Does common ownership really increase firm coordination?

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July 2020

Abstract

A growing body of evidence concludes that common ownership caused cooperation among firms to increase and competition to decrease. We take a closer look at four approaches used to identify these effects. We find that the effects the literature has attributed to common ownership are caused by other factors, such as differential responses of firms (or industries) to the financial crisis. We propose a modification to one of the previously used empirical approaches, which is less sensitive to these issues. Using this to re-evaluate the link between common ownership and firm outcomes, we find little robust evidence that common ownership affects firm behavior.

* We would like to thank Ian Appel, Jie Cai, Todd Gormley, Tom Griffin, Sungjoung Kwon, Felipe Severino, and Irene Yi, seminar participants at Dartmouth College, and conference participants at the 2019 Midwest Finance Association Meeting, the 2019 SFS Cavalcade North America, the 2019 Norges Bank Investment Management Research Seminar, and the 2019 BI Corporate Governance Conference. Michelle Lowry thanks the Raj & Kamla Gupta Governance Institute for financial support.

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1. Introduction

As has been well-documented, common ownership has increased substantially over the past three decades. This increase is due to a combination of consolidation in the asset-management industry and growth in index investing. Whereas only 17% of S&P500 firms had a blockholder that also owned a block in a competitor firm in 1990, this had increased to 81% by the end of 2015. A growing number of academic papers conclude that the rise in common ownership has caused cooperation among firms to increase and competition to decrease. Cross-owners' incentives to maximize returns across multiple firms in their portfolio rather than returns on any single firm allegedly lead them either to actively encourage cooperation between firms or to put less pressure on firm managers to aggressively compete against their rivals.

Commensurate with this evidence, there have been several policy proposals to regulate common ownership. Elhauge (2016) suggests using existing antitrust law to assess mergers, e.g., mergers between institutional investors, that result in increased cross-holdings within the same industry. Posner, Morton and Weyl (2017) propose requiring institutions to either face ownership restrictions or commit to being purely passive, i.e., to not influence firm governance through shareholder voting, communications, or trading.[1] However, Rock and Rubinfeld (2017) point out that these recommendations may be misguided, and that the costs of institutional investors disengaging from corporate governance may outweigh the benefits.

A survey of the literature finds six published papers and 13 working papers written since 2016 that conduct empirical analyses of common ownership (see list in Table 1). Across these 19

[1] Specifically, Posner, Morton and Weyl (2017) propose that institutions have the choice between: (1) owning no more than 1% of any oligopolistic industry, (2) owning only one firm within any oligopolistic industry, or (3) being entirely passive with respect to firm governance.

papers, all but three conclude that common ownership influences firm behavior, specifically competition and/or coordination between cross-owned firms. Based on this research, one could reasonably conclude that the trend in common ownership justifies the increased scrutiny by regulators. However, there are grounds for skepticism. On the theoretical side, Gilje, Gormley, and Levit (2020) and Lewellen and Lewellen (2019) question the incentives of institutions to encourage anti-competitive practices.[2] Consistent with such concerns, on the empirical side, Dennis, Gerardi and Schenone (2020) and Kennedy, O'Brien, Song, and Waehrer (2017) question the conclusions of Azar, Schmalz and Tecu (2018) that common ownership within the airline industry resulted in anti-competitive practices [see also a reply in Azar, Schmalz and Tecu (2018b)].

Empirically testing whether common ownership influences firm behavior is challenging. On an aggregate level, increases in common ownership have coincided with substantial consolidation within nearly every industry (see, e.g., Doidge, Karolyi and Stulz, 2017), and this trend could have caused shifts in profitability or investment. In addition, most studies of common ownership rely disproportionately on the years around the 2008 financial crisis, which further confounds the analysis (as we discuss in more detail below).

The first objective of the paper is to evaluate the empirical approaches used in the literature to identify the effects of common ownership. We focus on four types of events used to identify exogenous shifts in cross-ownership: a broad sample of mergers between financial institutions,

[2] Gilje, Gormley, and Levit (2020) point out that institutions may lack these incentives if the firms represent only small fractions of their portfolios. Lewellen and Lewellen (2019) show that while dollar cash-flows to institutions from promoting anticompetitive practices can be substantial, they tend to be small in the more concentrated industries where the potential for anticompetitive behavior would be greatest.

the Blackrock/BGI merger, additions to the S&P500, and reconstitutions of Russell 1000 / 2000 indices.[3]

With a more thorough understanding of the advantages and shortcomings of each approach, we then revisit the conclusions of prior empirical studies, that common ownership affects firm behavior. We are interested broadly in any explicit or implicit coordination between connected firms. Coordination could potentially take many forms, and we do not have a strong prior on which is more likely. For example, rival firms could coordinate to increase profits by engaging in joint innovation, by maintaining higher product prices, or by lowering costs via decreases in investment or product quality. Several authors and policy makers cite the recent evidence on coordination between cross-owned firms as an indication of anticompetitive behavior.[4]

Conceptually, mergers between financial institutions, proposed by He and Huang (2017), offer many advantages as a source of identification, and thus we focus first on this approach. As highlighted by prior literature, mergers influence cross-ownership but, compared to index additions, they are less likely motivated by policies or performance of the portfolio firms. Using a broader set of mergers (rather than the Blackrock-BGI merger alone) allows for both cross-sectional and time-series variation, lessening concerns about correlated factors. We identify 64 financial-institution mergers that have the potential to affect common ownership, which are listed in Appendix Table A1. The selection criteria are similar to those in He and Huang: we require that one partner (i.e., financial institution) in the merger holds a block in a firm while the other partner

[3] Several papers have also used mutual fund outflows. See Berger (2019) for an analysis of problems related to this approach.
[4] See, e.g., OECD Directorate for Financial and Enterprise Affairs Competition Committee, 'Common ownership by institutional investors and its impact on competition, background note by the secretariat.' December 5-6, 2017.

holds a block in the firm's industry peer. After merging, the newly-formed institution will be a significant cross-holder in the pair of firms.

We find that the financial-institution mergers cause substantial and lasting increases in cross-ownership for the pairs of affected firms, providing an attractive setting to identify the effects on coordination. Following similar approaches in the literature, we aggregate the cross-ownership stakes across cross-owners of each pair of firms, taking into account both the size of these stakes and their symmetry across the firms within each pair. We name this measure the Cross-ownership Index (C-Index). We find that the C-Index for treatment pairs jumps discretely in the merger quarter.

The key and previously overlooked challenge with using a broad set of financial-institution mergers for identification is that the data are clustered in time. While the merger *events* are distributed fairly evenly during the 1980-2015 sample period, close to half of the treatment *firms* are associated with mergers occurring during 2008 and 2009 (the largest of which is the Blackrock-BGI merger). This means that the post-merger period for these firms coincides with the aftermath of the financial crisis. This problem is compounded when control firms are sampled from different (i.e., "unaffected") industries than treatment firms, as these industries could have responded differently to the crisis even in the absence of the merger event.[5] These concerns apply both to studies that use a broad set of financial-institution mergers and to studies that use the Blackrock-BGI merger as the exogenous shock. We address this issue in two ways. First, we examine separately mergers outside of the 2008-2009 period, and second, we test the robustness of the results to alternative control samples, including the "untreated" industry peers.

[5] By 'unaffected' industries, we mean industries in which cross-ownership is not expected to increase as a result of the merger because the two merger partners do not hold blocks in different industry peers.

Our findings are striking. First, consistent with prior studies, when we use the full merger sample including the crisis years, and a control sample comprised of firms from unaffected (i.e., different) industries, treatment firms exhibit significantly better financial performance relative to the control firms after the event. Consistent with conclusions of He and Huang (2017), this suggests that cross-ownership improves performance, as might be expected if the affected firms engaged in tighter cooperation. There is also evidence of reduced investment in R&D, consistent with the hypotheses of Gutiérrez and Philippon (2017) that connected firms reduce investment as they face lower incentives to compete. These results disappear, however, when we use mergers outside of the financial crisis period. Moreover, they also disappear when we select control firms that are either drawn from the same industries as the treated firms or that are from different industries but are more closely matched to the treated firms.

Examining the data more closely reveals that the patterns which prior literature has attributed to common ownership can be traced to the differential responses of firms in the years coming out of the financial crisis. When using either the Blackrock-BGI merger or a broader set of financial-institution mergers (in which treatment firms are still concentrated in 2008 – 2009) as the identifying event, treatment firms disproportionately represent high growth firms. These types of firms performed better in the years after the crisis.

One reason for the lack of effects on firm-level financial performance or investment might be that these effects are difficult to detect or only materialize in the long run. As an alternative, we look for more direct evidence of cooperation by focusing on the pairs of affected firms, over a three-year period following the increase in cross-ownership. Here again, we find no evidence of increased cooperation. There is no evidence that firms that became cross-owned as a result of the financial institution merger are more likely to merge or to engage in joint ventures or strategic

alliances. In fact, these events of more explicit coordination between any specific pair of firms are extremely rare, less than 0.1% of all potential firm pairs.

Finally, we examine the two alternative approaches the literature has used to identify exogenous shifts in cross-ownership: S&P500 additions and the reconstitution of the Russell indices. We conclude that neither of these two events are appropriate for this purpose. In both cases, there are obvious concerns about endogenous entry. Incremental to this concern, in the case of S&P500 additions, we find that the entry events affect institutional ownership on multiple levels that are inherently difficult to separate. While entry into the index does cause discrete shifts in cross-ownership, it is also associated with increases in total institutional ownership and with drops in block ownership. Index tracking institutions increase their ownership in the added firms, contributing positively to both total institutional ownership and to cross-holdings with other portfolio firms; at the same time other blockholders decrease their positions.

