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Stealth Mergers and Investment Outcomes

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Abstract

"Stealth mergers" are not reported to the government because they fall below the required size threshold. We study stealth mergers involving public targets for which manipulation of transaction sizes is unlikely. These stealth mergers result in less R&D spending, patenting, and capital expenditures, and in lower value patents for both acquiring firms and their competitors relative to non-stealth mergers. Industry concentration increases, and product market competition decreases for stealth acquirers. Stealth acquirers and their competitors earn higher cumulative abnormal returns relative to non-stealth mergers. Our results suggest more government scrutiny is warranted for stealth mergers.

I. Introduction

Mergers below a size threshold for the target company do not require Federal Trade Commission (FTC) and Department of Justice (DOJ) premerger antitrust notification, as stipulated in the Hart–Scott–Rodino Antitrust Improvements Act of 1976 (HSR) and amended in 2000. The logic for exempting small mergers from premerger notifications assumes that these transactions are unlikely to raise significant antitrust concerns. If this assumption is correct, then existing policy reduces regulatory costs and delays, and enhances efficiency (Howell (2002)). In contrast, Wollmann (2019), (2021) argues these transactions can have important consequences in segmented industries, resulting in what he calls "stealth consolidation." Along these lines, Kepler, Naiker, and Stewart (2023) show that stealth mergers involving private targets increase prices and profits. We make two key contributions to this recent, growing literature. First, we provide economy-wide evidence that

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stealth mergers also harm investment and innovation. Second, we document these and other anticompetitive effects using data from public targets, which are effectively free from manipulation designed to avoid reporting requirements.

Historically, the government investigates a fraction of the mergers reported under the HSR Act. In 2020, 1,637 non-stealth mergers were reported to government agencies. Of these, 169 were investigated by the agencies, including approximately 8.4% of those that were within $50 million above the HSR threshold (https://www.ftc.gov/system/files/documents/reports/hart-scott-rodino-annual-report-fiscal-year-2020/fy2020_-_hsr_annual_report_-_final.pdf). Stealth mergers, those below the threshold, are not investigated as they are not reported. Because they are not investigated, stealth mergers could contribute to industry consolidation and reductions in corporate investment and social welfare (Gutierrez and Philippon (2017), Grullon, Larkin, and Michaely (2019)). Wollmann (2019) shows a sharp increase in horizontal mergers after an increase in the HSR threshold in 2001, which decreased the number of mergers subject to premerger notification. This seems to validate that premerger notification is effective in deterring anticompetitive behavior.[1]

Kepler et al. (2023) show a bunching of transactions below the HSR threshold due to strategic manipulation of transaction values in mergers involving privately held targets. After the merger, these private target transactions lead to higher prices, implying that the relaxation of premerger notification thresholds now allows anticompetitive transactions to occur.

By focusing on private targets with manipulated transaction values, Kepler et al. (2023) cannot say what would happen if merging firms had to report these transactions to the government. If they still choose to merge, would they not manipulate transaction values but still engage in ex post anticompetitive behavior? Alternatively, would they not engage in anticompetitive behavior because of government notification? Are HSR exemptions still beneficial for merging firms that are unlikely to manipulate, namely mergers involving public targets? Alternatively, do HSR exemptions allow acquisitions of public targets below the threshold to engage in ex post anticompetitive behavior?

Our paper addresses these questions by shutting down the possibility of manipulating transaction values. We do this by focusing on publicly traded acquirers with publicly traded targets rather than private targets. The values of publicly traded targets are unlikely to be manipulated as a readily observable public stock market value exists. Inducements offered to private targets to accept a low premium, such as board seats, founder compensation arrangements, and low earnout targets, are much more constrained for public targets, which typically have more independent boards as well as a shareholder vote on the merger.

While we do not observe manipulation of transaction values for the public targets in our data, there are several reasons why we could still observe more transactions below the threshold than above. Because of government scrutiny, mergers above the threshold are at risk of being blocked, which could reduce the number of completed mergers above the threshold. Second, mergers above the

[1]There is a substantial literature in finance on the anticompetitive effects of horizontal mergers including Eckbo (1983), Kim and Singal (1993), Prager and Hannan (1998), Shahrur (2005), Bhattacharya and Nain (2011), and Ahern (2012).

threshold could be deterred from even being attempted. Third, acquiring firms may select targets to stay below the threshold. All of these could result in a bunching of transactions below the threshold. Nonetheless, we do not find bunching below the HSR threshold for public targets. There does not appear to be strategic manipulation of public targets, and merging firms do not seem to be deterred from undertaking mergers above the threshold.

This leaves the question of whether government notification prevents ex post anticompetitive activity for mergers involving public targets. To address this question, we compare mergers that are within the HSR threshold of ±$50 million using an ordinary least squares (OLS) specification. Those below the threshold are stealth mergers, and those above the threshold are non-stealth mergers. However, a simple comparison of stealth and non-stealth mergers may still involve some selection effects, so we employ two other forms of variation for identification.

The first uses a feature of the size of the transaction threshold. Prior to 2001, HSR set the size of the transaction threshold to $15 million, which was not indexed. Few transactions were below this threshold. In 2000, HSR was amended to raise the size of the transaction threshold to $50 million for the years 2001 to 2004. Thereafter, the threshold increased by annual GDP growth. As in Wollmann (2019), we exploit the transaction threshold change in 2001 to conduct a difference-in-differences analysis. We examine mergers whose transaction values were within the HSR threshold ±$50 million before and after the change in the threshold in 2001.

Second, we employ a regression discontinuity design (RDD) for transactions from 2001 to 2019 that are within the HSR threshold of ±$10 million. This is a tighter band than in the OLS and difference-in-differences specifications. We argue that mergers subject to government notification just above the threshold will not show anticompetitive effects on investment and innovation, while those just below the threshold will show anticompetitive effects. As previously noted, mergers involving public targets do not exhibit strategic manipulation or bunching of transactions below the threshold. Thus, our RDD has internal validity. If our RDD results are consistent with our OLS results, then our RDD results also have external validity.

The results across all our specifications are consistent. First, both target premiums and acquirer cumulative abnormal returns (CARs) around merger announcement are higher for stealth than non-stealth mergers, similar to the results in Kepler et al. (2023). Higher public target premiums for stealth than non-stealth mergers are inconsistent with target premiums being manipulated to stay below the threshold. Combined firm profitability (return on assets) increases for stealth relative to non-stealth mergers. These results are consistent with two possibilities: Stealth mergers are efficient by reducing regulatory costs, or stealth mergers are anticompetitive.

Second, we find that the combined firms reduce R&D spending, innovation, patenting, and investment more for stealth relative to non-stealth mergers. This result is consistent with stealth mergers preempting the creation of new products that might challenge an incumbent's rents.

Third, we find that the acquiring firm's product market becomes less competitive and more concentrated after stealth relative to non-stealth mergers.

Fourth, we find that competitor CARs around announcements in the same industry are higher for stealth than non-stealth mergers. Competitor profitability

increases for stealth relative to non-stealth mergers. If stealth mergers are efficiency-enhancing, competitor CARs should be weakly negative, and return on assets should decline as competitor profits decrease. Conversely, if stealth mergers are anticompetitive by reducing the number of competitors in an industry, competitor CARs should be positive, and return on assets should increase as remaining competitors increase profitability. Our results are inconsistent with stealth mergers being efficiency-enhancing.

We also find that competitors of stealth acquirers reduce their R&D spending, innovation, patenting, and investment relative to competitors of non-stealth acquirers. These competitor results suggest that stealth mergers lead to anticompetitive behavior within the acquirer's industry. Our difference-in-differences and RDD results provide causal support for government premerger notification deterring anticompetitive behavior in investment and innovation.

We further explore whether agency notification deters anticompetitive behavior by examining the timing of mergers. Avoiding agency review is valuable even if a merger is unlikely to be blocked, as post-merger scrutiny is less likely if there is no premerger review (Wollmann (2019)). Once an acquirer is subject to government scrutiny for a merger above the threshold, then that acquirer is subject to more scrutiny in future mergers and in the product market, even if the government takes no action on the initial merger. This is also true for future stealth mergers. Section 7 of the Clayton Act permits the government to initiate a review and challenge a merger after it is completed.

We hypothesize that stealth mergers before an acquirer undertakes a non-stealth merger are anticompetitive, while stealth mergers after an acquirer undertakes a non-stealth merger are not anticompetitive. We find that investment and innovation are reduced for stealth mergers before subsequent non-stealth mergers. This is relative to stealth mergers that occur after non-stealth mergers. We also find that acquirer and competitor returns are higher and competitor investment is lower, consistent with stealth mergers prior to non-stealth mergers reducing competition more than stealth mergers after non-stealth mergers.

The significant reductions in investment and innovation for stealth acquirers and their competitors suggest that firms in the acquirer's industry view the acquisition as reducing competition. Given concerns that increased market concentration has reduced corporate investment and social welfare (Gutierrez and Philippon (2017)), our results suggest that consolidation of industries from large mergers is not the only cause. The relaxation of premerger notification for small mergers has also played a role. Many firms try to engage in ex post anticompetitive behavior unless they are deterred from doing so. Agency premerger notification seems to prevent or deter this behavior, and more stringent agency notification of many more mergers may be warranted.[2]

[2]The FTC has recently announced that it will require premerger review of all mergers of firms previously found to have engaged in anticompetitive behavior, specifically firms that engaged in prior mergers that required asset divestitures as a condition for approval. As a result, even mergers with small targets can now require review if the acquiring firm previously engaged in anticompetitive behavior. See https://www.reuters.com/business/us-ftc-restrict-future-deals-firms-that-pursue-anticompetitive-mergers-2021-10-25/.

