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The Effects of Antitrust Laws on Horizontal Mergers: International Evidence

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Abstract

This study examines how antitrust law adoptions affect horizontal merger and acquisition outcomes. Using the staggered introduction of competition laws in 20 countries, we find antitrust regulation decreases acquirers' 5-day cumulative abnormal returns surrounding horizontal merger announcements. A decrease in deal value, target book assets, and industry peers' announcement returns are consistent with the market power hypothesis. Exploiting antitrust law adoptions addresses a downward bias to an estimated effect of antitrust enforcement. The potential bias from heterogeneous treatment effects does not nullify our results. Overall, antitrust policies seem to deter post-merger monopolistic gains, potentially improving customer welfare.

I. Introduction

Countries around the world began adopting antitrust laws (or "competition laws") in the late 19th century. Antitrust laws promote competition within an industry by deterring a single firm from gaining monopolistic market power (e.g., Baker (2003)). Without antitrust regulation, a product market exhibits high prices and a less-than-optimal amount of goods and services at the expense of consumer welfare. Legislative authorities, therefore, strive to control within-industry (or horizontal) mergers when such mergers may exert anticompetitive pressures on the product market.

In this article, we examine a potential gain or loss from a producer's standpoint by exploring how adopting antitrust laws affects acquirers' gain from a horizontal merger in an international setting. Prior studies focus on the United States or European countries that adopted competition laws decades ago and whose

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governments challenged horizontal merger and acquisitions (M&As) due to antitrust violations (e.g., Stillman (1983), Eckbo (1983), and Eckbo and Wier (1985)). Because the antitrust laws in those countries were introduced in an era with limited M&A data, prior studies could not exploit the law adoptions for a quasi-experimental setting. Moreover, with the major antitrust regulation already adopted, previous studies may be subject to a downward bias to an estimated effect of antitrust regulation (Baker (2003)) because "mergers likely to have significant anticompetitive effects may not have been attempted" (Prager (1992)). To address such concern, we exploit the exogenous variation in the stringency of antitrust regulation by focusing on countries that adopted antitrust regulations in recent decades.

Based on the extant literature, we set up competing hypotheses on how antitrust laws affect the shareholder wealth of acquiring firms surrounding horizontal merger announcements. One group of studies argues that competition laws reduce horizontal M&A performance. The "market power hypothesis" claims that horizontal mergers increase monopolistic gain for newly combined firms due to the significant, post-merger market power. For instance, Robinson (1969) posits that merged firms can easily collude with their peers and reduce input prices by exploiting their suppliers. Similarly, Stigler (1964) argues that horizontally merged firms are likely to collude with peers to set monopolistic prices and quantities to the detriment of customers. Accordingly, antitrust regulation should reduce acquirers' gains from horizontal deals because the law deters large, within-industry business combinations and subdues the post-merger market power and monopolistic gains of acquirers.

The "cost-efficiency hypothesis" posits that horizontal M&A improves a merging firm's economies of scale, improving post-merger cost efficiency and productivity (Dewey (1961), Manne (1965)). Horizontal mergers may yield productive market efficiencies (Eckbo and Wier (1985), Eckbo (1992)) and the gains from horizontal deals arise not from oligopolistic collusion but rather from improved productivity and purchasing efficiencies (Fee and Thomas (2004)). Accordingly, competition laws may limit an acquirer's returns from a horizontal takeover because merger control curbs the scale of the deal, which reduces the post-merger improvement in the acquirer's cost efficiency.

The "managerial-entrenchment hypothesis" argues that managers may engage in value-decreasing activities because antitrust laws insulate executives from external governance pressures. Antitrust regulation may thus reduce a firm's likelihood of becoming a merger target and have an adverse impact on shareholder value (Frattaroli (2020)). Dissanaike, Drobetz, and Momtaz (2020) also argue that merger control reduces shareholder returns for acquiring firms because managers become less discerning in their M&A plans due to managerial entrenchment induced by merger control. If so, adopting antitrust laws may reduce acquirers' gains from horizontal mergers by exacerbating firm-level agency problems. Taken together, the market-power hypothesis, the cost-efficiency hypothesis, and the managerial-entrenchment hypothesis imply that adopting antitrust laws reduces acquirers' gains from horizontal mergers.

The other group of studies (the "fair-competition hypothesis") argues that antitrust laws may increase an acquirer's shareholder wealth surrounding a horizontal merger announcement. Product market competition can promote efficient

resource allocation and enhance corporate governance. Competitive environments improve within-industry efficiency by weeding out inefficient firms; thus, managers become discerning in their decision-making (Giroud and Mueller (2011), Dasgupta, Li, and Wang (2018)). Because antitrust laws promote fair competition (e.g., merger control, banning between-firm coordination), they may also improve acquirers' gains from horizontal mergers by limiting managerial opportunism. In sum, whether antitrust laws increase or decrease acquirers' gains from horizontal deals is an empirical question.

Our sample covers 9,931 unique acquirers and 27,113 completed, domestic M&A deals in 20 countries from 1989 to 2015.[1] We run a difference-in-differences-in-differences (DDD) analysis based on the staggered introduction of antitrust laws in multiple countries in recent decades. The treatment status is based on whether a country adopts an antitrust law during the sample period. We further interact AFTER × TREAT with an indicator for a horizontal merger.[2] A horizontal merger is defined based on the common 4-digit SIC code between an acquirer and a target (Alfaro and Charlton (2009)). Using the DDD setting, we compare M&A performance of horizontal mergers in a treated country with that of horizontal deals in a control group and of non-horizontal deals in both groups; we then test how the performance changes in response to the antitrust law adoptions. To measure the value implications of a merger, we use the 5-day cumulative abnormal returns (CAR) surrounding a merger announcement based on the market-adjusted model.

We first corroborate that the antitrust law adoptions in this paper increase the stringency of competition laws in the treated countries. We use the country-year panel data provided by Bradford and Chilton (2019), which present a normalized numerical score showing the intensity of competition laws for each country. We find that the antitrust law adoptions in this paper are effective in tightening the competition regulation in the treated countries, which validates our setting.

Moving to our baseline results, we find that antitrust laws reduce acquirers' 5-day CARs surrounding horizontal merger announcements. After controlling for the firm-, deal-, and country-level factors, the 5-day CARs of horizontal mergers under a treated country's merger control, on average, are lower by 45.7% relative to the sample standard deviation, compared with the CARs in other groups. The result is not an extension of a pre-treatment trend and remains robust to propensity score matching. The baseline result implies that antitrust laws subdue an acquirer's gain from a horizontal merger, which rules out the fair-competition hypothesis.

Next, we ascertain the channel that drives our results and find that the market-power hypothesis is consistent with our findings. Deal value and a target's book asset value on a balance sheet of a horizontal deal shrink by 33.2% and 50.1%, respectively, after antitrust law adoptions, compared with other groups. The lower post-merger monopolistic rents under merger control seem to drive the decrease in

[1]We exclude foreign acquirers from our analyses because a decision to approve cross-border M&As often involves political considerations unrelated to the product market implication (Dinc and Erel (2013)). Also see https://www.scmp.com/comment/opinion/article/3086070/should-china-wield-antitrust-laws-counter-us-attacks-huawei-amid.

[2]We treat horizontal and nonhorizontal mergers separately because non-horizontal mergers are less likely to create anticompetitive problems such as predatory pricing, price signaling, or exclusionary provisions.

acquirers' gains from horizontal mergers because a small target size implies a low potential for expansion through horizontal M&As. We do not find evidence that the post-merger change in cost efficiency for a horizontal deal deteriorates with merger control. Thus, the limited improvement in post-merger economies of scale does not seem to explain our results. The change in acquirers' stock market reactions to merger announcements, deal value, and target book assets after the law adoption seems to support the market-power hypothesis, not the cost-efficiency hypothesis.

We also look into how acquirers' industry peers react to horizontal merger announcements. One stream of literature predicts that industry peers collude with newly combined firms and enjoy anticompetitive rents (e.g., Stigler (1964)). If so, antitrust laws should not only reduce acquirers' CARs, but also they should reduce industry peers' stock returns surrounding horizontal M&A announcements because merger control may reduce post-merger oligopolistic gains through industry concentration.

Peers may also react in the opposite direction. A newly combined firm's improvement in post-merger economies of scale may put industry peers at a competitive disadvantage (Eckbo and Wier (1985)). Then, if antitrust laws limit the improvement in an acquirer's post-merger cost efficiency, industry peers should react less negatively to an acquirer's horizontal merger announcement under merger control. We find that antitrust laws reduce industry-peers' CARs surrounding a horizontal merger announcement. Thus, both acquirers' and industry-peers' stock market reactions are consistent with the market-power hypothesis.

Next, we examine whether governance-related factors can explain acquirers' subdued gain from horizontal deals under merger control (the managerial-entrenchment hypothesis). We conduct subsample analyses based on a country's legal origin and governance proxies (emerging-market country classification, the quality of government index, industry concentration, and free cash flow). We largely find that governance-related factors do not seem to explain the relation between antitrust regulation and acquirers' gains from a horizontal deal. Thus, we conclude that agency problems do not significantly affect our inference and find support only for the market-power hypothesis.

To establish the robustness of our findings, we run a series of sensitivity analyses. The results remain robust to using the subsample that only covers the treated countries, omitting the Great Recession years, and excluding the observations in the United States, the United Kingdom, China, Australia, and Japan. Narrowing the time window to 6 years surrounding the law adoption for each treated country does not change our inference. We also mitigate the concern related to omitted-variable bias by showing that the result is robust to including stringent levels of fixed effects (e.g., country-by-industry-by-year fixed effects). Using the alternative time windows for CARs (e.g., 3-day, 7-day, 11-day) does not change our inference, either.

Lastly, we show that the potential bias from heterogeneous treatment effects across units and over time does not nullify our findings. Recent literature points out that the standard staggered difference-in-differences (DiD) or DDD design may not be reliable (e.g., Goodman-Bacon (2021)). Gardner (2021) presents a remedy to this problem by subtracting the estimated group and period fixed effects from an outcome variable and re-running the analysis, which isolates the treatment effect

on the treated even with the heterogeneous treatment effects. We find that our results remain robust to using the methodology of Gardner (2021).

