urban consumers (current series) to convert nominal money supply to 2012 constant dollar. *Data Download Program*, FED. RSRV. SYS., https://www.federalreserve.gov/datadownload/Download.aspx?rel=H6&series=de0d7f93074e32cae3e619735d9cc5ce&filetype=spreadsheetml&label=include&layout=seriescolumn&from=01/01/1959&to=01/31/2020 [https://perma.cc/NY7G-JKSS] (last visited Feb. 12, 2020); U.S. BUREAU LAB. STAT., CONSUMER PRICE INDEX (CPI) DATABASES, https://www.bls.gov/cpi/data.htm [https://perma.cc/KWK8-NPKX] (last visited Feb. 12, 2020).

84. UNIV. MICH. SURV. CONSUMER, EXPECTED CHANGES IN INFLATION RATES, http://www.sca.isr.umich.edu/files/tbcpx1px5.pdf [https://perma.cc/ZLF3-QMFY].

C. INDEPENDENT VARIABLES

Table 1 presents the subset of independent variables I use for the 2SLS regression and provides their sources and definitions. Below, I explain why I control for these variables.

Population, Human Capital Index, Capital Stock, & Total Factor Productivity. These variables are the backbone of my regression model. I include them in the model because leading economic theories suggest that the growth of economies is driven by the aggregate of capital stock, labor, and productivity.[85] I control for population rather than the number of people employed. This is because the former affects not only the supply of labor but also the demand for goods and services. *Human Capital Index* is included to reflect the productivity of labor, while *Total Factor Productivity* is included to control the state of technology, which explains the portion of growth not explained by changes in labor and capital used in production.

Real GDP/Capita (lagged 1 yr.). This refers to the real GDP per capita in the United States for the period from 1953 to 2018. Recall that the output variable is real GDP per capita from 1954 to 2019. Thus, the model regresses the current output on the output one year ago. This controls the effect of output today on output one year ahead. If the country had a high GDP per capita in a given year, it would probably have a similarly high GDP per capita in the following year.

Openness Index. Previous empirical studies suggest that trade openness affects antitrust policy[86] and economic growth; consequently, they control for an openness index.[87] I do the same to capture the impact of the degree of the openness of the United States to the world trade system over the years.

Real Gov. Expenses & Real Money Supply & Real Gov. These variables are used to capture the effect of government interventions on growth. *Real Gov. Expenses* is included as a proxy to control for the impact of fascial policies, while *Real Money Supply* is added to the model to control for the impact of monetary policies. It is noteworthy that the Federal Reserve only provides money supply data since 1959, so

85. PHILIPPE AGHION & PETER HOWITT, ENDOGENOUS GROWTH THEORY Ch. 1-3 (1998).

86. Petersen, *supra* note 17, at 613.

87. Dutz & Hayri, *supra* note 17, at 6; Clougherty, *supra* note 17, at 120.

employing such data would cost 8 percent of observations.[88] Thus, I did not give priority to this variable when adding controls to the regression models.

Time. This variable is not listed in Table 1, as it is simply a self-created variable to control for the time trend on growth, if any, and does not have a source. For *Time*, I code 1 for the first year (1954), 2 for the second year (1955), etc.

Real Unit Labor Cost & Real Unit Total Cost. Either one of these variables is included to control for the cost of production, which affects the aggregate supply in the economy. These costs are calculated using the data of the U.S. business sector because the U.S. Bureau of Labor Statistics does not publish productivity measures for the total economy.[89] The business sector data is the best alternative because it is the broadest measure of productivity the Bureau publishes, and it accounts for approximately 75 percent of the nation's GDP.[90]

Public Enforcement. Like private enforcement, antitrust cases filed in court by antitrust authorities should have an impact on GDP. Therefore, I control for it. I do not consider *Public Enforcement* as the variable of interest, and it is always included it in the model because I could not find a suitable instrumental variable to cope with the endogeneity problem associated with public actions.[91] In any event, in the case of the United States, the impact of public enforcement should be far less significant than the impact of private enforcement, as merely 3 percent of total antitrust cases filed in the country in the last decade were public enforcement actions.[92]

Expected Inflation. This variable is used to capture people's perception of future economic conditions. Expected inflation is a critical indicator, since it affects both aggregate demand and aggregate supply. On the one hand, when people expect inflation to increase, their demand for money decreases.[93] Holding all else equal, this causes the real interest rate to fall. Consequently, firms are more eager to invest and

88. FED. RSRV. SYS., *supra* note 83.

89. *Labor Productivity and Cost - Frequently Asked Questions (FAQs)*, U.S. BUREAU LAB. STAT., https://www.bls.gov/lpc/faqs.htm#P08 [https://perma.cc/24R4-XACU] (last visited Feb. 12, 2020).

90. *Id.*

91. *See* discussions *supra* Section II.A.

92. STAT. TABLES FOR FED. JUDICIARY, *supra* note 51.

93. ANDREW B. ABEL ET AL., MACROECONOMICS 327, 346 (8th ed. 2014).

aggregate demand rises.[94] On the other hand, when firms expect goods and services to sell at a higher price in the future, they will be less willing to sell now.[95] Thus, an increase in expectations for inflation causes the short-run aggregate supply to drop.[96] The University of Michigan publishes *Surveys of Consumers* that reports consumers' expected change in the inflation rate for the next year.[97] However, the survey data do not cover years 1954 to 1965 and 1977,[98] so employing the survey data would cost 20 percent of observations. In light of this limitation, I did not give priority to this variable while adding controls to the regression models.

D. VARIABLE OF INTEREST & INSTRUMENTAL VARIABLE

The variables of interest of the short run model and the long run model are different. In the short run model, the variable of interest is *Private*, the number of private antitrust cases filed to the District Courts in the United States for the period from 1954 to 2019. Such data are acquired from multiple sources. To offer a more holistic view of the changes in private antitrust case filings over time, I prepared Figure 1, which covers the filing numbers from 1890, when the country passed its first antitrust law, the Sherman Act,[99] to 2019.[100] Note that, similar to some of the

94. *Id.*

95. *Sellers' Expectations, Supply Determinant*, AMOSWEB, http://www.amosweb.com/cgi-bin/awb_nav.pl?s=wpd&c=dsp&k=sellers%27+expectations,+supply+determinant [https://perma.cc/MGC5-KK5T] (last visited Feb. 12, 2020).

96. *Id.*

97. UNIV. MICH. SURV. CONSUMER, *supra* note 84.

98. *Id.*

99. An Act to Protect Trade and Commerce Against Unlawful Restraints and Monopolies, ch. 647, 26 Stat. 209 (1890) (current version at 15 U.S.C. §§ 1-7) [hereinafter Sherman Act].

100. All data presented in this figure, except for those between 1890 and 1936, are the actual number of private antitrust cases filed. Data from 1890 to 1936 were estimates of cases filed based on the actual number of cases decided in each year, obtained from records of the 89th U.S. Congress Hearings. *Nolo Contendere and Private Antitrust Enforcement: Hearings on S. 2512 Before the Subcomm. on Antitrust & Monopoly of the Senate Comm. on the Judiciary*, 89th Cong. 180-99 (1966). These estimates are the product of actual case numbers and multipliers suggested by Hon. Richard A. Posner in his empirical study. Richard A. Posner, *A Statistical Study of Antitrust Enforcement*, 13 J.L. & ECON. 365, 371 (1970). Data from 1937 to 1940 and from 1944 to 1992 were obtained from the *Annual Report of the Director* published by the Administrative Office of the U.S. Courts on various issues between 1940 and 1992.

other variables, the filing numbers are transformed to log form for the regression analysis.