Our paper proceeds as follows: In the next section, we discuss the evolution of the government merger review process. We also discuss our conceptual framework for examining whether government notification reduces anticompetitive behavior. Section III describes our data and the measures we construct for our tests. Section IV explains our identification strategy. Section V presents our empirical results. Section VI concludes.

II. Institutional Setting and Motivation

A. Discussion of U.S. Premerger Review

The Hart–Scott–Rodino Antitrust Improvements Act of 1976 (HSR) established premerger notification requiring both acquirers and targets to notify the Federal Trade Commission (FTC) and Department of Justice Antitrust Division (DOJ) about their intent to merge. After notification, the parties must wait 30 days before proceeding. Either the FTC or DOJ reviews the submissions and can request more information, allow the 30-day waiting period to elapse, or terminate the waiting period early.[3] If the agencies request more information, then the waiting period is extended. Ultimately, the agencies can negotiate conditions for the merger to proceed, restructure the transaction, refer the transaction to the DOJ to sue to block the merger, or just allow the merger to proceed.

In addition to establishing a premerger review, HSR provides for exemptions from the premerger review process. In the case of an exemption, the parties do not need to undergo premerger review, no notification to the agencies is required, and the transaction can close immediately. Wollmann (2019) shows that of the several tests for determining exemption, the crucial one is the size of the transaction test.[4] Under HSR, if the transaction value, which is the value of the securities and assets acquired, is less than $15 million, then the transaction is exempt from premerger review. Transaction value is determined by the buyer but must conform to common valuation practice. As we examine public targets, the transaction value is simply the publicly disclosed deal value paid by the acquirer. The purpose of these exemptions was that small transactions were unlikely to be anticompetitive. Therefore, they need not incur the regulatory burden of notification.

HSR did not index the $15 million transaction threshold, so more and more transactions were subject to premerger review over time. In 2000, HSR was amended to increase the size of the transaction threshold to $50 million starting Feb. 2, 2001 (the 2001 Amendment). In addition, for transactions closing after Sept. 30, 2004, the size of the transaction threshold was indexed to GDP growth. Figure 1 graphs the size of the HSR transaction threshold over time. The threshold increases from $15 million prior to 2001 to $50 million in 2001 through 2004 and then increases annually to $90 million in 2019.

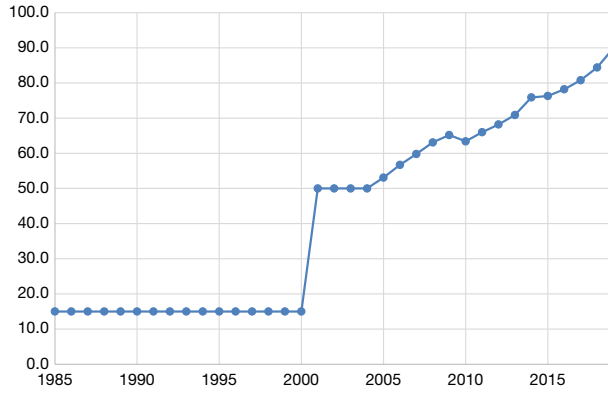
[3]We refer to this notification process and review as just agency or government premerger review for simplicity.

[4]There is also a size of the person test that examines the relative sizes of the assets for the target and acquirer. Wollmann (2019) shows that this test is essentially immaterial.

FIGURE 1

HSR Thresholds as Amended by Congress in 2001

Figure 1 plots the Hart–Scott–Rodino (HSR) thresholds over time. Mergers with a transaction size below the HSR threshold for that year do not require FTC premerger review. The threshold increases with GDP growth after Sept. 30, 2004, after initially being increased to $50 million on Feb. 2, 2001.



B. Conceptual Framework

We view premerger review as deterring anticompetitive behavior. Barrios and Wollmann (2024) provide a framework to consider the issues that arise when stealth mergers are possible. In their model, information disclosure allows gains from mergers to be rapidly incorporated into stock prices, but at the cost of potentially revealing to the agencies information sufficient to block the merger. As mergers above the HSR threshold require disclosure, acquirers will be deterred from engaging in anticompetitive behavior to minimize the risk of mergers being blocked.

While we do not formally model this point, we extend Barrios and Wollmann (2024) in the following way. In their model, HSR filings affect competition by deterring anticompetitive transactions from being attempted. Hence, their model implicitly assumes that conditional on premerger market structure, HSR filings do not affect post-merger behavior or actions. However, HSR filings may increase the likelihood that the antitrust agencies monitor the combined firm's ex post behavior or enhance the agencies' ability to do so. This violates the implicit assumption made by their model but provides more scope for HSR filings to maintain competition. Moreover, HSR filings may increase the likelihood that agencies monitor the combined firm's subsequent acquisitions or enhance the agencies' ability to do so regardless of the size of the subsequent acquisitions. Thus, HSR filings may impact the types of transactions that acquirers propose in the future. Conversely, the lack of review for an initial merger below the threshold allows acquirers to avoid scrutiny for future mergers. As a result, both acquirers and competitors may engage in anticompetitive behavior, such as reducing investment and R&D expenditures.

We hypothesize that stealth mergers result in anticompetitive behavior due to lack of disclosure. Non-stealth mergers are less likely to result in anticompetitive behavior due to the deterrence effect of premerger review. Anticompetitive behavior benefits acquirers, targets, and competitors to the acquirers. If R&D investment

is a strategic complement, then competitors can also reduce R&D investment as industry concentration and product differentiation increase. Conversely, if stealth mergers result in similar or higher levels of investment and innovation as non-stealth mergers, then premerger notification is unnecessary and burdens acquirers and targets with regulatory costs, delays, and risks.

Regardless, avoiding premerger notification is beneficial for an acquirer. Kepler et al. (2023) demonstrate that these benefits are sufficiently large that acquisition values of private targets are manipulated around the threshold. Because the set of mergers we consider have publicly traded targets and acquirers, the ability to manipulate a merger around the HSR threshold is limited. Our setting shuts down the manipulation channel, allowing us to isolate purely anticompetitive effects on investment and innovation.

III. Data

We use data from a variety of sources. Many of our outcome variables — stock returns, return on assets (ROA), R&D spending, and capital expenditures — come from CRSP and Compustat. For measures of innovation, we use the number of patents and dollar-weighted patent value based on Kogan et al. (2017).[5] The product market characteristics data come from the Hoberg and Phillips data library, which we discuss in greater detail below.

The domestic mergers and acquisitions data are from Refinitiv Securities Data Corporation (SDC). We use standard filters on the data. The announcement dates for these acquisitions are between 1998 and 2019. Acquirers and targets must be publicly traded. For many of our tests, we require financial and operating data for the acquirer and target pre-acquisition, and these data are only consistently available for publicly traded firms. We require that acquirers report accounting data from the fiscal year immediately before the announcement of the acquisition to the fiscal year immediately after the completion of the acquisition. We also require that target firms report accounting data for the fiscal year immediately before the acquisition. Acquirers must own less than 50% of the target before the announcement and obtain 100% of the target's shares.[6] We only consider completed deals. We exclude acquirers from the financial services and utility sectors.

Between 1998 and 2004, the deal value must be greater than $1 million and less than $100 million. This forms our difference-in-differences sample with 1998 to 2000 as the pre-period and 2002 to 2004 as the post-period. From 2005 to the end of our sample period, the deal value must be within the HSR threshold ±$50 million at closing. This excludes large transactions from our sample, as these are less likely

[5]The data are available at https://aseru.people.stanford.edu/data.

[6]One might be concerned that a 2-step transaction—acquiring an initial toehold and then the remainder of the securities in a target—would allow an acquirer to evade agency review as the size of the second transaction could be below the threshold. The FTC and DOJ state that they aggregate all securities in the target: "Because the objective of the Program is to analyze the effects of combining once separate businesses, the Rules generally require that assets, voting securities, or NCI of the acquired person that have already been acquired must be aggregated with those that will be acquired in the proposed transaction." (p. 4, *To File or Not to File-When You Must File a Premerger Notification Form*, available at www.ftc.gov/sites/default/files/attachments/premerger-introductory-guides/guide2.pdf (2008)).

to be comparable to stealth mergers. We also exclude transactions more than $50 million but less than the HSR threshold to maintain comparability by eliminating the smallest transactions. For example, the HSR threshold for a transaction closing in 2019 is $90 million, so a transaction in 2019 must have a size of less than $140 million and greater than $40 million to be included in the sample. While we restrict our general sample to transactions within the HSR threshold ±$50 million, this still implies potentially large differences in targets, as transaction values could differ by as much as $100 million. For this reason, our RDD sample is limited to transactions within the HSR threshold ±$10 million.

During our sample period, there are a total of 483 transactions involving public targets and public acquirers where the transaction value is within $50 million below the HSR threshold at closing, as well as greater than $1 million from 1998 to 2004. There are a total of 469 transactions where the transaction value is within $50 million above the HSR threshold. Those transactions below the HSR threshold are the treated sample or stealth mergers, and those above the HSR threshold are the control sample (non-stealth). As the numbers of transactions in both samples are relatively equal, we do not match transactions. Instead, we control for firm characteristics, as described below.

Table 1 provides summary statistics. Panel A reports means and standard deviations for the two samples, as well as tests of differences in means in columns 5 and 6. The target premium is the difference between the offer price and closing stock price the day before the deal announcement divided by the closing stock price. Target premiums are significantly higher by 15.32 percentage points for stealth mergers, as can be seen in column 5.[7] Part of the higher premium could be due to the smaller size of the stealth transactions, so we control for size and other firm characteristics in our subsequent empirical work.