Russell index reconstitutions have the potential to somewhat lessen the concerns about endogenous index inclusion because the reconstitutions are more transparent and are based on market capitalization alone. However, we find that they have no effect on cross-ownership on the institutional level, which disqualifies this setting for studies of institutional cross-ownership. This is related to the fact that Russell reconstitutions affect holdings of mutual funds that track the Russell indices but not of 13F institutions, as previously documented by Schmidt and Fahlenbrach (2017).

To summarize, our main contributions are twofold. First, we show that using either the financial-institution merger or index addition events as a source of identification, there is little robust evidence that common ownership affects firm profitability, investment, mergers, or strategic alliances. This is in spite of the large number of studies arguing that common ownership

has anticompetitive effects. Second, we highlight the channels through which using these instruments can lead to misleading conclusions.

2. Literature overview

The recent concerns about the rising institutional ownership of U.S firms and its potential anticompetitive effects prompted a surge of new empirical research in this area. We list the most recent papers along with some key parameters of each in Table 1. This table highlights the broad set of firm outcomes the literature has examined and the variety of sources of identification used to isolate these effects.

Two initial studies examine the effects of common ownership on prices of airline tickets (Azar, Schmalz, and Tecu, 2018) and banking products (Azar, Raina, and Schmalz, 2019), and both find evidence that common ownership leads to anticompetitive outcomes. However, Dennis, Gerardi and Schenone (2020) and Kennedy, O'Brien, Song, and Waehrer (2017) argue that after accounting for endogeneity of market shares in the measures of industry concentrations there is no evidence that common ownership increases ticket prices.[6]

If common ownership reduces competition, it may decrease firms' incentives to invest. Consistent with this prediction, Gutiérrez and Philippon (2017) find that industries with higher levels of cross-ownership exhibit unusually low investment levels. Alternatively, Lopez and Vives (2019) show that common ownership can promote certain types of investments, specifically those in technologies with positive spillovers on competitors. Based on their framework, Kini, Lee and

[6] O'Brien and Waehrer (2017) discuss more broadly concerns with the methodology in Azar, Schmalz, and Tecu (2018) and Azar, Raina, and Schmalz (2019), including endogeneity. See also the reply in Azar, Schmalz, and Tecu (2018b).

Shen (2019) argue that, in industries with high spillovers, common ownership can encourage innovation and intensify competition. They find supportive evidence for these predictions.

Another set of studies asks whether common ownership increases other types of coordination between connected firms, thus improving their outcomes. He and Huang (2017) find that increases in common ownership by blockholders lead to greater market shares of the connected firms and improvements in performance relative to competitors. They also find increases in the likelihood that the firms merge, enter into a joint venture, or form a strategic alliance. Similarly, Brooks, Chen and Zeng (2018) find increased merger likelihood for firms in which cross-owners own large blocks. However, Harford, Jenter, and Li (2011) point out that, in general, cross-owners' holdings are too small to incentivize them to influence merger outcomes. Several papers find evidence of coordination along other dimensions. For example, Kostovestsky and Manconi (2020) find that increases in cross-ownership between pairs of firms increase the likelihood that those firms cite each other's patents, and Xie and Gerakos (2018) find that cross-owned firms involved in patent litigation are more likely to settle out-of-court. Freeman (2019) provides evidence of tighter customer-supplier relationships among connected firms. However, Koch, Panayides and Thomas (2020) conclude that the link between cross-ownership and profitability or other measures of non-price competition in a broad cross-section of firms is not robust. In contrast to their paper, our focus is on understanding the advantages and shortcomings of the different approaches to identify the effects of common ownership.

Several studies investigate the channel(s) by which common owners could potentially influence firm policy. Literature to date has considered two potential channels: managerial incentives and "doing nothing" (i.e., refraining from pushing for more aggressive competitive

strategies).[7] With respect to managerial incentives, Antón, Ederer, and Schmalz (2018) conclude that common ownership lowers CEOs' wealth-to-performance sensitivity, thus reducing their incentives to compete. However, Liang (2016) and Kwon (2017) examine the effect of common ownership on relative performance evaluation, and using different methodologies, arrive at contradictory conclusions. In a different context, Edmans, Levit and Reilly (2019) and He, Huang and Zhao (2019) examine the effects of cross-ownership on corporate governance. Edmans, Levit, and Reilly (2019) show theoretically that common ownership can improve price informativeness and, indirectly, strengthen governance. He, Huang, and Zhao (2019) argue that common owners have stronger incentives to engage in governance because of positive spillovers on other firms in the industry, and they find that cross-holdings induce institutions to vote against management on governance proposals.

Two points about this stream of literature are worth noting. First, most empirical papers find support for the hypothesis that common ownership affects firms' behavior. Second, the literature has employed a wide variety of methodologies to investigate these effects, and the choices are not uniform across studies. For example, researchers have used in various contexts three types of events to identify exogenous changes in cross-ownership. Following Berger's (2019) criticism of mutual fund flows as a source of identification, our focus in this paper is on the remaining two events: mergers of financial-institutions (both a broad sample of mergers and just the Blackrock-BGI merger of 2009) and index reconstitutions (both S&P500 and Russell 1000/2000).

[7] See Hemphill and Kahan (2020) and Elhauge (2020) for a broader discussion of channels by which common ownership potentially influences firm outcomes.

3. Data

Our primary source of data on institutional holdings is Refinitiv (formerly known as Thomson Reuters). For the earlier portion of our sample, the 1980 to March 2013 period, we obtain data from the Refinitiv 13F Institutional Holdings dataset. Following information on the WRDS website regarding problems with the more recent years of this dataset, we rely on the WRDS SEC Analytics Suite – 13F Holdings dataset for the June 2013 to 2015 period.[8]

We clean these data along several dimensions. First, we identify the ten largest institutions each year based on total assets under management listed in the 13F data, where institution represents the level at which institutional holdings are recorded. For each of these institutions, we ensure that there are no missing quarters, i.e., no quarters in which the institution would plausibly be expected to own shares but there is no record in the data. For all quarters that we identify as missing, we search through the raw 13F data as provided on EDGAR, and we fill in any missing data.[9] Second, for each of these ten large institutions, we verify that holdings are consistent between the Refinitiv Institutional 13F Holdings dataset (covering the 1980 to March of 2013 period) and the WRDS SEC Analytics Suite – 13F Holdings dataset (covering the June of 2013 to 2015 period), ensuring that there is no sudden and unexplained change in holdings between the end of the Refinitiv data and the beginning of the WRDS data. We reconcile any differences using raw 13F data as provided on EDGAR. Third, following Ben-David, Franzoni, Moussawi, and Sedunov (2018) we aggregate the holdings of Blackrock, which are listed under six different manager numbers, into a single entity. Fourth, given the importance of obtaining accurate holdings

[8] See WRDS for more information: https://wrds-www.wharton.upenn.edu/pages/support/research-wrds/research-guides/research-note-regarding-thomson-reuters-ownership-data-issues/ .

[9] As discussed in Lowry, Rossi, and Zhu (2019), EDGAR only contains 13F filings for 1999 and later, thus restricting this process to this period.

around the time of financial institution mergers (described in Section 5.1), we manually verify holding data during these periods.[10]

In addition to ownership data, we obtain information on mergers, joint ventures, and strategic alliances from the Securities Data Company (SDC) database of Refinitiv (formerly Thomson Reuters). Stock return data and information on S&P500 additions comes from CRSP. We use financial statement information from Compustat, and information on Russell index reconstitutions from FactSet. Samples used for the financial-institution merger analyses are described in Section 5.1, and samples used for the index addition analyses are discussed in Sections 6.1 and 6.2.

4. Measurement of cross-ownership

Conceptually, cross-ownership represents the extent to which shareholders own multiple firms within an industry. The measurement of this begins at the firm-pair level, i.e., between each pair of rival firms. We form a product of a shareholder's stakes in the two firms and aggregate the products across all common shareholders:

$$\textit{Pair-level C-Index}_{j,k} = \sum_{i=1}^{N} \mu_{i,j} * \mu_{i,k} \quad (1)$$

where $\mu_{i,j}$ ($\mu_{i,k}$) equals the ownership percentage of investor i in firm j (firm k). While an alternative would be to use an arithmetic average instead of a product, for example $\sum_{i=1}^{N}(\mu_{i,j} + \mu_{i,k})/2$, we choose a product-based measure as a baseline to account for symmetry between $\mu_{i,j}$ and $\mu_{i,k}$., i.e., the extent to which a shareholder owns a similar stake in both firms

[10] This process led to corrections in the manager numbers and/or holdings of Fidelity, Invesco, Mellon Bank, Capital Research, Barclays, and Bank of America.

(this approach is similar to Gilje, Gormley, and Levit (2020) and Lewellen and Lewellen (2019)).[11] We compute this pair-level measure using both all shareholdings and just blockholdings.

This firm-pair measure can be aggregated across all of a firm's rivals to form a firm-level measure:

$$\textit{Firm-level C-Index}_j = \sum_{k=1}^{K} \sum_{i=1}^{N} w_k * \mu_{i,j} * \mu_{i,k} \quad (2)$$

where w_k represents the weight of each rival firm k, and μ_{ij} and μ_{ik} represent investor i's ownership percentages in each firm. In our main empirical analyses, we use either equal-weighted or value-weighted measures, weighting by firm k's share of total industry market capitalization.[12] Value weighting accounts for the fact that the same fractional ownership of the owner of firm j in a larger competitor k translates into a larger dollar stake in that competitor, and thus, stronger incentives to consider k's interests.

