ONLINE APPENDIX

A New Era of Midnight Mergers: Antitrust Risk and Investor Disclosures

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1 Supplementary tables and figures

The following is a list of the supplementary tables and figures referenced in the body of the main text.

1. Figure A.1 replicates the main result from Section IV (i.e., Figure 1) except that we construct the running variable from level rather than log values of the transaction-value-to-acquiror-assets ratio. Similar results obtain, which mirrors comparisons of the first three and last three columns of Table 3 in the body of the main text.

2. Table A.1 provides estimates of τ under the falsification test. That is, it replicates Table 3 except that the underlying sample is HSR rather than non-HSR mergers. Estimates of τ are near zero, reflecting the fact that the horizontal share of mergers trends smoothly through the cutoff value.

3. Figure A.2 plots the density of the running variable among non-HSR mergers. Consistent with investor disclosures posing antitrust risk, the plot exhibits a discontinuity at the cutoff among horizontal mergers. Consistent with non-horizontal mergers facing an extremely low threat of enforcement, no discontinuity is present among those transactions.

4. Figure A.3 replicates the prior figure except that the underlying sample is HSR rather than non-HSR mergers. In line with the idea that the government is already fully informed about HSR mergers vis-a-vis premerger notifications, no discontinuity exists among either horizontal or non-horizontal HSR mergers.

5. Table A.2 presents results for various placebo experiments. It reports estimates of $(\hat{\alpha}_{y,\downarrow} - \hat{\alpha}_{y,\uparrow})$, where y is one of the following outcomes: the date that the merger is completed, a dummy variable indicating the deal was structured to transfer assets rather shares, the proportion of the transaction value paid in cash, a dummy variable indicating that all of the transaction value was paid in cash, the transaction value, and the assets of the acquiror. See the body of the paper for an interpretation of the findings.

6. Table A.3 reports estimates of τ by sector. The first two columns consider mergers for which the target is operates in a manufacturing and service industry, respectively, while the second two consider mergers where the acquirer operates in one of those respective sectors. No obvious heterogeneity emerges. $\hat{\tau}$ is qualitatively

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similar to our main estimate but but less precisely estimated, as sector-specific estimates are based on 2/3 fewer observations, which mechanically increases the standard errors.

7. Table A.4 reports estimates of τ by US Presidential administration. We split the sample into mergers completed before or after January 1, 2009 (Barack Obama took office from George W. Bush on January 20, 2009). We then re-estimated τ for each sub-sample separately. $\hat{\tau}$ is much larger under President Obama's administration than President Bush's. The former estimate is not significant at the 10% level, while the latter is significant at the 5% level. These estimates are consistent with the notion that Republican Presidents have a more lax approach to antitrust enforcement.

8. Table A.5 reports estimates of τ by previous enforcement. We scrape the FTC and DOJ websites and extract the acquirer name and date associated with each challenged HSR merger between 1998 and 2016. In our transaction-level merger dataset, we could then construct a dummy indicating whether each acquirer SIC4 experienced an enforcement action at some point in the past five years. We place mergers for which this was true in one group ("Acquirer SIC faced enforcement") and remaining mergers in a second group ("Acquirer SIC did not face enforcement") We then estimate τ separately for these two groups. The results are consistent with the notion that antitrust risk is higher in industries that have recently experienced an HSR enforcement action, i.e., been under recent scrutiny.

9. Table A.6 reports estimates of τ by whether the acquirer has a contemporaneous HSR merger. We constructed a dummy indicating whether each acquirer was involved in at least one HSR merger in the same year. We placed mergers for which this was true in one group ("Involved in one or more contemporaneous HSR mergers") and remaining mergers in a second group ("Not involved in any contemporaneous HSR merger") We then estimated τ separately for these two groups. The results are consistent with the idea that antitrust risk among non-HSR mergers is higher for firms that have recently proposed or completed an HSR merger.

10. Table A.8 assesses heterogeneity in τ across the agencies authorized and responsible for overseeing mergers in particular industries per the 2002 Memorandum of Understanding between the DOJ and FTC. The findings show qualitatively consistent results, with larger effects observed for DOJ-enforced mergers compared to those enforced by the FTC. However, due to the wide confidence intervals, we exercise caution in drawing definitive conclusions regarding heterogeneity across the agencies.

11. Table A.7 reports estimates of τ using broader industry definitions—horizontal overlap is defined by acquirer-target overlap at the SIC2 rather than SIC4 industry. $\hat{\tau}$ is around 1/4 the value we obtained in our main specification. This is consistent with the idea that, for example, a merger between a manufacturer of home furniture and a manufacturer of office furniture typically does not reduce competition.

12. Figure A.4 plots revenue and capital expenditures over time.

[Figure A.1 about here.]

[Table A.1 about here.]

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[Table A.8 about here.]

[Figure A.4 about here.]

2 Thomson/SDC coverage and characteristics

We rely on data from Thomson/SDC for various reasons. It is by far the most widely used. For example, Bollaert and Delanghe [2015] report that "in the top four finance journals from 2000 to 2012, more than 75% of papers use [Thomson/SDC]. (Our informal, updated tally of "top four" finance journals indicates the figure is even higher after 2012.) It is also the most complete source for US mergers involving public companies. The most commonly employed alternative, Zephyr, is mainly used to study foreign transactions. Likewise, rival sources such as PitchBook and crunchbase focus on recent acquisitions that typically involve private equity sponsors. Moreover, Thomson/SDC has held this distinction for many years. For instance, Netter et al. [2011] report that as early as 1989, Thomson/SDC was "complete" relative to Grimm, the leading alternative at the time. Perhaps most compelling, Thomson/SDC publishes the most widely cited "league tables" used to rank investment banks and law firms by total deal volume. Since these periodic tabulations of advisory and legal fees are used to measure the performance and judge reputation, there are strong incentives to report transactions.

This is not to say that Thomson/SDC records every transaction. An industry-specific source such as Pharma Intelligence, which narrowly focuses on biotechnology, pharmaceuticals, and medical devices, may report deals that our source misses, but in our experience additional records are often missing critical information such as transaction values. If one believes the right definition of a basic disclosure reflects every possible information source, regardless of whether it is open or proprietary, then our definition overstates undisclosed merger activity. However, one should bear in mind that the dataset (a) would be prohibitively costly to construct by any organization throughout the sample period and at the time of writing, (b) will encounter other technical problems, described in Appendix 3, that Thomson/SDC solves, and (c) still will not cover the cash acquisitions reflected in the firms' cash flow statements.[1]

Other studies indicate Thomson/SDC provides not only comprehensive data but also accurate records. As one example, Barnes et al. [2014] draws from a wide range of sources (e.g., Wall Street Journal Index, Dow Jones Online/Factiva, Lexis-Nexis, media reports, et cetera) to determine announcement dates of mergers and reports that Thomson/SDC is very accurate. However, it implicitly restricts attention to larger mergers, which may be especially easy for Thomson/SDC to correctly characterize. As another example, Fuller et al. [2002] examines effectively all mergers involving publicly traded US acquirers and assesses accuracy in a random sample of 500 of them. Notably, it imposes similar restrictions to the ones we impose when downloading transactions from Thomson/SDC. The authors find that in 93% of cases, SDC was exactly correct, and "in the other cases [SDC's announcement date] was only off by two days at most." Moreover, while the authors study 1990-2000, we study

[1]For example, one of the authors has studied acquisitions of dialysis facilities by large chains, some of which are publicly traded. Data from Medicare reveals small transactions for which there are no other public record (i.e., no information from fuzzy and verbatim searches of Google, Bing, various news archives (e.g., ProQuest), all know M&A datasets including sector-specific ones (e.g., Levin and Associates), and the firms' own websites). Since Medicare ownership changes are reported with several years' delay, these transactions were unambiguously "undisclosed" around the time of their completion.

2002-2016, meaning that the information we downloaded from Thomson/SDC is even more accurate than what they did (so long as data quality improves over time).

To illustrate the accuracy of Thomson/SDC in our context, we exploit the relationship between v_i and C_{ft} described in Section 6.[2] Conceptually, we can use cash flow statements to obtain "transaction true" values, compute the ratio of v_i to C_{ft}, and inspect the density for a spike at one, indicating that Thomson/SDC reports the exactly right value in many cases. To operationalize this, we restrict attention to firm-year observations where we observe (a) exactly one transaction and (b) that transaction is all cash. We then computed the ratio of v_i to C_{ft} and plotted the density of the ratio. Figure A.5, below, reports the result. The density spikes at exactly 1.00.

[Figure A.5 about here.]

3 Thomson/SDC record collection and creation

3.1 Do Thomson/SDC record collection efforts drive our results?

Our research design leverages changes around Item 2 cutoff values to examine antitrust risk. However, there are concerns that Thomson/SDC's data collection methods, particularly their reliance on Item 2 reports, may introduce confounding factors into our results. We address these concerns with three key points. First, our discontinuity-based research design does not depend on overall changes in the number of mergers at the cutoff. Instead, it focuses on comparing horizontal and nonhorizontal mergers. Even if we cannot completely rule out this possibility, it is unclear why Item 2 reports would affect these two differently. Second, if Item 2 reports biased Thomson/SDC's record collection towards nonhorizontal mergers, we would expect this pattern to emerge for both non-HSR and HSR mergers. However, our discontinuity-based findings only hold for non-HSR mergers and are absent among HSR mergers. Third and perhaps most importantly, 8-Ks are not an important means by which Thomson/SDC discovers mergers.