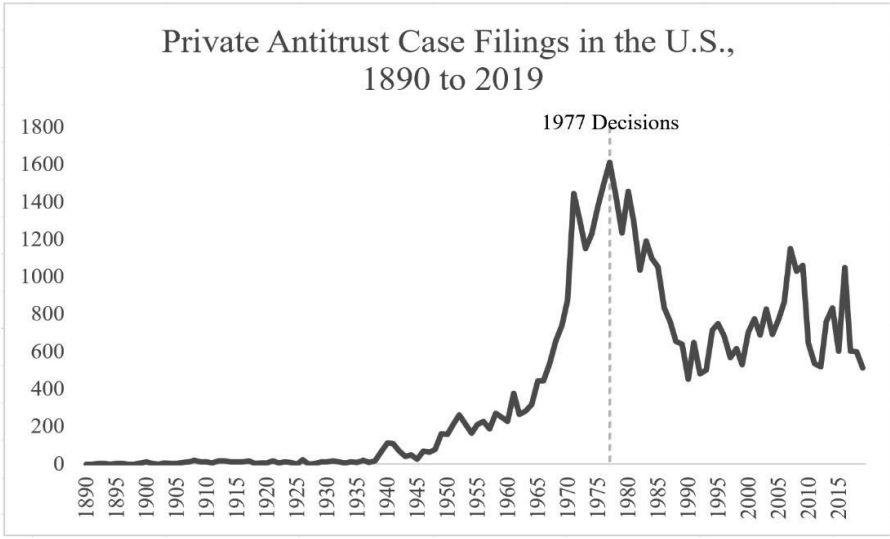

FIGURE 1

Data from 1941 to 1943 were obtained from records of the 89th U.S. Congress Hearings. *Nolo Contendere and Private Antitrust Enforcement: Hearings on S. 2512 Before the Subcomm. on Antitrust & Monopoly of the Senate Comm. on the Judiciary*, 89th Cong. 159 (1966). Data from 1993 to 2019 were obtained from the *Federal Judicial Caseload Statistics* published by the U.S. Courts on various issues between 1993 and 2019. Note that in 1960, the U.S. government brought an antitrust case against 29 companies that supplied electrical equipment. Then, from 1962 to 1967, more than one thousand and nine hundred private individuals separately filed followed-on suits against those companies. These private electrical equipment cases are not included in our figure because no other cartel before or after has generated a comparable level of private actions. Similar cases thereafter have been consolidated, and plaintiffs would be grouped. *See* Paul E. Godek, *Does the Tail Wag the Dog? Sixty Years of Government and Private Antitrust in the Federal Courts*, 9 ANTITRUST SOURCE 1, 1-2 (2009) (showing an example of a similar chart that reported electrical equipment cases separately for these reasons). In a 2012 publication, a similar chart was presented to show the private antitrust case filings from 1950 to 2011. In that chart, these electrical equipment cases were also excluded. Otherwise, the filing numbers from 1962 through 1967 reported in that chart would be higher. *See* William Kolasky, *Antitrust Litigation: What's Changed in Twenty-Five Years?*, 27 ANTITRUST 1, 9 (2012) (showing a similar figure published in 2012). Further, when the Administrative Office of the U.S. Courts publish the antitrust case filing statistics in the 1960s, they also report "Antitrust electrical equipment" separately from all other antitrust cases (i.e., "Antitrust other").

As explained, I introduce an instrumental variable and run a 2SLS regression to address the endogeneity of private antitrust enforcement.[101] The sole instrumental variable I use is a dummy variable created for two landmark antitrust decisions made by the Supreme Court in 1977: *Brunswick Corp. v. Pueblo Bowl-O-Mat*[102] and *Illinois Brick Co. v. Illinois*.[103] Both Supreme Court decisions have not been overruled and are still good law, so I created the *Decisions* variable and coded one for all the years on or after 1977, and zero otherwise.

In January 1977, the U.S. Supreme Court decided *Brunswick*, a classic antitrust case challenging the acquisition of bowling alleys. Brunswick Corp., a bowling equipment manufacturer, was acquiring and operating bankrupt bowling alleys.[104] Several owners of still-operating bowling alleys sued, arguing that they would have earned more profits if Brunswick had not acquired the bankrupt alleys, because they would have left the market and stopped competing with the small bowling alleys.[105] Thus, plaintiffs claimed, Brunswick's act had substantially lessened competition or tended to create a monopoly, violating Section 7 of the Clayton Act.[106] The Supreme Court rejected this argument and established the requirement that the injury asserted by plaintiffs in private actions must result from the anti-competitive effect of the defendant's conduct, but not flow from competition created by the defendant.[107] This is known as "antitrust injury."[108] The antitrust injury requirement has created an obstacle to private enforcement.[109]

In June of that same year, the Supreme Court decided *Illinois Brick*, where some concrete block producers were accused of conspiring to sell blocks at a marked-up price to wholesalers (direct purchasers).[110] The state of Illinois and hundreds of local governmental entities bought blocks from these wholesalers.[111] The government sued the concrete

101. *See supra* Section II.A.
102. 429 U.S. 477 (1977).
103. 431 U.S. 720 (1977).
104. *Brunswick Corp.*, 429 U.S. at 479.
105. *Id.*
106. *Id.* at 480.
107. *Id.* at 489-90.
108. *Id.* at 489; ERNEST GELLHORN ET AL., ANTITRUST LAW AND ECONOMICS IN A NUTSHELL 546 (5th ed. 2004).
109. *Id. See* Baker, *supra* note 33, at 42.
110. Ill. Brick Co. v. Illinois, 431 U.S. 720, 726-27 (1977).
111. *Id.*

block producers for damages under Section 1 of the Sherman Act, claiming that it had suffered harm from the alleged price-fixing scheme because the wholesalers passed some of the illegal overcharges along to the government (i.e., indirect purchasers) by increasing the wholesale price.[112] The Supreme Court ruled in favor of the defendant producers and held that indirect purchasers have no standing to sue for antitrust violations, meaning only direct purchasers could sue under federal antitrust law.[113] The creation of the "direct purchaser" rule removed many victims' right to bring antitrust lawsuits and reduced the number of private antitrust actions.[114]

Both 1977 decisions limit private parties' ability to file antitrust lawsuits and suppressed private antitrust enforcement thereafter. The private antitrust case filing numbers were at a historic high of 1,611 counts in the year *Brunswick* and *Illinois Brick* were decided.[115] After the two decisions, the number of filings dropped rapidly, and it has never climbed back anywhere close to the 1977 peak.[116]

The result of a 2SLS regression is only as good as the instrumental variable used. A valid instrumental variable must satisfy three assumptions, namely the relevance assumption, the independence assumption (also known as exchangeability assumption), and the exclusion restriction.[117] I argue that *Decisions* is a suitable instrumental variable because it largely meets the three assumptions.

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112. *Id.*

113. Michael D. Hausfeld et al., *Litigating Indirect Purchaser Claims: Lessons for the EU from the U.S. Experience*, 32 ANTITRUST 58, 59 (2018).

114. *See* Thomas E. Kauper & Edward A. Snyder, *An Inquiry into the Efficiency of Private Antitrust Enforcement: Follow-on and Independently Initiated Cases Compared*, 74 GEO. L.J. 1163, 1179 (1986) (suggesting that the *Illinois Brick* decision has caused the number of private antitrust actions to drop after 1978).

115. ADMIN. OFF. U.S. CTS., ANNUAL REPORT OF THE DIRECTOR 318 (1977), https://www.ojp.gov/ncjrs/virtual-library/abstracts/administrative-office-united-states-courts-annual-report-director-1 [https://perma.cc/8LTV-SHUN]. *See* Sinchit Lai, *Enabling and Incentivizing Standalone Private Antitrust Actions in Hong Kong—Lessons from the United States,* 16 BERKELEY BUS. L.J. 463, 495 (2019) (explaining the rising trend of private antitrust enforcement started from 1938).

116. *See supra* Figure 1.

117. Jeremy Labrecque & Sonja A. Swanson, *Understanding the Assumptions Underlying Instrumental Variable Analyses: A Brief Review of Falsification Strategies and Related Tools*, 5 CURRENT EPIDEMIOLOGY REPS. 214, 214 (2018).

The relevance assumption requires an instrument to be significantly related to the treatment.[118] In our case, this means the two 1977 Supreme Court decisions (*Decision*) had to be significantly related to the private antitrust case filing numbers (*Private*). To verify this, I ran the first-stage regression and present the results in Table 2. The results show that the coefficient on the instrumental variable *Decisions* has the predicted sign (negative).[119] More importantly, the *Decisions* variable is significantly correlated with the endogenous variable *Private*.[120]

The exclusion restriction and independence assumption present unique problems in verification because, unlike the relevance assumption, neither could rely on a formal statistical test for validity.[121] The independence assumption holds if the instrumental variable (*Decision*) does not share a common cause with the outcome variable (*GDP*).[122] Meanwhile exclusion restriction holds if an instrument variable (*Decision*) affects the outcome (*GDP*) only through the treatment (*Private*).[123] Because neither assumption can be verified through formal analysis, it is imperative that an analysis is undertaken based on subject matter knowledge.[124]

Starting with the independence assumption, adopting the 1977 decisions as an instrumental variable likely satisfies this assumption because the timing and outcome of the decisions are somewhat random. Generally, court decisions are preceded by underlying incidents and controversies, which could occur at any time. The courts have no control over when, where, or how potentially violative conduct arises; nor do they have control over whether those harmed ultimately bring suit. Further, the Supreme Court receives 7,000 petitions for a hearing on average annually, but it only agrees to hear 100 to 150 of them (a

118. *See id.* at 215.

119. *See supra* Table 2.

120. *Id.*

121. Mette Lise Lousadal, *An Introduction to Instrumental Variable Assumptions, Validation and Estimation*, 15 EMERG. THEMES EPIDEMIOL 1, 4-5 (2018).

122. Jeremy Labrecque & Sonja A. Swanson, *Understanding the Assumptions Underlying Instrumental Variable Analyses: A Brief Review of Falsification Strategies and Related Tools*, 5 CURRENT EPIDEMIOLOGY REPS. 214, 215 (2018).

123. *Id.* at 216.