We calculate cumulative abnormal announcement returns (CARs) for acquirers using the market model and the equally weighted stock market index. We calculate CARs for 3 days, which are trading days −1 to +1 relative to the announcement. Acquirer CARs are negative for both stealth and non-stealth mergers. Column 5 shows that stealth mergers have CARs that are 33 basis points higher than non-stealth mergers at the 3-day horizon. As a measure of profitability or operating performance, we calculate the combined acquirer and target firm change in ROA from 1 year before to 1 year after the merger. Stealth mergers show a significantly greater change in ROA than do non-stealth mergers.

We also examine several characteristics of the product market using measures from the Hoberg and Phillips data library. Hoberg and Phillips (2010) convincingly argue that using SIC codes to measure similarity or relatedness is too coarse to adequately capture the extent of true relatedness. They propose a text-based network industry classification (TNIC) method from annual reports (10Ks) to classify firms as similar based on the words firms use to describe their products. The variable *product similarity* measures the degree to which firms' products are similar based on the descriptors used in the firms' annual reports (Hoberg and Phillips (2016)). The variable *product market fluidity* measures the amount of competitive

[7]We have also calculated target premiums based on closing stock prices 1 week and 4 weeks prior to the announcement to account for the possibility of information leakage. All our results are robust to the choice of measure of target premiums.

TABLE 1

Summary Statistics

Table 1 presents summary statistics for stealth and non-stealth mergers using all public acquirers with public targets within $50 million of the HSR thresholds from 1998 to 2019. Panel A presents firm-level outcomes for targets and acquirers. Panel B presents firm-level outcomes for competitors to the acquirer. See Supplementary Material Appendix A for variable definitions. All variables are winsorized at 0.5%.

Panel A. Firms

	Stealth = 1 (N = 483)		Stealth = 0 (N = 469)			
	Mean	Std. Dev	Mean	Std. Dev	1–3	*t*-Stat
	1	2	3	4	5	6
Target premium	51.4315	55.0682	36.1115	35.6908	15.32	4.1436
Acquirer 3-day CAR	−0.0063	0.0634	−0.0097	0.0656	0.0033	2.7833
Combined ΔROA	0.0141	0.2231	−0.0051	0.2699	0.0192	2.5131
Premerger Product Market Competition						
Prod. mkt. fluidity	8.5882	3.6454	8.7419	3.4559	−0.1537	2.2119
Prod. similarity	26.7139	29.2765	27.417	28.1166	−0.7031	2.2507
TNIC HHI	0.1714	0.1969	0.1706	0.1916	0.0008	−0.2037
Post-Merger						
Prod. mkt. fluidity	8.3428	3.7064	8.8255	3.4642	−0.4827	2.0434
Prod. similarity	25.4624	28.6936	26.91	27.7562	−1.4476	2.4579
TNIC HHI	0.1739	0.2084	0.1751	0.2004	−0.0013	−0.2092
Premerger Combined Firm Investment and Innovation						
R&D/assets	0.0505	0.0937	0.0403	0.0654	0.0102	2.3073
CapEx/assets	0.0249	0.0468	0.0355	0.0496	−0.0105	−3.5731
No. patents/pssets	0.0099	0.0129	0.0124	0.0107	−0.0025	−1.9172
$ wgt patents/assets	0.0014	0.0158	0.0027	0.0053	−0.0013	−1.6923
Post-Merger						
R&D/assets	0.0435	0.0888	0.0414	0.0691	0.0021	1.5794
CapEx/assets	0.0209	0.0355	0.0337	0.0464	−0.0127	−4.8054
No. patents/assets	0.0084	0.0076	0.0143	0.0128	−0.0061	−3.485
$ wgt patents/assets	0.0012	0.0023	0.0026	0.0043	−0.0015	−1.892

Panel B. Competitors

	Stealth = 1 (N = 15,089)		Stealth = 0 (N = 14,677)			
3-day CAR	0.0002	0.0567	−0.0003	0.0585	0.0005	1.1488
ΔROA	0.0396	0.3125	−0.0211	0.3148	0.0607	−1.741
Pre-Merger Investment and Innovation						
R&D/assets	0.0499	0.0712	0.044	0.0691	0.0059	3.209
CapEx/assets	0.0362	0.0408	0.0422	0.0441	−0.0059	−2.6671
No. patents/assets	0.0118	0.0087	0.0121	0.0094	−0.0003	−1.8637
$ wgt patents/assets	0.0008	0.0012	0.0011	0.0014	−0.0003	−1.4732
Post-Merger						
R&D/assets	0.0455	0.1005	0.0459	0.0626	−0.0004	−0.0519
CapEx/assets	0.0332	0.0417	0.0409	0.0428	−0.0076	−2.8218
No. patents/assets	0.0084	0.0061	0.0143	0.0089	−0.0058	−2.0462
$ wgt patents/assets	0.0007	0.0014	0.0012	0.0021	−0.0004	−1.4928

threat and product market changes a firm faces using these text-based measures (Hoberg, Phillips, and Prabhala (2014)). To measure industry concentration, Hoberg and Phillips (2016) form industries based on the clustering of product market words and calculate a text-based version of the Herfindahl Index (TNIC HHI). Hoberg and Phillips (2016) show that the TNIC HHI dynamically updates year by year and is much more granular than static SIC-based measures of the Herfindahl Index.

Table 1, Panel A summarizes these measures premerger and post-merger. Whereas product market fluidity falls after stealth mergers, it increases after

non-stealth mergers. Product market similarity premerger is lower for stealth than non-stealth mergers and then declines for both samples post-merger. The decline is relatively larger for stealth mergers: the premerger difference between stealth and non-stealth mergers is −0.7, and increases in magnitude to −1.4 after the merger. In the univariate comparison, industry concentration is similar for both stealth and non-stealth mergers before and after the merger.

For innovation and investment, Table 1, Panel A shows that combined firm R&D spending divided by total assets declines from before to after the merger for stealth mergers. The same decline is seen for capital expenditures, the number of patents, and the dollar-weighted value of patents. For non-stealth mergers, the picture is mixed: R&D spending and the number of patents per dollar of total assets increase, while capital expenditures and the dollar-weighted value of patents slightly decrease from before to after the merger. These summary statistics are consistent with Cunningham et al. (2021), who find that incumbents in the pharmaceutical industry will acquire entrants to eliminate the entrants' innovations.

The premerger characteristics in Table 1, Panel A show there are significant differences between the stealth and non-stealth subsamples. For example, the combined target and acquirer in stealth mergers spend less premerger on capital expenditures/assets (0.0249 vs. 0.0355) and obtain fewer patents per dollar of assets (0.0099 vs. 0.0124 patents per dollar of assets) than do the combined target and acquirer in non-stealth mergers. That there are these differences is not surprising—a merger involving a target of size $40 million in 2019 is likely to be different than a merger involving a target of size $140 million. Significant premerger differences make drawing inferences harder. We address this issue when we discuss our identification strategy.

Table 1, Panel B provides summary statistics for outcomes for competitors to the acquiring firms. Competitors are firms in the same TNIC industry as the acquirer in the year of the acquisition. Interestingly, competitors to stealth acquirers engage in more R&D spending per dollar of assets prior to the merger relative to competitors to non-stealth acquirers. Nonetheless, the competitors to the stealth acquirers spend less on capital expenditures per dollar of assets, obtain fewer patents per dollar of assets, and have less valuable patents as a fraction of assets than the competitors to non-stealth acquirers prior to the merger.[8]

IV. Identification

We employ two identification strategies. First, we use a difference-in-differences specification around the 2001 amendment to the HSR thresholds. Second, we use an RDD around the HSR threshold throughout our sample period.

[8]Supplementary Material Table 1 provides summary statistics for the control variables we use in our empirical analysis. For both targets and acquirers, the controls include: firm size (total assets), growth opportunities and valuation (market-to-book value of assets), leverage (total liabilities/total assets), internally generated cash flow (EBIT/total assets), and liquidity (cash and short term investments/total assets). Not surprisingly, stealth targets are smaller than non-stealth targets. We also control for a number of bid and deal characteristics including the number of bidders, the percentage of cash used in the transaction, and whether the acquirer is a repeat acquirer. Because stealth mergers are more likely to be horizontal transactions than non-stealth mergers, we also include whether the merger is a horizontal merger. Supplementary Material Appendix A provides the list of variable definitions.

A. Difference-in-Differences Specification

On Feb. 2, 2001, the HSR threshold for premerger notification based on the size of the transaction increased from $15 million to $50 million and stayed at this level until Sept. 30, 2004. We examine mergers with a transaction size around the $50 million threshold both before and after the 2001 change. We define:

Post = 1 if a transaction occurs between Feb. 2, 2001, and Dec. 31, 2003; and 0 if between 1998 and 2000, inclusive.[9]

Below = 1 for transactions between $15 and $50 million; 0 for transactions between $50 and $100 million.

Our empirical specification is:

$$y_{it} = \alpha_0 + \alpha_1 Below_i + \alpha_2 Below_i \; X \; Post_t + \alpha^{!} X_{it} + \tau_t + \kappa_j + E_{it}. \tag{1}$$

In this specification, y_{it} is our outcome variable, such as target premium, acquirer returns, competitor returns, product market competition, and investment. *Below* and *Post* are as defined above. X_{it} is a set of covariates. These include lagged CARs from day −30 to day −3 and acquirer and target controls such as size, market-to-book, leverage, cash flow, and cash on hand. We also include the following deal controls: number of bidders, percentage of cash in the offer, a repeat acquirer indicator, a horizontal merger indicator, a tender offer indicator, and a deal attitude indicator.[10] We include industry fixed effects κ_j and year fixed effects τ_t. The year fixed effects subsume the standalone *Post* variable. Our coefficient of interest is α_2, which measures the difference in the dependent variable based on a merger being below the threshold relative to above and after the 2001 amendment relative to before. Dynamic difference-in-differences coefficients for our results are graphed in Supplementary Material Appendix B. These results allow us to assess whether there are pre-trends.