Our study contributes to several strands of literature. First, we broaden the understanding of the effects of antitrust policies on firm value. Although papers such as Kim and Singal (1993) imply that post-merger monopolistic/oligopolistic gains may drive the value creation from mergers, the literature in general has failed to find support for the market-power hypothesis (e.g., Stillman (1983), Eckbo (1983), and Fee and Thomas (2004)). Prager (1992) and Baker (2003) highlight a downward bias in the estimated effect of antitrust regulation in previous studies because the major antitrust regulation was already adopted during their sample period and may have induced managers to shy away from mergers with anticompetitive effects. In this study, we overcome such downward bias by exploiting the adoption of antitrust laws in recent decades with sufficient M&A data. Unlike previous studies, we document that antitrust laws significantly reduce both acquirers' and industry peers' gains from horizontal mergers. In addition to the findings in Seldeslachts, Clougherty, and Barros (2009) that antitrust laws discourage firms to engage in M&As, we find that competition laws induce firms to choose small targets when they implement horizontal mergers.

Second, our work is also related to other M&A studies that exploit antitrust reforms in non-U.S. countries. For example, Frattaroli (2020) exploits the French protectionist antitakeover law and find that the reduced takeover threats exacerbate managerial entrenchment. Dissanaike, Drobetz, and Momtaz (2020) further argue that merger control induces managers to engage in value-decreasing acquisitions based on the change in European Commission Merger Regulation. Aside from the channel related to managerial entrenchment, we find that antitrust laws reduce acquirers' gains from horizontal mergers at the international level by curbing the post-merger gain in market power.

Using international M&A data, we evaluate how antitrust regulation affects merger performance on a global scale. Much of the M&A literature focuses on the United States and European markets, which introduced antitrust regulation decades ago, and examines the effects of the breaches of antitrust laws.[3] Our empirical approach extends the analysis to countries with relatively new antitrust law adoptions and broadens the understanding of the deterrent effect of antitrust regulation in an international setting.

Lastly, our work contributes to the literature on the mechanism of antitrust enforcement. Corporate litigation may pose a threat to a firm because it often leads to significant wealth losses (e.g., Bhagat, Bizjak, and Coles (1998)). Thus, private antitrust enforcement through lawsuits may complement governmental antitrust authorities. For instance, Huth and MacDonald (1989) find that firms involved in an

[3]For example, Burns (1977) conducts an event study of antitrust policy convictions in the U.S. market and finds no significant stock market response. Bosch and Eckard (1991) examine a sample of 127 firms that are subject to antitrust indictments and find a negative market reaction around announcement dates. They attribute this negative response to the foregone potential profits from collusion, the legal costs, and the loss of firm reputation. Aguzzoni et al. (2013) investigate the stock market reaction to European antitrust law enforcement events and find a negative response. Günster and van Dijk (2016) confirm that stock markets expect a decrease in profitability and react negatively to European antitrust cases between 1974 and 2004.

antitrust suit see their stock prices decrease (increase) when a legal verdict is unfavorable (favorable). Bizjak and Coles (1995) argue that "the potential loss of the ability to engage in certain profitable business practices" explains why the stock price of a defendant firm involved in a private antitrust suit declines. In addition to private antitrust litigations, we confirm that governmental antitrust enforcement is binding for shareholders, consistent with prior works (e.g., Aguzzoni, Langus, and Motta (2013)).

The remainder of this paper is organized as follows: Section II discusses the institutional background on international antitrust policies. Section III describes the sample construction and the research design. Section IV presents empirical results, and Section V concludes the paper.

II. Institutional Background on International Antitrust Policies

Although some developed countries enacted antitrust laws by the earlier half of the 20th century, the worldwide expansion of competition laws took place around the turn of the 21st century (Bradford and Chilton (2019)). For instance, China, for the first time in its history, adopted the Anti-Monopoly Law in the late 2000s. The Chinese Anti-Monopoly committee and the Anti-Monopoly Agency monitor and regulate monopolistic activities, mostly related to accusations of exploiting dominant market positions.[4] Although competition laws have heterogeneously evolved across countries, Crandall and Winston (2003) find that antitrust laws have been broadly converging toward similar standards worldwide in recent years.

Among the various aspects of promoting fair competition (e.g., prohibiting cartel products, predatory pricing, fixed pricing, or exclusive contracts), merger control is the most influential tool in implementing antitrust policies (Viscusi, Vernon, and Harrington (1995)). Because M&As dramatically change the intensity of competition within an industry, they affect not only the firms directly involved in mergers but also customers, industry-peers, and suppliers. Due to the substantial repercussions of M&As, governmental authorities establish specific guidelines to determine whether merger attempts stem from a desire to gain monopoly power or other anticompetitive motives, and thus potentially violate the antitrust laws. For instance, the Indian government requires a merger filing if the combined assets of an acquirer and a target exceed 15 billion Indian Rupees.[5] As the value of a combined firm is larger and its sales account for a higher proportion of an industry's market share, it becomes less likely that the government will approve the merger.

The penalty for violating antitrust laws is significant. Infringements may lead to heavy monetary fines or the dissolution of a merged entity. For instance, the French merger-control regulation can impose a penalty of up to 5% of pretax turnover and may order firms involved in a merger to revert to the condition prior to the merger. Monetary fines and merger dissolution are common characteristics of merger-control regulations around the world.[6] Thus, companies must

[4]See the website for details at https://globalcompliancenews.com/antitrust-and-competition/antitrust-and-competition-in-china/.

[5]See the website for more details at https://globalcompliancenews.com/antitrust-and-competition/antitrust-and-competition-in-india/

[6]See "Competition Global Guide: Merger Control" from Thomson Reuters Practical Law.

consider whether their merger plans comply with current merger controls before implementation.

Even after a merged firm has long operated as a single entity, antitrust accusations may come years or decades later. For instance, American Telephone & Telegraph (AT&T) grew by gobbling up its rivals and became a monopoly in the U.S. telephone industry in the 20th century. However, in 1984, the company was ordered to break up into Ameritech, New York/New England EXchange (NYNEX), BellSouth, U.S. West, and others due to the antitrust accusations from the government. In recent years, governmental antitrust accusations targeted big tech companies. In 2019, the U.S. Justice Department geared up for antitrust investigations of Google due to its dominant position in online advertising and search engine. Elizabeth Warren, a senator from Massachusetts, explicitly called for the dissolution of the American tech giants due to their dominance in their industry.[7] Accordingly, because antitrust accusations may call for the break-up of an existing business entity, an acquirer must choose a target that will not trigger antitrust accusations (e.g., a small target).

III. Data, Variables, and Methodology

A. Data Sources and Sample

We collect international corporate acquisition information from the Securities Data Company (SDC) M&A database. The sample period starts in 1989, 3 years before the first antitrust reform in our sample, owing to a lack of international M&A data before the 1990s. The last year of the sample period is 2015, 3 years after the last antitrust reform in our sample. Our sample includes completed and domestic M&A deals for which an acquirer owns less than 50% of a target's shares before the merger announcement but acquires more than 50% of the shares through the transaction. If a deal's transaction value is less than $1 million in U.S. dollars, an acquirer is a limited partnership or a special purpose acquisition vehicle, or a deal is associated with a recapitalization or restructuring plan, we exclude it. We use online legal platforms such as International Comparative Legal Guides (ICLG), Global Legal Insights, and Getting the Deal Through, as well as academic articles to verify the nature of antitrust reform for each country.[8] We present a list of the antitrust reforms and information sources in Appendix A.

The accounting data come from Compustat. Our sample does not cover firms in financial and utilities sectors. We also require observations to have nonmissing values for industry classification and for the firm- and deal-level characteristics used in the main regression. An acquirer's nation code in the SDC database must be the same as the country of a company's headquarters in Compustat. The final sample contains 27,113 M&A deals in 20 countries from

[7]See https://www.wsj.com/articles/justice-department-is-preparing-antitrust-investigation-of-google-11559348795.

[8]The information sources identify the first antitrust law enactment or the most relevant antitrust regulation for each country in our sample. We acknowledge that a merger environment for each country may change over time due to reasons unrelated to each country's antitrust regulation. However, we include the country-level control variables and further address the concern in Panel B, Table 8.

1989 to 2015.[9] All continuous firm- and deal-level variables in the main regression are winsorized at the 1st and the 99th percentiles.

B. Control Variables

To reduce selection bias, we control for the firm- and deal-level characteristics that may affect a firm's M&A decision. The firm-level controls include an acquirer's size, asset tangibility, and growth opportunities. Following Jensen's (1986) free-cash-flow hypothesis, we control for leverage and cash holdings because a firm with low leverage and high internal excess cash may engage in value-decreasing M&As. Our deal-level controls include the method-of-payment indicators, a tender deal indicator, and a public target indicator. An acquirer's announcement returns may depend on the method of payment, because information asymmetry levels vary between cash and stock payments (Myers and Majluf (1984)). Tender deals may be associated with deals for nonfinancial purposes. A bidder's shareholders gain more when a firm purchases a private target owing to the liquidity discount (Fuller, Netter, and Stegemoller (2002)).

At the country level, we control for a country's annual GDP growth and foreign direct investment (FDI) inflow using the data from the World Development Indicators (World Bank).[10] We control for institutional quality at the country level using the Quality of Government Index in the International Country Risk Guide (ICRG). Appendix B presents variable definitions.

C. Methodology

To test how M&A outcomes change with the adoption of antitrust laws, we run the following DDD regression using the full sample:

$$\begin{aligned}(1)\qquad \text{CAR\_5DAY}_{it} = {} & \alpha + \beta_1 \text{AFTER}_t \times \text{TREAT}_i \times \text{HORIZONTAL}_i \\ & + \beta_2 \text{AFTER}_t \times \text{TREAT}_i + \beta_3 \text{TREAT}_i \times \text{HORIZONTAL}_i \\ & + \beta_4 \text{HORIZONTAL}_i + \gamma \text{CONTROLS}_{it} + \text{YEAR}_t \\ & + \text{INDUSTRY}_j + \text{COUNTRY}_k + \in_{it},\end{aligned}$$

where *i, t, j,* and *k* denote an acquiring firm, year, 2-digit SIC industry, and a country, respectively.[11] To measure an acquirer's gain from an M&A deal, we use the 5-day

[9]We focus on countries that adopt antitrust regulations at least 10 years before the start of our sample period (control group) or during our sample period (treated group). Thus, we exclude countries such as South Korea, which introduced an antitrust law in 1980. We introduce the 10-year gap to ensure that the effect of adopting antitrust laws in the control group was already absorbed by the firms in those countries.

[10]The World Bank does not provide information on Taiwan. Thus, we use the United Nations Conference on Trade and Development database for the two macroeconomic variables for Taiwan.