To support this last claim, we downloaded and analyzed information derived from an additional variable, *Sources of Deal Info*, which lists the sources from which Thomson/SDC obtain information about the transaction. Examples include "Dow Jones Institutional News | Press Release," "Law Firm Survey | Press Release | News Release | 8-K Filing," and "Press Release | Street Event ID | Web Site of Company | Business Wire." We imposed on this data similar restrictions to the ones we impose on the sample that generates are our main results (e.g., we require that the acquirer is a public company, that the transaction was completed between 2002 and 2016, et cetera). We then counted the number of times each source appears. The 8-K accounts for only 4.8%. By comparison, press releases, including those distributed over news wires, account for 84.6%. If we restrict attention to non-HSR mergers and recompute these shares, we reach similar figures (5.39% and 83.9%, respectively). The same is true if we restrict attention for mergers around the Item 2 threshold. That is, if we restrict attention to mergers whose ratio of transaction value to acquirer assets are in the range of those used to construct our main results, we find 5.7% and 85.6%, respectively. The same is also true if we restrict attention to either horizontal or non-horizontal mergers (i.e., the shares are 5.38% and 84.54% for horizontal mergers only, respectively, and 4.42% and 84.65% for non-horizontal mergers, respectively). In summary, the fact that such a small proportion of Thomson/SDC deals are sourced from 8-Ks and the fact that this proportion does not vary based on whether the merger is horizontal/non-horizontal minimizes the concern.

[2]We thank an anonymous referee for this suggestion.

3.2 Do mergers in Thom./SDC reflect disclosures made long after completion?

Agencies must learn about mergers in their incipiency to police them effectively. If many mergers appear in our data as a result of disclosures made long after completion, then it is unlikely they would involve antitrust risk. However, nearly all of the transactions reported by Thomson/SDC were recorded within days, or earlier, of completion.

To see this, we downloaded and analyzed information derived from an additional variable, *Date Deal was Created in SDC Database*. As the name implies, it reports the date on which each transaction, defined as a unique SDC Deal Number, was first added to the dataset. Again, we imposed on this data similar restrictions to the ones we impose on the sample that generates are our main results. We then compared *Date Deal was Created in SDC Database* to the completion date. We find that within five days of the completion date, 95.2% of mergers appear in Thomson/SDC. When we restrict attention to non-HSR mergers or mergers around the Item 2 threshold, we obtain very similar figures (93.7% and 96.2%, respectively). The same is true if we restrict attentions to only horizontal or non-horizontal mergers (95.5% and 95.0%, respectively). In summary, a very small proportion of mergers are reported long after completion, and this proportion does not differ based on whether the merger was subject to premerger notification or whether it was horizontal.

4 Manipulation of thresholds

Item 2 reportability depends on a comparison between transaction value and acquirer assets. In the language of the SEC Financial Reporting Manual, it depends on the ratio of "total GAAP purchase price" to the "registrant's consolidated total assets." The numerator leaves little room for discretion. "Total GAAP purchase price" is defined as "consideration transferred", meaning "[i]t includes the acquisition-date fair value of all contingent consideration and excludes acquisition-related costs."[3] In turn, under FASB Accounting Standards Codification 805, "contingent consideration" as it applies to consideration transferred to a seller requires firms to employ a "scenario-based technique." Firms compute "scenario-specific cash flow estimates," which "are then probability weighted and discounted using an appropriate discount rate."[4] The denominator leaves even less room for discretion. Acquirer assets must be read off the most recent annual report's balance sheet. Each figure is audited by independent certified public accountants. Historically, intentional misstatements were taken very seriously. This is especially true since the Enron and WorldCom scandals, which occurred around 2000. As a result, we view manipulation of Item 2 thresholds as a very infrequent occurrence.

Interestingly, recent work by Kepler et al. [2020] suggests that manipulation of HSR thresholds, which are also based on transaction values,[5] can occur. The main mechanism that the authors identify involves earnout payments—contingent consideration paid to the seller after closing that are triggered by the acquired entity reaching performance milestones (see their page 25-26). Earnouts are added to baseline payments if and only if the board of directors determines that they are likely to be paid. Notice that this approach differs from the one employed by the SEC. Whereas Item 2 reportability requires firms to compute a weighted average over the probabilities that various milestones are met, earnout payments are all-or-nothing under the HSR Act and Rules. Moreover, whereas the former case involves independent outside auditors with specialized degrees, the latter depends on the business judgment of the board of directors. Thus, manipulation identified by Kepler et al. [2020] are not in immediate conflict with facts presented in the preceding paragraph (or the conclusions drawn from them). The differences in the mechanisms and decision-making processes between Item 2 reportability and earnout

[3] See https://www.sec.gov/corpfin/cf-manual/topic-2.

[4] See, e.g., PriceWaterhouseCooper's formal interpretation of ASC 805, which is available at https://viewpoint.pwc.com/dt/us/en/pwc/accounting_guides/fair_value_measureme/fair_value_measureme__9_US/chapter_7_applicatio_US/73_business_combinat_US.html.

[5] Reportability depends on the "value of what is being acquired." See 16 CFR Section 801.10.

payments under the HSR Act and Rules suggest that these forms of manipulation can coexist with the findings and conclusions discussed earlier.

In the data, manipulation around the threshold typically results in bunching—excess mass immediately to the left of the cutoff that results from merging parties immediately to the right of it adjusting their transaction-value-to-acquirer-asset ratios to just below 10%. Panel A of Figure A.2 does not appear to exhibit this pattern. However, assessing manipulation based on visual inspections is difficult, especially when the density is sloped. For a more formal test, we adapt an idea proposed by Chetty et al. [2011]. Conceptually, the test is constructed as follows. First, place the data into equally sized bins and count the number of observations in each bin. Second, define an excluded region immediately to the left of the threshold in which bunching might occur. Third, ignore the excluded region, fit an n^{th}-order polynomial function of the running variable to the number of observations in each bin, and predict the number of observations in each excluded bin. Fourth, compute the difference between the observed and predict number in the excluded bins. Fifth, obtain standard errors around the difference by bootstrapping (with replacement). One can then evaluate whether there is excess mass in the bins residing in the excluding region.

We adapt the logic of this test to our setting. We restrict attention to observations to the left of the threshold in Panel A of Figure A.2, and we place them into J bins of equal width, each indexed by j. We count the number observations in each bin and denote this value by c. We denote the midpoint of the bounds of each bin by z. We also denote the number of excluded bins by R. We then estimate the coefficients on a n^{th}-order polynomial for all bins $j \leq J - R$:

$$c_j = \sum_{i=0}^{n} \beta_i (z_j)^i + \epsilon_j. \tag{1}$$

We set $n = 4$, and then we repeat the exercise with $n = 5$ (for a sense of robustness). We use these estimates to predict the number of observations in each bin $j > J - R$, which equals $\hat{c}_j = \sum_{i=0}^{n} \hat{\beta}_i (z_j)^i$. The average excess mass in the excluded bins equals

$$\hat{B} = R^{-1} \sum_{j=J-R+1}^{J} \left(c_j - \hat{c}_j \right). \tag{2}$$

We then construct K bootstrap samples, drawing with replacement, compute $\hat{B}_k$ for $k = 1, 2, ..., K$, and estimate the standard error of $\hat{B}$. When we conduct the set, we set $K = 500$, $J = 64$, and, for robustness, consider $R \in \{1, 2, 3, 4, 5\}$. Table A.9 reports the results. There is no evidence of bunching just left of the Item 2 cutoff.

[Table A.9 about here.]

5 Compliance with reporting requirements

To understand noncompliance, we manually inspected mergers underlying Figure 1. As the SEC states that Item 2 should only be used for deals that satisfy reporting requirements, noncompliance cases consist not only of missing Item 2 reports for mergers whose transaction-value-to-acquirer-asset ratios exceed the cutoff but also existing Item 2 reports for mergers whose ratios fall short of the cutoff. Some discrepancies arise due to timing. Some are due to timing. The SEC stipulates that the acquirers' assets should be taken from the most recent audited balance sheet. This does not necessarily correspond with the balance sheet from which Thomson/SDC draws acquirer assets—sometimes it uses quarterly statements, which are not audited. Typically, transactions values are measured accurately. The only discrepancies encountered were due to contingent consideration, i.e., cases where Thomson/SDC did not count milestone payments towards the purchase price, even though it frequently mentioned them in the deal synopsis variable.