B. Regression Discontinuity Design (RDD)

We exploit the fact that there is a sharp threshold for whether a merger requires premerger notification. Figure 1 graphs the size of the transaction threshold over time. The threshold increases from $15 million to $50 million in 2001 and to $90 million in 2019. We focus on 2001 to 2019, and we examine transactions within the HSR threshold ±$10 million by year.

We implement our RDD as follows: The forcing variable in our RDD specification is the transaction value minus the HSR threshold. We define Distance from the HSR threshold as:

$$Dist\ HSR = \ln(Transaction\ Value) - -\ln(HSR\ threshold). \tag{2}$$

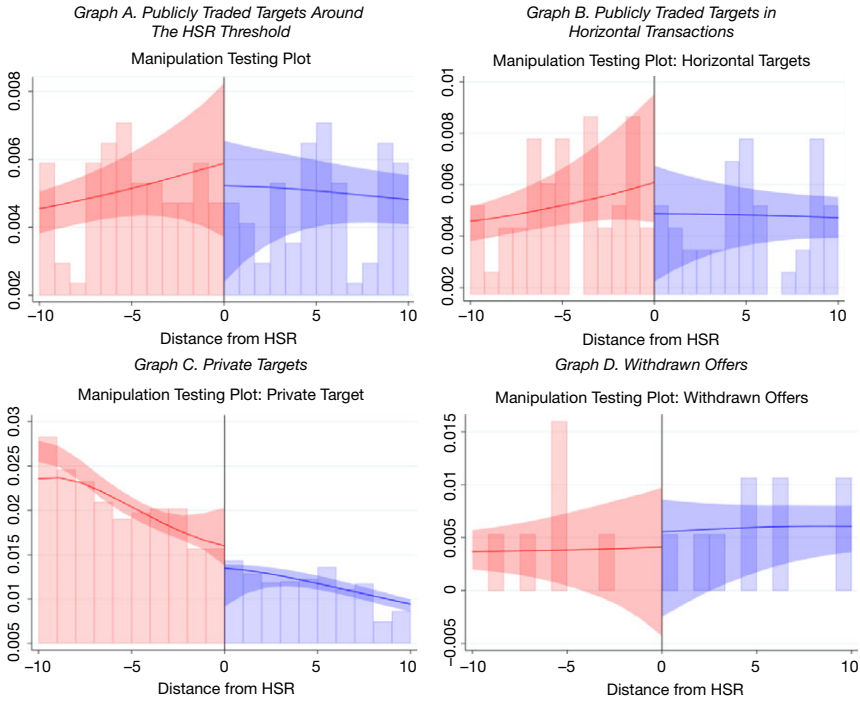
[9]We omit transactions between Jan. 1, 2001, and Feb. 2, 2001, and transactions in 2004. It is possible some transactions in these periods were delayed in anticipation of the increase in the HSR threshold, first on Feb. 2, 2001, and then on Sept. 30, 2004.

[10]Supplementary Material Table 2 presents summary statistics for our difference-in-differences sample. None of the premerger characteristics are significantly different between the stealth and non-stealth samples except for total assets, which are smaller for stealth mergers by definition.

FIGURE 2

Tests of Manipulation around the HSR Threshold

The graphs in Figure 2 plot whether there is a discontinuity at the HSR threshold for publicly traded targets. We plot the density of the forcing variable Distance from HSR threshold along with the estimated continuous density (and confidence intervals) above and below the threshold (McCrary (2008)). The number of bins is chosen optimally using McCrary's method. In all cases, the target size must be within $10 million above and below the HSR threshold.



In Figure 2, Graph A, we plot the density of the forcing variable *Dist HSR* along with the estimated continuous density and error bands above and below the threshold (McCrary (2008)). For ease of interpretation of the figures, we use the unlogged version of the Distance from HSR Threshold = Transaction Value – HSR Threshold, rather than the logged version. In the tables, we use the logged version for the empirical RDD results with optimally chosen bin widths.[11]

There is no statistically significant difference above and below the threshold in Figure 2, Graph A, ruling out manipulation of our forcing variable for public targets.

[11]We use the default choices in Stata for rdplot with the following exceptions: We use the $10 million bandwidth rather than optimally selecting the bandwidth in Stata, as optimally selecting the bandwidth will result in different bandwidths across different regressions. All of the reported coefficients are robust to the choice of bandwidth. For our graphs in Supplementary Material Appendix C, we use 5 for the number of bins, as 5 is the most frequent choice when we optimally select bins. We present the unlogged version with fixed bin widths of $2 million in the graphs in Supplementary Material Appendix C. The kernel is uniform. We use a first-order polynomial to avoid overfitting the data. We have examined alternate choices such as 10 bins, different kernels, and third-order polynomials. Our results are robust to other choices for the RDD. The cutoff is 0 as we have a sharp discontinuity when Transaction value – HSR threshold = 0.

In Graph B, we further restrict the analysis to the 152 public targets in a horizontal merger. These transactions are most likely to gain from being below the threshold and evading premerger review. We again find no evidence of a significant discontinuity. For comparison, Graph C shows that substantially more transactions involving private targets occur below the threshold than above, as previously demonstrated by Kepler et al. (2023).

In private transactions, the scope for direct manipulation is substantial. A target founder can be employed post-merger at an inflated salary, or the acquirer can establish easy earn-out targets. Unlike in private transactions, the scope of direct manipulation for public targets is very limited. There is a public market value and well-understood parameters for target premiums in public mergers. We do not expect bunching below the HSR threshold for direct manipulation reasons.

Firms above the threshold could withdraw their transactions more frequently in the face of premerger review. To address this, Graph D plots the density of withdrawn deals involving public targets around the threshold. We do not see a statistically significant difference around the threshold in withdrawn deals. We conclude that there is no ex ante manipulation of transaction values or strategic selection of targets below the threshold for public targets. Therefore, using the HSR threshold is valid in our RDD. If transactions below the HSR threshold show evidence of anticompetitive behavior in investment and innovation after the merger using our RDD, but transactions above the threshold do not, then premerger review reduces or deters ex post anticompetitive behavior in investment and innovation.

Having established internal validity, we define an indicator for stealth mergers as:

$$Stealth_{it} = 1 \ if \ DistHSR_{it} < 0, 0 \ otherwise. \tag{3}$$

Following Imbens and Lemieux (2008), we estimate:

$$y_{it} = \beta_0 + \beta_1 Stealth_{it} + \beta_2 DistHSR_{it} + \beta_3 DistHSR_{it} \times Stealth_{it} + \beta^{l} X_{it} + \tau_t + \kappa_j + E_{it}. \tag{4}$$

X_{it} is a vector of covariates, including acquirer controls, target controls, and bid controls, κ_j is a set of industry fixed effects, and τ_t is a set of year fixed effects. The coefficient of interest is β_1, which measures the impact of stealth mergers on the outcome variable.

Our RDD results are the most convincing as we use a relatively tight range around the HSR threshold, and the distribution of transaction values shows that our RDD is valid for public targets. There are 201 observations in our RDD sample, roughly evenly distributed between stealth (104) and non-stealth (97) mergers.[12] Our RDD sample does not have any of the selection concerns that our OLS sample in Table 1 might have. If we find similar results in the RDD samples and OLS

[12]Supplementary Material Table 3 presents summary statistics for our RDD sample. We winsorize the variables in the RDD sample at 1% to address outliers induced by scaling by total assets. None of the premerger characteristics are significantly different between the stealth and non-stealth samples except for total assets, which are smaller for stealth mergers by definition. That there are no significant differences ex ante between stealth and non-stealth transactions further establishes internal validity.

samples, this suggests that our RDD results have external validity. In our subsequent empirical results, we first present OLS results for the full sample to establish the baseline correlation. We then present difference-in-differences results and RDD results for identification.

V. Empirical Results

A. Target Premiums and Acquirer Returns

We hypothesize that stealth mergers result in anticompetitive behavior for publicly traded targets. As previously shown in Table 1, stealth mergers seem to generate positive value for both targets and acquirers relative to non-stealth mergers. Such value creation is consistent with either stealth mergers being efficiencyenhancing or fostering anticompetitive behavior. Nonetheless, we next establish the value creation results for targets and acquirers more formally.