[11]Our research design is similar to that of Chhaochharia and Grinstein (2009) and Fauver, Hung, and Taboada (2017) in the sense that the control group consists of observations that received the same treatment prior to the sample period. Chhaochharia and Grinstein (2009) examine how the regulatory changes regarding board structure affect CEO compensation. They compare firms that do not follow the requirements before the event with those that voluntarily follow them before the regulatory change occurs. Fauver et al. (2017) study the effect of board reform on firm value; they compare firms in countries that adopt board reform after 2000 to firms in the United Kingdom, which enacted a similar reform before 2000.

CARs around merger announcement dates, estimated using the market-adjusted model.[12] TREAT takes a value of 1 if an acquirer is in a country that adopts an antitrust law during the sample period, and 0 otherwise. AFTER is an indicator variable that is equal to 1 if an M&A deal occurs after the adoption of an antitrust law in a treated country, and 0 otherwise. HORIZONTAL takes a value of 1 if an acquirer and a target share the same 4-digit SIC code, and 0 otherwise (Alfaro and Charlton (2009)). The control variables are a set of firm-, deal-, and country-level factors that may affect a firm's acquisition decisions and outcomes. We include year fixed effects to account for time-varying macroeconomic conditions during the sample period, and industry- and country-fixed effects to control for time-invariant, industry- and country-level factors. Standard errors are adjusted for heteroscedasticity and clustered at the country level to account for serial correlations in residuals within a country.[13]

With the DDD model, seven terms (AFTER, TREAT, HORIZONTAL, and their four interaction terms) should be included in the regression, but some of them are omitted. AFTER × HORIZONTAL and AFTER are absorbed by AFTER × TREAT × HORIZONTAL and AFTER × TREAT, respectively, because the value of AFTER in the baseline regression shows variation only for the treated countries. The value of AFTER for the control group is all 0. TREAT is also subsumed by a linear combination of country fixed effects. So, in equation (1), we only include our main DDD term (AFTER × TREAT × HORIZONTAL), AFTER × TREAT, TREAT × HORIZONTAL, and HORIZONTAL.

The main coefficient of interest in equation (1) is β_1, which shows the effect of antitrust laws on horizontal M&A outcomes in the treated countries compared to the M&As in the other 3 groups: non-horizontal M&As in treated countries, and both horizontal and non-horizontal M&As in a control group. The countries in the control group are the ones that adopted antitrust laws by 1979, 10 years before the beginning of our sample period. We assume that the countries that adopted antitrust laws by 1979 have well-established merger-control regimes (e.g., the United States, the United Kingdom), thus exhibiting insignificant variation in the stringency of antitrust regulation during our sample period relative to the countries that more recently adopted antitrust laws. Later, we show that antitrust regulations in the treated countries become strict relative to those in the control group after the law adoptions. Country fixed effects should control for any time-invariant, country-specific factors that may affect the M&A environment of each country. In subsequent analyses, we include country-by-year or country-by-industry-by-year fixed effects to account for any change in the M&A environment over time for each country not driven by antitrust regulations (e.g., country-specific industry merger waves).

D. Summary Statistics

Table 1 shows the countries that adopted competition laws during the sample period (treated) and those that had already adopted an antitrust regime by 1979

[12]We estimate CAR using the International Event Study based on Compustat Global from WRDS.

[13]In untabulated analyses, we also cluster standard errors at the industry level and adopt 2-way clustering following Petersen (2009) at the industry-year, country-year, and country-industry levels. The baseline result is robust.

TABLE 1

Sample Distribution by Country

Table 1 presents the distribution of countries in the sample. We provide the year of antitrust law enactment for each country, the number of unique acquirers, the number of domestic M&A observations, and the proportion of horizontal M&As for each country in the full sample. We include only completed mergers in our sample. The sample period ranges from 1989 to 2015. The final sample includes 27,113 domestic M&A deal observations and 9,931 unique acquirers. Treatment year for a treated country corresponds to the first year of the post-treatment period.

Country (SDC Acquirer Nation Code)	Treatment Year	Number of Unique Acquirers	No. of Obs.	% of Horizontal M&As
Australia (AU)	Before 1980	617	1,261	11.90
Brazil (BR)	Before 1980	57	158	22.78
Chile (CE)	Before 1980	18	25	12.00
China (CH)	2009	925	1,532	10.31
France (FR)	Before 1980	117	180	15.00
Germany (WG)	Before 1980	82	108	13.89
Hong Kong (HK)	2012	17	20	5.00
India (IN)	2003	134	167	16.77
Indonesia (ID)	2000	36	55	9.09
Japan (JP)	Before 1980	1,066	1,883	14.55
Malaysia (MA)	2011	230	333	5.71
Mexico (MX)	1992	16	26	34.62
Norway (NO)	1993	71	132	10.61
Singapore (SG)	2005	129	192	6.25
Switzerland (SZ)	1995	25	35	14.29
Thailand (TH)	1999	46	60	8.33
Türkiye (TK)	1994	36	43	9.30
Taiwan (TW)	1992	88	112	23.21
United Kingdom (UK)	Before 1980	966	3,256	12.41
United States (US)	Before 1980	5,255	17,535	27.43
Total		9,931	27,113	

TABLE 2

Descriptive Statistics

Table 2 presents descriptive statistics for the variables in our baseline regression. The sample period spans from 1989 to 2015. The sample contains 27,113 domestic M&A deal observations. We take data on complete domestic M&A deal observations from 20 countries based on SDC Platinum and Compustat North America and Global. All continuous variables except for macroeconomic and country-level variables are winsorized at the 1% level. Appendix A presents the variable definitions.

Variable	No. of Obs.	Mean	Median	STD	P25	P75
TREAT	27,113	0.100	0.000	0.300	0.000	0.000
AFTER	27,113	0.081	0.000	0.273	0.000	0.000
HORIZONTAL	27,113	0.221	0.000	0.415	0.000	0.000
CAR_5DAY	27,113	0.022	0.010	0.092	−0.025	0.058
LOG_ASSET	27,113	6.000	5.899	1.908	4.679	7.208
ROA	27,113	0.008	0.041	0.162	0.005	0.076
TOTAL_LEVERAGE	27,113	0.211	0.189	0.179	0.042	0.333
CASH	27,113	0.124	0.077	0.134	0.027	0.173
TANGIBILITY	27,113	0.244	0.170	0.223	0.074	0.344
TOBIN_Q	27,113	2.177	1.614	1.780	1.192	2.412
PURE_CASH	27,113	0.303	0.000	0.460	0.000	1.000
PURE_STOCK	27,113	0.159	0.000	0.366	0.000	0.000
TENDER	27,113	0.041	0.000	0.199	0.000	0.000
PUBLIC_TARGET	27,113	0.124	0.000	0.330	0.000	0.000
GDP_GROWTH	27,113	0.032	0.030	0.022	0.020	0.041
FDI_INFLOW	27,107	0.022	0.017	0.024	0.010	0.028
ICRG_QOG	26,472	0.852	0.889	0.116	0.833	0.926

(control). We identify 27,113 domestic M&A deals with 9,931 unique acquirers that fit our sample-selection criteria. The time-series average proportion of horizontal M&As in each country ranges from 5% to 34.6% in our sample.

Table 2 presents the summary statistics. 10% of the acquisitions in our sample occur in the treated group. Horizontal acquisitions account for about 22% of the deals in the sample. On average, an acquirer in our sample has a leverage ratio of

21.1%, a return on assets of 0.8%, and a Tobin's Q of 2.18. Moreover, cash is 12.4% of an average acquirer's assets, and tangible assets constitute 24.4% of total assets. Deals with a tender offer account for 4.1% of all deals in our sample, 12.4% involve public targets, 30.3% of the deals are funded entirely by cash, and 15.9% are funded entirely by stock. A country in a year, on average, shows an annual GDP growth rate of 3.2%, an FDI inflow of 2.2% relative to GDP, and the ICRG Quality of Government Index equal to 0.852.

IV. Results

A. Are the Antitrust Laws in Our Sample Effective?

Before examining the relation between merger outcomes and antitrust regulations, we first ascertain that the competition law adoptions we identify increase the stringency of antitrust regulation in each country. In other words, we show that the antitrust laws used in this paper have "teeth."

To do so, we rely on the data set provided by Bradford and Chilton (2019). Their work identifies 126 countries that adopted an antitrust law by 2010, codes the content of a competition law for each country, and creates a measure of the stringency of each country's antitrust regulation, called the Competition Law Index (CLI). The CLI is available for each country every year. The index aggregates the restrictive factors of a country's antitrust laws. The more binding a law is, the higher the CLI is. The index is based on four subcategories: merger control, anticompetitive agreements, the abuse of a dominant position, and the antitrust authority. Merger control covers provisions related to M&A notification, merger review process, and the mechanisms available to a firm to argue against the anticompetitive effects of a merger. Anticompetitive agreements refer to restrictions on within- or cross-industry coordination among firms, such as price fixing, market sharing, and resale price maintenance. Abuse of dominance covers prohibitions on a firm's conduct that may lead to anticompetitive abuse of a dominant position in a market. Lastly, antitrust authority covers the enforcement mechanisms available to a regulatory agency. The CLI score and the subcategory scores are normalized to be between 0 and 1.[14]

We focus on the 20 countries included in our sample and run the following DiD test based on the Bradford and Chilton (2019) country-year panel data:

$$\text{CLI (or the subcategory score)}_{k,t} = \beta_1 \text{AFTER} \times \text{TREAT}_{k,t} + \gamma' \mathbf{X}_{k,t} + \delta_k + \mu_t + \varepsilon_{k,t}, \quad (2)$$

where k and t denote country and year, respectively. The Bradford and Chilton (2019) data end in 2010, and we have 394 country-year observations from 1989 to 2010.[15] CLI is the Competition Law Index for each country in a year, which we use as a dependent variable in column 1 of Table 3. From column 2 to 5 in Table 3, we use the score for each subcategory: antitrust authority, merger control, the abuse

[14]For more detailed information, see Bradford and Chilton (2019).

[15]Bradford and Chilton (2019) do not cover Hong Kong.

TABLE 3

Are the Antitrust Laws in Our Sample Effective?

Table 3 examines whether the antitrust law enactments we identify in this paper are effective. We use the country-year panel data provided by Bradford and Chilton (2019) from 1989 to 2010. The dependent variables are the overall Competition Law Index (CLI) that measures the intensity of competition law enforcement and the index's score for the four subcategories: antitrust authority, merger control, abuse of dominance, and anticompetitive agreements for each country in year *t*. The control variables are TRADE_SHARES, the economic globalization index (GLOBALIZATION; Dreher (2006)), POPULATION, and the relative budget for an antitrust-specific agency (POLICY_BUDGET) recorded in Bradford and Chilton's (2019) data set. We include country and year fixed effects. We compute *t*-statistics (in parentheses) using robust standard errors clustered at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The constant is omitted for brevity. Appendix B presents variable definitions.