Our analysis focuses on the firm-pair and firm measures. These measures can be further aggregated to obtain industry-level measures, as used in other studies. For example, aggregating the firm-level measure across all firms in an industry yields:

$$\textit{Industry-level C-Index} = \sum_{j=1}^{J} \sum_{k \neq j}^{K} \sum_{i=1}^{N} w_j * w_k * \mu_{i,j} * \mu_{i,k} \quad (3)$$

[11] In the case where investors 1 and 2 each own 50% of the firm, a geometric average will be equivalent to an arithmetic average, with average overlap equaling 0.5. However, in a case where investor 1 owns 90% and investor 2 owns 10%, the arithmetic average overlap again equals 0.5, whereas the geometric overlap equals a lower 0.09. Thus, the product-based measure will be higher when a shareholder's ownership stakes are more symmetrically divided among the two firms. Gilje, Gormley, and Levit (2020) use a similar approach, except that they adjust the pair-level products with a measure of the institution's attention. The approach is also analogous to that in Lewellen and Lewellen (2019) except that they focus on industry level measures. In section 5.3, we report tests based on the arithmetic-average measures.

[12] While we could alternatively weight by the firm's sales, we find that the correlation between the firm-level C-Index computed using market values versus sales as weights is close to 97%, and market values have the advantage of being available more reliably on a quarterly basis. As discussed later, we find similar patterns in cross-ownership for the sales-based measures.

Deflating this expression by the squared holdings of manager i in firm j provides a measure that is analogous to the Modified Hirschmann-Herfindahl Index (MHHI) Delta developed by O'Brien and Salop (2000) and employed by Azar, Schmalz, and Tecu (2018).[13]

$$MHHI\ Delta = \sum_{j=1}^{J} \sum_{k \neq j}^{K} \frac{\sum_{i=1}^{N} w_j * w_k * \mu_{i,j} * \mu_{i,k}}{\sum_{i}^{N} \mu_{i,j} * \mu_{i,j}} \quad (4)$$

Intuitively, the denominator in equation (4) measures ownership concentration across all shareholders of firm j while the numerator measures the extent of their cross-ownership in the firm's competitors. Roughly speaking, the ratio captures the relative weight that the more powerful shareholders of firm j put on the interests of firm j's competitors versus the firm itself.

5. Evidence on cross-ownership using financial-institutions mergers

5.1. Sample

We form a sample of financial institution mergers broadly following the criteria outlined in He and Huang (2017), with several modifications. First, we download from the SDC mergers and acquisitions database all mergers for which the announcement date is between 1980 and 2015, the target firm is incorporated in the U.S., the acquirer and target primary SIC codes are between 6000 – 6999, and firm names are provided for both the target and acquirer firm. For each target firm and each acquirer firm across these deals, we use text matching algorithms to match to firm names provided in the 13F data. We further impose the requirement that either the target firm stops filing 13F statements within 15 months of the completion date of the merger or that the target's assets under management decline by more than 80% from quarter -6 to quarter 6 relative to the

[13] The difference is that O'Brien and Salop (2000) use sales-based market shares as weights instead of w_j and w_k, and they allow for cash flow rights of firm j's shareholders to differ from their control rights. Finally, we can express the MHHI as *MHHI = HHI + MHHI Delta.*

completion quarter. Throughout, we are careful to account for the fact that manager numbers in the Refinitiv 13F data are recycled, and we manually check each merger to ensure accuracy. This process generates a sample of 248 financial institution mergers, 64 of which meet our criteria for the selection of treatment firms (described below). The final list of 64 financial-institution mergers is provided in Appendix Table A1.

We construct both treatment and control samples around the financial institution mergers following the procedure in He and Huang (2017).[14] Fig. A1 in the Appendix provides an illustration. To construct the treatment sample, we identify firms that are likely to become cross-owned as a result of the financial-institution merger. The selection procedure follows two steps. First, we identify all firms in which one of the merger partners holds a block of 5% or more in the quarter prior to the merger announcement.[15] This list consists of 7,100 event-firms (41 event-firms in which both merger partners hold a block are deleted). In the remainder of the paper, we refer to "event-firms" as "firms". Second, we form firm pairs (Firm1, Firm2), where both firms are from the same 3-digit SIC industry, Firm1 is block-held by one merger partner, and Firm2 is block-held by the other merger partner. This process yields 2,492 firm pairs (1,246 firm combinations) formed from 1,048 firms (i.e., event-firms). Third, we require that ownership data are available in the quarter prior to the effective date of the merger and that the firm is on Compustat in the fiscal year of the merger. The resulting sample of treatment firms consists of 1,894 pairs (947 firm combinations), across 934 firms.

[14] Kini, Lee, and Shen (2019) use this same procedure, and many other papers (see, e.g., Table 1) use related methodologies.

[15] We use the announcement date (rather than the completion date) to ensure that we define the treatment and control samples using only ex ante information.

We construct control samples at the firm level for the 934 treatment firms and at the firm pair level for the 1,894 treatment pairs. For the firm-level control sample, we select all firms that are block-held by one of the merger partners prior to the merger but are not included in the treatment sample. This means that these control firms are from different industries than the treatment firms (i.e., unaffected industries, defined as industries in which the other merger partner holds no blocks). We refer to this sample as Control Firms^{DI} for "different industry". After requiring that ownership data are available in the quarter before the effective date of the merger and that the firm is on Compustat in the fiscal year of the merger, Control Firms^{DI} consists of 3,249 firms. Descriptive statistics for the treatment and control samples are in Table 2, and they are discussed below. Finally, for regressions in Tables 4 and 5, we impose the additional requirement that the firm is on Compustat in fiscal years -3 to +3 around the merger.

To construct the Control Pairs sample, for each treatment pair (Firm1, Firm2), we form a control pair (Firm1, Firm2'), where Firm2' is matched to Firm2 (from the treatment pair) based on industry and market capitalization. Specifically, we select a matched control firm in the quarter prior to the merger that (1) is in the same 3-digit SIC industry; (2) is block-held by a financial institution other than the merging institutions; and (3) is closest in market capitalization to the treatment firm. The Control Pairs sample consists of 1,956 firm pairs, half of which represent cases in which Firm1 is held by the target and half of which represent cases in which Firm1 is held by the acquirer.

5.2. Identification challenges

As discussed above, the treatment sample consists of firms that are likely to become cross-owned by blockholders as a result of the financial-institution mergers. A key identifying assumption is that the financial-institution mergers are exogenous to the firms themselves, i.e.,

they were not caused by the firms' investment or product-market strategies. At least in the case of the Blackrock-BGI merger, this appears to have been the case, as discussed, for example, in Azar, Schmalz, and Tecu (2018). Analogously, He and Huang (2017) argue that this requirement was similarly satisfied for other mergers.

One potential concern is that the occurrence of the financial-institution mergers was correlated with broader trends in the affected industries. To evaluate this, it is worthwhile to examine the distribution of the merger events over time. Looking first at Panel A of Fig. 1, the incidence of mergers is distributed fairly evenly across the sample period from 1983 through 2010 (with a somewhat higher frequency during the 1990s).[16] However, as shown in Panel B, the number of the treated firms is concentrated in 2009, the year of the Blackrock-BGI merger. Approximately 40% of the firms (414 of the 1048 firms) are associated with the Blackrock-BGI merger.[17] Moreover, an additional 201 treatment firms are associated with mergers occurring in 2008. This concentration of firms reflects the fact that the 2008-2009 mergers involved much larger institutions as measured by their Assets Under Management, as illustrated in Panel C.[18] This concentration is particularly problematic because these events coincided with the financial crisis, so any differential effects on treatment versus control firms could be contaminated by how these firms responded to the crisis. The fact that the treatment and Control Firms^{DI} samples come from different industries (by definition) increases this concern.

[16] While our process of identifying financial-institution mergers covers the 1980 – 2015 period, we find no events that satisfy our criteria after 2010.
[17] As illustrated in Fig. A1 in the Internet Appendix, the largest two mergers account for 47% of firms (65% of firm pairs), and the largest six mergers account for 69% of firms (80% of firm pairs).
[18] The fact that the size of the merging institutions in our sample increased over time is consistent with the growing importance of large institutional shareholders more broadly as documented, for example, in Ben-David, Franzoni, Moussawi, and Sedunov (2018) and Lewellen and Lewellen (2019).

Table 2 shows the financial characteristics of the treatment sample and of Control Firms^{DI} in the fiscal year prior to the financial institution merger completion date. As described in detail in Section 5.1 and illustrated in Fig. A1 in the Appendix, the treatment sample consists of firms in which (1) one merger partner holds a block and (2) the other merger partner holds a block in at least one of its industry peers. Control Firms^{DI} satisfy the first but not the second criterion, which is what causes them to be drawn from different industries than the treatment firms. The table highlights a challenge with selecting the control sample this way: while Control Firms^{DI} have similar market capitalization to the treatment firms, they have substantially lower R&D ratios (0.02 vs. 0.07), higher book-to-market ratios (0.81 vs. 0.66) and higher ratios of PPE to total assets (0.39 vs. 0.30). If growth firms responded differently to the financial crisis than more mature firms, this discrepancy could bias any analyses based on a comparison between these samples. We also find that firms in Control Firms^{DI} have substantially higher market shares than Treatment Firms (0.22 vs. 0.09). This is likely hardwired since a smaller number of industry peers makes it less likely that the merger partner holds a block in the peers.