In Table 2, Panel A, we present OLS regressions of target premiums, acquirer CARs, and the combined firm change in ROA on an indicator for whether the merger is a stealth merger. We include controls for acquirer and target premerger

TABLE 2

Target Premiums, Acquirer Returns, and Return on Assets for Stealth Mergers

Table 2 examines the effects of stealth mergers on target premiums, acquirer 3-day cumulative abnormal announcement returns (CAR), and change in combined firm (target plus acquirer) return on assets (ROA). Panel A presents the results of OLS regressions on an indicator for whether the merger is a stealth merger (Stealth = 1). Panel B presents the results of differences-in-differences specifications. The sample for Panel B is transactions completed between 1998 and 2003. *Below* denotes an indicator equal to 1 when the deal size is below $50 million, and equal to 0 otherwise. *Post* denotes an indicator equal to 1 if the transaction occurs after Feb. 2, 2001, when the HSR threshold was increased to $50 million. The coefficient of interest is for the interaction of *Below* × *Post*. Year fixed effects subsume the standalone Post variable. Panel C presents the results of regression discontinuity design (RDD) specifications. *Dist HSR* = ln(Transaction Value) − ln(HSR threshold). *Stealth* denotes an indicator equal to 1 if *Dist HSR* ≤ 0, and equals 0 otherwise. The coefficient of interest is for the *Stealth* variable, which shows whether there is a discontinuity at the HSR threshold. For this specification, we only include transactions with a deal size within $10 million above and below the HSR threshold for the year in which the transaction occurs. Controls in all panels include target and acquirer premerger characteristics as well as bid characteristics, which are unreported. See Supplementary Material Appendix A for variable definitions. We include industry fixed effects and year fixed effects in all specifications, and report robust standard errors clustered by year and industry in parentheses below the estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	Panel A. OLS			*Panel B. Difference-in-Differences Estimates*			*Panel C. Regression Discontinuity Design (RDD) Estimates*		
	Dependent Variable								
	Target Premium	Acquirer CAR	Combined ΔROA	Target Premium	Acquirer CAR	Combined ΔROA	Target Premium	Acquirer CAR	Combined ΔROA
	1	2	3	4	5	6	7	8	9
Stealth	9.218***	0.008**	0.025***				8.527***	0.030**	0.024**
	(3.150)	(0.004)	(0.008)				(2.913)	(0.013)	(0.011)
Below × Post				6.773**	0.029**	0.013**			
				(3.028)	(0.012)	(0.005)			
Below				−3.526	−0.000	−0.002			
				(4.806)	(0.017)	(0.018)			
Dist HSR							−9.073	0.217	0.017
							(39.018)	(0.141)	(0.162)
Stealth × *Dist HSR*							5.067	−0.169	−0.013
							(6.532)	(0.112)	(0.023)
Controls and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	952	952	952	453	453	453	201	201	201
R^2	0.0738	0.0319	0.196	0.0588	0.0358	0.303	0.055	0.026	0.412

characteristics as well as deal characteristics. We also include industry fixed effects to account for any time-invariant industry characteristics and year fixed effects. We report the primary coefficient of interest to conserve space.

Column 1 shows that stealth targets receive an incremental 9.22% premium relative to non-stealth targets. Column 2 shows that stealth acquirers earn a significant 81 basis points higher 3-day cumulative abnormal announcement return (CAR) than non-stealth acquirers.[13] As is well-known, it is difficult to infer much about the nature of mergers from changes in stock prices of acquirers, targets, and competitors due to signaling, selection, and other confounding factors (see, e.g., Eckbo (1983)). To address these issues, we examine real changes in operating performance (ROA), investment, and innovation, as well as employing two identification strategies.

Column 3 shows that ROA increases for the combined firm from 1 year prior to the merger to 1 year after by a significant 2.48 percentage points more for stealth than non-stealth mergers. Collectively, the results in Panel A suggest that stealth mergers are more value-increasing for targets and acquirers than non-stealth mergers.

To establish causality, we first use the 2001 Amendment to HSR as a natural experiment. We use the difference-in-differences specification in equation (1). We examine changes from before to after the 2001 Amendment for transactions below the $50 million threshold relative to transactions above the $50 million threshold. Those transactions below the $50 million threshold are the treated sample, and our coefficient of interest is on the interaction *Below* × *Post.*

Table 2, Panel B contains the results. We report the primary coefficients of interest to conserve space. The standalone Post variable is subsumed by the year fixed effects. Column 4 shows that 1-day target premiums are 6.77% higher for mergers below $50 million after 2001 relative to mergers above the threshold and prior to 2001. Column 5 shows that acquirer 3-day CARs are 294 basis points higher. Column 6 shows that combined firm ROA is 1.27 percentage points higher. These results are statistically significant and large in magnitude.[14]

To further establish causality, we employ the RDD around the HSR threshold from 2001 until 2019. We are interested in the average treatment effect for mergers with transaction values just below the threshold relative to those just above the threshold. We use mergers with transaction values within the HSR threshold ±$10 million at merger closing. This yields a total of 201 mergers from 2001 to 2019. Our previous results in Figure 2 show no evidence of manipulation around the threshold for public targets.

We estimate equation (4) in Table 2, Panel C, and our variable of interest is *Stealth.*[15] Column 7 shows that premiums are a significant 8.53% higher for mergers just below the threshold than for mergers just above the threshold. If target

[13]In unreported results, we have also examined 5-day CARs (92 basis points) and 2-week CARs (177 basis points). All our empirical results are robust to using the other horizons.

[14]Supplementary Material Appendix B plots the dynamic difference-in-differences coefficients for these specifications and shows that there are no pre-trends.

[15]In unreported robustness tests, all of the RDD results are quite similar if we exclude the covariates, further validating our RDD. Including the covariates slightly reduces the standard errors on our coefficients of interest but does not alter levels of statistical significance.

values are manipulated to stay below the threshold, then target premiums should be artificially low below the threshold. The results do not support manipulation of premiums. Column 8 shows that 3-day acquirer CARs are 296 basis points higher for mergers just below than those just above the threshold. Column 9 shows that combined firm ROA is 2.36 percentage points higher for mergers below relative to above the threshold.[16] CARs, premiums, and combined firm ROA are higher for mergers just below the threshold, showing these mergers are valuable for both acquirers and targets.

B. Product Market Competition

Our previous results demonstrate that stealth mergers are valuable to the participants but do not address whether they are efficiency-enhancing or anticompetitive. We next turn to the product market effects of stealth mergers to see whether these mergers are anticompetitive. We use three measures of product market competition.

First, we use product market fluidity, which measures the amount of competitive threat and product market change a firm faces using text-based analysis (Hoberg, Phillips, and Prabhala (2014)). If a merger simply eliminates a rival, then we expect product market fluidity to decrease. Second, we use product similarity, which measures the degree to which firms' products are similar based on descriptors in firms' annual reports (Hoberg and Phillips (2016)). If a merger eliminates a rival and is anticompetitive, we expect product similarity to decline and, therefore, product differentiation to increase substantially. Third, we use the text-based version of the firm's industry Herfindahl Index (TNIC HHI) from Hoberg and Phillips (2016), which dynamically updates year by year. We expect industry concentration will increase whether a merger is efficiency-enhancing or anticompetitive. However, substantial increases in concentration indicate that stealth mergers may be anticompetitive.

Table 3, Panel A presents OLS results. Columns 1 and 2 show that product market fluidity and product similarity significantly decline after stealth mergers relative to non-stealth mergers. Using the mean stealth premerger values from Table 1, Panel A, product market fluidity declines by about 2% (−0.1769/8.5882) for stealth relative to non-stealth mergers, and product market similarity declines by about 1% (−.2763/26.7139).

Column 3 shows that industry concentration significantly increases. Using the mean stealth premerger value of TNIC HHI in Table 1, Panel A, industry concentration increases by almost 11% (0.0187/0.1714) for stealth relative to non-stealth mergers. An important feature of the TNIC HHI is that an industry or market updates dynamically and is not fixed year to year.[17] Over time, industries will naturally experience changes in concentration as firms reposition themselves in product market space. Mergers may accelerate this repositioning. Smaller stealth

[16]Supplementary Material Appendix C provides RDD plots for these results.

[17]There is an intuition that markets and industries are stable and fixed over time. Using TNIC HHIs demonstrates that this is not the case—there is substantial movement within the network definition of markets and industries (Hoberg and Phillips (2010), (2016)).

TABLE 3

The Effect of Stealth Mergers on Product Market Competition

Table 3 examines the effect of stealth mergers on product market competition. Panel A presents the results of OLS regressions of the change from the year before to the year after the merger of product market fluidity, product similarity, and Herfindahls (HHI) on an indicator for whether the merger is a stealth merger (Stealth = 1). Panel B presents the difference-in-difference results for the same product market variables. Panel C presents the RDD results for the same product market variables. The product market variables come from the Hoberg and Phillips data library. Controls include target and acquirer premerger characteristics and bid characteristics (unreported). See Supplementary Material Appendix A for variable definitions. We include industry fixed effects and year fixed effects in all specifications, and report robust standard errors clustered by year and industry in parentheses below the estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	Panel A. OLS			*Panel B. Difference-in-Differences Estimates*			*Panel C. Regression Discontinuity Design (RDD) Estimates*		
	Dependent Variable								
	ΔProd.		ΔTNIC	ΔProd.		ΔTNIC	ΔProd.		ΔTNIC
	Mkt. Fluid	Simil.	HHI	Mkt. Fluid	Simil.	HHI	Mkt. Fluid	Simil.	HHI
	1	2	3	4	5	6	7	8	9
Stealth	−0.177***	−0.276**	0.019**				−0.306**	−0.403***	0.009**
	(0.066)	(0.128)	(0.008)				(0.1361)	(0.134)	(0.0039)
Below × Post				−0.104***	−0.256**	0.008**			
				(0.0368)	(0.1213)	(0.0040)			
Below				−0.084	−0.194	−0.059			
				(0.093)	(0.146)	(0.039)			
Dist HSR							−0.847	−1.122	0.193*
							(1.192)	(1.272)	(0.099)
Stealth × *Dist HSR*							0.765	0.645*	−0.185
							(1.092)	(0.381)	(0.198)
Controls and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	893	893	893	426	426	426	189	189	189
R^2	0.355	0.657	0.429	0.437	0.547	0.348	0.224	0.331	0.265

mergers may increase industry concentration more than larger mergers as competitors move away from stealth acquirers in product market space.

Panel B presents results from the difference-in-differences specification, which are similar in sign and significance. Product market fluidity and product similarity decline from before to after a merger below the threshold and after 2001 relative to a merger above the threshold and prior to 2001 industry concentration increases. The magnitudes of all the effects are reduced relative to the OLS results but are still economically significant.