	CLI	ANTITRUST_ AUTHORITY	MERGER_ CONTROL	ABUSE_OF_ DOMINANCE	ANTICOMPETITIVE_ AGREEMEENTS
	1	2	3	4	5
AFTER × TREAT	0.300***	0.203**	0.453***	0.164	0.242**
	(3.34)	(2.49)	(5.65)	(1.34)	(2.22)
TRADE_SHARES	−0.002	0.017	0.233	−0.129	−0.139
	(−0.01)	(0.09)	(1.35)	(−0.50)	(−0.49)
GLOBALIZATION	0.000	−0.003	0.000	0.011	0.000
	(0.03)	(−0.63)	(0.03)	(1.12)	(0.09)
POPULATION	0.732	0.724	−0.138	0.355	0.983**
	(1.41)	(1.53)	(−0.31)	(0.51)	(2.18)
POLICY_BUDGET	−0.025	−0.014	−0.054	−0.029	−0.000
	(−0.66)	(−0.66)	(−1.16)	(−0.38)	(−0.01)
Year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
No. of obs.	394	394	394	394	394
Adj. R^2	0.786	0.754	0.835	0.685	0.740

of a dominant position, and anticompetitive agreements as dependent variables. TREAT follows the same definition as equation (1). $\mathbf{X}_{k,t}$ is a vector of time-varying country-level controls in Bradford and Chilton's (2019) data set: trade shares, the economic globalization index (Dreher (2006)), population, and an antitrust regulatory agency's budget relative to GDP. We include country and year fixed effects, which absorb TREAT and AFTER, respectively, adjust standard errors for heteroscedasticity, and cluster them at the country level.

Column 1 in Table 3 shows that the treated countries after the law adoption show a 30% higher score for CLI than the control group does. Thus, the set of law adoptions we identify makes antitrust regulations binding in each treated country relative to the competition laws in the control group. Column 2 implies that a regulatory agency becomes more powerful in enforcing competition laws after the reform. Column 3 corroborates that the stricter merger control regime makes a country's antitrust law more stringent. Column 5 shows that the set of laws we identify also makes within- and cross-industry coordination between companies more difficult than before. In sum, Table 3 shows that the competition law reforms we identify are effective in empowering an antitrust regulatory agency, as well as in restricting anticompetitive mergers and between-firm collusion.

B. Baseline Results

Table 4 shows how antitrust law adoption affects acquirers' horizontal M&A performance. Column 1 presents the results without the control variables. Column 2 includes the firm- and the deal-level factors, and column 3 further controls for the

TABLE 4

Antitrust Law Enactment and Announcement Returns for Horizontal Mergers

Table 4 reports how antitrust law enactment affects acquirers' announcement returns for horizontal acquisitions. A horizontal merger is a case in which an acquirer and a target share the same 4-digit SIC code (Alfaro and Charlton (2009)). We use the 5-day CARs surrounding merger announcements as a dependent variable. We include 2-digit SIC industry, year, and country fixed effects. We compute *t*-statistics (in parentheses) using robust standard errors clustered at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The constant is omitted for brevity. Appendix B presents variable definitions.

	Dependent Variable: CAR_5DAY		
	1	2	3
AFTER × TREAT × HORIZONTAL	−0.040***	−0.038***	−0.042***
	(−4.02)	(−3.10)	(−3.50)
AFTER × TREAT	0.030**	0.027**	0.025**
	(2.47)	(2.13)	(2.57)
TREAT × HORIZONTAL	0.018***	0.017**	0.021***
	(3.55)	(2.48)	(3.30)
HORIZONTAL	−0.001	0.001	0.001
	(−0.85)	(1.57)	(1.57)
LOG_ASSET		−0.007***	−0.007***
		(−14.70)	(−14.95)
ROA		0.010	0.010
		(1.70)	(1.59)
TOTAL_LEVERAGE		0.027***	0.029***
		(4.95)	(5.92)
CASH		−0.013**	−0.012**
		(−2.19)	(−2.26)
TANGIBILITY		−0.010**	−0.010**
		(−2.72)	(−2.72)
TOBIN_Q		0.002***	0.002***
		(4.77)	(4.45)
PURE_CASH		0.001	0.001
		(0.29)	(0.53)
PURE_STOCK		0.008**	0.008**
		(2.38)	(2.29)
TENDER		0.019**	0.019*
		(2.14)	(2.11)
PUBLIC_TARGET		−0.025***	−0.025***
		(−7.06)	(−7.63)
GDP_GROWTH			−0.232**
			(−2.28)
FDI_INFLOW			0.047
			(0.92)
ICRG_QOG			−0.052
			(−0.91)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
No. of obs.	27,113	27,113	26,472
Adj. R^2	0.0243	0.0479	0.0494

country-level characteristics. The estimated coefficient of AFTER × TREAT × HORIZONTAL in all columns is negative and significant at the 1% level.[16] Based on column 3, an acquirer's CAR surrounding a merger announcement decreases by 45.7% (= −0.042/0.092) relative to the sample standard deviation. The median market capitalization in our full sample is about $445 million. So, the coefficient of

[16]The result is robust to defining industry fixed effects at the 3-digit SIC level and the Fama–French 48-industry level.

AFTER × TREAT × HORIZONTAL implies a loss of approximately $18.69 million in market capitalization. An interesting comparison can be made between AFTER × TREAT × HORIZONTAL and TREAT × HORIZONTAL. The difference between the two terms is 6.3% (= 0.021 − (−0.042)).[17] In other words, an acquirer's CARs from a horizontal merger in a treated country decrease by 6.3% after the antitrust law adoption. Thus, Table 4 suggests that the deterrent effect of antitrust regulation seems to subdue acquirer's gains from horizontal mergers.

The result is consistent with the market-power and the cost-efficiency hypotheses. The decrease in gain from a horizontal merger after the antitrust law adoption may be attributable to the lower monopolistic gain from an M&A or to subdued improvement in the post-merger economies of scale. Table 4 rules out the possibility that antitrust laws increase an acquirer's return from a horizontal merger through the positive influence of fair competition on corporate governance.

Regarding the control variables, LOG_ASSET is significantly negatively associated with M&A outcomes. This result is consistent with Masulis, Wang, and Xie (2007), who find that managers in larger firms are more likely to be entrenched and pursue value-destroying M&As. TOTAL_LEVERAGE and CASH have significantly positive and negative associations, respectively, with M&A outcomes. The results for TOTAL_LEVERAGE and CASH align with Jensen's (1986) free cash flow hypothesis, as we find that higher leverage and lower internal cash holdings are associated with better M&A outcomes. Also, the gains from M&As are lower when a target firm is public, consistent with Fuller et al. (2002). As to the other controls, we find that PURE_STOCK, TENDER, and TOBIN_Q have significantly positive associations with M&A outcomes, whereas TANGIBILITY and GDP_GROWTH have negative associations.

In Table OA1 of the Supplementary Material, we show that the results in Table 4 do not merely reflect a time trend in acquirers' CARs surrounding horizontal merger announcements. We find that the coefficient of AFTER × TREAT × HORIZONTAL is negative and significant only in the post-treatment period. Also, in Table OA2 of the Supplementary Material, we show that our results are robust to propensity score matching. Thus, the difference in firm- or deal-characteristics between the treated and the control group does not drive our findings.

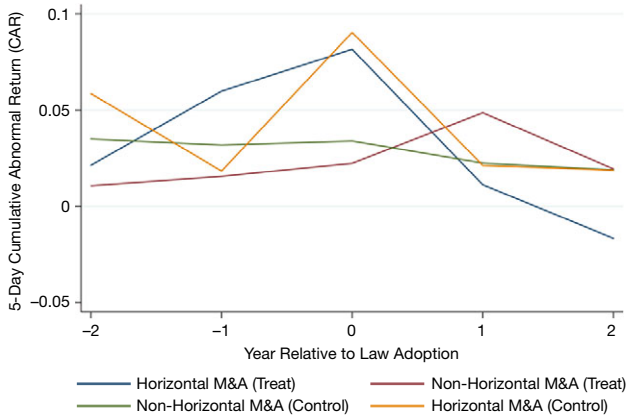
For a graphical illustration of our findings, we plot M&A announcement returns based on the matched sample. Figure 1 shows the variation in the outcomes for horizontal and non-horizontal M&As over the 5 years surrounding the adoption of antitrust laws. The vertical axis shows the cross-sectional mean 5-day CAR in a year, and the horizontal axis shows the year relative to the adoption of antitrust law. Year 0 corresponds to the first year in the post-treatment period. In Figure 1, the blue solid line shows that horizontal M&A outcomes for the treatment group decline after antitrust laws are adopted, consistent with Table 4. Horizontal M&A outcomes for the control group (the yellow solid line) show no consistent trend over time, and we observe no noticeable variation in the outcomes of non-horizontal M&As in both groups (the green and the brown sold lines) over the 5 years. Thus, Figure 1 corroborates the findings in Table 4 and implies that the deterrent effect of antitrust regulation on merger outcomes is largely limited to horizontal deals.

[17]The Wald test confirms that the difference is statistically significant.

FIGURE 1

Horizontal and Non-Horizontal M&A Outcomes Relative to Antitrust Law Adoption

Figure 1 shows the 5-day CARs acquirers experience in each group relative to the enactment years of antitrust laws in each country. We use a sample of matched firms from the treatment and control groups, as well as horizontal and non-horizontal M&As over the 5-year window surrounding the law adoption. Year 0 is the first year in the post-treatment period.



C. Potential Channels

Next, we examine which of the two hypotheses drives our findings. First, the market-power hypothesis implies that antitrust policy reduces acquirers' CARs surrounding horizontal merger announcements due to the decrease in post-merger monopolistic gains. Because merger control regulation makes it less likely for a large merger to get approved, an acquirer is induced to choose a relatively small target (e.g., Prager (1992)). Choosing a small target in a horizontal merger implies that the post-merger market power of an acquirer under merger control is likely limited.