However, even after these adjustments, noncompliance exists (see, e.g., Panel A of Figure **??**). There are four likely sources. One relates to an alternative means by which Item 2s are required. Even if the transaction value is less than 10% of the acquirers assets, an Item 2 is mandatory if the after-tax net income of the target meets or exceeds 10% of the after-tax net income of the acquirer. Mergers are unlikely to pass the second test when they have failed the first, but we cannot rule this out entirely, so it may account for some noncompliance to the left of the cutoff. Another source involves equity-based consideration. When shares of the acquirer comprises some or all of the consideration, the transaction value moves with the stock price, meaning Thomson/SDC cannot measure it exactly. Third, firms undoubtedly make mistakes.[6] Mistakes are especially plausible in the support of the x-axis values plotted in Figures 1 and 2, since firms whose acquisitions are around the cutoff are relatively small, meaning they may have less advanced accounting and compliance departments. Fourth, our merging process undoubtedly also produces mistakes that our manual inspection did not correct.

6 Deterrence by enforcement

In our context, deterrence occurs because orchestrating and structuring a merger requires large fixed costs that are not recouped if the deal is blocked or abandoned, i.e., potential acquirers that expect their deals will be blocked will not attempt them in the first place. Hence, the degree of deterrence depends on the probability of enforcement.[7] Historically, only a small proportion of US mergers faced enforcement. For example, from 2000 to 2020, only about 3% of HSR mergers received a Second Request (i.e., intensive scrutiny), and many of these were completed without any remedies. Among non-HSR mergers, the comparable figures are even lower. From these statistics, one might reasonably think that deterrent effects are weak, especially among smaller mergers. However, the risk of enforcement and its relationship with deal attributes are fundamentally hard to infer from this information. Along the lines of the Lucas [1976] critique, historical merger enforcement rates, or differences therein, are usually uninformative in equilibrium. For instance, in the extreme case of perfect deterrence, no enforcement is observed because no anticompetitive mergers are attempted.

A better source of information is a large merger that will affect many markets. In this case, an acquirer may anticipate divestitures in markets where competition will be reduced but still rationally propose the deal under the expectation that large profits will be generated from acquiring assets in other markets. As a result, a researcher may observe components of a merger that would never be attempted on their own, as they would be deterred. What do these types of quasi-experiments reveal? In the dialysis industry, acquisitions that would result in market-level monopoly face enforcement (i.e., divestitures of facilities previously owned by the target) about 90% of the time [Wollmann, 2021].

Likewise, while observed enforcement rates are lower among smaller mergers, deterrent effects may be stronger. One reason is that their competitive effects are easier to predict. For example, larger mergers often affect many business lines and geographies, which may in turn involve complicated demand- and cost-side interdependencies, whose net effects require some subjective evaluation. In contrast, smaller mergers are often simpler, which makes their net effects easier to estimate, enforcement more predictable, and deterrence more effective. The other reason is that orchestrating and structuring a merger may require large fixed costs that are not recouped if the deal is blocked or abandoned. As the transaction's cost rises in relation to its potential benefit, deterrence rises, too.

[6]As one possible example, see the acquisition of Innovative Concepts by Hurley Industries. The transaction value is $20 million (see https://www.sec.gov/Archives/edgar/data/47035/000120180005000074/hrly8kapril2005-ici.txt) and the total assets of the acquirer are $220.971 million (see https://www.sec.gov/Archives/edgar/data/47035/000004703504000013/filing10k10152004.txt.). The ratio of the first figure to the second is less than 10%, so, putting extenuating circumstances aside, an Item 2 event did not occur. Yet, Hurley's management reported one.

[7]By "enforcement" we mean any agency action that effectively eliminates the profits from the transaction derived from reducing competition. Examples include abandonment, behavioral remedies, divestitures, or litigation resulting in the courts finding the merger is illegal. Note that under this definition, the agencies need not file a complaint. Merely filing a Request for Additional Information (i.e., "Second Request") may cause the parties to abandon the deal.

7 Number of deterred mergers

The number of mergers deterred over the sample period cannot easily be inferred from our estimates of τ. A discontinuity-based research design provides credible results but the underlying assumptions only restrict the relationship between mergers just above and below the Item 2 cutoff. This exercise must incorporate information from a wider range of running variable values. Based on our earlier assumption that premerger notifications fully inform the agencies about transactions in their incipiency, only non-HSR mergers are deterred. Hence, we restrict attention to non-HSR mergers in this exercise. Based on Panel B of Figure A.2, which reports that the number of nonhorizontal mergers trends smoothly through the cutoff, we assume that only horizontal mergers are deterred. This lets us further restrict attention to horizontal mergers. We also assume that $F_H(a)$ does not vary with Z (i.e., among horizontal non-HSR mergers, the distribution of the degree to which a merger reduces competition does not vary with the ratio of the transaction value to the acquirer's assets). This lets us extrapolate out from changes occurring at the cutoff to larger values of the running variable.

First, we estimate the number of mergers that would have received a basic disclosure but were deterred due to Item 2. For any value of the running variable to the right of the cutoff, this equals the number of observed mergers receiving a basic disclosure multiplied by $F_H(b^\star)/F_H(b^\star/\mu) - 1$. In Panel A of Figure A.6, below, this number is represented by the difference between the dashed and solid lines for positive x-axis values. To obtain $F_H(b^\star)/F_H(b^\star/\mu)$, we fit an fifth-order polynomial to the count of horizontal non-HSR mergers that receive a basic disclosure for running variable values to the left of the cutoff, and we do the same to the right of the cutoff. In Panel B of Figure A.6, below, the best fit lines are plotted to the left and right of the vertical red line, respectively. Our estimate of $F_H(b^\star)/F_H(b^\star/\mu)$ equals y-axis value at which $Z = 0$ is intersected from the left divided by the y-axis value at which $Z = 0$ is intersected from the right.

[Figure A.6 about here.]

We then integrate out over all values of the running variable to the right of the cutoff. Table A.10 reports the result. To ensure our estimates are are robust, we use different ranges of the running variable and different sized bins for it as well. We find that 212-362 horizontal non-HSR mergers that would have received basic disclosures are deterred. Most estimates are between 290 and 330. We then further assume that the average transaction value of a deterred merger, conditional on Z, is the same as the average transaction value of an observed one, which allows us to compute the predicted value of these transactions. Over the panel, they total \$3.8-6.5 billion. If at first it seems that around 20 mergers per year with an average value of \$20 million each are comparatively unimportant, then recall why these transactions are deterred. This set of mergers is composed entirely of ones that would have been blocked were the agencies to read the associated 8-K associated with them (or the information contained therein).

[Table A.10 about here.]

8 Proofs

Proposition 1. Let $\psi(\mu)$ denote the horizontal share of mergers in transaction level data. $\psi(1) < \psi(0)$.

Proof. The claim is that $Pr.(H|Y = 1, D \geq 1, \mu = 1) < Pr.(H|Y = 1, D \geq 1, \mu = 0)$, which is equivalent to the claim that $Pr.(H|Y = 1, D \geq 1, \mu = 1) - Pr.(H|Y = 1, D \geq 1, \mu = 0) < 0$. For notational convenience, we denote the unconditional share of horizontal mergers opportunities by ξ. Then

$$\begin{aligned}
&Pr.(H|Y=1, D\geq 1, \mu=1) - Pr.(H|Y=1, D\geq 1, \mu=0)\\
&= Pr.(H|Y=1, D=\bar{D}) - Pr.(H|Y=1, D=1)\\
&= Pr.(H|a<b^{\star}/\bar{D}) - Pr.(H|a<b^{\star})\\
&= \frac{\xi F_H(b^{\star}/\bar{D})}{\xi F_{NH}(b^{\star}/\bar{D}) + (1-\xi)F_{NH}(b^{\star}/\bar{D})} - \frac{\xi F_H(b^{\star})}{\xi F_{NH}(b^{\star}) + (1-\xi)F_{NH}(b^{\star})}\\
&\propto \frac{F_H(b^{\star}/\bar{D})}{F_{NH}(b^{\star}/\bar{D})} - \frac{F_H(b^{\star})}{F_{NH}(b^{\star})}\\
&= \int_{b^{\star}/\bar{D}}^{b^{\star}} \frac{-f_{NH}(a)F_H(a)}{F_{NH}(a)^2}\partial a + \int_{b^{\star}/\bar{D}}^{b^{\star}} \frac{f_H(a)}{F_{NH}(a)}\partial a\\
&\propto \int_{b^{\star}/\bar{D}}^{b^{\star}} \left[\frac{f_H(a)}{F_H(a)} - \frac{f_{NH}(a)}{F_{NH}(a)}\right]\partial a < 0. \qquad (3)
\end{aligned}$$

If $\mu = 1$, then $D = \bar{D}$, which implies that $D \geq 1$. Thus, we can replace replace $Pr.(H|Y=1, D\geq 1, \mu=1)$ with $Pr.(H|Y=1, D=\bar{D})$. If $\mu = 0$, then $D < \bar{D}$, which implies $D \leq 1$, since $D \in \{0, 1, \bar{D}\}$ and $\bar{D} > 1$. If $D \geq 1$ and $D \leq 1$, then $D = 1$. Thus, we can also replace $Pr.(H|Y=1, D\geq 1, \mu=0)$ with $Pr.(H|Y=1, D=1)$. Hence, we can move from the first line to the second. To arrive at the third line, we re-write the conditional probability as the probability of drawing certain values of a. To arrive at the fourth line, we follow Bayes' theorem. To arrive at the fourth line, we multiply through by the product of the denominators and then factor out like terms, which are all positive. To arrive at the fifth line, we integrate by parts. That is, we let $u(a) \equiv F_H$ and $v(a) \equiv 1/F_{NH}(a)$ so that the fifth line equals $u(b^{\star})v(b^{\star}) - u(b^{\star}/\bar{D})v(b^{\star}/\bar{D})$, and then substitute accordingly. To arrive at the sixth line, we factor out like terms, which are all positive. The final inequality follows from reverse hazard rate dominance. ■

Proposition 2. Assume the distribution of a does not depend on μ or ρ. If $Pr.(\mu=0, \rho=0) > 1$, then $Pr.(D=0|Y=1) > 0$.