124. Lousadal, *supra* note 121.

success rate of some 2 percent).[125] William Rehnquist, one of the Associate Justices who decided the two 1977 cases,[126] commented that whether or not a case is accepted by the Supreme Court is "a rather subjective decision, made up in part of intuition and in part of legal judgment."[127] Regarding antitrust in particular, the vagueness and breadth of federal competition statutes are such that the Court has relied less on statutory interpretation and more on the common law process to develop the law.[128] Therefore, case outcomes depend heavily on, *inter alia*, the time and composition of the Court when the case is heard. This means that a case could result oppositely if it was heard in a different year or by a different group of justices. For instance, the decision in *Illinois Brick Co. v. Illinois* was not unanimous.[129] And some jurisdictions, such as the European Union, do have a rule different from the United States that allows indirect purchasers to sue for damages.[130]

Turning to the exclusion restriction, to understand why the two 1977 decisions likely satisfy this assumption and serve as a valid instrumental variable, one may consider an example of an *invalid* instrument, *Bell Atlantic Corp. v. Twombly*.[131] *Twombly* is a 2007 landmark civil procedure and antitrust case, where one of the key allegations was that several large telecommunication companies engaged in parallel conduct to hinder the growth of their smaller competitors.[132] The defendants moved to dismiss the complaint for failing to state a claim under Rule 12(b)(6) of the Federal Rules of Civil

125. *Supreme Court Procedures*, U.S. CTS., https://www.uscourts.gov/about-federal-courts/educational-resources/about-educational-outreach/activity-resources/supreme-1 [https://perma.cc/5DTF-WV9E] (last visited Apr. 12, 2022).

126. William Rehnquist served on the U.S. Supreme Court for 33 years, first as an Associate Justice from 1972 to 1986 and then as a Chief Justice from 1986 to 2005. *Justices 1789 to Present*, SUP. CT. U.S., https://www.supremecourt.gov/about/members_text.aspx [https://perma.cc/T7XZ-GWE2] (last visited Apr. 12, 2022).

127. *Choosing Cases*, WASH. POST, https://www.washingtonpost.com/wp-srv/national/longterm/supcourt/history/choosing.htm [https://perma.cc/9N7V-5TNW] (last visited Apr. 12, 2022).

128. Rebecca Allensworth, *Amicus Briefs and the Sherman Act: Why Antitrust Needs a New Deal*, 89 TEX. L. REV. 1247, 1248 (2011).

129. This was a 6-3 decision. Ill. Brick Co. v. Illinois, 431 U.S. 720, 720-23 (1977).

130. Hausfeld et al., *supra* note 113, at 58.

131. 550 U.S. 544 (2007).

132. Thomas P. Gressette Jr., *The Heightened Pleading Standard of* Bell Atlantic Corp. v. Twombly *and* Ashcroft v. Iqbal: *A New Phase in American Legal History Begins*, 58 DRAKE L. REV. 401, 418 (2010).

Procedure (FRCP).[133] The Supreme Court ruled in favor of the defendants, clarifying the pleading requirements under FRCP Rule 8(a)(2), and holding that alleging parallel conduct alone is insufficient to prove a violation of Section 1 of the Sherman Act.[134] Instead, a complaint must allege "enough facts to state a claim to relief that is plausible on its face."[135] As such, *Twombly* heightened the pleading requirements that had been set forth by the same court in *Conley v. Gibson* in 1957.[136] Since cartels are normally conducted secretly and critical evidence is controlled by the cartels, *Twombly* is unduly harsh on plaintiffs who can struggle to meet the pleading standard.[137] It is no wonder *Twombly* has discouraged victims from filing antitrust actions. As shown in Figure 1, private antitrust case filing numbers increased from the early 1990s to 2007, the year *Twombly* was decided, and then, in subsequent years, the numbers dropped sharply.[138] Thus, *Twombly* is likely to satisfy the instrument relevance assumption but, as discussed below, it cannot satisfy the exclusion restriction.

This inability to act as the exclusion restriction is because Twombly's impact extends beyond the field of antitrust: the decision changed the pleading standard for dismissal under the FRCP, affecting all types of federal civil actions.[139] As such, the chilling effect of *Twombly* applies not only to antitrust lawsuits, but also to all other federal civil actions. This means *Twombly* could influence GDP through channels other than the private antitrust case filing numbers (*Private*).

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133. *Id.* at 419.

134. *Id.* at 420.

135. *Id.*; *Twombly*, 550 U.S. at 570.

136. Gressette Jr., *supra* note 132, at 420. In *Conley*, the Court set forth that "a complaint should not be dismissed for failure to state a claim unless it appears beyond doubt that the plaintiff can prove no set of facts in support of his claim which would entitle him to relief." Conley v. Gibson, 355 U.S. 41, 45-46 (1957).

137. HERBERT HOVENKAMP, PRINCIPLES OF ANTITRUST 156 (2017).

138. *Supra* Figure 1, *supra* note 100.

139. In the two years following *Twombly*, there were vigorous disagreements about whether *its* plausibility standard applied to all types of federal actions (i.e., not just antitrust cases). That said, in those two years, "the courts generally assumed a more restricted application." The discussion was halted when the Supreme Court confirmed in *Ashcroft v. Iqbal* the applicability of the heightened pleading standard in non-antitrust cases. Twombly & Iqbal – *The New Federal Pleading Standard Plaintiffs' Attorneys Must Stop and Take Notice Of*, TRIAL REP. (Dec. 7, 2010), https://www.hcinjurylaw.com/twombly-iqbal-new-federal/ [https://perma.cc/L3Q6-CUCZ]. *See generally* Ashcroft v. Iqbal, 556 U.S. 662 (2009).

For instance, the harsher pleading requirements might discourage victims from bringing employment discrimination lawsuits, adversely affecting GDP by reducing labor market efficiency). Therefore, because *Twombly* affects the outcome *GDP* not only through the treatment *Private*, *Twombly* violates the exclusion restriction and cannot serve as a valid instrument for the 2SLS regression analysis.

In contrast, the instrument selected for this Study–the 1977 Supreme Court decisions–does not suffer from the same problem. The 1977 Supreme Court decisions satisfy exclusion restriction because they only changed antitrust law, more specifically, they only changed the rule in relation to private antitrust enforcement. Conceptually, the sole channel through which the 1977 decisions (*Decisions*) could influence GDP is by affecting the number of private antitrust enforcement actions (*Private*). Therefore, the 1977 decisions satisfy exclusion restriction along with the other two assumptions, and can serve as a valid instrument for the short-run regression model.

TABLE 2 – U.S. PRIVATE ANTITRUST ENFORCEMENT REGRESSIONS (DEPENDENT VARIABLE: NO. OF PRIVATE ANTITRUST CASE FILINGS IN LOG FORM)

	MODELS									
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Decisions	-0.426***	-0.422***	-0.389***	-0.447***	-0.370***	-0.378**	-0.371**	-0.378**	-0.251*	-0.480**
	(0.137)	(0.143)	(0.137)	(0.146)	(0.136)	(0.143)	(0.141)	(0.144)	(0.131)	(0.185)
Population	-3.432	-3.010	-0.106	-1.031	2.840	5.360	2.829	3.545	5.857	33.18**
	(4.036)	(4.352)	(5.176)	(4.933)	(4.304)	(5.038)	(4.346)	(4.759)	(7.655)	(13.34)
Human Capital Index	10.22***	10.29***	9.466***	7.898***	6.634***	5.158***	6.632***	6.726***	5.138***	-1.836
	(0.789)	(0.811)	(1.052)	(1.532)	(1.225)	(1.827)	(1.238)	(1.216)	(1.893)	(3.601)
Capital Stock	-6.001***	-6.377**	-7.183**	-4.756*	-1.735	-0.713	-1.721	-2.040	0.488	5.371
	(1.740)	(2.669)	(2.792)	(2.793)	(2.846)	(2.924)	(2.923)	(2.914)	(3.264)	(5.859)
Total Factor Productivity	7.982***	7.909***	8.388***	9.392***	9.734***	7.633***	9.728***	9.688***	2.674	-1.154
	(2.182)	(2.189)	(2.173)	(2.386)	(2.253)	(2.860)	(2.252)	(2.244)	(3.061)	(3.504)
GDP/Capita (lagged 1 yr.)		0.243	1.080	1.618	1.472	2.977	1.468	1.600	1.737	-1.072
		(1.621)	(1.557)	(1.557)	(1.340)	(2.019)	(1.339)	(1.403)	(1.559)	(2.231)
Openness Index			-3.208	-6.681*	-3.822	-5.488	-3.818	-3.813	2.589	-10.43
			(2.618)	(3.944)	(3.459)	(3.777)	(3.474)	(3.499)	(5.369)	(6.303)
Real Gov. Expenses				-1.468	-2.526***	-3.126***	-2.526***	-2.548***	-2.571**	-3.028**
				(0.981)	(0.809)	(0.946)	(0.817)	(0.853)	(0.969)	(1.268)
Time					-0.0946***	-0.129***	-0.0947***	-0.0976***	-0.201***	-0.369***
					(0.0318)	(0.0402)	(0.0325)	(0.0341)	(0.0540)	(0.0798)
Real Unit Labor Cost						-3.129				
						(2.649)				
Real Unit Total Cost							-1.418	-0.986	-7.871	-93.21
							(79.06)	(79.03)	(80.61)	(69.43)
Public Enforcement								-0.0279	-0.0706	-0.0352
								(0.115)	(0.115)	(0.124)
Real Money Supply									1.576***	2.480***
									(0.535)	(0.712)
Expected Inflation										0.102**
										(0.0397)
Constant	88.78***	90.26***	82.45***	66.25***	16.14	-0.676	22.83	21.20	-3.145	234.2
	(10.36)	(13.06)	(15.67)	(18.44)	(20.86)	(24.45)	(375.6)	(376.3)	(392.5)	(330.5)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.817	0.817	0.822	0.827	0.851	0.854	0.851	0.851	0.804	0.731

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

After reviewing the variable of interest and instrumental variable of the short run model, I now turn to explain the subset of variable of interest of the long run model–*Private Lagged.*[140] Under the set, there are three variables:

Private Enforcement Lagged 3 Years. This refers to the number of private antitrust cases filed to the U.S. district courts for the period from 1951 to 2016 (instead of 1954 to 2019). Recall that the output variable is level per capita real GDP from 1954 to 2019. Thus, the model regresses the current output on the private enforcement 3 years ago. This analyzes the effect of private enforcement today on output 3 years later.