5.3. Cross-ownership changes around financial-institution mergers

To identify the effects of cross-ownership on firm fundamentals, financial institution mergers must cause significant increases in cross-ownership for treated (but not control) firms. We begin by illustrating the changes in ownership around the mergers in Figs. 2 and 3, which show the mean indices in the quarters surrounding the events (corresponding tests are in Table 4). Table 3 describes the distribution of the cross-ownership indices prior to the merger events.

Looking first at Fig. 2, Panel A includes all shareholdings in the pair-level C-Index, and Panel B includes only blockholdings. In both cases, the treatment pairs exhibit substantial and sudden increases in the C-Index in the event quarter, compared to little change among the control

pairs. Using all stakes, the C-index (as calculated from eq. (1)) for the average treatment firm pair jumps by close to 200 points (from 304 to 482) in quarter zero and remains above 400 throughout quarter 5. Using just blockholdings, the magnitude of the increase is similar in absolute terms (slightly less than 200 points) but much larger in percentage terms. The C-Index increases by a factor of four, from 52 to 217, though it declines slightly to 153 by quarter five. To put these magnitudes in perspective, two cross-holders, each holding 5% stakes in each firm would result in an index of 50 (=25+25), so an increase of 150 would be equivalent to adding a third cross-holder with 12.25% stakes in each firm (150=12.25*12.25).[19] For the median firm, the increase is smaller and corresponds to an additional cross-holder with two 6% stakes. The smaller magnitudes for the median firm are consistent with the skewed distribution of the C-Indices, as shown in Table 3.

Panel A of Table 4 shows a series of regressions that test the statistical significance of the increases in the pair-level C-Index around the merger events. The sample consists of both the treatment pairs and the control pairs in each of the (-5, +5) quarters around the financial institution merger completion date. We estimate difference-in-difference regressions, where the dependent variable is the C-Index of each firm pair. The independent variable of interest is *Treat*×*After*, where Treat is a dummy equal to one if the pair belongs to the treatment sample and *After* is a dummy equal to one if the quarter end follows the merger completion date. The regressions include firm-merger and calendar quarter fixed effects as well as the *After* dummy (not tabulated). The left-hand columns include the full sample, the middle columns the Blackrock-BGI sub-sample, and the right-hand columns the "All but 2008 and 2009" sub-sample. Results indicate that the

[19] In Appendix Fig. A2, we plot these patterns after excluding mergers occurring in 2008-2009. We find similar magnitudes for all stakes and smaller though still sizable magnitudes for blocks. Appendix Fig. A3 shows analogous patterns for the firm-level C-Index, discussed below.

patterns observed in Fig. 2 are highly statistically significant: the interaction term is positive and significant at the 1% level in all samples.

Fig. 3 shows the firm-level C-Index around the merger events, for the treatment firms as well as Control Firms^{DI}. To construct the firm-level C-Index, for each firm, we average the pair-level indices (defined using all shareholdings) across all of the firm's competitors, either equal-weighting (Panel A) or value-weighting (Panel B) them (see details in Section 4). The figure shows a steady increase in the firm-level indices throughout the event horizon, but also a larger increase in quarter zero for the treatment firms. Not surprisingly, the firm-level effects are smaller than the pair-level effects in Fig. 2 because the firms' cross-ownership with some (or most) of their industry peers (those not held by the merger partner) is unaffected by the mergers. Nevertheless, regressions in Panel B of Table 4 show statistically significant increases of between 2.33 points and 3.92 points in the full sample, relative to Control Firms^{DI}. This compares to mean levels in quarter -1 of 47.1 points (equal-weighted) and 61.5 points (value-weighted). Across the 'Blackrock-BGI' and the 'All but 2008 and 2009' subsamples, most of the regressions show similar effects, with three of the four coefficients being statistically significant.[20] In Tables A1 and A2 in the Internet Appendix, we show that results are similar when we compute the firm-level C-Index weighting industry rivals using their sales rather than market capitalization, and computing the pair-level C-Index based on an arithmetic average rather than a geometric average, respectively. Panels A and B of Table A3 shows that results are also similar using a tighter winsorization level, or using a trimming instead of winsorizing.

[20] It is potentially informative to evaluate the significance of *Treat×After* in Table 4 relative to common benchmarks in a typical first-stage regression (which would be an alternative to the difference-in-difference framework used here). In Panel A of Table 4, t-statistics range from 3.3 to 10, which corresponds to F-statistics between 11.2 and 99.5, well above the common criteria of 10. In Panel B, five out of the six coefficients are statistically significant with the associated F-statistics between 11.9 and 40.0.

Overall, these results show that the merger events caused significant shifts in cross-ownership, particularly for pairs of firms held by the two merger partners. The effects on cross-ownership at the firm level are significant but smaller in magnitude. It follows that effects at the industry level would be commensurately smaller. This suggests that the financial institution merger setting is more likely to detect effects working through pair-level channels (such as cooperation that potentially increases firm-level profits or leads to mergers between firms) rather than industry-wide shifts in competition.

5.4. The effects of cross-ownership on firm choices

5.4.1. Baseline Results

The literature has identified multiple channels through which cross-ownership could affect firm choices, and thus financial performance (see overview in Section 2). To explore these effects, Table 5 examines measures of profitability and investment for the treatment and control firms during fiscal years -3 through 3 around the financial-institution mergers. We define profitability as operating income before depreciation divided by assets, which we refer to henceforth as ROA, and we use R&D divided by assets as a measure of investment. Section 5.4.4 discusses other measures. Descriptive statistics on the full panel dataset used in regressions is provided in Panel A of Table A4 in the Internet Appendix. All variables are winsorized at 1%.

The regressions in the left panel of Table 5 verify the results from prior literature. Based on column 1, there is a significant improvement in operating performance following the merger-induced increase in common ownership. The coefficient on *Treat*×*After* shows an increase in ROA by 1.2 percentage points (p-value<0.1). Similarly, based on column 2, there is a significant decline in R&D by 0.4% of assets (p-value<0.05).

The middle panel reports similar regressions, but the sample is restricted to the Blackrock-BGI merger as the identifying event. Strikingly, we find that the coefficient on *Treat*×*After* in the ROA regression doubles relative to the left panel and remains significant in spite of a smaller sample. There is also a large and significant effect on R&D. The Blackrock-BGI merger occurred in 2009, so the *After* period coincides with the years following the financial crisis. This raises the concern that the estimated effects are influenced by the firms' responses to the crisis. For example, if high-growth industries suffered smaller declines in profitability as a result of the crisis or experienced a stronger rebound, the fact that the treated sample consists disproportionately of such industries (as shown in Table 2) would induce an upward bias. Full sample results are also potentially affected by this issue, because as discussed earlier, even within this larger sample the treatment firms are disproportionately concentrated in 2008 and 2009.

To address this concern, in the right panel we restrict the sample to mergers outside of the 2008 to 2009 period. Consistent with the conjecture that the crisis period is influential, the coefficient on *Treat*×*After* flips sign and becomes insignificant in both the ROA and the R&D regressions. The 95% confidence intervals around these coefficients highlight the contrast with the full sample results. From column 5, we can reject with 95% confidence that the mergers increased ROA by more than 1.4 percentage points, i.e., by a magnitude similar to the point estimate in column 1 (1.2 percentage points). Similarly, column 6 indicates that we can reject that they reduced R&D by more than 0.5 percentage points (which is similar to the -0.004 point estimate in column 2). To ensure that results are not driven by outliers, Table A3, Panel C in the Internet Appendix re-estimates our main regressions using a tighter winsorization level or using trimming instead of winsorizing. The results are qualitatively similar.

Fig. 4 provides additional insights into the contradictory results. Panel A shows coefficients on *Treat×Event Year* from a regression of ROA similar to that in columns 3 and 5 of Table 5, with the exception that the *After* dummy is replaced by indicators for event years, such that the regression now includes *Treat×Year-3, Treat×Year-2, …, Treat×Year+3*. The left figure, in which the sample is restricted to the Blackrock-BGI merger, shows evidence of differential trends in ROA prior to the merger event, suggesting a violation of the parallel trends assumption (see also tests in Table A5 in the Internet Appendix). Specifically, it appears that the treatment firms became increasingly more profitable relative to the control firms in the years leading up to the merger, and that their relative performance continued to improve subsequently. Since the merger occurred in 2009, these patterns could reflect the differential effects of the financial crisis across the two samples.[21] To control for this, the right panel uses mergers outside of the 2008-2009 period. The contrast is striking. After removing mergers that occurred around the time of the financial crisis, we find no evidence that treatment firms' post-event profitability improved relative to control firms. This is consistent with the results in Table 5.

Panel B of Fig. 4 yields similar conclusions. We plot average ROA for each sample (Treatment and Control Firms^{DI}) over the same event period, years -3 to +3 around the financial institution mergers. Profitability of treatment firms increased relative to control firms following the Blackrock-BGI merger that occurred in 2009. However, after excluding mergers that occurred around the crisis, we find no evidence of a post-merger improvement.

[21] Fig. 1 shows that outside of the 2008-2009 period, the merger events are distributed more evenly across the sample period, with the next largest spike in year 2000. We find no evidence of significant pre-trends leading up to this event, however we do observe some evidence that the treatment firms' performance *declined* after the mergers, which is contrary to the hypothesized effect of cross-ownership. This could be caused by higher R&D firms doing worse after the Internet bubble burst.

The overall conclusion from Fig. 4 and Table 5 is that tests based disproportionately on the Blackrock-BGI merger (or other mergers during the 2008-2009 period) can mistakenly attribute the effects of the financial crisis to the effects of cross-ownership, particularly when treatment and control samples are not closely matched. We propose alternative approaches to construct control samples in Section 5.4.3.