To test whether our results are consistent with the market-power hypothesis, we examine how M&A deal size changes after an antitrust law goes into effect. In Panel A of Table 5, we use the natural log of deal value and of target book assets based on the SDC database as dependent variables.[18] The coefficient of AFTER × TREAT × HORIZONTAL is negative and significant in both columns. The deal value for a horizontal merger decreases by 33.2% ($=e^{-0.403}-1$) and a target's book asset value is lower by 50.1% ($=e^{-0.695}-1$) after an antitrust law becomes effective. Panel A of Table 5 corroborates the argument that an acquirer chooses a relatively small target due to antitrust regulation, which reduces the monopolistic gain from a horizontal merger. Thus, the result is consistent with the market power hypothesis.

Second, we evaluate the post-merger change in the cost of goods sold (COGS) and the selling, general & administrative costs (SG&A) to test whether the lower gain from a horizontal merger after the law adoption arises from the change in

[18]Using a target firm's book asset value in a balance sheet mitigates the concern that our inference may be confounded by deal premium. The number of observations for column 2 in Panel A of Table 5 is relatively low due to data limitations.

TABLE 5

Market-Power Hypothesis and Cost-Efficiency Hypothesis

Table 5 examines the channel that drives the subdued gain from a horizontal merger for an acquirer after an antitrust law goes into effect. Panel A examines whether the target of a horizontal merger becomes smaller due to the law's restriction; it uses the natural log of deal value and target book asset value as dependent variables, respectively. Panel B tests the cost-efficiency channel by looking into the post-merger change in cost behavior. The dependent variables are the change from year $t+1$ to $t+2$ (or $t+3$) in industry (3-digit SIC)-median-adjusted cost of goods sold or selling, general & administrative costs scaled by book assets, with t corresponding to the year of the merger announcement. We include 2-digit SIC industry, year, and country fixed effects. We compute t-statistics (in parentheses) using robust standard errors clustered at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The constant is omitted for brevity. Appendix B presents variable definitions.

Panel A: Deal Value and Target Book Assets

	LOG_DEALVALUE	LOG_TARGET_BOOK_ASSET
	1	2
AFTER × TREAT × HORIZONTAL	−0.403***	−0.695*
	(−3.42)	(−2.06)
AFTER × TREAT	0.722***	0.931**
	(3.55)	(2.78)
TREAT × HORIZONTAL	0.303*	0.636*
	(1.79)	(1.96)
HORIZONTAL	0.176***	0.228***
	(7.99)	(13.08)
Controls	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
Country FE	Yes	Yes
No. of obs.	26,472	6881
Adj. R^2	0.518	0.509

Panel B: Post-merger Change in Cost Behavior

	$\Delta COGS_{t+1 \text{ to } 2}$	$\Delta COGS_{t+1 \text{ to } 3}$	$\Delta SG\&A_{t+1 \text{ to } 2}$	$\Delta SG\&A_{t+1 \text{ to } 3}$
	1	2	3	4
AFTER × TREAT × HORIZONTAL	−0.083***	−0.039	0.006	0.003
	(−4.04)	(−0.84)	(1.11)	(0.40)
AFTER × TREAT	−0.002	−0.003	0.002	−0.013
	(−0.09)	(−0.10)	(0.29)	(−0.73)
TREAT × HORIZONTAL	0.060***	0.024	−0.003	0.009
	(3.26)	(0.51)	(−0.49)	(1.02)
HORIZONTAL	0.006***	0.009**	−0.001	−0.004
	(3.13)	(2.35)	(−0.14)	(−1.11)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
No. of obs.	23,637	22,130	23,900	22,376
Adj. R^2	0.0084	0.0160	0.0001	0.0058

post-merger cost behavior. The cost-efficiency hypothesis implies that the antitrust regulation reduces the gain from a horizontal merger because an acquirer cannot significantly improve economies of scale through a large M&A deal. If so, then the post-merger improvement in cost efficiency for a horizontal deal should be less pronounced after the antitrust law goes into effect.

In Panel B of Table 5, we examine the change in the industry-median-adjusted change in COGS and SG&A from year $t+1$ to $t+2$ or 3, with t corresponding to a merger announcement year. If the cost-efficiency hypothesis is correct, then the coefficient of AFTER × TREAT × HORIZONTAL should be positive. The positive sign implies that a newly merged firm cannot improve its post-merger cost behavior as much as it could have without the antitrust regulation. However, Panel

B of Table 5 shows a largely insignificant or negative association between the post-merger cost behavior and AFTER × TREAT × HORIZONTAL. Thus, the results in Panel B of Table 5 are inconsistent with the cost-efficiency hypothesis. The subdued gain for an acquirer from a horizontal merger after the antitrust law adoption does not seem to be driven by the limited improvement in the post-merger economies of scale. In summary, Table 5 implies that the market-power hypothesis explains the negative relation between antitrust regulation and horizontal M&A outcomes.

Next, we investigate how acquirers' industry-peers react to horizontal merger announcements to test which of the two hypotheses explains horizontal merger outcomes under competition laws. On one hand, Stigler (1964) argues that horizontal mergers increase the probability of collusion within an industry. If so, industry peers are likely to benefit from horizontal mergers that yield anticompetitive gains, consistent with the market-power hypothesis. Accordingly, competition laws that limit industry concentration though horizontal mergers should lead to less positive (or more negative) announcement returns for acquirers' peers after the law adoption because the prospect of post-merger oligopolistic gain diminishes under merger control. On the other hand, the cost-efficiency hypothesis predicts negative reactions from rival firms upon the announcement of horizontal mergers, because the deal gives merging firms a competitive advantage through enhanced productivity and cost efficiency (Eckbo and Wier (1985)). Accordingly, competition laws that prevent the emergence of large, combined firms should lead to more positive reactions from acquirers' peers surrounding merger announcements, because the post-merger improvement in acquirers' economies of scale is limited under merger control.

We define peers as the Compustat firms headquartered in the same country as acquirers and that share the same 4-digit SIC code in the merger announcement year. We exclude firms that experience a major merger event themselves (identified by Compustat footnote code "AB") from a peer group. We compute the 5-day CARs for peers surrounding an acquirer's merger announcement using the same methodology as before. Then, we take the peer-average (equal-weighted) of the 5-day CARs surrounding a merger announcement of each acquirer in a year. To control for peer characteristics, we include the peer-averages of the firm-level controls in equation (1) each year in the regression.

Columns 1 and 2 in Table 6 show the results without and with the controls, respectively. In column 2, we find that the effect of antitrust laws on peers' reactions to horizontal M&A announcements is negative and significant at the 1% level. Thus, industry-peers seem to recognize the subdued potential for greater industry concentration and oligopolistic gain because antitrust laws deter large, horizontal deals. Our result is different from that of earlier studies, which fail to find evidence of anticompetitive effects based on rivals' stock market reactions to a merger (Eckbo (1983), Stillman (1983)). Our findings are consistent with Prager (1992), which argues that the insignificant results of earlier studies are attributable to the deterrent effect of antitrust regulation. The earlier works cover the sample period after a major antitrust law is adopted. During those periods, mergers with anticompetitive potential are unlikely to occur due to anticipated antitrust challenges. We find significant results because we exploit the exogenous variation in the stringency of antitrust regulation in our sample. In sum, Tables 5 and 6 support the market-power hypothesis, not the cost-efficiency hypothesis.

TABLE 6

Antitrust Law Enactment and Industry-Peers' Announcement Returns for Horizontal Mergers

Table 6 reports how an acquirers' peers' stock returns react to horizontal-merger announcements after an antitrust law goes into effect. We use the equal-weighted average of the peers' 5-day CARs surrounding the acquirer's merger announcement as the dependent variable and the average values across the set of peers for firm-level peer controls. Peers are firms headquartered in the same country and that share the same 4-digit SIC code as the acquirer in merger announcement year *t*. We include 2-digit SIC industry, year, and country fixed effects. We compute *t*-statistics (in parentheses) using robust standard errors clustered at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The constant is omitted for brevity. Appendix B presents variable definitions.

	Dependent Variable: PEER_CAR_5DAY	
	1	2
AFTER × TREAT × HORIZONTAL	−0.012*	−0.016***
	(−1.90)	(−3.66)
AFTER × TREAT	0.001	−0.002
	(0.29)	(−0.46)
TREAT × HORIZONTAL	0.009*	0.013***
	(1.87)	(4.05)
HORIZONTAL	0.001***	0.001**
	(3.46)	(2.35)
PEER_LOG_ASSET		−0.001**
		(−2.22)
PEER_ROA		0.001***
		(7.77)
PEER_TOTAL_LEVERAGE		0.000
		(0.90)
PEER_CASH		0.002
		(0.83)
PEER_TANGIBILITY		0.005*
		(1.90)
PEER_TOBIN_Q		0.001***
		(4.87)
PURE_CASH		0.000
		(1.33)
PURE_STOCK		0.000
		(0.86)
TENDER		0.002*
		(1.77)
PUBLIC_TARGET		0.003***
		(6.97)
GDP_GROWTH		−0.037
		(−0.80)
FDI_INFLOW		−0.031
		(−1.50)
ICRG_QOG		0.001
		(0.06)
Year FE	Yes	Yes
Industry FE	Yes	Yes
Country FE	Yes	Yes
No. of obs.	21,769	21,229
Adj. R^2	0.011	0.016

Lastly, in Section IV.C, we test whether governance-related factors affect the relation between antitrust regulation and horizontal merger outcomes in our sample. For instance, a competition law may reduce takeover threats, exacerbate managerial entrenchment, and induce value-decreasing acquisitions (e.g., Dissanaike et al. (2020)). Thus, we test whether antitrust regulation has a pronounced effect on horizontal mergers in a setting with potential agency problems. To do so, we conduct subsample analyses based on country- and firm-level governance proxies.

We first compare countries with a common-law origin to those with a civil-law origin. La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998) argue that investor protection is associated with a country's legal origin and find that countries with common law frameworks offer better shareholder protections than those with civil law frameworks. If so, the negative relation between antitrust regulation and horizontal-merger CARs may be more pronounced in countries with a civil law origin than those with a common law origin if merger control exacerbates managerial entrenchment.

To identify a country's legal origin, we follow Lopez-de-Silanes et al. (1998) and various legal sources. COMMON_LAW is a dummy variable equal to 1 if a deal occurs in a country with a common law origin, and 0 otherwise. We follow Lopez-de-Silanes et al. (1998) and various legal sources to determine a country's legal origin. In our sample, the countries with a civil law origin are Brazil, Chile, China, France, Germany, Indonesia, Japan, Mexico, Norway, Switzerland, Taiwan, Thailand, and Türkiye; countries with a common law origin are Australia, Hong Kong, India, Malaysia, Singapore, the United Kingdom, and the United States.[19] Then, we conduct a subsample analysis based on the COMMON_LAW indicator. In columns 1 and 2 in Table 7, we find that the negative effect of antitrust regulation on an acquirer's CARs surrounding a horizontal deal is more pronounced in countries with a civil law origin than in those with a common law origin. The coefficient of AFTER × TREAT × HORIZONTAL is significantly different between the 2 columns (p-value <0.0001). So, based on columns 1 and 2 alone, it seems that agency problems may also have a role in the decrease in an acquirer's gain from a horizontal deal under merger control.