Private Enforcement Lagged 6 Years. Similarly, this variable of interest is obtained by lagging *Private*. The sole difference between this variable and the previous variable is that the former is lagged 6 years, while the latter is lagged 3 years. This means that the current variable incorporates the number of private antitrust cases filed in the United States for the period from 1948 to 2013. The current variable is created to study the effect of private enforcement today on output 6 years later.

Private Enforcement Lagged 9 Years. This variable refers to the number of private antitrust cases filed in the U.S. district courts for the period from 1945 to 2010 (lagged 9 years). It is employed to analyze the effect of private enforcement today on output 9 years ahead.

The data source of these three variables of interest is the same as that of *Private.*[141] As explained, there is no endogeneity of private enforcement in the long run, so a 2SLS regression is not required to analyze the long-run output effect. Instead, I applied OLS regression;[142] therefore, the long-run model has no instrumental variable. Only one of the three variables of interest appears in the regression each time. In other words, I ran three OLS regressions to examine the 3-year, 6-year, and 9-year output effects separately. Comparing the results of these three regressions allows for an evaluation of how long-lasting the effect of private enforcement is on output, if there is any effect.

140. *See* Section II.A.2.

141. *See supra* Figure 1; *supra* note 100.

142. *See* GUJARATI & PORTER, *supra* note 23, at 33-36 (explaining the OLS regression method).

III. RESULTS & ROBUSTNESS CHECK

A. RESULTS

Table 3 summarizes the descriptive statistics for the explanatory variables used in the analyses before log transformation. In bivariate analysis, a statistical analysis of two variables to determine the relationship between them, the dependent variable real GDP per capita is weakly and positively correlated with the number of private antitrust case filings in the current year, lagged 3 years, lagged 6 years, and lagged 9 years (Pearson coefficients 0.14, 0.26, 0.34, and 0.46, respectively).

TABLE 3– DESCRIPTIVE STATISTICS ON VARIABLES BEFORE LOG TRANSFORMATION

VARIABLES	MEAN	SD	MINIMUM	MAXIMUM	OBSERVATIONS
Dependent Variables:					
Real GDP/Capital (millions)	35,468	12,781	16,504	58,113	66
Explanatory Variables:					
Actual Private Enforcement (counts)	757.14	370.83	163	1,611	66
Private Enforcement lagged 3 yrs. (counts)	741.48	385.46	16	1,611	66
Private Enforcement lagged 6 yrs. (counts)	709.80	403.54	78	1,611	66
Private Enforcement lagged 9 yrs. (counts)	684.65	425.53	27	1,611	66
Population (millions)	246.40	49.38	162.39	328.53	66
Human Capital Index	3.32	0.35	2.63	3.75	66
Capital Stock (millions)	37,187,081	17,849,101	12,073,594	69,059,072	66
Total Factor Productivity	0.84	0.10	0.65	1.02	66
Real GPD/Capita of last year (millions)	34,844	12,664	16,504	57,006	66
Openness Index	0.17	0.09	0.06	0.32	66
Real Government Expenses (millions)	2,145,194	755,272	971,608	3,307,322	66
Real Unit Labor Cost (dollars)	74.71	3.18	67.93	79.29	66
Real Unit Total Cost (dollars)	121.89	0.068	121.60	121.99	66
Public Enforcement (counts)	28.88	13.62	9	73	66
Real Money Supply (billions)	6,255	2,903	2,345	13,831	61
Expected Inflation (%)	3.61	1.59	2.10	9.70	53

Table 4 reports the 2SLS regression results of the impact of private enforcement on real GDP per capita in the short run. The results are reasonably consistent with existing growth theories and my expectations, as the coefficients on all independent variables, except for openness index, government expenses, and labor cost, have the predicted sign. The three variables that do not have the predicted sign are not worrying because they do not always have statistical significance across the 10 models under Table 4. The regression analysis reverses the positive correlations observed in the bivariate analysis. The associations achieve statistical significance on the $p < 0.1$, $p < 0.05$ or $p < 0.01$ levels.[143] Thus, 1 percent increases in private enforcement are associated with approximately 0.0389 to 0.138 percent decreases in real GDP per capita.

Next, it is important to look at the OLS regression results of the impact of private enforcement on real GDP per capita in the long run. Tables 5 and 6 show that private enforcement has a negative effect on real GDP per capita 3 and 6 years ahead, respectively. Again, the results reverse the positive correlations observed in the bivariate analysis and are statistically significant on the $p < 0.1$, $p < 0.05$, or $p < 0.01$ levels. One percent increase in private enforcement is associated with approximately 0.0284 to 0.0362 percent decrease in real GDP per capita 3 years later, and 0.0178 to 0.0280 percent decrease 6 years later.

The situation is different for the effect of private enforcement on real GDP per capita in the very long run. Table 7 reveals that private enforcement lagged 9 years also has a negative effect on output. However, the associations do not achieve statistical significance in almost all the 10 models under Table 7. This means that the negative effect recorded is likely to have occurred by chance only, hence the impact private enforcement has on real GDP per capita in the long run is undetermined

143. Statistical significance helps to measure whether a finding is likely due to chance or to some factor of interest, hence determine the meaningfulness of the research finding. A statistically significant finding is likely due to the factor of interest rather than by chance, hence is meaningful, and vice versa. GUJARATI & PORTER, *supra* note 23, at 503-07. To test statistical significance, there are three commonly used p-value thresholds: 0.1, 0.05 and 0.01. They correspond to a confidence interval of 90%, 95% and 99% respectively. The lower the p-value thresholds, the harder is to show statistical significance. Thus, when an association achieves statistical significance at a lower p-value threshold (e.g., $p<0.01$), the stronger is the evidence that the association was not obtained at random. *Id.* at 75-76.

TABLE 4–U.S. OUTPUT REGRESSIONS (DEPENDENT VARIABLE: REAL GDP PER CAPITA IN LOG FORM)

	MODELS									
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Private Enforcement	-0.0389*	-0.0590**	-0.0667**	-0.0568**	-0.0756**	-0.0764***	-0.0710**	-0.0728**	-0.0970*	-0.138***
	(0.0213)	(0.0243)	(0.0259)	(0.0234)	(0.0300)	(0.0287)	(0.0286)	(0.0288)	(0.0533)	(0.0410)
Population	-1.888***	-1.097***	-0.828**	-0.888***	-0.483	-0.573	-0.543	-0.603	-0.255	1.802
	(0.215)	(0.293)	(0.378)	(0.322)	(0.372)	(0.417)	(0.361)	(0.366)	(0.809)	(1.873)
Human Capital Index	0.255	0.607***	0.602***	0.405*	0.415*	0.474**	0.377*	0.381*	0.471	-0.00388
	(0.216)	(0.235)	(0.233)	(0.226)	(0.227)	(0.213)	(0.225)	(0.225)	(0.301)	(0.410)
Capital Stock	1.053***	0.166	0.0364	0.268	0.510**	0.471*	0.575**	0.600**	0.589*	1.171*
	(0.166)	(0.268)	(0.309)	(0.260)	(0.251)	(0.251)	(0.251)	(0.258)	(0.308)	(0.644)
Total Factor Productivity	1.352***	1.363***	1.472***	1.456***	1.670***	1.754***	1.601***	1.622***	1.225***	1.129***
	(0.299)	(0.288)	(0.314)	(0.322)	(0.375)	(0.344)	(0.361)	(0.365)	(0.316)	(0.406)
GDP/Capita (lagged 1 yr.)		0.495***	0.581***	0.606***	0.621***	0.567***	0.598***	0.589***	0.702***	0.558**
		(0.149)	(0.157)	(0.140)	(0.141)	(0.174)	(0.135)	(0.137)	(0.175)	(0.270)
Openness Index			-0.322	-0.520	-0.333	-0.275	-0.299	-0.306	0.167	-0.568
			(0.245)	(0.341)	(0.302)	(0.320)	(0.290)	(0.293)	(0.490)	(0.710)
Real Gov. Expenses				-0.0973	-0.241**	-0.221*	-0.229**	-0.232**	-0.270	-0.557***
				(0.0817)	(0.106)	(0.117)	(0.105)	(0.106)	(0.167)	(0.213)
Time					-0.0104***	-0.00917**	-0.0105***	-0.0104***	-0.0198	-0.0397**
					(0.00370)	(0.00456)	(0.00351)	(0.00351)	(0.0132)	(0.0178)
Real Unit Labor Cost						0.114				
						(0.192)				
Real Unit Total Cost							-5.716	-5.757	-5.470	-12.51
							(4.616)	(4.755)	(6.274)	(8.176)
Public Enforcement								0.00252	-0.00415	0.00143
								(0.00645)	(0.00892)	(0.0130)
Real Money Supply									0.117	0.235*
									(0.113)	(0.137)
Expected Inflation										0.00292
										(0.00450)
Constant	0.815	5.628**	5.535**	3.646	-0.598	0.0280	26.27	26.46	22.20	42.01
	(2.242)	(2.476)	(2.473)	(2.343)	(1.875)	(1.950)	(22.10)	(22.71)	(30.94)	(37.59)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.997	0.993