Panel C presents the RDD results for mergers around the HSR threshold. The results are similar in sign and significance. Product market fluidity declines from before to after the merger by 3.6% (−0.3061/8.5428) for stealth mergers relative to non-stealth mergers, a larger effect than in either the OLS or difference-in-difference specifications when using the mean stealth premerger value of 8.5428 for the RDD sample. Product similarity declines by 1.5% (−0.4027/26.6645) for stealth relative to non-stealth mergers. Industry concentration increases for stealth relative to non-stealth mergers, and the magnitude of the effect is about 5.3% (0.0091/0.1718).

C. Changes in Investment and Innovation

Innovation is a primary dimension along which stealth mergers can impact social welfare. Stealth mergers could allow a smaller firm to scale more rapidly,

bring products to market, and reach more consumers. Alternatively, recent research has argued that some mergers occur to effectively eliminate innovation and future competition (Cunningham, Ederer, and Ma (2021), Kamepalli, Rajan, and Zingales (2020)).

We consider several measures of investment and innovation. First, we examine the change in combined firm R&D spending from 1 year prior to 1 year after the merger. Second, we examine the change in the combined firm's number of patents. Third, to measure the value of patents, we examine the change in the combined firm's dollar-weighted value of patents following Kogan et al. (2017). Fourth, we examine the combined firm change in capital expenditure. Finally, as asset divestitures could drive reductions in capital expenditures, we examine the change in asset sales.

Table 4, Panel A presents OLS results. In column 1, stealth mergers show a significant reduction in R&D spending as a fraction of total assets relative to non-stealth mergers. The reduction is substantial. Using mean combined firm R&D spending premerger from Table 1, Panel A, R&D spending decreases by 14% for stealth relative to non-stealth mergers.

In column 2, stealth mergers show a significant reduction in the number of patents per dollar of assets relative to non-stealth mergers. This is not surprising as stealth mergers have reduced patenting while non-stealth mergers have increased patenting from before to after the merger. Using mean combined firm patents per dollar of assets premerger from Table 1, Panel A, patenting decreases by 34% for stealth relative to non-stealth mergers.

Column 3 shows that the dollar-weighted value of patents decreases for stealth relative to non-stealth mergers from before to after the merger. Using the mean premerger combined firm dollar-weighted value of patents from Table 1, Panel A, patenting value decreases by 29% for stealth mergers compared to non-stealth mergers. These results suggest that innovative activity decreases after stealth mergers relative to non-stealth mergers.

Column 4 shows that stealth mergers are associated with a significant reduction in investment in the form of capital expenditures from before to after the merger. The coefficient of -0.0021 is a reduction of 8.4% in investment for stealth relative to non-stealth mergers using the premerger combined firm investment from Table 1, Panel A. Column 5 shows that this reduction is not due to asset sales in the year after the merger.

Panel B presents the difference-in-differences results. The results are similar: The change in R&D spending, patenting, the dollar value of patents, and investment spending are all lower for merging firms below the threshold and after the 2001 Amendment relative to firms above the threshold and prior to 2001. The magnitudes for the change in R&D spending, patenting, and the dollar value of patents are smaller than for the OLS results, but still economically and statistically significant. The change in capital expenditures is larger in magnitude than for the OLS specification, and the change in asset sales is again insignificant.

Panel C presents the RDD results. We find similar results: The average treatment effects for the change in R&D spending, patenting, the dollar value of patents, and investment spending are all lower for mergers just below than just

TABLE 4

The Effect of Stealth Mergers on Changes in Investment and Innovation for the Combined Firm

Table 4 examines the effect of stealth mergers on changes in investment and innovation for the combined firm (target and acquirer) from 1 year before to 1 year after the merger. Panel A presents the results of OLS regressions of R&D spending, the number of patents, the dollar-value of patents, capital expenditures, and asset sales on an indicator for whether the merger is a stealth merger (*Stealth* = 1). Panel B presents the difference-in-difference results for the same investment and innovation variables, and Panel C presents the RDD results for the same investment and innovation variables. Controls include target and acquirer premerger characteristics and bid characteristics (unreported). See Supplementary Material Appendix A for variable definitions. We include industry fixed effects and year fixed effects in all specifications, and report robust standard errors clustered by year and industry in parentheses below the estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. OLS Estimates

	ΔR&D/ Assets	ΔNo. Patents/ Assets	Δ$ Wgt Patents/ Assets	ΔCapex/ Assets	ΔAsset Sale/ Assets
	Dependent Variable				
	1	2	3	4	5
Stealth	−0.0071**	−0.0034***	−0.0004***	−0.0021**	−0.0539
	(0.0032)	(0.0011)	(0.0001)	(0.0010)	(0.0454)
Acquirer controls	Yes	Yes	Yes	Yes	Yes
Target controls	Yes	Yes	Yes	Yes	Yes
Bid controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
No. of obs.	952	654	654	952	952
R^2	0.118	0.326	0.347	0.217	0.155

Panel B. Difference-in-Differences

Below × Post	−0.0017**	−0.0025**	−0.0003**	−0.0062**	−0.0317
	(0.0008)	(0.0012)	(0.0001)	(0.0029)	(0.0592)
Below	−0.0238	−0.0036	−0.0008	−0.0028*	−0.0208
	(0.0209)	(0.0031)	(0.0036)	(0.0016)	(0.0678)
Acquirer controls	Yes	Yes	Yes	Yes	Yes
Target controls	Yes	Yes	Yes	Yes	Yes
Bid controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
No. of obs.	453	363	363	453	453
R^2	0.182	0.352	0.362	0.216	0.162

Panel C. RDD Estimates

Stealth	−0.0061**	−0.0040**	−0.0005**	−0.0042**	−0.0331
	(0.0031)	(0.0019)	(0.0002)	(0.0021)	(0.0288)
Dist HSR	0.0265	−0.0474	−0.0385	−0.0165	−0.1421
	(0.0364)	(0.0371)	(0.0296)	(0.0392)	(0.2026)
Stealth × *Dist HSR*	−0.0579	0.4742	0.5732	−0.0054	0.4903*
	(0.0462)	(0.3739)	(0.4834)	(0.0693)	(0.2921)
Acquirer controls	Yes	Yes	Yes	Yes	Yes
Target controls	Yes	Yes	Yes	Yes	Yes
Bid controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
No. of obs.	201	159	159	201	201
R^2	0.205	0.291	0.284	0.206	0.126

above the HSR threshold. The magnitudes are again consistent with our previous results.[18] Change in asset sales is insignificant.

[18]In unreported results, we examine the change in capital expenditures and change in R&D spending for the combined firm from 1 year before the merger to 2 years after the merger to see if the decrease in R&D and capital expenditures for stealth relative to non-stealth mergers persists over time. Across all specifications, the reduction in R&D spending for stealth mergers doubles at the 2-year post-merger horizon. The reduction in capital expenditures is similar in magnitude at the 2-year and 1-year

TABLE 5

Establishment Level Evidence on Target Closures Post Stealth Acquisitions

Data in Table 5 are from the U.S. Census Bureau Longitudinal Business Database (LBD). We define an indicator variable *Shutdown* equal to 1 if a target establishment is closed (no longer appears in the LBD) within 3 years of acquisition, and equal to 0 otherwise. We define *AgeAfter* as the number of years that a target establishment exists in the LBD after being acquired. We only present the OLS specification due to Census disclosure requirements.

Panel A. Summary Statistics (N = 17,500)

	Stealth = 1	Stealth = 0	Difference	*t*-Stat
	1	2	3	4
Shutdown	0.5332	0.3185	0.2147	12.16
AgeAfter	2.888	4.847	−1.959	−11.9

Panel B. OLS

	Shutdown	AgeAfter
	Dependent Variable	
	1	2
Stealth	0.0637***	−0.1852***
	(0.024)	(0.042)
Controls	Yes	Yes
No. of obs.	17,500	17,500
R^2	0.264	0.292

To further understand the mechanism behind the change in investment and innovation, we use data from the U.S. Census Bureau Longitudinal Business Database (LBD). We examine whether stealth acquirers are more likely to shut down target establishments than non-stealth acquirers. Table 5 contains the results. We find that stealth target establishments are 21.47 percentage points more likely to be shut down within 3 years of acquisition than non-stealth establishments. The average age post-acquisition for stealth target establishments is almost 2 years less than non-stealth target establishments. Shutdowns are not accompanied by significant asset sales, as demonstrated in Table 4, so these are complete shutdowns rather than divestitures. Stealth mergers result in innovation, investment, and future competition being shut down.

A potential concern is that the number of public target and public acquirer stealth mergers is relatively small—483 stealth mergers over a 22-year period. However, there are 6,458 private target and public acquirer stealth mergers over our sample period. We do not examine the private target sample as we do not have data for capital expenditures, R&D spending, and patents for private targets prior to the stealth merger. Nonetheless, we believe that our results generalize to the private target sample for several reasons.

First, we argue that disclosure to government agencies deters anticompetitive behavior. For private targets, disclosure should matter more as less is known about private targets ex ante. Second, while public target values are not manipulated to be below the HSR threshold, they still result in anticompetitive behavior in investment and innovation. Manipulated private target transactions may be even more anticompetitive than non-manipulated public target transactions. Section V.E. includes

post-merger horizon. These effects are statistically significant and suggest that reductions in R&D and capital expenditures are not transient.

private transactions and finds consistent results. Third, our RDD specification, which is our tightest specification with the best identification, yields estimates that are very similar to our OLS specification. We believe our results have external validity and generalize beyond our sample to private transactions as well.