5.4.2. Industry effects behind the spurious results

This subsection takes a closer look at the economic forces behind the differential effects of the Blackrock-BGI merger on the Treatment Firms versus Control Firms^{DI}. Specifically, we examine the extent to which firms that are similar along various dimensions to Treatment firms, but which were not actually treated (i.e., did not become common owned as a result of a financial institution merger), experienced changes in profitability or investment. As shown in Table 2, Treatment Firms exhibit characteristics of higher-growth firms relative to Control Firms^{DI}. For example, they have higher R&D (mean of 0.07 vs. 0.02) and lower B/M (mean of 0.66 vs. 0.81). This is also reflected in the top five industries most strongly represented in each sample (listed in Appendix Table A6). Treatment Firms come disproportionally from high-growth industries such as drugs (SIC 283, representing 15.0% of treatment sample) and computer & data processing services (SIC 737, 11.0% of sample). In contrast, the two most common industries in Control Firms^{DI} include commercial banks (SIC 602, 9.4% of sample) and electronic components & accessories (SIC 367, 3.1% of sample).[22]

Fig. 5 illustrates how these differences affect the cross-ownership tests in Table 5. We plot average ROA over the years prior to and following the merger year, across firms with positive

[22] The two most heavily represented industries in the treatment sample, SIC codes 283 and 737, have B/M ratios of 0.34 and 0.52, respectively. In contrast, the two most heavily represented industries in the control sample, SIC codes 602 and 367, have BM ratios of 0.93 and 0.65, respectively.

R&D (more similar to the treatment sample) versus those with zero R&D (more similar to the control sample). The samples are drawn from Control Firms^{DI}, and Treatment Firms are excluded. Thus, any differences in outcomes are unlikely due to the effects of cross-ownership.

The figure shows that the two sets of firms experienced dramatically different outcomes, in the years surrounding the financial crisis: profitability of 'control-type' firms with zero R&D decreased following the crisis, whereas profitability of 'treatment-type' firms with positive R&D increased. These patterns provide more direct evidence on the factors underlying the results in Table 5.

5.4.3. Effects of common ownership based on alternative control samples

The previous sections show that the effects of common ownership documented in prior literature are not present for mergers outside of the 2008-2009 period. In this section, we offer an alternative test that uses the full set of mergers but constructs control samples that are more closely matched to the treatment firms. Selecting control firms in this way helps account for the effects of the financial crisis on the sample firms.

The first set of control firms comes from the same industries as the treatment firms and is denoted Control Firms^{SI} for "same industries". For each treatment firm, we select a matched control firm in the quarter prior to the merger that (1) is in the same 3-digit SIC industry; (2) is block-held by a financial institution other than the merging institutions; and (3) is closest in market capitalization to the treatment firm. Appendix Fig. A1 shows an illustration. After requiring that ownership data are available in the quarter before the effective date of the merger and that the firm is on Compustat in the fiscal year of the merger, Control Firms^{SI} consists of 941 firms. Table A7 in the Internet Appendix shows that Control Firms^{SI} are similar to Treatment Firms based on most

characteristics (for example, mean R&D are 0.07 and 0.08 and mean B/M are 0.66 and 0.63). Exceptions include a somewhat lower institutional ownership for Control Firms^{SI}.

Panel A of Table 6 shows regressions similar to those in Table 5, but Treatment Firms are compared to Control Firms^{SI}, instead of Control Firms^{DI}. Results are starkly different. Here, the differential effects between the samples are statistically insignificant and, in most cases, have opposite signs to those predicted by the main hypothesis. This is irrespective of whether the sample includes all financial institution mergers, just the Blackrock-BGI merger, or the mergers outside the 2008-2009 period. Based on column 1, we can reject with 95% confidence that common ownership increased ROA for treatment firms by more than 0.9 percentage points.[23]

One potential concern with Control Sample^{SI} is that financial-institution mergers might have caused industry-wide improvements in operating performance (or declines in R&D), which the regressions in Panel A cannot detect. We therefore construct an alternative control sample, drawn from different industries than treatment firms but matched on both size and R&D. We refer to this sample as Control Firms^{DIM}. Specifically, for each treatment firm we select a matched control firm in the quarter prior to the merger that: (1) is drawn from industries that are not affected by the merger; (2) is block-held by a financial institution; and (3) is matched with the treatment firm on market capitalization and R&D rank.[24] Table A7 in the Internet Appendix shows that this control sample is matched closely with Treatment Firms on all key characteristics, including R&D, ROA,

[23] Table A8 in the Internet Appendix presents a different way of controlling for industry effects. In Panel A, we re-estimate the Table 5 regressions (where we draw control firms from different industries than treatment firms), but include industry-year fixed effects. We again find that the coefficients on *Treat×After* switch signs relative to those in Table 5 and become statistically insignificant. In Panel B, we cluster standard errors by industry and year (instead of firm and year) and obtain somewhat weaker significance compared to Table 5.

[24] The R&D ranks are set to 1, 2, or 3 if R&D equals 0, is between 0 and 0.05, and is greater than 0.05. The 0.05 cutoff was chose as it is close to the sample median for Compustat firms with positive R&D.

and institutional ownership (also see Table A4 in the Internet Appendix for descriptive statistics on the full panels).

Results using Controls Firms^{DIM} are presented in Panel B of Table 6. They are qualitatively similar to those using Control Firms^{SI}, as reported in Panel A. We continue to find no evidence that common ownership caused either the profitability of Treatment Firms to increase or the R&D investments of these firms to decrease.

The lack of effects for both control samples is illustrated in Fig. 6. The figure shows the patterns in average ROA around the merger events using all mergers. The top panel shows Treatment Firms and Control Firms^{SI}, and the bottom panel shows Treatment Firms and Control Firms^{DIM}. In both panels, ROA traces similar patterns around merger events for all samples, and there is no evidence of differential responses to the events.

Overall, the results in this section reinforce our earlier findings that tests based on the Blackrock-BGI event (or on a broader sample of events in which a disproportionate share of treatment firms come from years around the financial crisis) can lead to misleading conclusions. As shown in section 5.4.2, significant differences in firm type across Treatment Firms and Control Firms^{DI} could account for this bias. When control samples are more appropriately matched, using either Control Firms^{SI} or Control Firms^{DIM}, we find no significant effects of common ownership on either firm profitability or firm R&D.

5.4.4. Effects of common ownership on alternative outcome variables

We focus to this point on profitability and R&D as two commonly examined outcome variables. This section considers alternative financial outcomes that have been used in broad studies of cross-ownership, and for which researchers found some evidence of a response to cross-

ownership. Table 7 reports regressions using operating margin, change in market share, R&D plus capital expenditures scaled by total assets, and cash holdings scaled by assets as dependent variables. Market share is defined as the ratio of the firm's sales to total industry sales, and the change is measured relative to the previous fiscal year.[25] In addition, we include regressions using an alternative measure of ROA based on operating income after depreciation.[26]

Panels A – C show our baseline regressions using Control Firms^{DI}, analogous to those in Table 5. Panel A, which uses the full sample of mergers, shows significant increases in *ROA(After Depr.)*, *Margin*, and *ΔMarket Share*, and a significant decline in *Cash*, consistent with the hypothesized effects of cross-ownership. The effect on *R&D+CapEx* is negative but not statistically significant. Similar to prior results, many of these effects appear to be influenced by the Blackrock-BGI merger sub-sample (Panel B). When mergers occurring during 2008-2009 are excluded (Panel C), the coefficients on *Treat×After* are statistically insignificant and often switch signs. Similarly, estimates are close to zero when we use Control Firms^{SI} (Panel D) or Control Firms^{DIM} (not tabulated for brevity). In the case of *ΔMarket Share*, the regressions using Control Firms^{SI} are especially informative as they test directly whether treatment firms increased market shares after the events *relative to their industry peers*. The evidence in Panel D suggests that this was not the case.

[25] To minimize the influence of outliers, we exclude firms whose industries experience a change in the number of firms of 50% or more (in absolute value) in a single year during the event period. We find that such shifts are often caused by SIC reclassifications, and that they can distort the calculation of changes in market shares.

[26] These variables have been used, for example, in He and Huang (2017), Gutierrez and Philippon (2017), Bindal (2019), and Semov (2017). Similar effects have been tested in industry specific settings, for example, in Azar, Schmalz, and Tecu (2018), Azar, Raina, and Schmalz (2019), and Torshizi and Clapp (2019). In section 5.4.6, we also investigate the incidence of mergers, joint ventures, and strategic alliances, which have been examined in He and Huang (2017) and Brooks, Chen, and Zeng (2018). Finally, we examined asset growth, stock returns, and industry-adjusted profitability and failed to find any evidence of the effects of cross-ownership, consistent with the results reported throughout the paper. These results are not tabulated for brevity.

The insignificant results in Panels C and D do not appear to be caused by insufficient power. For example, based on Panel D, we can reject with 95% confidence that the merger events caused an increase in profit margin by 1.7%. This compares to the 3.4% effect estimated without controlling for industry effects (Panel A). Similarly, we can reject with 95% confidence a -2.0% effect on *Cash* and an 0.2% effect on *ΔMarket Share*. Overall, the results in Table 7 suggest that the biases we document for our baseline measures of profitability and R&D could affect tests based on other commonly used financial outcomes.