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 5–U.S. OUTPUT REGRESSIONS (DEPENDENT VARIABLE: REAL GDP PER CAPITA IN LOG FORM)

	MODELS									
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Private Enforcement	-0.0362***	-0.0294***	-0.0297***	-0.0308***	-0.0299***	-0.0321***	-0.0284***	-0.0294***	-0.0297***	-0.0193
(lagged 3 yrs.)	(0.00969)	(0.00829)	(0.00886)	(0.00871)	(0.00872)	(0.00912)	(0.00862)	(0.00856)	(0.0107)	(0.0123)
Population	-2.012***	-1.333***	-1.362***	-1.404***	-1.333***	-1.702***	-1.339***	-1.503***	-1.656**	-2.988***
	(0.210)	(0.309)	(0.374)	(0.369)	(0.415)	(0.474)	(0.410)	(0.426)	(0.642)	(0.934)
Human Capital Index	0.190*	0.235***	0.246**	0.180	0.148	0.360**	0.130	0.120	0.217	0.275
	(0.0987)	(0.0867)	(0.111)	(0.124)	(0.114)	(0.175)	(0.120)	(0.123)	(0.156)	(0.336)
Capital Stock	1.182***	0.654***	0.659***	0.762***	0.824***	0.697***	0.862***	0.933***	0.727***	0.775
	(0.0845)	(0.175)	(0.180)	(0.204)	(0.191)	(0.205)	(0.208)	(0.218)	(0.243)	(0.470)
Total Factor Productivity	1.169***	0.956***	0.956***	1.035***	1.025***	1.313***	1.000***	1.008***	0.989***	1.054***
	(0.153)	(0.173)	(0.174)	(0.198)	(0.202)	(0.253)	(0.204)	(0.203)	(0.266)	(0.293)
GDP/Capita (lagged 1 yr.)		0.374***	0.366***	0.394***	0.392***	0.188	0.385***	0.355***	0.437***	0.552***
		(0.103)	(0.114)	(0.105)	(0.109)	(0.149)	(0.114)	(0.112)	(0.115)	(0.194)
Openness Index			0.0308	-0.140	-0.0922	0.125	-0.0762	-0.0798	0.0336	0.457
			(0.179)	(0.214)	(0.252)	(0.268)	(0.271)	(0.266)	(0.375)	(0.409)
Real Gov. Expenses				-0.0714	-0.0912*	-0.0146	-0.0891	-0.0868	-0.0505	-0.124
				(0.0546)	(0.0538)	(0.0659)	(0.0594)	(0.0589)	(0.0634)	(0.0993)
Time					-0.00168	0.00298	-0.00225	-0.00162	0.00162	0.0114**
					(0.00240)	(0.00300)	(0.00271)	(0.00267)	(0.00455)	(0.00523)
Real Unit Labor Cost						0.410*				
						(0.212)				
Real Unit Total Cost							-4.683	-4.749	-3.799	-2.681
							(3.604)	(3.601)	(3.473)	(3.334)
Public Enforcement								0.00541	0.00308	0.00397
								(0.00605)	(0.00618)	(0.00664)
Real Money Supply									-0.0236	-0.104**
									(0.0390)	(0.0430)
Expected Inflation										-0.00545*
										(0.00313)
Constant	-0.391	1.127	1.234	0.634	-0.380	1.965	21.62	21.89	20.13	21.10
	(0.871)	(0.870)	(1.106)	(1.219)	(1.453)	(1.979)	(16.76)	(16.72)	(16.67)	(15.21)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.998	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.998	0.998

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 6 - U.S. OUTPUT REGRESSIONS (DEPENDENT VARIABLE: REAL GDP PER CAPITA IN LOG FORM)

	MODELS									
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Private Enforcement	-0.0280***	-0.0178**	-0.0180**	-0.0222***	-0.0212**	-0.0192**	-0.0192**	-0.0193**	-0.0219*	-0.00810
(lagged 6 yrs.)	(0.00759)	(0.00861)	(0.00833)	(0.00818)	(0.00806)	(0.00854)	(0.00759)	(0.00776)	(0.0119)	(0.0117)
Population	-1.834***	-1.251***	-1.187***	-1.279***	-1.204***	-1.390***	-1.204***	-1.195***	-1.264**	-2.865***
	(0.196)	(0.302)	(0.379)	(0.372)	(0.421)	(0.466)	(0.416)	(0.442)	(0.619)	(0.978)
Human Capital Index	0.0650	0.0840	0.0648	-0.0270	-0.0549	0.0608	-0.0648	-0.0634	0.0496	0.0790
	(0.0736)	(0.0797)	(0.0990)	(0.115)	(0.114)	(0.165)	(0.120)	(0.129)	(0.165)	(0.361)
Capital Stock	1.259***	0.792***	0.778***	0.960***	1.016***	0.917***	1.035***	1.031***	0.821***	0.907*
	(0.0866)	(0.192)	(0.203)	(0.235)	(0.237)	(0.254)	(0.237)	(0.260)	(0.265)	(0.520)
Total Factor Productivity	0.828***	0.712***	0.718***	0.824***	0.819***	0.983***	0.802***	0.803***	0.710**	0.898***
	(0.151)	(0.159)	(0.162)	(0.179)	(0.181)	(0.217)	(0.184)	(0.186)	(0.301)	(0.307)
GDP/Capita (lagged 1 yr.)		0.333***	0.351**	0.372***	0.372***	0.256	0.371***	0.372***	0.444***	0.545**
		(0.120)	(0.132)	(0.123)	(0.124)	(0.166)	(0.125)	(0.129)	(0.140)	(0.213)
Openness Index			-0.0763	-0.344	-0.286	-0.131	-0.252	-0.253	-0.101	0.380
			(0.188)	(0.240)	(0.254)	(0.322)	(0.269)	(0.272)	(0.394)	(0.448)
Real Gov. Expenses				-0.108	-0.127*	-0.0729	-0.121	-0.121	-0.0735	-0.160
				(0.0678)	(0.0748)	(0.0857)	(0.0760)	(0.0765)	(0.0813)	(0.102)
Time					-0.00174	0.00110	-0.00233	-0.00236	-0.000364	0.0119**
					(0.00233)	(0.00273)	(0.00253)	(0.00254)	(0.00424)	(0.00535)
Real Unit Labor Cost						0.259				
						(0.247)				
Real Unit Total Cost							-4.611	-4.597	-4.560	-3.024
							(3.480)	(3.524)	(3.580)	(3.498)
Public Enforcement								-0.000330	-0.00450	0.00111
								(0.00611)	(0.00734)	(0.00766)
Real Money Supply									-0.00654	-0.119**
									(0.0446)	(0.0519)
Expected Inflation										-0.00568*
										(0.00327)
Constant	-2.046***	-0.656	-0.874	-1.883	-2.856*	-1.483	18.94	18.88	20.97	21.23
	(0.698)	(0.853)	(1.001)	(1.202)	(1.648)	(1.923)	(16.47)	(16.63)	(17.52)	(15.64)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 7 – U.S. OUTPUT REGRESSIONS (DEPENDENT VARIABLE: REAL GDP PER CAPITA IN LOG FORM)