Proof. By the rules of conditional probabilities, $Pr.(D=0|Y=1) = Pr.(\mu=0, \rho=0)Pr.(D=0|Y=1, \mu=0, \rho=0)$. Thus, it suffices to show that $Pr.(\mu=0, \rho=0)$ and $Pr.(D=0|Y=1, \mu=0, \rho=0)$ are both positive. The first holds by assumption. To see that the second is positive, notice that

$$Pr.(D=0|Y=1, \mu=0, \rho=0) = Pr.(D=0|\mu=0, \rho=0) = Pr.(a>b^{\star}|\mu=0, \rho=0) = Pr(a>b^{\star}) > 0. \qquad (4)$$

The first equality holds because management always choose $Y = 1$ when $\mu = 0$ and $\rho = 0$. The second equality holds because management choose $D = 0$ to avoid enforcement whenever a exceeds $b^{\star}$. The third equality holds because because the distribution of a does not depend on μ or ρ. The inequality holds because a has positive support on $[0, \infty)$ and $b^{\star} > 0$. Thus, $Pr.(D=0|Y=1) > 0$. ■

Proposition 3. Assume the distribution of a does not depend on μ or ρ. $Pr.(H|D{=}0, Y{=}1) > Pr.(H|D\neq 0, Y{=}1)$.

Proof. By Bayes' theorem,

$$Pr.(H|D=0, Y=1) = \frac{\xi Pr.(D=0, Y=1|H)}{\xi Pr.(D=0, Y=1|H) + (1-\xi)Pr.(D=0, Y=1|NH)} = \frac{\xi(1-F_H(b^{\star}))}{\xi(1-F_H(b^{\star})) + (1-\xi)(1-F_{NH}(b^{\star}))}. \qquad (5)$$

Define the details disclosed through an HSR filing by $\tilde{D}$ (i.e., if $\rho = 1$, then $D = \tilde{D}$). For notational convenience, define $\phi_1 = Pr.(\mu = 0, \rho = 0)$, $\phi_2 = Pr.(\mu = 1, \rho = 0)$, and $\phi_3 = Pr.(\rho = 1)$. By Bayes's theorem and the rules of conditional probabilities,

$$Pr.(H|D>0,Y=1) = \frac{\xi Pr.(D>0,Y=1|H)}{\xi Pr.(D>0,Y=1|H)+(1-\xi)Pr.(D>0,Y=1|NH)}$$
$$= \frac{\xi(\psi_1 F_H(b^\star)+\psi_2 F_H(b^\star/\bar{D})+\psi_3 F_H(b^\star/\tilde{D}))}{\xi(\psi_1 F_H(b^\star)+\psi_2 F_H(b^\star/\bar{D})+\psi_3 F_H(b^\star/\tilde{D}))+(1-\xi)(\psi_1 F_{NH}(b^\star)+\psi_2 F_{NH}(b^\star/\bar{D})+\psi_3 F_{NH}(b^\star/\tilde{D}))}. \quad (6)$$

Thus, Proposition 3 holds if and only if

$$\frac{\xi(1-F_H(b^\star)}{\xi(1-F_H(b^\star)+(1-\xi)(1-F_{NH}(b^\star)}$$
$$> \frac{\xi(\psi_1 F_H(b^\star)+\psi_2 F_H(b^\star/\bar{D})+\psi_3 F_H(b^\star/\tilde{D}))}{\xi(\psi_1 F_H(b^\star)+\psi_2 F_H(b^\star/\bar{D})+\psi_3 F_H(b^\star/\tilde{D}))+(1-\xi)(\psi_1 F_{NH}(b^\star)+\psi_2 F_{NH}(b^\star/\bar{D})+\psi_3 F_{NH}(b^\star/\tilde{D}))}, \quad (7)$$

which requires

$$\frac{1}{1+\frac{1-\xi}{\xi}\frac{1-F_{NH}(b^\star)}{1-F_H(b^\star)}} > \frac{1}{1+\frac{1-\xi}{\xi}\frac{\psi_1 F_{NH}(b^\star)+\psi_2 F_{NH}(b^\star/\bar{D})+\psi_3 F_{NH}(b^\star/\tilde{D})}{\psi_1 F_H(b^\star)+\psi_2 F_H(b^\star/\bar{D})+\psi_3 F_H(b^\star/\tilde{D})}}, \quad (8)$$

which requires

$$[1-F_{NH}(b^\star)][\psi_1 F_H(b^\star)+\psi_2 F_H\left(\frac{b^\star}{\bar{D}}\right)+\psi_3 F_H\left(\frac{b^\star}{\tilde{D}}\right)] < [1-F_H(b^\star)][\psi_1 F_{NH}(b^\star)+\psi_2 F_{NH}\left(\frac{b^\star}{\bar{D}}\right)+\psi_3 F_{NH}\left(\frac{b^\star}{\tilde{D}}\right)], \quad (9)$$

which is true if

$$(1-F_{NH}(b^\star))F_H(b^\star) < (1-F_H(b^\star))F_{NH}(b^\star),$$
$$(1-F_{NH}(b^\star))F_H(b^\star/\bar{D}) < (1-F_H(b^\star))F_{NH}(b^\star/\bar{D}),$$
$$\text{and } (1-F_{NH}(b^\star))F_H(b^\star/\tilde{D}) < (1-F_H(b^\star))F_{NH}(b^\star/\tilde{D})].$$

The immediately preceding inequalities all hold if $F_H(A')/F_{NH}(A') - (1-F_H(A))/(1-F_{NH}(A)) < 0$ for all $A' < A$, which is true because

$$\frac{F_H(A')}{F_{NH}(A')} - \frac{F_H(A)}{F_{NH}(A)} = \int_{A'}^{A} \frac{-f_{NH}(a)F_H(a)}{F_{NH}(a)^2}\partial a + \int_{A'}^{A} \frac{f_H(a)}{F_{NH}(a)}\partial a \propto \int_{A'}^{A}\left[\frac{f_H(a)}{F_H(a)} - \frac{f_{NH}(a)}{F_{NH}(a)}\right]\partial a < 0. \quad (10)$$

The second expression follows from integration by parts. (See the proof of Proposition 1 for more detail.) The third expression follows from multiplying through by $\frac{F_{NH}(a)}{F_H(a)}$. The inequality follows from reverse hazard rate dominance. Thus, Proposition 3 holds. ■

9 Dataset construction

9.1 Thomson/SDC

We downloaded the Thomson/SDC records via Thomson One, a web-based interface. To ensure that we did not miss transactions at the collection stage, the query included (a) all transactions with an announcement or completion date between 1990 and the date of the query and (b) all transactions had either the acquiror, the target, or the acquiror's or target's intermediate or ultimate parent located in the United States. Once the records were downloaded, we eliminated transactions with missing completion dates (i.e., those that were not completed). We also remove transactions where the target and acquiror have the same name or CUSIP; these transactions mainly comprise stock buybacks that were erroneously coded as mergers. We also drop a small number of duplicates at the "deal number" level. In a few cases where the transaction value is missing but the "ranking value" (i.e., the value used to calculate rankings of investment banks and law firms) is not missing, we replace the former with the latter. We then eliminate transactions with missing transaction values. If the cash proportion of consideration is missing, we correct it whenever possible using information from the "Final Consideration" and "Synopsis" columns. To be precise, if the cash proportion is missing and if (a) the final consideration is "cash only" or (b) the synopsis indicates only cash consideration was involved, then we set the cash proportion to 100%. If the cash proportion is missing and if (a) the final consideration is "stock only" or (b) the synopsis indicates only stock consideration was involved, then we set the cash proportion to 0%. If the cash proportion is missing, the synopsis is indeterminate, and the final consideration is "hybrid," "unknown," or "choice of," we set the cash proportion to 50%.

9.2 Item 2 reports

We downloaded Form 8-K Item 2 reports from Wharton Research Data Services, accessing its "List of 8-K Items" product within the SEC Analytics Suite. Again, to ensure we did not miss any records at the collection stage, the query included (a) all Form 8-K and 8-K/A filings (b) for all dates, spanning from the mid-1990s to the present. Once downloaded, we kept records where the item number, NITEM, equals 2 or 2.01. (See the body of the main text for information about the renumbering of 8-k Items, which has no impact on our analysis.)