5.4.5. Do lower common ownership thresholds lead to similar outcomes?

Previous analysis constructs the treatment sample using a 5% threshold for blockholdings. This relatively high ownership cutoff allows us to examine shifts in cross-ownership by large blockholders who have the potential to influence corporate policies. In this section, we examine instead smaller shareholders with positions of 1% to 2% and 0.5% to 1%. These shareholders are less likely to have sufficient power (or incentives) to intervene in corporate decision making. We show however, that firms classified as "treated" based on these lower ownership cutoffs also experience performance improvements around the Blackrock-BGI event. This finding casts further doubt on the interpretation that these shifts in firm outcomes are the result of cross-owners' pressure.

The construction of the alternative treatment and control samples is identical to that for the main analysis, as described in Section 5.1, except that we now use the lower cutoffs to define a potential cross-holder. Details of the sample construction, as well as descriptive statistics on the two samples are provided in Table A9 in the Internet Appendix. In Table A10 in the Internet Appendix we show that, as expected, the treatment firms experience no significant increases in

firm-level cross-ownership around the merger events relative to control firms.[27] A notable difference to the main analysis is that, because of the lower ownership cutoffs, a larger number of financial institution mergers "qualify" as identifying events (that is, there are a larger number of cases in which the merger partners own 1% to 2 %, or 0.5% to 1% of two industry rivals, compared to the 5% minimum threshold in our main sample). The number of events for the 1%-2% ownership cutoff is 103, and it is 127 for the 0.5% -1% cutoff, compared to 64 events in the main sample. As a result, the Blackrock-BGI event represents a smaller proportion of the overall sample and has less influence on the full-sample results.

Table 8 shows regressions similar to those in Tables 5 and 7 in the main analysis, except that we are now using the new Treatment Firms and Control Firms^{DI} constructed using the 1%-2% ownership cutoff (Panel A) or the 0.5%-1% ownership cutoff (Panel B). As in the main analysis, we find that treatment firms experience improvements in *ROA*, *Margin* and *ΔMarket Share* around the Blackrock-BGI event (middle panel). These effects are similar in magnitude to those reported in Tables 5 and 7 and are significant at the 10% level. The coefficient on *R&D* is negative but is statistically not significant. The operating improvements are again concentrated in the Blackrock-BGI sample: when the 2008-2009 period is excluded (the right panel), the effects are insignificant and close to zero (except for a small but statistically significant increase in *ΔMarket Share* when we use the 0.5%-1% cutoff for cross-ownership).

Because the cross-ownership increases examined in this section involve, by construction, smaller ownership stakes (and cross-ownership does not significantly increase, as shown in Table A10), it is unlikely that they cause the operating improvements we document in Table 8. Such an

[27] In fact, for mergers outside the 2008-2009 period, we observe small but statistically significant declines in cross-ownership relative to control firms.

explanation would require that the smaller shareholders exerted significant pressure on firms to compete less (or cooperate more) with rivals. The fact that the effects are generally not present outside of the 2008-2009 period makes it more likely that they reflect confounding effects of the financial crisis.[28]

5.4.6. The effects on mergers, joint ventures, and strategic alliances

This section focuses on the possibility that the effects of cross-ownership can be better observed at the firm-pair level. As previously shown in Figs. 2 and 3, the magnitude of the change in cross-ownership is much greater at this level. Moreover, this enables us to focus on two explicit channels of cooperation: mergers and strategic alliances / joint ventures. The hypothesis put forward in prior literature is that an increase in cross-ownership between a pair of firms increases the likelihood of these events for that pair of firms.

We download from the SDC Mergers and Acquisition database all mergers, joint ventures, and strategic alliances between publicly traded firms, from 1980 to 2015. This represents a total of 6,609 mergers and 13,721 joint ventures / strategic alliances. To include the broadest sample of firms, we relax the requirement that firms have Compustat data, thereby increasing our treatment sample to 1,048 firms. Panel A of Table 9 provides, as a baseline, descriptive statistics for the overall frequencies of these events for the Treatment Firms. Across the 1,048 treatment firms, 28% engage in one of these events over the following three years. For the event to have been influenced by common ownership, it must be the case that the two firms belong to the same industry. After instituting this requirement, the percent decreases to 15%. Finally, for the event to be driven by cooperation, it must be the case that it occurs with a specific firm, i.e., with a firm that is owned

[28] This conjecture is supported by the fact that, based on Table A9 in the Internet Appendix, the Treatment Firms exhibit higher R&D levels before the merger events than the control firms. We show in Section 5.4.2 that higher-R&D firms did relatively better after the financial crisis in the main sample, even after excluding treatment firms.

by the partner financial institution (in the case of treatment firms). Clearly, this should further decrease the frequency substantially, and Panel B shows that this is the case.

In Panel B, we report the frequencies of mergers and joint ventures for both the Treatment Pairs and Control Pairs. The first observation worth noting is that the probability that a given pair of industry peers experiences any of the three events is extremely low. Focusing on the Control Pairs as a baseline, we find that only 0.08% of Control Pairs experience an event during the three years after the financial-institution merger. This corresponds to two events out of 2,448 Control Pairs. Similarly, we observe only two events prior to the financial-institution merger (not tabulated). These low frequencies make it difficult to conduct meaningful statistical analysis.[29]

The second observation from Panel B is that the frequency of events is similarly low for the Treatment Pairs, 0.08%.[30] Based on this comparison, there is no evidence that the treatment firms are more likely to engage in cooperation with those industry peers with which they have recently become connected via common owners (vs. with their other matched industry peers). These results put into question the conclusions in the recent studies that common-ownership causes the connected firms to cooperate more strongly with each other, in particular via mergers, joint ventures, or strategic alliances.

6. Evidence on cross-ownership using index additions

This section explores two alternative settings used in the literature to identify exogenous changes in cross-ownership: additions to the S&P500 index and Russell index reconstitutions. To

[29] The analysis is further complicated by survivorship biases stemming from the fact that one of the merging firms by definition delists after the merger. While joint ventures would have the advantage of not being subject to such issues, sample sizes prohibit us from conducting an analysis on these events. We observe no joint ventures for the control sample prior to the event, and only one for the treatment sample.

[30] As explained in Section 5.1 and Fig. A1 in the Appendix, (Firm1, Firm2) and (Firm2, Firm1) constitute two pairs. These two treatment pairs correspond to control pairs (Firm1, Firm2') and (Firm2, Firm1').

briefly preview our findings, neither of these settings appears to be suitable for studies of cross-ownership. We highlight the reasons below.

6.1. Additions to S&P500

To form the sample of index additions, we obtain from CRSP the date on which any firm either joined or exited the S&P500 Index, over the 1980 – 2015 sample period. There are 873 such entries in total during the sample period, of which 804 have required Compustat data and available ownership data (from either Refinitiv or SEC Analytics Suite) in the quarter prior to index entry. We also create a benchmark sample of 766 firms, matched with the entering firms on 3-digit SIC codes and market capitalization in the quarter prior to entry.[31]

One challenge with using S&P500 additions as an identifying event is apparent from Table 10, Panel A, which shows the characteristics of the entering and benchmark firms. Not surprisingly, entering firms exhibit better financial performance than the benchmark firms in the entry year: on average, they have higher stock returns (0.22 vs. 0.18), and lower book-to-market ratios (0.49 vs. 0.58). A matching algorithm would need to control for past performance of entering firms, in addition to their other attributes. This presents a challenge given the limited number of large industry peers in some industries, and given that the selection criteria for inclusion to the S&P500 are not perfectly observed.[32]

[31] In some cases, the lack of a firm in the same industry with Compustat data prevents us from obtaining a match.
[32] According to the Dow Jones September 2018 documentation, discussion of the S&P500 index: "Constituent selection is at the discretion of the Index Committee and is based on the eligibility criteria. The indices have a fixed constituent company count of 500…. Sector balance, as measured by a comparison of each GICS sector's weight in an index with its weight in the S&P Total Market Index, in the relevant market capitalization range, is also considered in the selection of companies for the indices." https://us.spindices.com/documents/methodologies/methodology-sp-us-indices.pdf

A second obstacle to using these events for studies of cross-ownership is illustrated in Fig. 7. While the additions appear to induce increases in cross-ownership in the entry quarter, they also cause increases in the overall institutional ownership in the same quarter. These two effects are closely linked: as institutions increase their holdings in the entering firms (because they are now part of the index), cross-ownership between these firms and any other firms in these institutions' portfolios mechanically goes up. An added complication (apparent from the top right panel) is that the entry events are accompanied by significant declines in ownership by large (5% or more) blockholders. To the extent that blockholders exert stronger influence on firms than smaller investors, this decline alone could affect firm policies independently of any changes in common ownership.

In sum, the entry events affect firms' ownership structures on multiple levels, and the different effects are inherently difficult (if not impossible) to separate. Given this (and the difficulties with controlling for factors that affect entry), S&P500 additions are not a useful setting for studies of cross-ownership.

6.2. Russell reconstitutions

The annual reconstitution of the Russell indices similarly influences ownership, so it is worthwhile to explore it as a potential instrument for cross-ownership research. Compared to entry into the S&P500, the criteria for inclusion into the Russell indices are more transparent as they are based only on market capitalization.[33] This allows researchers to address the concern about endogenous index entry more effectively than in in case of S&P500 additions.

[33] Up through 2006, membership in the Russell indices was based solely on a ranking of firms' market capitalizations. Beginning in 2007, Russell switched to a banding policy, such that a firm only enters (exits) an index if its market capitalization exceeds the market capitalization breakpoint by a minimum threshold percent. See Chang, Hong, and Liskovich (2015) for more details.