	MODELS									
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private Enforcement	-0.0129**	-0.00132	-0.00368	-0.00606	-0.00408	-0.00345	-0.00408	-0.00402	-0.00324	0.0160
(lagged 9 yrs.)	(0.00617)	(0.00671)	(0.00879)	(0.00897)	(0.00956)	(0.00930)	(0.00944)	(0.00945)	(0.0117)	(0.0115)
Population	-1.670***	-1.038***	-0.959**	-0.973**	-0.901**	-1.174**	-0.939**	-0.991**	-1.101*	-2.381**
	(0.227)	(0.299)	(0.372)	(0.367)	(0.404)	(0.473)	(0.397)	(0.442)	(0.609)	(0.951)
Human Capital Index	-0.0577	-0.0119	-0.0318	-0.0996	-0.127	0.0301	-0.132	-0.139	-0.000261	0.0345
	(0.0664)	(0.0689)	(0.0761)	(0.101)	(0.0949)	(0.163)	(0.101)	(0.108)	(0.157)	(0.333)
Capital Stock	1.279***	0.686***	0.688***	0.797***	0.859***	0.751***	0.911***	0.936***	0.635**	0.720
	(0.0920)	(0.190)	(0.192)	(0.209)	(0.198)	(0.229)	(0.208)	(0.238)	(0.247)	(0.455)
Total Factor Productivity	0.765***	0.704***	0.696***	0.746***	0.748***	0.968***	0.733***	0.733***	0.900***	1.085***
	(0.204)	(0.196)	(0.198)	(0.206)	(0.199)	(0.258)	(0.199)	(0.200)	(0.279)	(0.305)
GDP/Capita (lagged 1 yr.)		0.434***	0.438***	0.452***	0.456***	0.298*	0.441***	0.431***	0.511***	0.638***
		(0.115)	(0.115)	(0.109)	(0.114)	(0.152)	(0.117)	(0.124)	(0.130)	(0.185)
Openness Index			-0.112	-0.303	-0.195	-0.00486	-0.172	-0.172	-0.206	0.485
			(0.242)	(0.316)	(0.372)	(0.426)	(0.382)	(0.384)	(0.409)	(0.445)
Real Gov. Expenses				-0.0645	-0.0860	-0.0218	-0.0840	-0.0829	-0.0361	-0.168*
				(0.0623)	(0.0634)	(0.0799)	(0.0680)	(0.0685)	(0.0812)	(0.0986)
Time					-0.00224	0.00144	-0.00297	-0.00277	0.00232	0.00955
					(0.00248)	(0.00259)	(0.00278)	(0.00281)	(0.00464)	(0.00570)
Real Unit Labor Cost						0.332				
						(0.247)				
Real Unit Total Cost							-6.507*	-6.554*	-5.901	-2.013
							(3.697)	(3.755)	(3.813)	(3.581)
Public Enforcement								0.00190	0.000636	0.00553
								(0.00691)	(0.00663)	(0.00694)
Real Money Supply									-0.0583	-0.138***
									(0.0350)	(0.0436)
Expected Inflation										-0.00721**
										(0.00312)
Constant	-2.931***	-0.826	-1.218	-2.001	-3.083**	-1.291	27.66	27.84	28.74	16.17
	(0.651)	(0.842)	(1.161)	(1.398)	(1.447)	(1.993)	(17.36)	(17.60)	(19.15)	(16.51)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

B. ROBUSTNESS CHECK

For the robustness check, I focused on the negative short-run, 3-year, and 6-year effects where statistical significance has been achieved in almost all of the 10 regression models, as presented in Tables 4, 5 and 6 respectively.[144] I added *Private Enforcement lagged 3 years* and *Private Enforcement lagged 6 years* to the original 2SLS model as variables of interest. This allows an evaluation of both short-run, 3-year, and 6-year effects together. Since two new variables are included in the model, I re-ran the first-stage regression to check whether *Decisions* was still an acceptable instrumental variable. The result was positive. Table 8 shows that the *Decisions* variable is negatively and significantly correlated with *Private*, meaning that it satisfies the relevance assumption.

Next, I ran 2SLS regressions for the robustness check models. The results are reported in Table 9. Concerning the short-run effect, the regression analysis identified negative and significant associations between private enforcement and output on the $p < 0.1$ or $p < 0.05$ levels. One percent increases in private enforcement are associated with approximately 0.0571 to 0.151 percent decreases in the growth of real GDP per capita. Such a result is largely consistent with the result of our main model.[145]

Table 9 also shows that private enforcement has a negative effect on output in the long run (i.e., 3 and 6 years ahead), which is consistent with our main model in Tables 5 and 6. However, the results are *not* statistically significant in any of the models even at $p < 0.01$ level, meaning that they are likely to have occurred by chance only and are not meaningful findings.

Thus, overall, my analysis provides evidence that private enforcement has a negative effect on output in the short run but no robust evidence that it has a negative effect in the long run.

144. *Supra* Tables 4-6.
145. *Supra* Table 4.

TABLE 8 – U.S. PRIVATE ANTITRUST ENFORCEMENT REGRESSIONS (DEPENDENT VARIABLE: NO. OF PRIVATE ANTITRUST CASE FILINGS IN LOG FORM)

	MODELS									
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Decisions	-0.539***	-0.533***	-0.489***	-0.509***	-0.428***	-0.424***	-0.448***	-0.452***	-0.347*	-0.508**
	(0.157)	(0.156)	(0.152)	(0.156)	(0.145)	(0.155)	(0.154)	(0.161)	(0.180)	(0.214)
Private Enforcement (lagged 3 yrs.)	0.114	0.128	0.167	0.163	0.212	0.235*	0.220	0.227	0.189	0.145
	(0.155)	(0.157)	(0.165)	(0.168)	(0.135)	(0.135)	(0.137)	(0.141)	(0.151)	(0.139)
Private Enforcement (lagged 6 yrs.)	0.219**	0.282**	0.257**	0.231**	0.267***	0.240***	0.287***	0.282***	0.160	0.114
	(0.102)	(0.118)	(0.108)	(0.107)	(0.0923)	(0.0872)	(0.0997)	(0.0963)	(0.188)	(0.166)
Population	-2.606	1.369	4.455	3.690	9.193**	11.43**	9.181*	9.907*	10.82	33.96***
	(4.128)	(4.935)	(5.985)	(5.712)	(4.431)	(5.206)	(4.607)	(5.348)	(9.374)	(12.37)
Human Capital Index	7.479***	7.520***	6.495***	5.844**	3.875**	2.467	3.678**	3.731**	3.306	-3.803
	(1.590)	(1.595)	(1.931)	(2.328)	(1.667)	(2.174)	(1.739)	(1.718)	(2.642)	(4.132)
Capital Stock	-4.508**	-7.597***	-8.276***	-6.897**	-3.796	-2.722	-3.485	-3.762	-1.334	5.287
	(1.712)	(2.589)	(2.822)	(2.648)	(2.536)	(2.638)	(2.688)	(2.779)	(3.767)	(6.046)
Total Factor Productivity	6.898***	6.061**	6.305***	6.939***	6.757***	4.848	6.398**	6.334**	3.607	-0.511
	(2.255)	(2.314)	(2.268)	(2.504)	(2.331)	(3.288)	(2.437)	(2.510)	(3.929)	(4.122)
GDP/Capita (lagged 1 yr.)		2.220	3.018	3.152	3.291**	4.560**	3.280**	3.403**	2.722	-0.638
		(1.570)	(1.825)	(1.900)	(1.518)	(1.988)	(1.526)	(1.666)	(1.835)	(2.287)
Openness Index			-3.262	-5.121	-1.620	-3.308	-1.317	-1.357	0.240	-11.29*
			(2.517)	(4.295)	(3.972)	(4.083)	(3.886)	(3.929)	(6.072)	(6.134)
Real Gov. Expenses				-0.771	-1.857**	-2.427**	-1.810*	-1.839*	-2.283**	-3.214**
				(1.057)	(0.877)	(0.962)	(0.911)	(0.945)	(1.058)	(1.315)
Time					-0.109***	-0.139***	-0.114***	-0.117***	-0.178***	-0.335***
					(0.0316)	(0.0402)	(0.0311)	(0.0329)	(0.0661)	(0.0783)
Real Unit Labor Cost						-2.754				
						(2.351)				
Real Unit Total Cost							-46.48	-45.86	-31.00	-104.3
							(76.11)	(75.74)	(79.25)	(73.33)
Public Enforcement								-0.0269	-0.0491	-0.0108
								(0.110)	(0.127)	(0.129)
Real Money Supply									0.887	1.833**
									(0.835)	(0.872)
Expected Inflation										0.0941**
										(0.0402)
Constant	66.38***	75.02***	65.20***	57.46***	-2.917	-19.22	215.4	212.4	106.4	291.8
	(14.90)	(15.10)	(17.76)	(21.02)	(21.68)	(27.02)	(360.1)	(358.7)	(382.3)	(350.2)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.833	0.838	0.842	0.844	0.874	0.877	0.876	0.876	0.814	0.739

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

TABLE 9–U.S. OUTPUT REGRESSIONS (DEPENDENT VARIABLE: REAL GDP PER CAPITA IN LOG FORM)