9.3 Company name crosswalk

We construct a crosswalk of company names over time using Wharton Research Data Services' "Historical Company Names" product within the SEC Linking Tables. Our query includes all records.

9.4 Compustat

We downloaded Compustat data from Wharton Research Data Services, accessing "S&P Capital IQ's Compustat Snapshot North America" product. We use Annual data "As First Reported." Note that this snapshot is a significant improvement over the "Current" data and appears to be a relatively recent release. In short, while S&P initially reports organization identifiers (i.e., "header" variables such as name and CUSIP) as they appear at the time companies release the results of their operations, the data provider overwrites these these objects over time, which complicates matching to other data sources (e.g., Thomson/SDC) that always report historical identifiers. Thus, the "As first reported" version of Compustat limits the need for linking tables, which in turn reduces error in the matching process when we merge records using legal names and six-digit CUSIP's.

We download variables AQC, CAPX, IVACO, and REVT, which correspond to cash flows from acquisitions, capital expenditures, other cash flow from acquisitions, and revenue. These items reflect annual flows. We also obtain the variable AT, which corresponds to total assets recorded at fiscal year end.

9.5 Standardizing names

We apply the same company name standardization procedure across all four raw data sources. To standardize names, we complete the following steps.

- Convert all text to lowercase.
- Replace all foreign language characters with closest-match English language characters (e.g, the Spanish "n" with an overset tilde becomes English "n").
- Remove remaining non-standard ASCII characters.
- Remove US state suffixes (e.g., "/ FL" for "Florida).
- Remove issue suffixes (i.e., "/ NEW" for "new issuances").

- Replace commas, periods, dashes, slashes, colons, asterisks, and semicolons with spaces.
- Remove duplicate interior spaces and any exterior spaces (i.e., trim the strings and interior-trim the strings).
- Replace common abbreviations that are not corporate suffixes (e.g., "ctr," "sys," "finl," "labs," "intl," "info," "natl," "tech," "hldg," "assoc") with abbreviated text.
- Remove corporate suffixes (e.g., "corp," "llc," "inc," "group").
- Again, remove duplicate interior spaces and any exterior spaces (i.e., trim the strings and interior-trim the strings).

9.6 Creating the transaction level dataset

The "core" of this dataset is the set of records from Thomson/SDC. We begin by eliminating acquisitions that Thomson/SDC involved (a) coal, (b) real oil/gas assets, (c) hotels without casinos, (d) REITs and related real estate investment vehicles, (e) banks, (f) creditors, (g) leveraged buyouts, and (h) spin-outs. Note that deals in (a)-(d) are omitted, as in Wollmann [2019], because these mergers do not require HSR mergers, indicating enforcement issues are not a first order concern here. Deals in (e) are omitted because all bank mergers are notified to the government, so HSR and Item 2 issues are irrelevant for enforcement purposes. Deals in (f)-(h) are omitted because they are intrinsically unlikely to involve competitive concerns. We then eliminate acquisitions where the acquiror's parent company is foreign or not public.[8] We also eliminate very small acquisitions (i.e., less than $1 million in transaction value, as is customary in the literature), acquisitions where the target or acquiror name is "undisclosed" (for obvious reasons), and acquisitions where the proportion of the target being acquired is missing.

We then match the clean Thomson/SDC acquisitions to Item 2 reports if two criteria are satisfied. The first is that either (a) the standardized parent names match or (b) any historical variant of the Thomson/SDC parent name, as determined by the WRDS historical names file, matches any historical variant of the 8-K company name, as determined by the WRDS historical names file. The second criteria is that the date of the 8-K is no more than four days earlier or ten days later than the completion date of the merger.

We define $ITEM2$ as a match to an Item 2 report, and we define H as a match between the primary four-digit SIC codes of the target and acquiror.

9.7 Creating the firm-year level dataset

Again, we start with the set of Thomson/SDC records. First, we map each acquisition's completion date to a fiscal year-end. To do so, we merge the Thomson/SDC data to the Compustat data, keeping only the fiscal year-end variable from Compustat. Second, we eliminate acquisitions where the acquiror already owns greater than or equal to 50% of the target; in these cases, from the standpoint of financial accounting, the target is already part of the acquiror, so the purchase of any part of the remaining stake will now show up as cash flow from acquisitions, regardless of the type of consideration. (Substantively speaking, the target is probably not autonomous if another firm owns 50% or more of its outstanding shares, so increasing the stake should not be considered any acquisition anyway.) Third, we calculate each merger's cash and stock value by multiplying v_i by c_i and $1 - c_i$. Fourth, we collapse the data down to the parent and fiscal year level.

We then merge the panel to the Compustat data. Specifically, we merge to Compustat once at the parent's six-digit CUSIP and the fiscal year, and then we merge again at the parent's name and the fiscal year. Since each merge matches one Thomson/SDC observation to one or more Compustat records, this step creates duplicates at

[8] As per the body of the main text, the focus of this paper is "publicly traded US companies."

the parent name and fiscal year level. We eliminate them by selecting the observation with the largest C_{ft}.[9] As in the transaction level dataset, we then eliminate acquisitions where the acquiror's parent company is foreign or not public. We also ensure that the value of disclosed cash mergers does not exceed the value of all cash mergers. To do so, for each firm-year, we compute the sum of the value of disclosed cash mergers in the prior year, present year, and subsequent year. We then compute an analogous sum for total cash mergers. If the first exceeds the second, we reduce the value of disclosed cash mergers accordingly. Finally, to ensure that we correctly match entities between Thomson/SDC and Compustat, we require that revenues reported by the two sources differ from one another by no more than 10%. The 10% margin of error allows for timing differences, as Compustat obtains revenue from from annual reports whereas Thomson/SDC revenue may come from quarterly reports.

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[9]In most cases, duplicate records are created because Compustat includes certain subsidiaries. For example, if XYZ LLC is the operating subsidiary of XYZ Inc., and we eliminate corporate suffixes, we merge our Thomson/SDC record for "XYZ" to both Compustat records. However, the subsidiary entity is correctly eliminated when we keep only the larger of the two companies in Compustat.

Figures

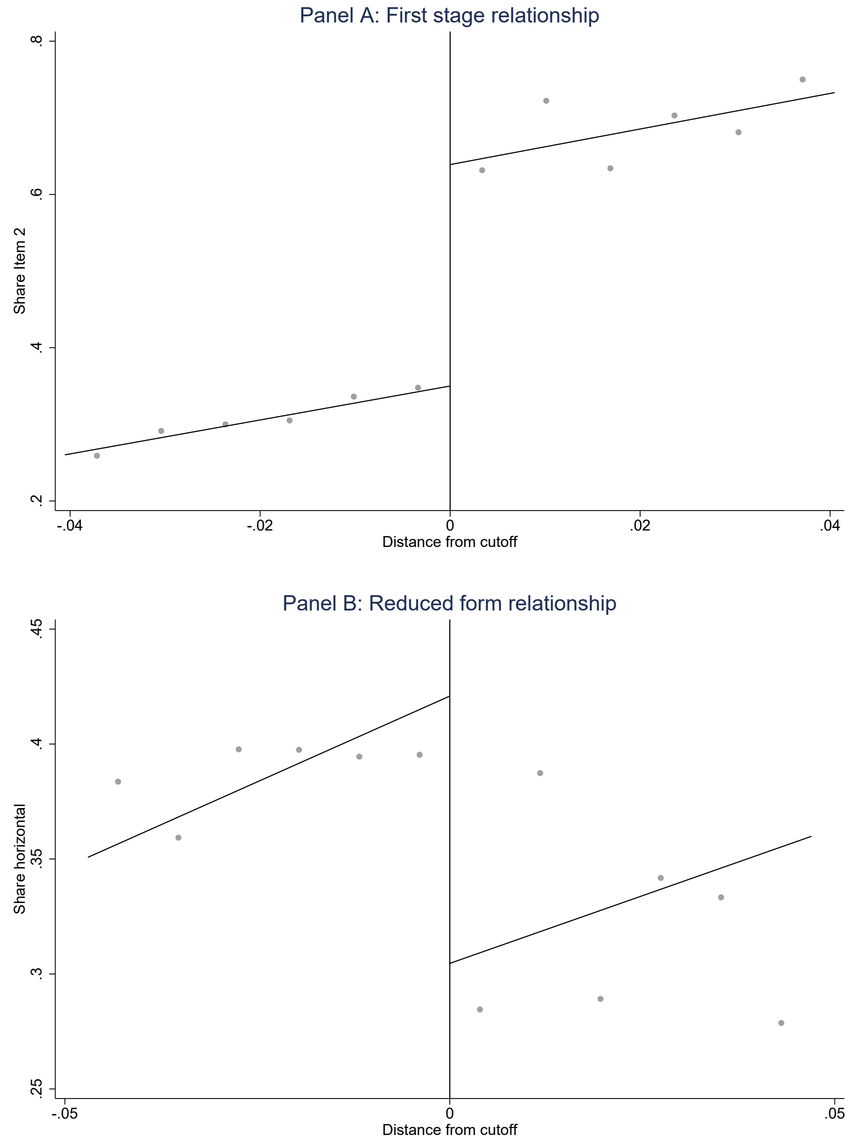

Figure A.1: *First stage and reduced form when running variable is calculated in levels*

This figure replicates Figure 1 except that we construct the running variable as the level rather than log value of the ratio of the transaction value to acquiror assets. The discontinuities at the cutoff witnessed here are similar in magnitude to the size of the jumps witnessed in the figure that appears in the body of the main text, which mirrors the comparisons of the first three and last three columns of Table 3 in the body of the main text.