However, in Table OA3 of the Supplementary Material, when we extend our analysis to the "four-legal framework" of Lopez-de-Silanes et al. (1998), we do not find evidence consistent with the managerial-entrenchment hypothesis. The coefficient of AFTER × TREAT × HORIZONTAL is neither negative nor significant in French-civil-law countries. Lopez-de-Silanes et al. (1998) argue, "common-law countries generally have the strongest, and French-civil-law countries the weakest, legal protections of investors, with German- and Scandinavian-civil-law countries located in the middle." Thus, if agency problems drive the negative relation between antitrust regulation and horizontal merger performance, our result should be most pronounced in French-civil-law countries, which is not what we find. Thus, it is questionable whether a country's legal origin affects the relation between antitrust laws and horizontal merger outcomes.

We also test whether other country-level governance factors affect our results. The two country-level governance proxies are the emerging-market countries classification following Morgan Stanley Capital International (MSCI) and the Quality of Government index (ICRG_QOG).[20] Emerging countries may have less established institutions than developed countries do, and ICRG_QOG is an annual country-level rating that is based on a country's political, financial, and economic

[19]Lopez-de-Silanes et al. (1998) state "Thailand's first laws were based on common law but since received enormous French influence." Other legal sources such as Poapongsakorn (2002) state that Thailand is a country with a civil law tradition. Thus, we assign Thailand to a group of civil law countries. Also, note that the SDC Platinum database assigns a unique country code to Hong Kong.

[20]See https://www.msci.com/market-classification for the MSCI market classification.

TABLE 7

Cross-Sectional Tests: Country- and Firm-Level Governance Proxies

Table 7 presents the results of the cross-sectional tests based on the country-level and firm-level governance proxies: a country's legal origin, the MSCI classification of emerging countries, the Quality of Government index (ICRG_QOG), the Herfindahl–Hirschman Index (HHI) at the 3-digit SIC level, and the free cash flow for each firm in a year, following Lehn and Poulsen (1989). We conduct subsample tests based on each indicator or the median value of a proxy for the full sample. We use the 5-day CARs surrounding merger announcements as a dependent variable. We include 2-digit SIC industry, year, and country fixed effects. We compute *t*-statistics (in parentheses) using robust standard errors clustered at the country level. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively. The constants are omitted for brevity. Appendix B presents variable definitions.

	Common Law	Civil Law	Emerging Countries	Developed Countries	High-Quality Government	Low-Quality Government	High HHI	Low HHI	High Free Cash Flow	Low Free Cash Flow
	1	2	3	4	5	6	7	8	9	10
AFTER × TREAT × HORIZONTAL	−0.020***	−0.047***	−0.048***	−0.035**	−0.041**	−0.040**	−0.042**	−0.041***	−0.037*	−0.061***
	(−5.04)	(−7.50)	(−11.95)	(−3.21)	(−3.27)	(−2.82)	(−2.44)	(−4.81)	(−1.81)	(−3.31)
	H0:β(1) = β(2) (*p*-value: <0.001)		H0:β(3) = β(4) (*p*-value: 0.252)		H0:β(5) = β(6) (*p*-value: 0.945)		H0:β(7) = β(8) (*p*-value: 0.961)		H0:β(9) = β(10) (*p*-value: 0.396)	
AFTER × TREAT	−0.019	0.030***	0.004	0.002	−0.017***	0.027***	0.022*	0.027***	0.015	0.048**
	(−1.00)	(3.91)	(0.27)	(0.38)	(−6.65)	(3.54)	(1.77)	(3.01)	(1.40)	(2.49)
TREAT × HORIZONTAL	0.010	0.021**	0.044***	0.015***	0.007*	0.016	0.024	0.018***	0.015	0.033
	(1.00)	(2.36)	(5.97)	(10.10)	(2.31)	(1.56)	(1.56)	(7.06)	(0.85)	(1.69)
HORIZONTAL	0.000	0.008*	−0.007	0.001	−0.002***	0.005***	−0.001	0.002**	0.005***	−0.001**
	(1.65)	(2.15)	(−1.50)	(1.67)	(−4.88)	(4.23)	(−0.63)	(2.74)	(6.21)	(−2.55)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	22,411	4061	2178	24,294	14,582	11,890	13,056	13,416	12,182	12,399
Adj. R^2	0.042	0.092	0.122	0.043	0.047	0.065	0.054	0.059	0.044	0.053

risk, with a higher value indicating lower risk. We conduct subsample analyses based on the emerging-market country indicator and the median value of ICRG_QOG for the full sample. Columns 3 and 4 in Table 7 show that the coefficient of AFTER × TREAT × HORIZONTAL is negative and significant for both emerging and developed market subsamples, and the difference in the coefficient is not significant between the 2 columns (p-value: 0.252). Columns 5 and 6 in Table 7 show that acquirers' gains decrease after an antitrust law goes into effect both in high-quality and low-quality government countries, with no statistical difference in the coefficient of AFTER × TREAT × HORIZONTAL between the 2 columns (p-value: 0.945). Thus, the difference in country-level governance or institutional quality does not seem to explain subdued gains for acquirers from horizontal mergers under merger control.

Next, we test whether industry concentration or free cash flow (Lehn and Poulsen (1989)) drives our results. Giroud and Mueller (2010), (2011) argue that product market competition can act as a corporate governance mechanism because a firm is less likely to survive in a more competitive environment if a self-interested manager wastes a company's resources. Relatively concentrated industries are then more likely to exhibit firm-level agency problems than competitive industries. We measure industry concentration using the Herfindahl–Hirschman Index (HHI). If managerial entrenchment drives the negative relation between horizontal-merger CARs and antitrust regulation, then the result should be more pronounced for acquirers in relatively concentrated industries. Also, Jensen (1986) argues that a manager likely engages in suboptimal empire-building. A manager attempts to expand through M&As, which often do not benefit shareholders. Such practice is more pronounced when a firm holds excess cash. If agency problems drive our results, the decrease in acquirers' gains should then be more pronounced among acquirers with higher free cash flow.

Columns 7 and 8 in Table 7 show that the coefficient of AFTER × TREAT × HORIZONTAL is negative and significant for both high and low HHI subsamples, with insignificant difference in the coefficient between the two (p-value: 0.961). The influence of industry concentration on corporate governance does not seem to drive the relation between antitrust regulation and horizontal M&A performance. When it comes to the subsample analysis based on free cash flow, we again find that the coefficient of AFTER × TREAT × HORIZONTAL is not significantly different (p-value: 0.396) between columns 9 and 10 (high vs. low free cash flow). Thus, we are not able to conclude that industry concentration and free cash flow significantly influence how antitrust regulation subdues acquirers' horizontal-merger CARs in our sample.

In summary, Table 7 does not find that a country- or a firm-level governance factor affects the relation between antitrust regulation and horizontal M&A performance. We only find support for the market-power hypothesis.

D. Sensitivity Analyses

Table 8 examines whether our baseline findings remain robust to various specifications. Column 1 of Panel A only uses the observations in the treatment group. We use the following model in column 1, Panel A only:

TABLE 8
Sensitivity Analyses

Table 8 reports a series of robustness tests. Columns 1–3 in Panel A show the results when we only use the treatment group, exclude observations from 2008 and 2009 to rule out the effect of the Great Recession, and omit the observations from the United States, the United Kingdom, China, Australia, and Japan, respectively. Column 4 uses the sample that only covers the last 3 years ($t = -3, -2, -1$) of the pre-treatment period and the first 3 years ($t = 1, 2, 3$) of the post-treatment period for the treated group. Panel B presents the results based on various fixed effects levels. Panels A and B use the 5-day CARs surrounding the merger announcement as a dependent variable. Panel C reports the results with the alternative time windows. We use 3-, 7-, and 11-day CARs surrounding merger announcements. We include 2-digit SIC industry, year, and country fixed effects. We compute *t*-statistics (in parentheses) using robust standard errors clustered at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The constants are omitted for brevity. Appendix B presents variable definitions.

Panel A. Subsample Tests

	Dependent Variable: CAR_5DAY			
	Treatment Group Only	Excluding 08 & 09	Excluding USA, UK, CHN, AUS & JPN	6-Year Window for the Treated Group
	1	2	3	4
AFTER × TREAT × HORIZONTAL	-- --	−0.037*** (−4.63)	−0.029*** (−4.81)	−0.051*** (−11.53)
AFTER × TREAT	-- --	0.031** (2.51)	−0.010 (−0.44)	0.008 (0.96)
AFTER × HORIZONTAL	−0.041*** (−6.42)	-- --	-- --	-- --
TREAT × HORIZONTAL	-- --	0.011** (2.87)	0.034** (2.77)	0.033*** (3.30)
HORIZONTAL	0.027*** (5.04)	0.001 (1.18)	−0.013** (−2.29)	0.001 (1.45)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
No. of obs.	2,354	24,833	1,005	24,623
Adj. R^2	0.118	0.050	0.049	0.038

Panel B. Additional Fixed Effects

	Dependent Variable: CAR_5DAY						
	1	2	3	4	5	6	7
AFTER × TREAT × HORIZONTAL	−0.029* (−1.76)	−0.038** (−2.95)	−0.049*** (−4.71)	−0.052*** (−3.70)	−0.025** (−2.64)	−0.040*** (−15.97)	−0.038** (−2.45)
AFTER × TREAT	-- --	0.028** (2.51)	0.026*** (3.42)	-- --	0.017 (1.39)	-- --	-- --
TREAT × HORIZONTAL	0.010 (0.76)	0.023** (2.28)	0.028*** (3.17)	0.041*** (4.61)	0.025** (3.42)	0.054*** (57.77)	-- --
HORIZONTAL	0.001 (1.51)	0.001 (1.56)	0.001 (1.07)	0.001 (1.04)	0.004** (3.46)	0.004*** (5.09)	-- --
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	No	Yes	No	No
Industry FE	Yes	No	No	No	No	No	Yes
Acquiring firm FE	No	No	No	No	Yes	Yes	No
Country FE	No	Yes	No	No	No	No	No
Country × year FE	Yes	No	No	No	No	No	Yes
Industry × year FE	No	Yes	No	No	No	No	No
Country × industry FE	No	No	Yes	No	No	No	No
Country × industry × year FE	No	No	No	Yes	No	Yes	No
Horizontal × year FE	No	No	No	No	No	No	Yes
Country × horizontal FE	No	No	No	No	No	No	Yes
No. of obs.	26,472	26,472	26,472	26,472	26,472	26,472	26,472
Adj. R^2	0.053	0.056	0.053	0.044	0.222	0.138	0.051