The Russell 3000 Index consists of the largest 3000 firms based on market capitalization, where the Russell 1000 includes the largest 1000 firms, and the Russell 2000 includes the subsequent 2000. A useful feature of Russell indices is that a firm's weight in the overall index increases discretely as the firm crosses the boundary from Russell 1000 to 2000.[34] We obtain from FactSet a sample of 2,081 firms that cross the 1000 boundary during the period of 1980 to 2015 (we refer to these events as entries to Russell 2000 from 1000). After requiring that 13F data and Compustat data are available, we have 1,972 firm-years. Following the approach used in the S&P500 analysis, we form a benchmark sample, matching on 3-digit SIC industries and market capitalization in the year prior to entry. As shown in Panel B of Table 10, consistent with expectations, the entrants show poor stock market performance in the year prior to entry, with the average stock return of -6% compared to 13% for benchmark firms. This highlights the challenges of finding a good match.

Beyond the issues related to matching, Fig. 8 illustrates a perhaps more serious problem, and highlights why Russell additions are not suitable for cross-ownership research. Based on the figure, there is no evidence that institutional ownership or cross-ownership increases in the entry quarters.[35] This is consistent with the evidence in Schmidt and Fahlenbrach (2017) who show that, while ownership by mutual funds tracking Russell indices increases at the 1000 boundary, no such increase can be detected for broader categories of index funds or 13F institutions. It is therefore not surprising to see that cross-ownership by institutions is also unaffected by these events (as with S&P500 additions, the changes in institutional ownership and cross-ownership are mechanically

[34] This is because a firm at the top of the Russell 2000 represents a larger percentage of its respective index than a firm at the bottom of the Russell 1000. In contrast, market capitalizations change more smoothly around the boundary. Russell announces the new index additions on May 30th of each year, and they become effective on June 30th. Details are in Appel, Gormley, and Keim (2016) and Chang, Hong, and Liskovich (2015).

[35] As an aside, it is interesting to note that ownership (and cross-ownership) *declines* sharply prior to the Russell 2000 events. This decline coincides with poor stock market performance in the year prior to entry, as reported in Table 10.

linked). Assuming that institutions pay attention to their overall holdings, this fact alone disqualifies Russell reconstitutions as a potential instrument for cross-ownership changes.

7. Conclusions

The increasing propensity of institutional investors to own shares in rival firms has led both academics and policy makers alike to consider the potential consequences on the underlying firms. Of particular interest is the question of whether this shared ownership incentivizes the firms to compete less aggressively against each other or to cooperate more closely, as a way to maximize joint profits. A growing body of evidence suggests that the answer to these questions is yes, leading some to suggest limits on common ownership. However, there would likely be costs to limiting common ownership: many of the common owners represent large investors that provide many benefits, for example by pressuring firms toward better governance practices or, in the case of index funds, offering lower fees to individual investors. Given the high costs and benefits at stake, a careful examination of the issue is warranted.

Across multiple potential sources of identification, we conclude that most do not represent viable methods of isolating the effects of common ownership. The only potentially appropriate approaches are to use mergers between financial institutions outside of the 2008-2009 period, or to use a more complete sample of these mergers but select control firms that are closely matched to treatment firms. We show that the alternative methods, which rely on the Blackrock-BGI merger, the S&P additions, or the reconstitutions of Russell indices can lead to spurious results. After using a more appropriate source of identification, we find no evidence that common ownership causes increases in firm coordination, as measured by joint ventures, strategic alliances, or mergers between firms. We also find no evidence that it causes increases in operating

performance or decreases in investment spending. We attribute prior evidence that common ownership causes these effects to a combination of inappropriate instruments and inappropriate control samples.

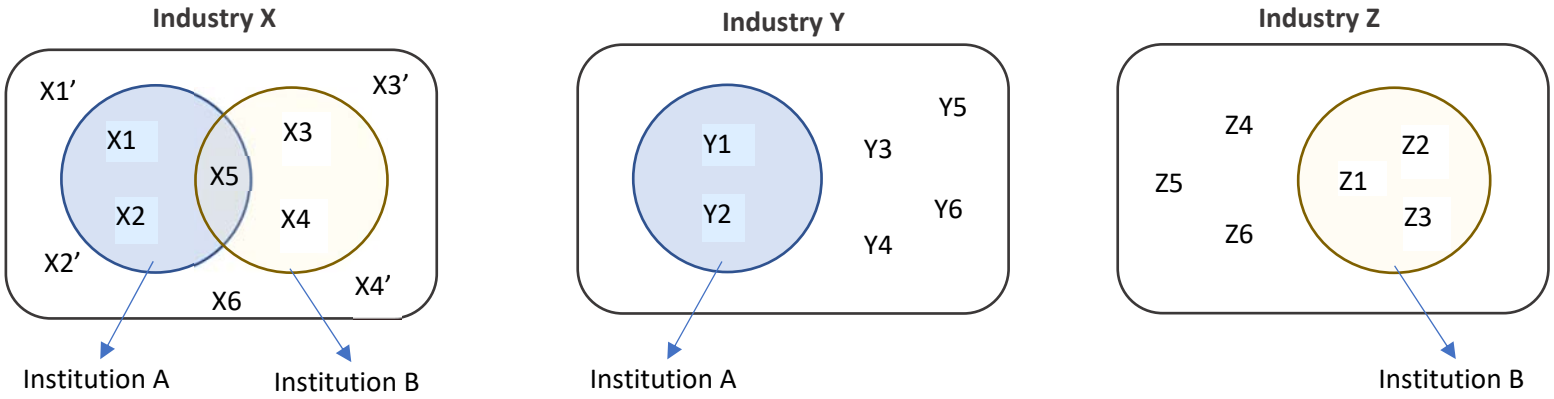
Appendix Table A1: Financial Institution Mergers

This table lists the sample of financial institution mergers that are used as identifying events. In addition to the announcement date, completion date, acquirer name and target name of the merger, it also lists the acquirer and target firm mgrnos, as used in the 13F database provided by Refinitiv (formerly Thomson Reuters). Permission was obtained from Refinitiv to publish these data.

Announcement Date	Completion Date	Acquiror Name	Target Name	Acquirer Mgrno	Target mgrno
6/20/2000	10/2/2000	Alliance Capital Mgmt Hldg LP	Sanford C Bernstein & Co Inc	25610	8650
10/27/2003	4/1/2004	Bank of America Corp	FleetBoston Financial Corp,MA	62890	38260
9/14/2008	1/1/2009	Bank of America Corp	Merrill Lynch & Co Inc	62890	56780
9/16/2009	12/1/2009	Blackrock Inc	Barclays Global Fund Advisors	9385	7900
8/28/1995	3/31/1996	Chemical Banking Corp,New York	Chase Manhattan Corp	15345	15230
11/5/1984	2/1/1985	Equitable Life Assurance	Donaldson Lufkin & Jenrette	25610	23375
3/6/1994	6/30/1994	First Union Corp,Charlotte,NC	Evergreen Asset Management Crp	37700	26100
6/19/1995	1/2/1996	First Union Corp,Charlotte,NC	First Fidelity Bancorp,NJ	37700	29580
3/14/1999	10/1/1999	Fleet Financial Group Inc,MA	BankBoston Corp,Boston,MA	38260	6000
8/1/1985	3/17/1986	Fleet Finl Group,Providence,RI	First Connecticut Bancorp	38260	29445
10/25/2000	4/10/2001	Franklin Templeton Investments	Fiduciary Trust Co Intl	39300	28060
4/14/2003	4/30/2003	Goldman Sachs Group Inc	Ayco Co LP	41260	5500
4/6/2010	4/6/2010	Goldman Sachs Group Inc	Level Global Investors LP	41260	10194
4/5/1995	4/5/1995	Heine Securities Corp	Chase Manhattan Corp	44438	15230
1/14/2004	7/1/2004	JPMorgan Chase & Co	Bank One Corp,Chicago,IL	58835	5955
4/23/2008	9/30/2008	Lehman Brothers Holdings Inc	David J Greene & Co LLC	50200	42120
12/6/1984	7/9/1985	Marshall & Ilsley,Milwaukee,WI	Heritage Wisconsin Corp	54280	44465
9/14/1992	5/21/1993	Mellon Bank Corp,Pittsburgh,PA	Boston Co	55390	9750
1/20/1997	5/20/1997	Mellon Bank Corp,Pittsburgh,PA	Ganz Capital Management Inc	55390	39800
12/11/1997	4/1/1998	Mellon Bank Corp,Pittsburgh,PA	Founders Asset Management Inc	55390	38870
6/28/1995	1/3/1996	Morgan Stanley Asset Mgmt	Miller Anderson & Sherrerd	58950	57980
10/31/2006	12/4/2006	Morgan Stanley	FrontPoint Partners LLC	58950	7759
8/30/1996	1/6/1997	NationsBank Corp,Charlotte,NC	Boatmen's Bancshares,St Louis	62890	9480
4/13/1998	9/30/1998	NationsBank Corp,Charlotte,NC	BankAmerica Corp	62890	5980
3/18/1992	10/15/1992	NBD Bancorp,Detroit,Michigan	INB Financial Corp	59800	45800
1/21/1997	11/5/1997	PIMCO Advisors LP	Oppenheimer Capital LP	70470	67463
11/5/1997	12/1/1997	PIMCO Advisors LP	Oppenheimer Capital LP	70470	67463
6/30/1986	2/27/1987	PNC Financial,Pittsburgh,PA	Citizens Fidelity, Louisville	67600	16575
9/22/1988	3/31/1989	PNC Financial,Pittsburgh,PA	Bank of Delaware, Wilmington	67600	6500
9/16/1991	7/23/1992	PNC Financial,Pittsburgh,PA	First National Pennsylvania	67600	34640
6/23/1995	9/29/1995	New England Investment Cos LP	Harris Associates LP	63120	43485
9/24/1984	9/3/1985	Society Corp	Centran Corp	79295	15015