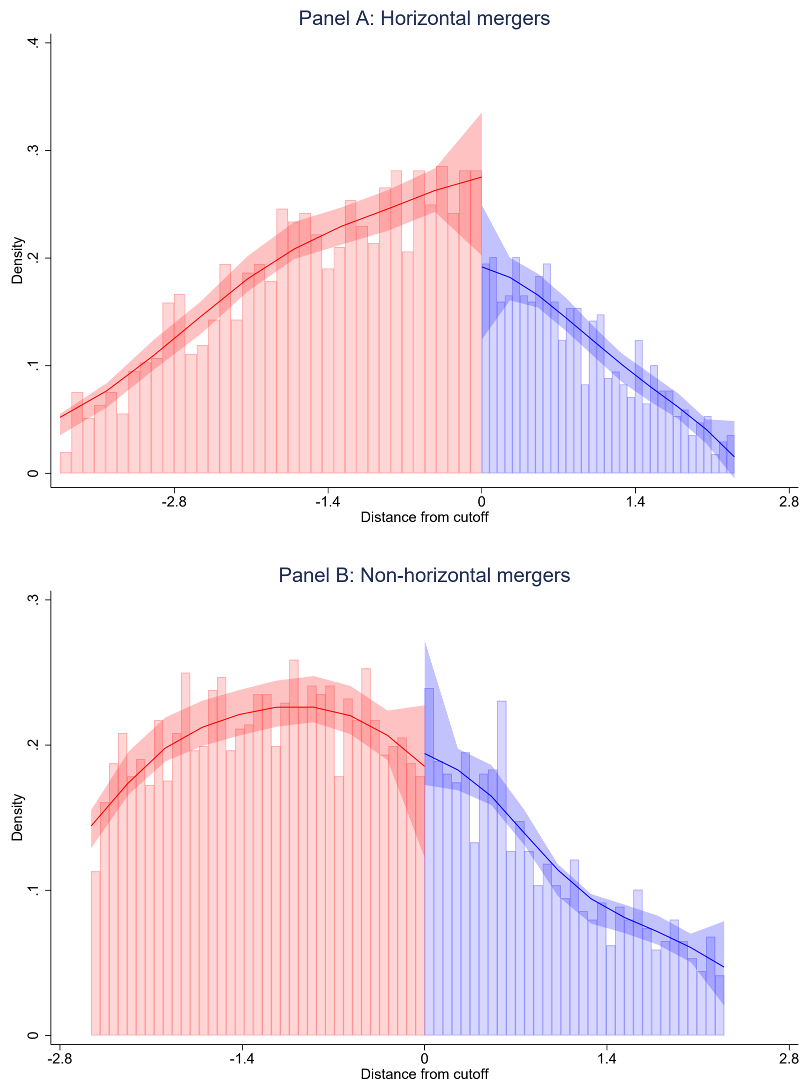

Figure A.2: *Density plot of non-HSR mergers around the Item 2 cutoff*

This figure plots the density of the difference between the running variable and the cutoff. The sample consists of non-HSR mergers. Bands around the curve represent valid 95% confidence intervals [Cattaneo et al., 2021]. The abrupt decline among horizontal mergers reflects deterrence, while the smooth trend among non-horizontal mergers reflects the fact that they pose little competitive threat and face very little threat of enforcement. Neither group present a pattern consistent with sorting around the threshold (i.e., a spike in density just to the left of the threshold and/or a trough just the left of it).

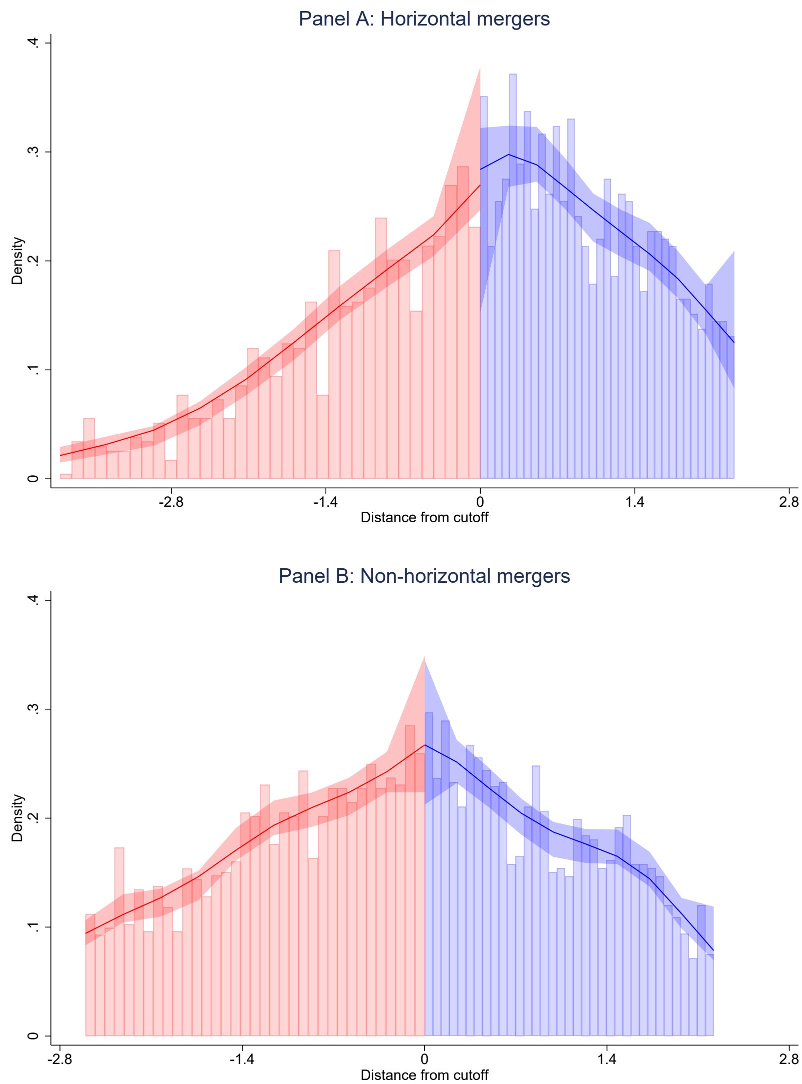

Figure A.3: *Density plot of HSR mergers around the Item 2 cutoff*

This figure replicates Figure A.2 except that the underlying sample consists of HSR rather than non-HSR mergers. Bands around the curve represent valid 95% confidence intervals [Cattaneo et al., 2021]. Neither plot witnesses sorting around the cutoff. Moreover, neither plot exhibits an abrupt change in density at the cutoff, consistent with the fact that the antitrust authorities are already fully informed about HSR mergers vis-a-vis premerger notifications, so Item 2 reports are uninformative for enforcement purposes.