	MODELS									
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private Enforcement	-0.0571***	-0.0593***	-0.0666***	-0.0596***	-0.0743***	-0.0739***	-0.0691***	-0.0705***	-0.139**	-0.151***
	(0.0211)	(0.0213)	(0.0217)	(0.0209)	(0.0253)	(0.0243)	(0.0231)	(0.0229)	(0.0565)	(0.0468)
Private Enforcement	-0.0187	-0.0158	-0.0114	-0.0131	-0.00620	-0.00746	-0.00637	-0.00689	-0.000744	0.00546
(lagged 3 yrs.)	(0.0146)	(0.0126)	(0.0126)	(0.0119)	(0.0119)	(0.0130)	(0.0112)	(0.0116)	(0.0194)	(0.0207)
Private Enforcement	-0.0144	-0.00103	-0.00125	-0.00733	-0.000650	0.000605	0.000321	0.00141	-0.0193	-0.0148
(lagged 6 yrs.)	(0.00929)	(0.00985)	(0.00944)	(0.00788)	(0.00801)	(0.00771)	(0.00779)	(0.00800)	(0.0149)	(0.0177)
Population	-2.073***	-1.273***	-0.990**	-1.151***	-0.588	-0.708	-0.638	-0.719	-0.148	2.304
	(0.274)	(0.325)	(0.418)	(0.360)	(0.461)	(0.534)	(0.437)	(0.447)	(1.229)	(2.141)
Human Capital Index	0.726***	0.751***	0.715***	0.559***	0.463***	0.533***	0.420**	0.419**	0.739**	-0.0365
	(0.192)	(0.199)	(0.199)	(0.207)	(0.167)	(0.194)	(0.169)	(0.168)	(0.352)	(0.526)
Capital Stock	0.796***	0.160	0.0444	0.336	0.521**	0.468*	0.577**	0.608**	0.753*	1.330*
	(0.140)	(0.256)	(0.285)	(0.271)	(0.264)	(0.257)	(0.257)	(0.260)	(0.395)	(0.714)
Total Factor Productivity	1.651***	1.497***	1.562***	1.626***	1.711***	1.806***	1.635***	1.652***	1.172***	0.960*
	(0.237)	(0.234)	(0.245)	(0.253)	(0.290)	(0.295)	(0.276)	(0.276)	(0.430)	(0.500)
GDP/Capita (lagged 1 yr.)		0.450***	0.537***	0.538***	0.598***	0.531***	0.579***	0.568***	0.715***	0.515*
		(0.149)	(0.163)	(0.142)	(0.146)	(0.191)	(0.139)	(0.139)	(0.224)	(0.293)
Openness Index			-0.289	-0.582*	-0.334	-0.246	-0.290	-0.287	0.395	-0.678
			(0.205)	(0.298)	(0.289)	(0.301)	(0.275)	(0.275)	(0.638)	(0.788)
Real Gov. Expenses				-0.131*	-0.243***	-0.213**	-0.227**	-0.226**	-0.403**	-0.614**
				(0.0743)	(0.0930)	(0.106)	(0.0910)	(0.0911)	(0.177)	(0.240)
Time					-0.0100***	-0.00842*	-0.0101***	-0.00993***	-0.0310**	-0.0469**
					(0.00358)	(0.00458)	(0.00341)	(0.00343)	(0.0144)	(0.0191)
Real Unit Labor Cost						0.141				
						(0.206)				
Real Unit Total Cost							-5.358	-5.503	-4.415	-13.01
							(4.799)	(4.908)	(8.848)	(8.928)
Public Enforcement								0.00347	-0.0113	-0.00295
								(0.00654)	(0.0144)	(0.0155)
Real Money Supply									0.240*	0.305**
									(0.123)	(0.148)
Expected Inflation										0.00437
										(0.00492)
Constant	4.794***	6.696***	6.371***	4.604**	-0.130	0.707	25.05	25.73	14.36	40.24
	(1.838)	(2.082)	(2.077)	(2.143)	(1.820)	(2.200)	(22.76)	(23.21)	(42.75)	(40.77)
No. of Observations	66	66	66	66	66	66	66	66	61	53
R-squared	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.994	0.992

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

IV. DISCUSSIONS: MECHANISM OF NEGATIVE IMPACT

Private antitrust enforcement could cause GDP to rise in both the short run and the long run by reversing wrongdoings and enhancing productivity, respectively. Inconsistent with the literature, my regression analysis found that private enforcement has caused output to drop in the short run and possibly in the long run as well. The following views are largely speculative and meant to initiate a discussion on the causes of a short-run negative output effect in the United States. In this Section, I hypothesize that the negative output effect found in the United States is an exceptional case that can be attributed to the expected high legal costs defendants face.

Jonathan M. Jacobson, the former Chair of the ABA's Section of Antitrust Law, commented that "[a]ntitrust cases can take forever and cost a fortune."[146] Jacobson shared that:

> One of the first cases I worked on . . . was a private action follow-on to *United States v. Greater Buffalo Press.* The events leading to that case started in 1954. A grand jury was convened in 1958. No bill was voted out, but Justice commenced a civil suit in 1960. A bench trial took place in 1967, with judgment for the defense. On direct appeal to the Supreme Court . . ., the judgment was reversed 9-0 and the case sent back for divestiture of one of the printing plants involved. So much time had passed, however, that by 1973 the case terminated for lack of an interested buyer. The plant could not be sold. The follow-on case was filed in 1974 . . . . The case was litigated for eight more years, after which it settled–long *before* expert reports, summary judgment, or trial. This was 28 years after the relevant events.[147]

While this private case is an extreme example, it is true that, in general, private antitrust actions are very costly in the United States. As shown in a study by Steven C. Salop and Lawrence J. White, based on a sample of non-multidistrict litigation ("non-MDL") cases, the average length of private antitrust litigations was 24.9 months.[148] Their study also revealed that when private plaintiffs win a judgment, courts order

146. Jonathan M. Jacobson, *Tackling the Time and Cost of Antitrust Litigation*, 32 ANTITRUST 3, 3 (2017).

147. *Id.*

148. Steven C. Salop & Lawrence J. White, *Economic Analysis of Private Antitrust Litigation*, 74 GEO. L.J. 1001, 1009 (1986).

defendants to compensate the plaintiffs $1,121,675 per case, on average (all figures hereinafter are in 2019 dollars).[149] Moreover, courts might order defendants to pay the plaintiff's legal fees.[150] If so, such fees might amount to some 20 percent of the compensation.[151] Salop and White found that over 70 percent of private antitrust cases were eventually settled.[152] For these cases, defendants, on average, paid settlements of $1,662,834.[153]

In addition to using the data from the non-MDL case sample, Salop and White conducted a survey to collect more data on costs.[154] With the additional information, the authors estimate that the average total legal costs per private antitrust case (for the plaintiff and defendant sides combined) were between $491,963 and $614,953.[155] Additionally, Kenneth G. Elzinga and William C. Wood suggest that most antitrust lawyers are remunerated from direct billings and contingency fees instead of fees awarded by courts.[156] However, when there is such an award, the defendant side is responsible for an average of $20,933 in costs and $839,382 in attorney fees for the plaintiff.[157]

In the United States, private antitrust actions are usually brought as class actions.[158] Elzinga and Wood find that the average length of private antitrust class action extends to 46.7 months, as compared to 24.9 months for non-class actions.[159] Similarly, Brian T. Fitzpatrick

149. *Id.* at 1012. The figure reported by Salop and White was in 1984 dollars. For easy comparison, I converted all figures in this part of the article into 2019-dollar equivalents using the Consumer Price Index urban consumers (current series). U.S. BUREAU LAB. STAT., *supra* note 83.

150. Lai, *supra* note 115, at 488-90.

151. Salop & White, *supra* note 148, at 1013.

152. *Id.* at 1010.

153. *Id.*

154. Salop & White, *supra* note 148, at 1013.

155. *Id.* at 1015; *See also* Baker, *supra* note 33, at 42-43 (suggesting that the cost of private antitrust enforcement might be higher than what was estimated by Salop and White (1986)).

156. Kenneth G. Elzinga & William C. Wood, *The Costs of the Legal System in Private Antitrust Enforcement*, *in* PRIVATE ANTITRUST LITIGATION: NEW EVIDENCE, NEW LEARNING 120 (Lawrence J. White ed., 1988).

157. *Id.* at 121.

158. Bill Baer, Assistant Att'y Gen., Dep't of Just., Remarks at the European Competition Forum: Public and Private Antitrust Enforcement in the United States (Feb. 11, 2014).

159. Elzinga & Wood, *supra* note 156, at 111.

found that antitrust class actions take an average of 37.38 months to reach a settlement.[160] Eventually, approximately 70 percent of antitrust class actions end in settlement.[161] Theodore Eisenberg and Geoffrey P. Miller studied class action settlements for the period from 1993 to 2008 and found that, in the category of antitrust, wrongdoers compensated plaintiffs an average amount of $194 million per class action settled (attorney fees included).[162] Fitzpatrick performed a similar study but merely looked at settlements between 2006 and 2007.[163] There, he identified a mean settlement amount of $74 million for antitrust class actions.[164]

On the damages side, an antitrust defendant can face substantial penalties if found to have committed an antitrust violation. Two papers by Robert H. Lande and Joshua P. Davis can give some insight into how much risk there is for antitrust wrongdoers. In their 2008 paper, Lande and Davis analyzed forty then-recent and large-scale private antitrust litigations.[165] On average, the wrongdoers compensated the victims $623 million per case.[166] That figure already includes attorney fees awarded to the plaintiff but does not include non-cash compensation, such as products, services, discounts, and coupons.[167] In 2013, the same authors published a similar paper, where they identified another twenty private antitrust cases and showed that wrongdoers compensated the plaintiff for $664 million, on average.[168]

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160. Brian T. Fitzpatrick, *An Empirical Study of Class Action Settlements and Their Fee Awards*, 7 J. EMPIRICAL LEGAL STUD. 811, 820 (2010).

161. Lai, *supra* note 115, at 498.

162. Theodore Eisenberg & Geoffrey P. Miller, *Attorney Fees and Expenses in Class Action Settlements: 1993-2008*, 7 J. EMPIRICAL LEGAL STUD. 248, 262 (2010).

163. I presume the figure reported by Fitzpatrick was in 2007 dollars. Fitzpatrick, *supra* note 160, at 828.

164. *Id.*

165. Robert H. Lande & Joshua P. Davis, *Benefits from Private Antitrust Enforcement: An Analysis of Forty Cases*, 42 UNIV. S.F.L. REV. 879, 889 (2008).