(continued on next page)

TABLE 8 (continued)
Sensitivity Analyses

Panel C. Alternative Time Windows

	CAR_3DAY 1	CAR_3DAY 2	CAR_7DAY 3	CAR_7DAY 4	CAR_11DAY 5	CAR_11DAY 6
AFTER × TREAT × HORIZONTAL	−0.022***	−0.016	−0.046***	−0.048**	−0.068**	−0.077**
	(−2.87)	(−1.26)	(−3.08)	(−2.81)	(−2.73)	(−2.75)
AFTER × TREAT	0.026***	0.020**	0.039**	0.037**	0.038*	0.035**
	(3.48)	(2.50)	(2.10)	(2.46)	(1.80)	(2.14)
TREAT × HORIZONTAL	0.009	0.004	0.019**	0.024**	0.033**	0.045**
	(1.64)	(0.51)	(2.04)	(2.20)	(2.14)	(2.59)
HORIZONTAL	−0.000	0.001	−0.000*	0.001	0.000	0.002**
	(−0.31)	(1.72)	(−0.20)	(1.06)	(0.27)	(2.47)
Controls	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	27,113	26,472	27,111	26,470	27,113	26,472
Adj. R^2	0.022	0.046	0.024	0.043	0.025	0.056

$$(3)\quad \text{CAR\_5DAY}_{it} = \alpha + \beta_1 \text{AFTER}_t \times \text{HORIZONTAL}_i + \beta_2 \text{HORIZONTAL}_i + \gamma \text{CONTROLS}_{it} + \text{YEAR}_t + \text{INDUSTRY}_j + \text{COUNTRY}_k + \in_{it},$$

where *i, t, j,* and *k* denote an acquirer, year, industry, and country, respectively.[21] We include the same controls used in Table 4 along with year, industry, and country fixed effects. We cluster the standard errors at the country level. In the treated group subsample, we find that an acquirer's return from a horizontal-merger announcement decreases after the antitrust law adoption. The coefficient of AFTER × HORIZONTAL is negative and significant at the 1% level in column 1 in Panel A of Table 8.

The remaining columns in Panel A of Table 8 follow equation (1). In column 2, we exclude deals in 2008 and 2009 to address the concern that the Great Recession may drive our results. However, the coefficient of AFTER × TREAT × HORIZONTAL is still negative and significant after excluding M&A deals during the Great Recession years.

In column 3, we exclude deals in the United States, the United Kingdom, China, Australia, and Japan from our sample. Table 1 shows that a significant portion of the observations in the treated (control) group come from China (the United States, the United Kingdom, Australia, and Japan). Although the full sample covers 20 countries, one may argue that those 5 countries drive our baseline results and that the merger-control regulation in the remaining countries is not binding. However, in column 3, Panel A, we show that our results remain robust excluding the United States, the United Kingdom, China, Australia, and Japan. This alleviates the concern that a few major countries drive our findings.

In column 4, we use the subsample that only covers the 6 years surrounding the antitrust law adoption for the treated countries. We focus on the last 3 years ($t = -3, -2, -1$) of the pre-treatment period and the first 3 years ($t = 1, 2, 3$) of

[21] AFTER is subsumed by year fixed effects.

the post-treatment period. We narrow the time window for each treated country to address the concern that the negative coefficient of AFTER × TREAT × HORIZONTAL may be driven by each country-specific event or change in M&A environment unrelated to the antitrust law adoption. However, the coefficient of AFTER × TREAT × HORIZONTAL is negative and significant in the 6-year subsample as well.[22] Thus, we corroborate that the introduction of merger control regime does indeed drive the subdued gain for an acquirer from a horizontal merger for each country.

In Panel B of Table 8, we examine whether our findings remain robust to using more stringent levels of fixed effects. In column 1, we introduce industry and country-by-year fixed effects to account for any unobserved, time-varying country-level factors. For instance, the M&A regulation environment for each country may change over time due to reasons unrelated to the antitrust law adoption. We show that the coefficient of AFTER × TREAT × HORIZONTAL is still negative and significant after including country-by-year fixed effects. In column 2, we include country and industry-by-year fixed effects to further account for any unobserved, time-varying industry-level factors. For instance, Harford (2005) documents that industry merger waves occur due to economic and technological shocks, which are not necessarily related to antitrust policy. However, in column 2, our findings remain robust to further controlling for industry-by-year fixed effects. In column 3, we include year and country-by-industry fixed effects because time-invariant industry-level factors may vary across countries. Country-by-industry fixed effects do not nullify our findings. In column 4, we introduce country-by-industry-by-year fixed effects to account for the possibility that time-varying industry-level factors may be different across countries. For instance, industry merger waves may occur at different points in time for each country. The coefficient of AFTER × TREAT × HORIZONTAL is still negative and significant in column 4. Thus, unobserved, time-varying industry-level factors in each country do not drive our findings.

In columns 5 and 6 in Panel B of Table 8, we further include acquiring firm fixed effects to address the concern that any unobserved, time-invariant acquirer characteristics may confound our inference. Column 5 includes firm and year fixed effects, and column 6 uses firm and country-by-industry-by-year fixed effects. In both columns 5 and 6, the coefficient of AFTER × TREAT × HORIZONTAL is negative and significant. Thus, unobserved acquirer characteristics do not seem to drive our results.

In column 7 in Panel B of Table 8, we use industry, horizontal-by-year, horizontal-by-country, and country-by-year fixed effects to fully isolate our DDD estimate. This specification further alleviates the concern that the time-varying returns from a horizontal merger may drive our findings or that country-specific factors may manifest differently between a horizontal and a non-horizontal M&A. However, column 7 shows that the coefficient of AFTER × TREAT × HORIZONTAL is still negative and statistically significant, which further mitigates the concern stemming from omitted variable bias.

[22] In an untabulated analysis, we impose the 6-year condition on the subsample that covers only the treated observations and use the same specification as that in column 1 in Panel A of Table 8. The result remains robust.

In Panel C of Table 8, we test whether the results are robust to using different time windows to estimate CARs. Instead of 5-day CARs, we use 3-, 7-, and 11-day CARs surrounding each merger announcement as the dependent variables. The coefficient of AFTER × TREAT × HORIZONTAL is negative and significant in all columns, except for column 2. Our results remain robust to using different time windows to estimate CARs.

In the Supplementary Material, we further show that requiring a minimum deal value of $1 million neither tilts our sample toward developed countries nor nullifies our findings (Table OA4 and OA5 of the Supplementary Material). The baseline findings are robust to removing other M&A sample filters as well (Table OA6 and OA7 of the Supplementary Material). Also, in our untabulated analysis, we find that other deal characteristics for horizontal M&As (tender offer, 100% cash or stock payment, friendly takeover, and competing bids from other parties) hardly related to antitrust regulation do not show significant variation surrounding the law adoptions. Thus, it seems unlikely that changes in deal characteristics that occur after, but are unrelated to antitrust law adoptions, may lower acquirers' gains from horizontal deals.

E. Addressing Bias from Heterogeneous Treatment Effects

Recent studies question whether the staggered DiD/DDD design provides reliable inferences (e.g., Goodman-Bacon (2021)). The problem arises when the magnitude of a treatment effect varies across units (e.g., firms or countries) or over time. In our context, the stringency of antitrust regulation is likely to be different across countries. The content of antitrust regulation in one country is likely not identical to one in a different country. Also, even in one country, relatively minor regulatory adjustments subsequent to a major law adoption can make the treatment effect vary over time. Thus, the staggered DDD design in our paper may also be subject to the biases noted by the literature.

To address this concern, Gardner (2021) proposes a 2-stage methodology. In the first stage, one regresses an outcome variable on group and period fixed effects, using only the untreated observations in the data set. In the second stage, one subtracts the estimated group and period fixed effects from an observed outcome, and the adjusted outcome variable is regressed on the treatment variable of interest. Gardner (2021) argues that this methodology isolates the overall average treatment effect on the treated, even with the heterogeneous treatment effects across units or over time.

Using the methodology of Gardner (2021), we check the robustness of the three results: i) whether the law adoptions in this study make the antitrust regulatory environment stringent, ii) whether the antitrust law adoptions decrease acquirers' gains from horizontal deals, and iii) whether deal value shrinks for horizontal deals after law adoptions.[23] Panel A of Table 9 uses the Competition Law Index (CLI) data set provided by Bradford and Chilton (2019) and Panel B uses our full sample. In the first stage, we include the controls and the fixed effects. In the second stage,

[23]We use the "did2s" STATA package to follow the methodology of Gardner (2021). The package does not report R^2 or adjusted R^2.

TABLE 9

Addressing Bias Due to Heterogeneous Treatment Effects Across Units/Over Time

Table 9 shows that our results are robust to using the methodology of Gardner (2021), which addresses the bias in the staggered DID design due to heterogeneous treatment effects across units and over time. In Panel A, the dependent variables are the overall Competition Law Index (CLI), which measures the intensity of competition law enforcement and the index's score for the four subcategories: antitrust authority, merger control, abuse of dominance, and anticompetitive agreements for each country in year *t* (Bradford and Chilton (2019)). We include country and year fixed effects. In Panel B, the dependent variables are an acquirer's 5-day CARs surrounding a merger announcement and the natural logarithm of deal value. We include 2-digit SIC industry, year, and country fixed effects. We compute *z*-statistics (in parentheses) using standard errors clustered at the country level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The constants are omitted for brevity. Appendix B presents variable definitions.