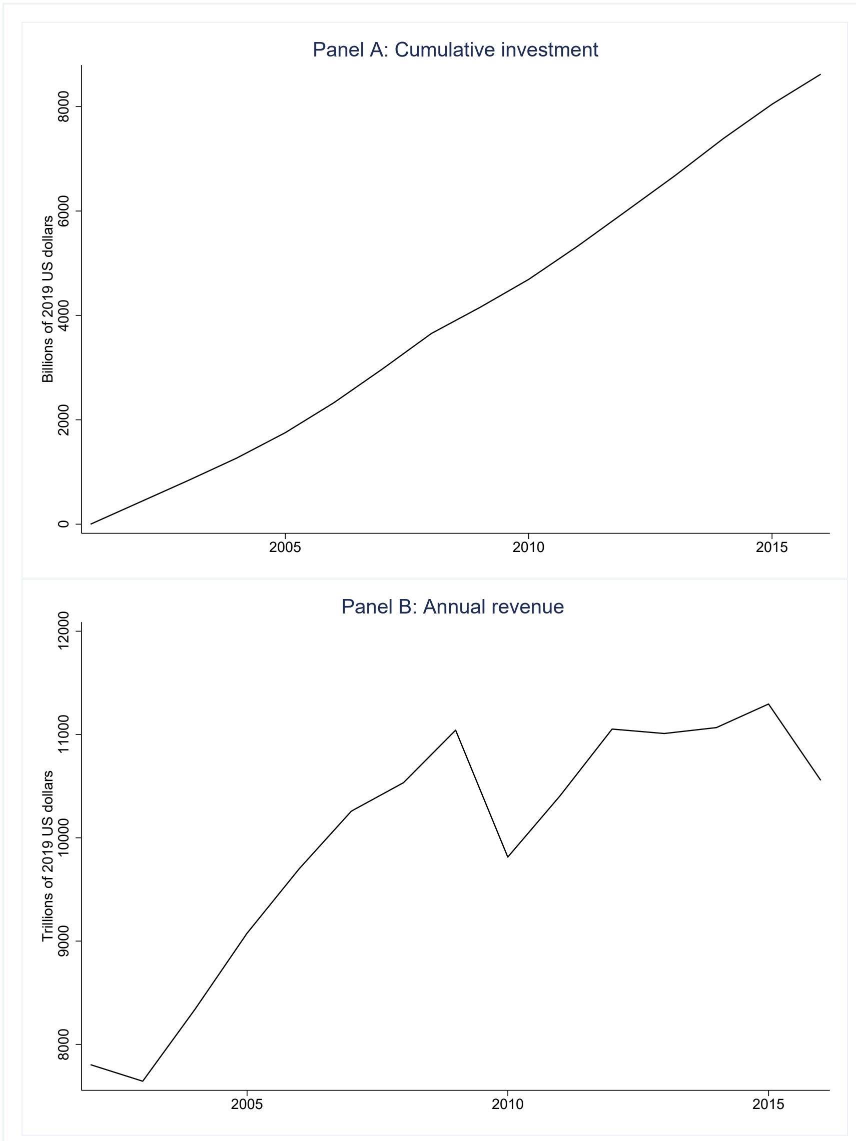

Figure A.4: *Revenue and capital expenditures between 2002 and 2016*

Panels A plots investment (i.e., capital expenditures) cumulatively, while Panel B plots revenue annually. (Since investment produces long-lived revenue-producing assets, it is better represented as an accumulated "stock" than a "flow.") All figures are in constant 2019 dollars.

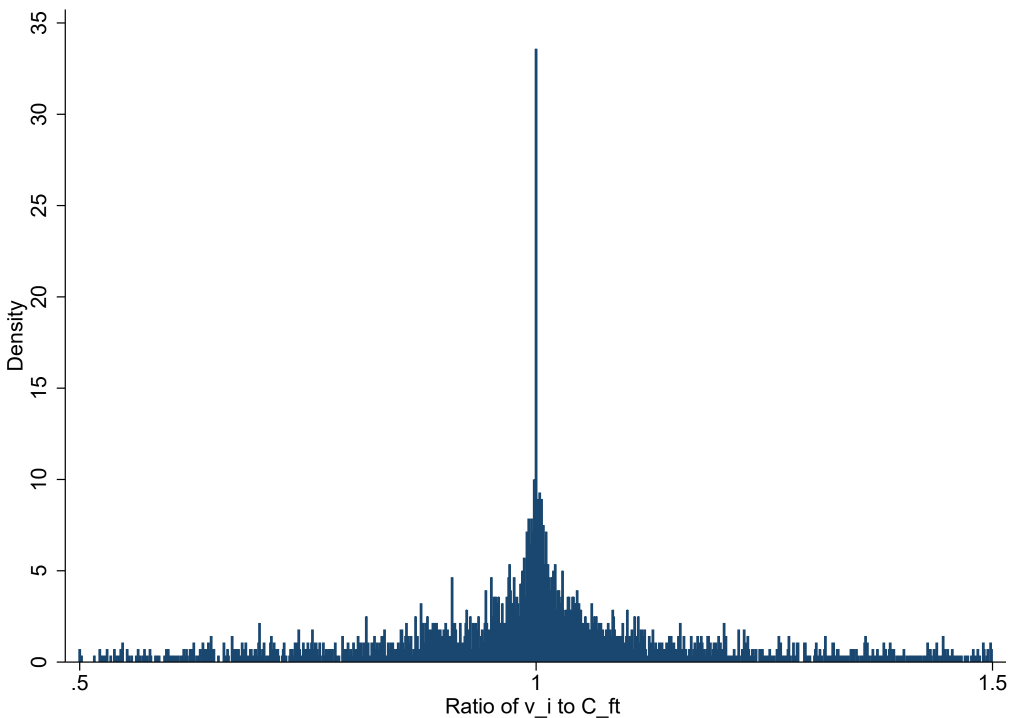

Figure A.5: *Comparing v_i and C_{ft} for firms with exactly one transaction*

To produce this figure, we restrict attention to firm-year observations where we observe (a) exactly one transaction and (b) that transaction is all cash. We then compute v_i/C_{ft}, the ratio of the cash value of mergers measured using Thomson/SDC and Compustat, respectively, and plot its density. We truncate the support at 0.5 and 1.5 merely to increase legibility. A ratio of 1 corresponds to an exact match between the two sources. In support of our proposed methodology, there is a spike in the density exactly at 1.00.

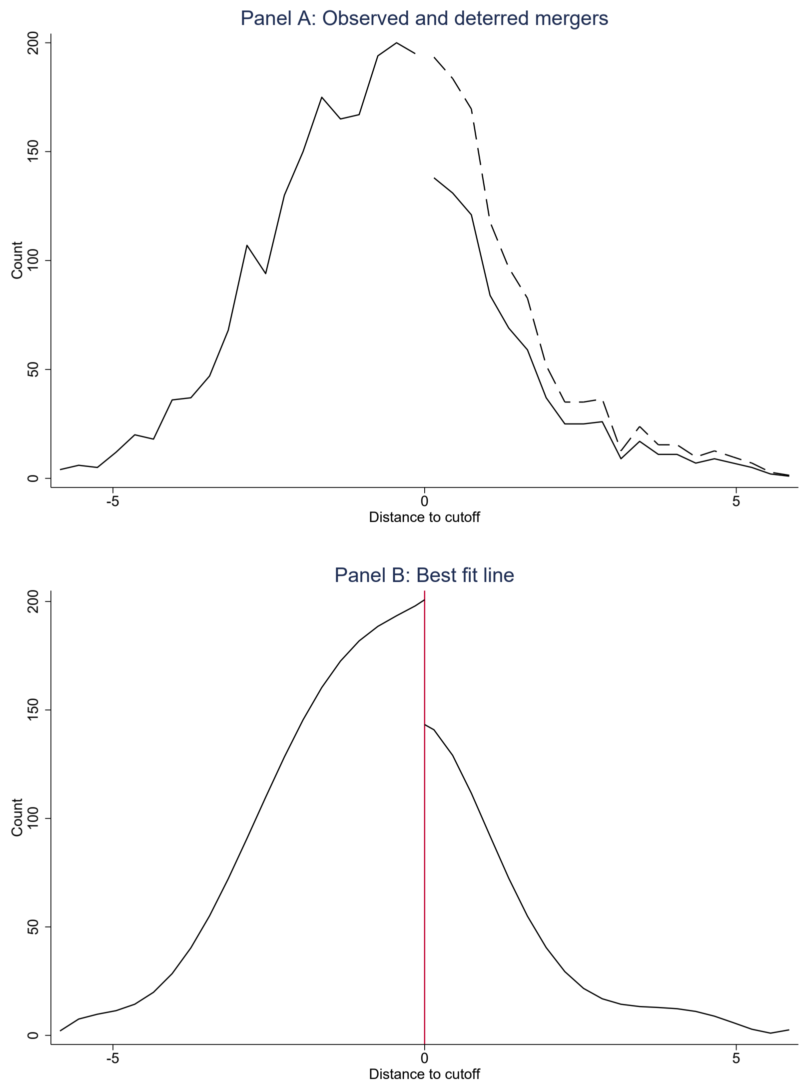

Figure A.6: *Observed, deterred, and predicted mergers*

To produce this figure, we set the range of the running variable to [-6,6], set the bin width to 0.2, and restrict attention to horizontal mergers. In each panel, we plot distance to the cutoff on the x-axis. Along the y-axis in Panel A, we plot the observed mergers in each bin using the solid line, and we plot the sum of the observed and deterred mergers using the dashed line (the text describes how we predict the number of deterred mergers). Along the y-axis in Panel B, we restrict attention to negative values of the running variable, plot predicted values from a fifth-order polynomial, and then repeat this process for positive values of the running variable.

Tables

Table A.1: *Falsification test: estimates of τ*

VARIABLES	(1) Horizontal	(2) Horizontal	(3) Horizontal	(4) Horizontal	(5) Horizontal	(6) Horizontal
RD_Estimate	0.00402	-0.0577	-0.00185	0.0149	0.0202	0.0156
	(0.121)	(0.121)	(0.117)	(0.112)	(0.108)	(0.111)
Observations	6402	6402	6402	6402	6402	6402
Kernel Type	Triangular	Uniform	Epan.	Triangular	Uniform	Epan.
Robust p-value	0.882	0.609	0.843	0.997	0.962	0.995
BW Loc. Poly. (h)	0.937	0.762	0.924	1.456	1.084	1.356

*This table replicates Table 3 but restricts attention away from non-HSR mergers to HSR mergers. Since antitrust authorities are already fully apprised of HSR mergers, Item 2 reports are uninformative, so they should have no effect. Consistent with that intuition and the model's predictions, we find $\hat{\tau}$ is near zero for this subset of transactions. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.*

Table A.2: *Placebo experiments*

Variable	MSE-optimal bandwidth	RD estimate	Robust p-value	Robust conf. int.	Eff. number of observations
Year completed	.8200	.25505	.5160	[-.56513,1.12531]	2569
Asset acquisition	.4464	-.0298	.4313	[-.17703,.075605]	1429
Percent cash	.6353	.00058	.7844	[-.09006,.068009]	2035
All cash	.8625	.02444	.6645	[-.07372,.115612]	2707
Value	.6485	1.6841	.4445	[-2.5552,5.82355]	2072
Acquiror assets	.5019	16.229	.6664	[-36.565,57.1837]	1603

The figure reports estimates of the reduced form relationship between the distance to the cutoff and various "placebo" outcomes. These outcomes include the following: the date that the merger was completed (measured in fractional shares, so that, e.g., July 1, 2005 equates to 2005.5), a dummy variable indicating the deal was structured to transfer assets rather shares, the proportion of the transaction value paid in cash, a dummy variable indicating that all of the transaction value was paid in cash, the transaction value, and the assets of the acquiror. The specifications are identical to those used to estimate τ: we use MSE-optimal bandwidths, triangular kernels, and calculate the running variable in log values.