166. *Id.* at 891 n.46 (providing data according to its 2008 valuation); *id.* at 839. From Appendix II, I found when the forty cases were decided. U.S. BUREAU LAB. STAT., *supra* note 83. Then, I converted the recovery data (on the low side) in Table 1 to 2019-dollar equivalents. After the conversion, I calculated the average recovery per case.

167. Lande & Davis, *supra* note 165, 891 n.46.

168. Joshua P. Davis & Robert H. Lande, *Toward an Empirical and Theoretical Assessment of Private Antitrust Enforcement*, 36 SEATTLE UNIV. L. REV. 1269, 1275 (2013); U.S. BUREAU LAB. STAT., *supra* note 83.

Altogether, companies sued by private parties for antitrust law violations face significant financial consequences, ranging from a million to hundreds of millions of dollars. Additionally, the defendants need to spend substantial time handling the case. This likely contributes, at least in part, to the short-run negative output effect discovered using case filing numbers. It is because defendant companies likely have to set aside resources for contingency use after learning about private antitrust suits against them. For instance, defendant companies need to hire lawyers to study the allegation and either pursue litigation or negotiate a settlement with the plaintiffs. Then, when the case is resolved, defendants may need to fund settlement payments or damages, requiring additional capital. This is also not without opportunity costs for defendants who need to spend or reserve this large sum of money. If there were no lawsuits, the defendant companies could allocate those same resources for immediate production use (increased GDP in the short run) or for research and development purposes (increased GDP in the long run). Moreover, there is an organizational impact, as these cases require managerial focus to be directed towards the litigation and away from increasing production and sales, impacting output negatively.

Furthermore, these potentially severe consequences of private antitrust actions affect not only the target of such actions but also other businesses in the economy. A negative output effect might exist if private actions deter legal and productive business activities (overdeterrence).[169] For example, if a few competitors are considering forming a cooperation agreement with each other that could boost output. Such an agreement might involve some anticompetitive elements but does not necessarily violate antitrust law (e.g., when its procompetitive effect outweighs anticompetitive effect).[170] However, the companies may not be certain whether such an agreement is legal.[171] In

169. *See* Roger D. Blair & Christine Piette Durrance, *Antitrust Sanctions: Deterrence and (Possibly) Overdeterrence*, 53 ANTITRUST BULL. 643, 655 (2008).

170. ANDREW I. GAVIL ET AL., ANTITRUST LAW IN PERSPECTIVE, 246-48 (3d ed. 2017). This follows one of the fundamental principles in the U.S. antitrust law–rule of reason. In practice, courts perform a "reasonable test" to evaluate the anti-competitive and pro-competitive evidence submitted by the plaintiff and defendant, respectively. If evidence presented shows that the pro-competitive effect of the challenged conduct outweighs its anti-competitive effect, then the conduct is conceived as a reasonable restraint of trade that does not violate antitrust law.

171. *See* Blair & Durrance, *supra* note 169 (explaining that such uncertainty exists and depends on the intensity of antitrust sanctions).

any event, the companies could still decide whether to enter into the abovementioned agreement by comparing the expected benefit and cost of doing so. The expected cost of forming that agreement is calculated as the product of (1) the probability of being detected, sued, and convicted and (2) penalties and litigation costs. When the amount of private enforcements grows, companies perceive a higher chance of being sued. In other words, holding other things equal, the greater the number of private actions, the higher the expected cost rises. Companies might therefore give up forming the agreement, negatively impacting output in both the short and long run.[172]

If the negative output effect is indeed attributed to the high consequence of private actions in the United States, then other jurisdictions should not necessarily be discouraged from promoting private antitrust enforcement. This is because the U.S. legal system has some unique features that increase the incentive for antitrust plaintiffs to sue. For example, in the United States, antitrust law provides treble damages to victims,[173] and discovery procedures are generally costly and time-consuming, particularly in the context of antitrust.[174] In addition, some have expressed concern that the antitrust bar is overpaid in the United States as compared to other jurisdictions.[175] Moreover, U.S. antitrust law has a one-way fee-shifting rule that is unfavorable to defendants.[176] The rule provides prevailing plaintiffs a right to recover their cost of suit and attorney fees from the defendant's side, but not the other way around.[177] All the aspects enumerated above vary from jurisdiction to jurisdiction.[178] This incongruity between regions has two consequences, first, in a less costly jurisdiction, companies being sued

172. *See id.* (explaining that procompetitive or competitively neutral conduct may not be companies' best practice when there is overdeterrence).

173. SHARIS POZEN ET AL., ANTITRUST LITIGATION, CHAMBERS GLOB. PRACTICE GUIDE 12 (2019), https://practiceguides.chambers.com/practice-guides/antitrust-litigation-2021 [https://perma.cc/REV2-DVHD].

174. *Id.* at 9; Jacobson, *supra* note 146.

175. Zygimantas Juska, *The Effectiveness of Private Enforcement and Class Actions to Secure Antitrust Enforcement*, 62 ANTITRUST BULL. 603, 622-23 (2017).

176. POZEN ET AL., *supra* note 173, at 15.

177. *Id.*

178. Jacobson, *supra* note 146. *See* Sinchit Lai, *Incentivizing Private Antitrust Enforcement to Promote Leniency Applications: A Case Study of the United Kingdom*, 38 ARIZ. J. INT'L & COMP. L. 247, 295-313 (2022), https://repository.arizona.edu/handle/10150/663996 [https://perma.cc/49VC-C3AE] (showing some key features under the U.K. and German legal systems).

for antitrust violations face less severe consequences and may reserve fewer resources for the case. Second, companies considering launching a business activity that involves anticompetitive elements may be less hesitant to do so. This is because businesses in jurisdictions with lower penalties and litigation costs face a smaller expected cost for launching business activity, assuming that the probability of a claim being filed in such jurisdictions is identical to that of the United States. For these two reasons, the average impact on output in many countries would be smaller than that encountered in the United States.

The deterrent effect of antitrust litigation's high costs is not the sole factor in whether output rises or declines in response to a change in the private antitrust filing numbers. As explained in the Introduction of this Article, private enforcement could cause output to rise in the short run in a few ways. Note that such positive effect mechanisms are not contradictive or mutually exclusive to the negative effect mechanisms discussed above. In summary, private enforcement affects at least three groups of market players. The first category is comprised of the wrongdoers being sued, and the overall impact on output is undetermined. On the one hand, wrongdoers might suspend anticompetitive conduct and restore production at a competitive level, causing output to rise.[179] On the other hand, wrongdoers might reserve resources to prepare for penalties and litigation costs, causing output to decrease. The second group is that of existing wrongdoers that have not been sued and would-be wrongdoers; private enforcement imposes desistance and deterrence effects on them, respectively, causing output to rise.[180] Finally, concerning companies that have not and do not intend to violate antitrust law, private enforcement might discourage their productive business activities, causing output to decline. Overall, when the positive effects outweigh the negative effects, output rises, and vice versa.

My research is exploratory. More work should be done to determine: (1) whether private enforcement has a negative effect on output in the United States and (2) whether such an effect is attributable to the unique features of the U.S. legal system. If the answers to both questions are positive, the next question is whether the negative output effect could be reversed by reforming the legal system. One direction of reforms worth exploring is lowering damages or litigation costs. While

179. *See supra* Part I.

180. *Id.*

the United States has introduced reforms that make the legal system less costly,[181] there is still room for improvement.[182] Poignantly, as Jonathan M. Jacobson pointed out, while "130 or so nations have followed the United States' lead to create competition protection regimes, not one has sought to replicate [its] litigation methodology."[183] This should be a red flag. If future works do find that a negative output effect exists and is unavoidable, it should not immediately jump to the conclusion that society prefers fewer private actions. Policymakers should keep in mind that private antitrust enforcement has many costs and benefits to society.[184] Output in the short run or economic growth in the long run is merely one of the varied factors that policymakers should consider when deciding whether to promote private antitrust enforcement.

V. CONCLUSION

Using actual data for the period from 1954 to 2019, after addressing endogeneity, this Article shows that the number of private antitrust case filings has a negative and robust impact on the United States' real GDP per capita in the short run. Although such a negative effect might extend to the long run, private enforcement does not appear to be robustly associated with future real GDP per capita. I hypothesize that the findings of this article are specific to the United States because of the unique features of the country's legal system. Specifically, I hypothesize that the negative output effect is attributed to the severe consequences faced by defendants in U.S. private antitrust actions. Additional study of the effects of private enforcement on output and the role penalties and litigation costs play in such relationships is warranted. These factors are essential for legislators to evaluate the costs and benefits of private antitrust enforcement and design a suitable mechanism accordingly.

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181. Jacobson, *supra* note 146.

182. *See* RICHARD A. POSNER, ANTITRUST LAW 266 (2d ed. 2001) (suggesting antitrust enforcement mechanisms are deficient in "ensur[ing], at reasonable cost, a reasonable degree of compliance"). *See generally* David Klingsberg, *Balancing the Benefits and Detriments of Private Antitrust Enforcement: Detrebling, Antitrust Injury, Standing, and Other Proposed Solutions*, 9 CARDOZO L. REV. 1215 (1988).

183. Jacobson, *supra* note 146.

184. GELLHORN ET AL., *supra* note 108, at 543-44.