Panel A: Stringency of Antitrust Regulation

	CLI	ANTITRUST_ AUTHORITY	MERGER_ CONTROL	ABUSE_OF_ DOMINANCE	ANTICOMPETITIVE_ AGREEMEENTS
	1	2	3	4	5
AFTER × TREAT	0.313***	0.189**	0.501***	0.185	0.284***
	(3.63)	(2.09)	(6.35)	(1.39)	(3.13)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
No. of obs.	394	394	394	394	394

Panel B: Acquirer's Gain and Deal Value

	CAR_5DAY	LOG_DEALVALUE
	1	2
AFTER × TREAT × HORIZONTAL	−0.046***	−0.312**
	(−4.41)	(−2.24)
AFTER × TREAT	0.020	0.523
	(1.06)	(1.34)
TREAT × HORIZONTAL	0.014	0.141
	(1.60)	(1.33)
HORIZONTAL	0.001*	0.114***
	(1.68)	(9.27)
Controls	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
Country FE	Yes	Yes
No. of obs.	26,472	26,472

we only consider our indicators of interest: AFTER × TREAT in Panel A, and our main DDD term, AFTER × TREAT, TREAT × HORIZONTAL, and HORIZONTAL in Panel B. Panels A and B show that the three results remain robust to using the methodology of Gardner (2021). Thus, based on Table 9, we conclude that the heterogeneity in antitrust law regulations across countries and over time does not nullify our results.

V. Conclusion

Existing literature reaches divergent and even conflicting conclusions regarding how antitrust regulation affects horizontal merger performance. In addition, prior studies mostly focus on the United States and the European economies that adopted antitrust policies decades ago. Because the data on international mergers and acquisitions tend to be scarce prior to the 1990s, solely focusing on the relatively established economies makes it difficult for researchers to study the

effects of antitrust laws. Thus, this paper shifts attention to countries that introduced merger-control regulation in recent decades with relatively sufficient data availability. We use a quasi-experimental design using the staggered adoption of antitrust laws in each country from 1989 to 2015, covering 27,113 M&A deal observations and 9,931 unique acquirers in 20 countries.

The law adoptions we identify significantly increase the stringency of competition law enforcement in the treated countries. Effective antitrust laws reduce acquirers' 5-day CARs surrounding horizontal-merger announcements. The subdued gains for acquirers from horizontal mergers under merger control stems from the decrease in post-merger monopolistic gains through horizontal M&As. Our findings are most consistent with the market-power hypothesis. The bias from heterogeneous treatment effects does not nullify our results based on the staggered DDD design. Overall, our study suggests that antitrust policies reduce anticompetitive rents in countries that adopted merger control regulation in recent decades.

Appendix A. Source of Information for Antitrust Regulation in Each Country

Country	Name of the Law	Source
Australia	Trade Practices Act (1974)	Nagarajan (2013)
Brazil	Brazilian Competition Act of 1962	Cowie and de Mattos (1999)
Chile	Decree Law No. 211 of 1973	ICLG
China	Chinese Antimonopoly Law (AML)	Getting The Deal Through, ICLG, and Global Legal Insights
France	Law No. 77-806, 1977 J.O. 3833	Schwartz (1993)
Germany	Act against Restraints of Competition of 1958 (GWB)	Schwartz (1993) and Getting The Deal Through
Hong Kong	The Hong Kong Competition Ordinance (Cap 619)	Getting the Deal Through and Global Compliance News
India	The Competition Act of 2002	ICLG and Getting the Deal Through
Indonesia	Law No. 5 of 1999 on the Prohibition of Monopoly and Unfair Business Competition Practices	Getting the Deal Through and Global Legal Insights
Japan	The Act on Prohibition of Private Monopolisation and Maintenance of Fair Trade (Act No. 54 of 1947)	ICLG, Getting the Deal Through and Global Legal Insights
Malaysia	The Competition Act of 2010	ICLG, Global Compliance News, and Global Legal Insights
Mexico	Federal Law on Economic Competition (Ley Federal de Competencia Económica (FLEC))	Aydin (2016)
Norway	Norwegian Competition Act No. 65 of 11 June 1993.	Papadopoulos (2010)
Singapore	The Competition Act (Cap. 50B)	Global Competition Review
Switzerland	The Swiss Cartel Act of 1995 ("CartA")	Global Legal Insights
Thailand	The Trade Competition Act B.E. 2542 (1999)	Getting the Deal Through
Türkiye	The Law on Protection of Competition No. 4054 ("Law No. 4054")	Global Legal Insights
Taiwan	The Taiwan Fair Trade Act	ICLG
United Kingdom	The Monopolies and Restrictive Practices (Inquiry and Control) Act, 1948	The British Monopolies Act of 1948: A contrast with American policy and practice (1950)
United States	The Sherman Antitrust Act of 1890	ICLG, and Global Legal Insights

Appendix B. Variable Definitions

Main Independent Variables

TREAT: Indicator equal to 1 if the country where a firm is headquartered enacts an antitrust law during the sample period, and 0 otherwise.

AFTER: Indicator equal to 1 if a merger is completed with an acquirer in the treatment group after the enactment of an antitrust law in the country, and 0 otherwise.

HORIZONTAL: Indicator equal to 1 if the acquiring and target firms share the same 4-digit SIC code, and 0 otherwise (Alfaro and Charlton (2009)).

Dependent Variables

CLI: The Competition Law Index for each country in a year in the Bradford and Chilton (2019) data set. The CLI score is normalized to be between 0 and 1.

ANTITRUST_AUTHORITY: The subcategory score of the CLI showing the authority of an antitrust agency for each country in a year, in the Bradford and Chilton (2019) data set.

MERGER_CONTROL: The subcategory score of the CLI showing how restrictive a merger control regulation is for each country in a year, in the Bradford and Chilton (2019) data set.

ABUSE_OF_DOMINANCE: The subcategory score of the CLI showing how restrictive a country is in preventing a firm from exploiting its dominant position in a market, in the Bradford and Chilton (2019) data set.

ANTICOMPETITIVE_AGREEMENTS: The subcategory score of the CLI showing how restrictive a country is in preventing product market coordination between firms, in the Bradford and Chilton (2019) data set.

CAR_5DAY: An acquirer's 5-day CARs, calculated using the market-adjusted model. The abnormal return is the difference between the daily stock return and the market return in the acquirer's country.

LOG_DEALVALUE: The natural logarithm of the value of transaction (SDC Platinum: Dealval) for an M&A deal.

LOG_TARGET_BOOK_ASSET: The natural logarithm of a target firm's book asset value on a balance sheet. (SDC Platinum; TASS).

$\Delta COGS_{t+1 \text{ to } 2 \text{ or } 3}$: The change from year $t+1$ to $t+2$ (or $t+3$) in industry (3-digit SIC)-median-adjusted cost of goods sold scaled by book assets, with t corresponding to the year of the merger announcement.

$\Delta SG\&A_{t+1 \text{ to } 2 \text{ or } 3}$: The change from year $t+1$ to $t+2$ (or $t+3$) in industry (3-digit SIC)-median-adjusted selling, general, and administrative costs scaled by book assets, with t corresponding to the year of the merger announcement.

PEER_CAR_5DAY: The equal-weighted average of the 5-day CAR of the acquirer's peers. Peers are headquartered in the same country as the acquirer and share the same 4-digit SIC code as of the merger announcement in year t.

$\Delta PROFIT_{t+1 \text{ to } 2 \text{ or } 3}$: The change from year $t+1$ to $t+2$ (or $t+3$) in industry (3-digit SIC)-median-adjusted operating income before depreciation and amortization scaled by book assets, with t corresponding to the year of merger announcement.

$R\&D_{t+k}$: The ratio of R&D expenditures (Compustat: XRD) to book assets (Compustat: AT) for firm i in year t, where k = 1, 2, or 3 and t is the merger announcement year. We set XRD to 0 if it is missing.

Control Variables

LOG_ASSET: The natural logarithm of a firm's book assets (Compustat AT) in year t.

RELATIVE_SIZE: The ratio of the M&A deal value (SDC Platinum Dealval) to the acquirer's market value of equity. The market value of equity is the stock price in U.S. dollars multiplied by the firm's common shares outstanding.

ROA: Net income (Compustat NI) of a firm in year t scaled by its book assets (Compustat AT) in year t.

TOTAL_LEVERAGE: The sum of long-term debt (Compustat DLTT) and debt in current liabilities (Compustat DLC), scaled by book assets (Compustat AT).

CASH: A firm's cash (Compustat CH) scaled by book assets (Compustat AT).

TANGIBILITY: A firm's net property, plant, and equipment (Compustat PPENT) scaled by book assets (Compustat AT).

TOBIN_Q: A firm's stock price at fiscal year-end (Compustat PRCC_F) multiplied by its common shares outstanding (Compustat CSHO), plus book assets (Compustat AT) minus book shareholder equity (Compustat SEQ), scaled by the firm's book assets (Compustat AT).

PURE_CASH: An indicator equal to 1 if the percentage of consideration paid in cash (SDC Platinum PCT_CASH) is 100%, and 0 otherwise.

PURE_STOCK: An indicator equal to 1 if the percentage of consideration paid in stock (SDC Platinum PCT_STK) is 100%, and 0 otherwise.

TENDER: An indicator equal to 1 when a tender offer is launched for the target (SDC Platinum TENDER), and 0 otherwise.

PUBLIC_TARGET: An indicator equal to 1 if a target is a public firm (SDC Platinum TPUBLIC), and 0 otherwise.

GDP_GROWTH: The country's annual GDP growth rate (Indicator code: NY.GDP.MKTP.KD.ZG; Source: World Development Indicator).

FDI_INFLOW: The country's annual net inflow (% of GDP) of foreign direct investment (Indicator code: BX.KLT.DINV.WD.GD.ZS; Source: World Development Indicator).

ICRG_QOG: ICRG Quality of Government Index (Source: The Quality of Government Institute, University of Gothenburg).

TRADE_SHARES: Share of a country's trade with foreign countries, relative to its GDP each year, defined and recorded in the Bradford and Chilton (2019) data set.

GLOBALIZATION: The globalization index for each country in a year, developed by Dreher (2006) and recorded in the Bradford and Chilton (2019) data set.

POPULATION: The natural log of a country's population each year, in the Bradford and Chilton (2019) data set.

POLICY_BUDGET: The amount of government budget assigned to a country's antitrust regulatory agency each year, scaled by the country's GDP, in the Bradford and Chilton (2019) data set.

Conditioning Variables

COMMON_LAW (or CIVIL_LAW): An indicator equal to 1 if an observation is in a country with a common law (civil law) origin based on Lopez-de-Silanes et al. (1998) and other legal sources, and 0 otherwise.

EMERGING: An indicator equal to 1 if a country is an emerging market country based on MSCI's classification, and 0 otherwise.

HHI: Herfindahl–Hirschman Index computed at the 3-digit SIC level each year based on Compustat.

FREE_CASH_FLOW: The amount of free cash flow for a firm in year *t,* scaled by market capitalization (Lehn and Poulsen (1989)).

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023000467.

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