Table A.3: *Estimates of τ by sector*

VARIABLES	(1) Target industry: Manufacturing (SIC2 20-39)	(2) Target industry: Services (SIC2 70-89)	(3) Acq. industry.: Manufacturing (SIC2 20-39)	(4) Acq. industry: Services (SIC2 70-89)
RD_Estimate	-0.373*	-0.263	-0.254	-0.364*
	(0.217)	(0.188)	(0.246)	(0.199)
Observations	2487	2652	2871	2245
Kernel Type	Triang.	Triang.	Triang.	Triang.
Robust p-value	0.135	0.181	0.482	0.081
BW Loc. Poly. (h)	0.690	0.694	0.617	0.710

This table assesses heterogeneity in τ across industries. Columns 1 and 2 restrict attention to targets operating within manufacturing (SIC2 20-39) and services (SIC2 70-89), respectively. Columns 3 and 4 restrict attention to acquirers operating within these sets of industries, respectively. Other sectors have fewer than 1,000 observations around the threshold and thereby produce unreliable estimates of τ. $\star$, $\star\star$, and $\star\star\star$ denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.4: *Estimates of τ by US Presidential administration*

VARIABLES	(1) Bush Administration	(2) Obama Administration	(3) Full sample (all years)
RD_Estimate	-0.229	-0.569**	-0.373***
	(0.178)	(0.223)	(0.129)
Observations	4477	2809	7286
Kernel Type	Triang.	Triang.	Triang.
Robust p-value	0.319	0.008	0.006
BW Loc. Poly. (h)	0.912	1.036	1.102

This table assesses heterogeneity in τ across the administrations of two US Presidents who were in office during the sample period. Column 1 replicates the main result from Table **??** *for comparison. George W. Bush was in office until January 2009, so column 2 restricts attention from 2002, the start of the panel, through and including 2008. Barack Obama was in office afterwards, so column 2 restricts attention from 2009 through and including 2016, the end of the panel. The estimates imply that investor disclosures posed a much larger antitrust risk during the latter period than the former (though one should not take this statement to imply a causal relationship, as any time-varying factors may confound this relationship). $\star$, $\star\star$, and $\star\star\star$ denote significance at the 10%, 5%, and 1% levels, respectively.*

Table A.5: *Estimates of τ by whether the industry recently had a merger challenge*

VARIABLES	(1) Acquirer SIC faced enforcement	(2) Acquirer SIC never faced enforcement	(3) Target SIC faced enforcement	(4) Target SIC never faced enforcement	(5) All 4-digit SIC codes (main result)
RD_Estimate	-0.398*	-0.329*	-0.484***	-0.239	-0.373***
	(0.206)	(0.188)	(0.186)	(0.172)	(0.129)
Observations	3975	3311	4080	3206	7286
Kernel Type	Triang.	Triang.	Triang.	Triang.	Triang.
Robust p-value	0.070	0.101	0.013	0.219	0.006
BW Loc. Poly. (h)	0.958	0.879	1.033	1.266	1.102

This table assesses heterogeneity in τ based on whether the industry experienced a merger-related enforcement action in the past 5 years. To produce it, we compile a list of Complaints filed by either the DOJ or FTC in or after 1998. In column 1, we restrict attention to acquirers whose SIC4s have experienced in a year prior to the merger, and in column 2, we restrict attention to remaining mergers. In columns 3 and 4, we reproduce these results but make restrictions based on targets' SIC4s rather than acquirers' SIC4s. In column 5, we reproduce the main result, which is based on all industries, to facilitate comparisons. *, **, *and* *** *denote significance at the 10%, 5%, and 1% levels, respectively.*

Table A.6: *Differences in τ based on whether the acquirer has a contemporary HSR merger*

VARIABLES	(1) Involved in one or more contemporaneous HSR mergers	(2) Not involved in any contemporaneous HSR merger	(3) All acquirers (main result)
RD_Estimate	-0.614	-0.429**	-0.373***
	(0.688)	(0.171)	(0.129)
Observations	2397	4889	7286
Kernel Type	Triang.	Triang.	Triang.
Robust p-value	0.386	0.017	0.006
BW Loc. Poly. (h)	0.593	0.782	1.102

This table assesses heterogeneity in τ based on whether the acquirer is involved in an HSR merger in the same year. *, **, *and* *** *denote significance at the 10%, 5%, and 1% levels, respectively.*

Table A.7: *Estimates of τ using broader industry definitions*

VARIABLES	(1) Horizontal	(2) Horizontal	(3) Horizontal	(4) Horizontal	(5) Horizontal	(6) Horizontal
RD_Estimate	-0.100	-0.142	-0.100	-0.373***	-0.374***	-0.357***
	(0.129)	(0.141)	(0.127)	(0.129)	(0.133)	(0.119)
Observations	7286	7286	7286	7286	7286	7286
Kernel Type	Triangular	Uniform	Epan.	Triangular	Uniform	Epan.
Industry	SIC2	SIC2	SIC2	SIC4	SIC4	SIC4
Robust p-value	0.526	0.446	0.535	0.006	0.007	0.003
BW Loc. Poly. (h)	1.022	0.758	0.977	1.102	0.868	1.153

*Columns 1-3 report estimates of τ when we redefine horizontal mergers as ones where the target and acquirer occupy the same two-digit SIC code. Columns 4-6 report analogous estimates when four-digit SIC codes are used (i.e., these columns reproduce the main results to facilitate comparisons to the first three columns). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.*

Table A.8: *Estimates of τ by responsible agency*

VARIABLES	(1) Dept. of Justice	(2) Federal Trade Commission	(3) DOJ or FTC
RD_Estimate	-0.745*	-0.274**	-0.380**
	(0.421)	(0.139)	(0.152)
Observations	2280	4952	7232
Kernel Type	Triang.	Triang.	Triang.
Robust p-value	0.066	0.066	0.021
BW Loc. Poly. (h)	0.943	1.094	0.855

*This table assesses heterogeneity in τ across the agencies authorized and responsible for mergers in particular industries per the 2002 Memorandum of Understanding between the DOJ and FTC (see https://www.justice.gov/sites/default/files/atr/legacy/2007/07/17/10170.pdf). Column 1 restricts attention to mergers whose targets operated in industries that are primarily policed by the DOJ. Column 2 makes an analogous restriction for the FTC. Column 3 reports our estimate of τ for all industries that we can unambiguously characterize as either the responsibility of the DOJ and FTC. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.*

Table A.9: *Estimates of B by R and n*

Panel A: Using 4^{th} order polynomials to fit c_j				
Data excluded (R)	$\hat{B}$ estimate	$\hat{B}$ std. error	t-statistic	p-value
1 bins	4.2196	7.1315	.59168	.55405
2 bins	-.7996	3.0670	-.2607	.79431
3 bins	-.2887	1.8846	-.1532	.87822
4 bins	.25227	1.5010	.16806	.86652
5 bins	-.4382	1.3381	-.3275	.74327
6 bins	-.6795	1.2365	-.5495	.58262

Panel B: Using 5^{th} order polynomials to fit c_j				
Data excluded (R)	$\hat{B}$ estimate	$\hat{B}$ std. error	t-statistic	p-value
1 bins	5.4655	7.6617	.71335	.47562
2 bins	-.5816	3.5622	-.1632	.87030
3 bins	-.0595	2.3753	-.0250	.98000
4 bins	.73281	1.9224	.38118	.70306
5 bins	-.3496	1.9140	-.1826	.85505
6 bins	-.8913	1.8575	-.4798	.63134

Table A.10: *Predicted number and value of deterred mergers*

		Deterred mergers	
Running var. range	Width of bins	Predicted number	Predicted value
[-4,4]	0.1	314	5738
[-4,4]	0.2	336	6137
[-4,4]	0.3	279	5156
[-5,5]	0.1	349	6306
[-5,5]	0.2	362	6537
[-5,5]	0.3	212	3802
[-6,6]	0.1	294	5319
[-6,6]	0.2	299	5406
[-6,6]	0.3	318	5762

This table reports the predicted number and value of deterred horizontal mergers for different possible ranges of the running variable and different bin widths. Predicted values are reported in millions of 2019 constant dollars.