Overall, these results support the view that stealth mergers allow firms to reduce innovation and investment, but they are not conclusive with respect to aggregate social welfare. We cannot say if the investment spending or innovation that is foregone after a stealth merger would have created value or been wasteful for firms. However, less investment and innovation reduce consumer surplus, which is an important focus for antitrust policy.

In addition, the dollar-weighted patenting results suggest that the innovation foregone after a stealth merger was value-creating. For our RDD, the identifying assumption is that assignment is as good as random around the threshold. Yet, mergers below the threshold have less ex post investment and innovation than those above. It is unlikely that stealth targets would consistently have inefficient investment in capital expenditures and R&D, while non-stealth targets would have valuable investment. Nonetheless, to address the possibility that stealth mergers eliminate inefficient investment, we turn to an analysis of competitors of acquirers.

D. Effects on Competitors

We examine the effects of stealth mergers on competitor firms. We have already shown that product market competition decreases after stealth mergers, and stealth acquirers have lower investment in R&D and capital expenditures but greater profitability. This is due to one of two possibilities. Either stealth mergers reduce inefficient investment by targets, or stealth mergers consolidate an industry. Changes in competitor profitability, investment, and innovation will indicate whether stealth mergers are efficiency-enhancing or anticompetitive. We define competitors as all firms in the acquirer's TNIC, following Hoberg and Phillips (2010), (2016). If stealth mergers are efficiency-enhancing, then a stealth merger should weakly negatively affect competitors' returns and profitability as they face a more formidable competitor. Competitors should increase investment and innovation to compete with a stronger product market rival. Conversely, if stealth mergers are anticompetitive, then a stealth merger should increase competitors' returns and profitability and allow competitors to decrease investment and innovation due to industry consolidation. R&D investment would represent competition in strategic complements.

Table 6 examines the effects on competitor returns (CARs) and profitability (ROA). Panel A contains OLS results. Competitor CARs are measured from 1 day prior to the focal acquirer's merger announcement to 1 day after. Column 1 shows that competitor 3-day CARs are 21 basis points higher when a rival's merger is a stealth relative to a non-stealth merger.[19] Column 2 shows that the competitor change in profitability from before to after the merger is 1.41 percentage points higher for stealth than non-stealth mergers.

[19]Kepler et al. (2023) show a similar result for their sample of private targets, although smaller in magnitude.

TABLE 6

The Effect of Stealth Mergers on Competitor Returns

Table 6 examines the effect of stealth mergers on the returns to the competitors to the acquirer. The sample includes all competitor firms to the acquirer as defined by the acquirer's TNIC for the year in which the acquisition takes place. Panel A presents the results of OLS regressions of competitor firm 3-day cumulative abnormal announcement returns around the acquirer's announcement of the acquisition and the change in ROA from before to after the merger for the competitor firms on an indicator for whether the merger is a stealth merger (*Stealth* = 1). Panel B presents the difference-in-difference results for competitor CARs and the change in competitor ROA. Panel C presents the RDD results for competitor CARs and change in competitor ROA. Competitor firm controls include size, market-to-book, leverage, cash flow, and the level of cash balances (unreported). See Supplementary Material Appendix A for variable definitions. We include year fixed effects and industry fixed effects in all specifications, and report robust standard errors clustered by year and industry in parentheses below the estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	Panel A. OLS		*Panel B. Diff-in-Diff*		*Panel C. RDD*	
	3-Day CAR	ΔROA	3-Day CAR	ΔROA	3-Day CAR	ΔROA
	Dependent Variable					
	1	2	3	4	5	6
Stealth	0.0021***	0.0141***				
	(0.0007)	(0.0032)				
Below × Post			0.0099***	0.0098**		
			(0.0036)	(0.0041)		
Below			−0.0089***	0.0016		
			(0.0026)	(0.0032)		
Stealth (RDD)					0.0093***	0.0161**
					(0.0032)	(0.0079)
Dist HSR					−0.0082	0.0537
					(0.0285)	(0.0592)
Stealth × *Dist HSR*					0.0058	−0.0265
					(0.0267)	(0.0472)
Competitor firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	29,766	29,766	12,158	12,158	3945	3945
R^2	0.0031	0.363	0.0029	0.473	0.022	0.427

We also put these results in context relative to the summary statistics in Table 1, Panel B. There we showed that stealth mergers are associated with competitors having positive 3-day CARs in an absolute sense. Non-stealth mergers have negative 3-day CARs for competitors, although both are small in magnitude. In addition, stealth mergers are associated with competitors having positive ROA changes from before to after the merger, while non-stealth mergers are associated with competitors having negative ROA changes. These results are more consistent with stealth mergers leading to anticompetitive outcomes rather than stealth mergers creating a more efficient, formidable rival, and increasing competition.

Table 6, Panels B and C establish a causal interpretation for the previous competitor CAR and ROA results. Panel B presents the difference-in-difference results. Competitor CARs are 99 basis points higher for competitors to merging firms below the threshold and after 2001 relative to those above the threshold and prior to 2001. Competitor firm profitability (ROA) increases by 0.98 percentage points. Panel C presents the RDD results. Competitor CARs are 93 basis points higher for competitors to a stealth acquirer than a non-stealth acquirer. ROA also increases by 1.61 percentage points for competitors to a stealth acquirer relative to a non-stealth acquirer. These results are consistent with anticompetitive behavior.

Table 7 turns to competitor firm investment and innovation. Panel A contains OLS results. A rival engaging in a stealth merger is associated with the acquirer's

TABLE 7

The Effect of Stealth Mergers on Changes in Investment and Innovation by Competitors

Table 7 examines the effect of stealth mergers on changes in investment and innovation by the competitors to the acquirer. The sample includes all competitor firms to the acquirer as defined by the acquirer's TNIC for the year in which the acquisition takes place. Panel A presents the results of OLS regressions of changes in competitor firm R&D, number of patents, dollar value of patents, and capital expenditures from before to after the merger on an indicator for whether the merger is a stealth merger (*Stealth* = 1). Panel B presents the difference-in-difference results for the change in competitor investment and innovation. Panel C presents the RDD results for changes in competitor investment and innovation. Competitor firm controls include size, market-to-book, leverage, cash flow, and the level of cash balances (unreported). See Supplementary Material Appendix A for variable definitions. We include year fixed effects and industry fixed effects in all specifications, and report robust standard errors clustered by year and industry in parentheses below the estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. OLS

	ΔR&D/Assets	ΔNo. Patents/Assets	Δ$ Wgt Patents/Assets	ΔCapex/Assets
	Dependent Variable			
	1	2	3	4
Stealth	−0.0110***	−0.0018***	−0.0002**	−0.0014**
	(0.0031)	(0.0005)	(0.0001)	(0.0006)
Competitor firm controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	29,766	16,065	16,065	29,766
R^2	0.214	0.204	0.193	0.216
Panel B. Difference-in-Differences				
Below × Post	−0.0073**	−0.0013**	−0.0002**	−0.0029**
	(0.0033)	(0.0006)	(0.0001)	(0.0014)
Below	−0.0063**	−0.0072	−0.0001	−0.0015
	(0.0031)	(0.0094)	(0.0013)	(0.0042)
Competitor firm controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	12,158	6562	6562	12,158
R^2	0.262	0.229	0.216	0.201
Panel C. RDD Estimates				
Stealth	−0.0066**	−0.0023***	−0.0004***	−0.0042**
	(0.0031)	(0.0007)	(0.0001)	(0.0020)
Dist HSR	0.0002	−0.0065	−0.0001	0.0004
	(0.0009)	(0.0082)	(0.0023)	(0.0029)
Stealth × *Dist HSR*	−0.0005	0.0038	0.0043	−0.0007
	(0.0004)	(0.0042)	(0.0061)	(0.0026)
Competitor firm controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	3945	2169	2169	3945
R^2	0.360	0.208	0.192	0.234

competitors spending less on R&D, patenting less, having lower value patents, and investing less in capital expenditures than when a rival engages in a non-stealth merger. These results are consistent with a general reduction in industry investment and innovation as a consequence of stealth mergers. The magnitudes of the effects on innovation are large. The reduction in R&D spending for competitors to stealth relative to non-stealth mergers is 22% (−0.0110/0.0499) of the stealth premerger level of R&D spending from Table 1, Panel B. Competitors reduce the number of patents per dollar of assets from before to after the merger by 15.3% (−0.0018/0.0118) for stealth relative to non-stealth mergers, using the premerger number of competitor patents per dollar of assets from Table 1, Panel B. The dollar-weighted value of competitor patents falls by 25% from before to after for stealth

relative to non-stealth mergers. Compared to non-stealth mergers, the reduction in capital expenditures for competitors to stealth acquirers is 3.9% of the pre-stealth merger level of capital expenditures.

To provide a causal interpretation, we present the results of our difference-in-differences specifications in Table 7, Panel B, and our RDD in Table 7, Panel C. The results are consistent. Panel B shows that the change in competitor R&D spending, patenting, the dollar value of patents, and spending on capital expenditures are lower for competitors to merging firms below the threshold and after 2001 relative to above the threshold and prior to 2001. The magnitudes of the coefficients for R&D spending, patents, and dollar-weighted value of patents are smaller in the difference-in-differences specifications than in the OLS specifications, but still economically significant, while the magnitude of the coefficient is larger for capital expenditures.

Panel C shows that competitor R&D spending, patenting, the dollar value of patents, and spending on capital expenditures are all lower for competitors to a stealth acquirer than for competitors to a non-stealth acquirer. The magnitudes of the coefficients are similar in these specifications to those in our OLS and difference-in-differences specifications.

Although there is no evidence of manipulation of transaction values by public targets and acquirers, one might still be concerned about manipulation. However, manipulation should not impact the results we find for competitors. Competitors do not influence a rival's manipulation of a transaction value. Our results showing better competitor performance and reduced investment and innovation from a rival's stealth transaction are free from any concerns about whether manipulation occurred. Nonetheless, our results show industry consolidation and a reduction in social welfare due to decreases in innovation from stealth mergers.

E. Merger Timing

We argue that stealth mergers result in reduced investment and innovation because they face no government scrutiny. We further explore whether premerger review deters anticompetitive behavior by examining the timing of mergers. Once an acquirer is subject to government scrutiny for a non-stealth merger, then that acquirer is subject to more scrutiny in future mergers and the product market even if the government takes no action on the initial merger. This is also true for a future stealth merger, as Section 7 of the Clayton Act permits the government to initiate a review and challenge a merger after it is completed. We hypothesize that stealth mergers before an acquirer undertakes a non-stealth merger are anticompetitive, while stealth mergers after an acquirer undertakes a non-stealth merger are not. As non-stealth mergers open the acquirer to ongoing government scrutiny, this deters anticompetitive behavior in future mergers.

To test this hypothesis, we examine all instances of serial acquirers—those acquirers engaging in multiple transactions—in our data. We include both public and private targets, as private targets can trigger premerger notification. During our sample period, we examine two situations: 1) a serial acquirer completes stealth acquisitions and then completes a non-stealth acquisition, and 2) a serial acquirer completes a non-stealth acquisition and then completes stealth acquisitions. For any

given serial acquirer, only the first non-stealth acquisition is relevant. All stealth acquisitions prior to the first non-stealth acquisition are in category 1). All stealth acquisitions after the first non-stealth acquisition are in category 2). We create an indicator *BeforeNonStealth* = 1 for mergers in category 1), and 0 for mergers in category 2). We test whether stealth mergers where *BeforeNonStealth* = 1 are more anticompetitive than stealth mergers where *BeforeNonStealth* = 0.

Table 8 contains the results. Panel A presents summary statistics on acquirer and competitor returns for stealth mergers prior to non-stealth mergers, non-stealth mergers themselves, and stealth mergers after non-stealth mergers. Before a non-stealth merger, stealth mergers have positive and significant 3-day CARs for both acquirers at 56 basis points and competitors at 28 basis points. By contrast, both non-stealth mergers and stealth mergers after a non-stealth merger have insignificant 3-day CARs.[20]

Table 8, Panel B, columns 1 and 2 provide OLS regressions using the *BeforeNonStealth* indicator with controls and confirm the results in Panel A. As these tests involve a comparison of two categories of stealth mergers, before and after a non-stealth merger, and not non-stealth mergers themselves, we only use OLS specifications. Columns 3 and 4 provide confirmatory evidence using the change in ROA from before to after the merger as the dependent variable. For the combined target and acquirer change in ROA, we only use the public target sample in the regression as we need the target's ROA prior to the acquisition. We use the entire sample of public and private targets for the competitor results, as the ROA change is for the competitor itself. In columns 3 and 4, the change in ROA is significantly higher for stealth mergers prior to a non-stealth merger than for stealth mergers after a non-stealth merger.

We next examine changes in product market competition. Table 8, Panel C, provides OLS results for product market fluidity, product similarity, and industry concentration. In all three columns, product market competition is reduced for stealth mergers before a non-stealth merger relative to stealth mergers after a non-stealth merger. The magnitudes of these effects are substantial. These results include private targets.

We are primarily interested in investment and innovation. We examine changes in R&D, the number of patents, the dollar value of patents, and capital expenditures from before to after the merger for the combined firm. We use the public target sample as we need combined target and acquirer changes. Table 8, Panel D contains the results. We compare the change in investment for stealth mergers before a non-stealth merger to the change in investment for stealth mergers after a non-stealth merger. For all dependent variables, investment and innovation decline significantly more for stealth mergers before non-stealth mergers than stealth mergers after non-stealth mergers.

As a final set of tests, we examine changes in competitors' innovation and investment. In these tests, we again include public and private targets. The results are in Table 8, Panel E. We find that for all 4 dependent variables, investment and innovation decline significantly more for competitors of acquirers engaging in stealth mergers prior to a non-stealth merger than competitors of acquirers engaging in stealth mergers after a non-stealth merger.

[20]Summary statistics for all variables used in Panels B through E are available from the authors.

TABLE 8

Merger Timing in Serial Acquisitions

Table 8 examines the effect of merger timing on stealth mergers in serial acquisitions. We examine stealth mergers that occur prior to a first non-stealth merger relative to stealth mergers that occur after the first non-stealth merger by a serial acquirer. We examine both public and private targets. We create an indicator *BeforeNonStealth* equal to 1 for stealth mergers prior to a non-stealth merger, and equal to 0 for stealth mergers after a non-stealth merger. Panel A presents summary statistics for *BeforeNonStealth* = 1, Non-stealth mergers themselves, and *BeforeNonStealth* = 0. Panel B presents the results of OLS regressions of returns on *BeforeNonStealth* for both acquirers and competitors of acquirers. Panel C presents the results of OLS regressions of changes in product market competition on *BeforeNonStealth*. Panel D presents the results of OLS regressions of the change from the year before to the year after the merger of combined firm investment and innovation on *BeforeNonStealth* for mergers with public targets only. Panel E presents the results of OLS regressions of changes in competitor investment and innovation on *BeforeNonStealth* for mergers with public and private targets. Controls include target and acquirer or competitor premerger characteristics and bid characteristics (unreported). See Supplementary Material Appendix A for variable definitions. We include industry fixed effects and year fixed effects in all specifications, and report robust standard errors clustered by year and industry in parentheses below the estimates. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Summary Statistics

	BeforeNonStealth = 1	NonStealth	BeforeNonStealth = 0	Difference (1)–(3)
	1	2	3	4
3-day CAR (acquirer)	0.0056**	−0.0012	−0.0019	0.0075*
	[1189]	[796]	[683]	
3-day CAR (competitor)	0.0028*	0.0006	−0.0009	0.0037**
	[26,224]	[17,556]	[15,064]	

Panel B. OLS Returns

	3-Day CAR		ΔROA	
	Acquirer (Public and Private Targets)	Competitor (Public and Private Targets)	Combined (Public Targets Only)	Competitor (Public and Private Targets)
	Dependent Variable			
	1	2	3	4
BeforeNonStealth	0.0089***	0.0023**	0.0244***	0.0241***
	(0.0023)	(0.0011)	(0.0076)	(0.0072)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	1872	41,288	483	41,288
R^2	0.034	0.026	0.450	0.272

Panel C. OLS Product Market Competition

	ΔProd.		ΔTNIC
	Mkt. Fluid	Simil.	HHI
	Dependent Variable		
	1	2	3
BeforeNonStealth	−0.2937**	−0.3306**	0.0407**
	(0.1362)	(0.1343)	(0.0189)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
No. of obs.	1741	1741	1741
R^2	0.300	0.721	0.362

Panel D. OLS Changes in Combined Firm Investment and Innovation

	ΔR&D/Assets	ΔNo. Patents/Assets	Δ$ Wgt Patents/Assets	ΔCapex/Assets
	Dependent Variable			
	1	2	3	4
BeforeNonStealth	−0.0105*	−0.0016**	−0.0003**	−0.0012*
	(0.0061)	(0.0007)	(0.0001)	(0.0007)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	483	317	317	483
R^2	0.147	0.295	0.362	0.179

Panel E. OLS Changes in Competitor Firm Investment and Innovation

Dep. Var.	ΔR&D/Assets	ΔNo. Patents/Assets	Δ$ Wgt Patents/Assets	ΔCapex/Assets
BeforeNonStealth	−0.0148***	−0.0019**	−0.0004**	−0.0014*
	(0.0024)	(0.0009)	(0.0002)	(0.0008)
Competitor controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	41,288	20,231	20,231	41,288
R^2	0.185	0.183	0.196	0.205

Most of the anticompetitive effects of stealth mergers occur prior to the acquirer making a non-stealth acquisition, consistent with premerger notification deterring ex post anticompetitive behavior. An important caveat to these results is that there are significant endogeneity and selection concerns with respect to serial acquisitions. Early choices of targets impact government scrutiny of later choices. Our previous difference-in-difference and RDD specifications are insufficient to address this specific endogeneity concern.

VI. Conclusion

We examine whether stealth mergers reduce investment and innovation and are therefore anticompetitive. We provide several novel findings. First, stealth mergers involving public targets result in anticompetitive behavior, including reductions in investment and innovation. While private target transaction values can be manipulated, this is not the case for public targets. Antitrust concerns with high thresholds for government notification go beyond strategic manipulation. We provide causal evidence that premerger notification deters anticompetitive behavior regardless of whether there is strategic manipulation of transaction values.

Second, we document changes in product market outcomes. Stealth mergers are associated with less product market fluidity, product similarity, and greater industry concentration. Third, investment and innovation decrease for both stealth acquirers and their competitors relative to non-stealth acquirers and their competitors. In addition, both stealth acquirers and their competitors have higher returns and better operating performance relative to non-stealth acquirers and their competitors. These results are consistent with industry consolidation and a reduction in social welfare, due to decreases in aggregate innovation and product market churn from stealth mergers.

Our analysis shows that raising HSR thresholds increased anticompetitive behavior due to mergers. An important policy implication is that HSR thresholds may need to be revisited by lowering the threshold, requiring horizontal mergers to undergo premerger review, or requiring transactions in industries above some level of concentration to undergo premerger review.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109024000577. We have received no additional funding for this work.

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