5/15/1991	3/16/1992	Society Corp	AmeriTrust Corp,Cleveland,OH	79295	3700
4/27/1987	11/1/1987	Sovran Financial,Norfolk,VA	Commerce Union Corp,Nashville	80190	19600
9/26/1989	9/1/1990	Sovran Financial,Norfolk,VA	Citizens & Southern Georgia	80190	16910
7/20/1998	12/31/1998	SunTrust Banks Inc,Atlanta,GA	Crestar Finl Corp,Richmond,VA	82355	21650
4/6/1998	10/8/1998	Travelers Group Inc	Citicorp	84900	16260
10/3/2008	12/31/2008	Wells Fargo & Co	Wachovia Corp,Charlotte,NC	65850	37700
10/18/1995	4/1/1996	Wells Fargo Capital C	First Interstate Bancorp,CA	92035	29800
7/14/1986	7/15/1986	First Interstate Bancorp,CA	First Natl Bk & Tr,OK City,OK	29800	36140
9/17/1985	3/31/1986	Citizens and Southern GA Corp	Citizens and Southern Corp,SC	16910	16915
5/4/1988	12/26/1988	Boatmen's Bancshares,St Louis	Centerre Bancorp,St Louis,	9480	13000
12/21/1983	7/1/1984	Chase Manhattan Corp	Lincoln 1st Banks Inc	15230	51220
7/10/1990	4/19/1991	Norwest Corp,Minneapolis,MN	United Banks of Colorado	65800	87815
4/14/1992	3/31/1993	BANC ONE Corp,Columbus,Ohio	Valley National Corp,Phoenix	5955	89960
9/9/1992	7/13/1993	Bank of Boston Corp,Boston,MA	Multibank Financial Corp	6000	59400
3/9/1993	7/30/1993	Primerica Corp	Shearson Lehman Brothers Inc	71300	78685
10/18/1993	7/1/1994	First Union Corp,Charlotte,NC	Lieber & Co	37700	50800
11/3/1993	8/15/1994	BANC ONE Corp,Columbus,Ohio	Liberty National Bancorp	5955	50680
11/28/1994	4/12/1995	KeyCorp,Cleveland,Ohio	Spears,Benzak,Salomon & Farrel	49240	80200
2/21/1995	11/30/1995	Fleet Financial Group Inc,MA	Shawmut National Corp	38260	78660
5/8/1995	12/27/1995	US Bancorp,Portland,Oregon	West One Bancorp,Boise,Idaho	88855	92150
11/20/1995	11/15/1995	Donaldson Lufkin & Jenrette	First Interstate Bancorp,CA	25610	29800
9/6/1996	12/12/1996	First Union Corp,Charlotte,NC	Keystone Investments Inc	37700	49250
6/9/1997	10/1/1997	BankAmerica Corp	Robertson Stephens & Co	5980	74535
2/15/1999	7/6/1999	Credit Suisse Asset Management	Warburg Pincus Asset Mgmt	5720	91450
4/30/1999	9/20/1999	Firstar Corp,Milwaukee,WI	Mercantile Bancorp,St Louis,MO	38230	55510
9/13/2000	12/31/2000	Chase Manhattan Corp,NY	JP Morgan & Co Inc	15345	58835
10/18/2000	2/14/2001	Allianz AG	Nicholas-Applegate Capt Mgmt	1275	64240
4/16/2001	9/4/2001	First Union Corp,Charlotte,NC	Wachovia Corp,Winston-Salem,NC	37700	91000
5/26/2004	1/3/2005	Wells Fargo & Co	Strong Financial-Fund Asts	65850	82100
5/19/2005	8/4/2005	Transamerica Investment Mgmt	Westcap Investors LLC	84750	92160
7/7/2008	11/7/2008	RiverSource Investments LLC	J&W Seligman & Co	45639	78400
9/16/2008	9/22/2008	Barclays PLC	Lehman-Invest Bkg Bus	7900	50200

Appendix Figure A1: Sample construction for the financial institution-merger analysis: example. In this example, the universe consists of three industries (X, Y, and Z) and two merging institutions (A and B). Firms are numbered X1, X2, …, Y1, Y2, …, etc. *Treatment Firms* are firms that are block-held by one of the merger partners with some industry rivals being block-held by the other partner (firms block-held by both partners are excluded). *Control Firms*^{DI} are firms block-held by one merger partner with no industry rivals block-held by the other partner. *Control Firms*^{SI} are firms matched to Treatment Firms based on industry and size (a matched firm to firm X1 is denoted as X1', etc.). The formation of *Treatment Pairs* and *Control Pairs* is shown in the table below.



FOR PAIR-LEVEL ANALYSES:

Treatment Pairs	Control Pairs
X1 - X3	X1 - X3'
X1 - X4	X1 - X4'
X2 - X3	X2 - X3'
X2 - X4	X2 - X4'
X3 - X1	X3 - X1'
X4 - X1	X4 - X1'
X3 - X2	X3 - X2'
X4 - X2	X4 - X2'

FOR FIRM-LEVEL ANALYSES:

Treatment Firms: firms that are block-held by one of the merger partners with some industry rivals being block-held by the other partner (firms block-held by both partners are excluded).
X1, X2, X3, X4

Control Firms^{DI}: firms block-held by one merger partner with no industry rivals block-held by the other partner.
Y1, Y2, Z1, Z2, Z3

Control Firms^{SI}: firms matched to Treatment Firms based on industry and size and not block-held by the merging institutions:
X1', X2', X3', X4'

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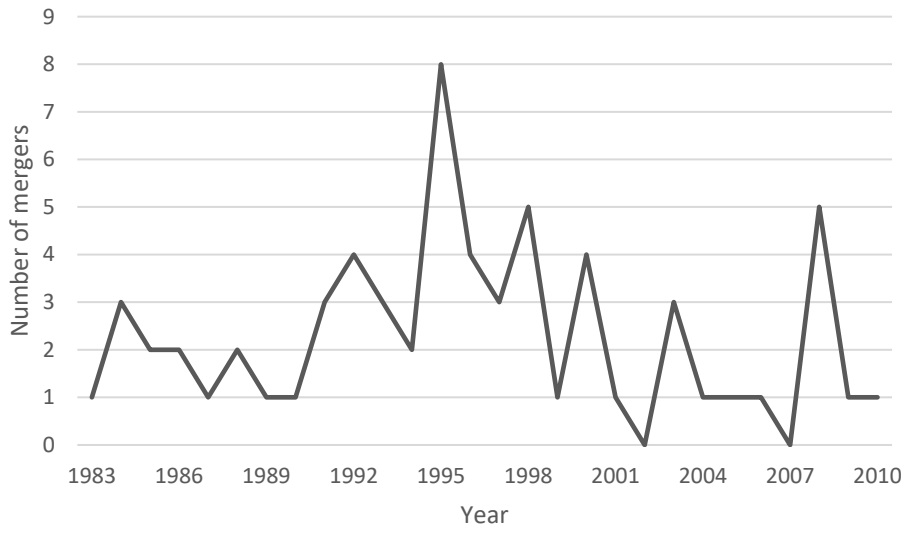
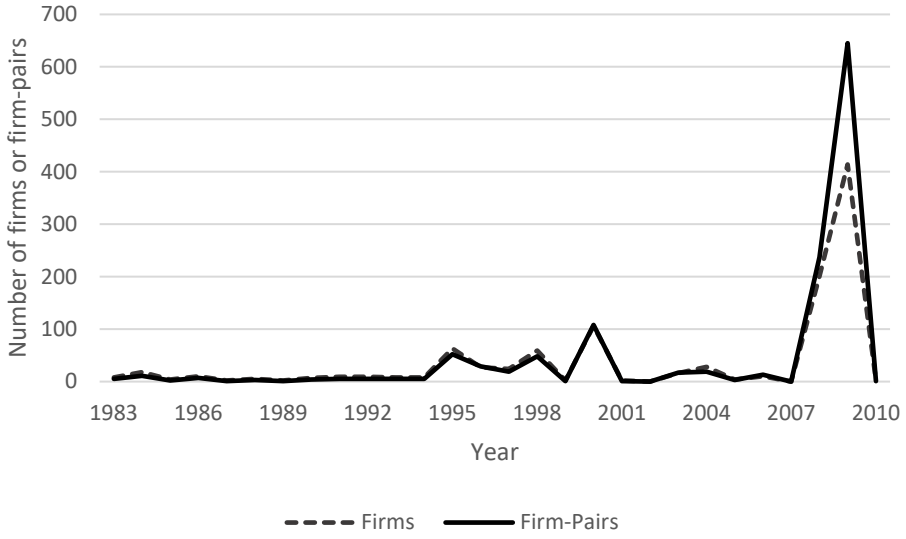
Figure 1: The frequency and size of financial-institution mergers and the number of Treatment Firms by quarter. Panel A includes the 64 financial-institution mergers that satisfy our criteria for sample selection described in Section 5.1. Panel B includes the 1,048 treatment firms and the 1,246 treatment firm pairs. Panel C includes the same 64 financial-institution mergers as Panel A, and it plots the average assets under management of the merging institutions each year. Year is the year of the last 13F reporting quarter for the treatment firm before the merger announcement.

Panel A: Financial institution mergers



Panel B: Treatment firms and firm pairs associated with the financial institution mergers



Panel C: Average size of merging institutions

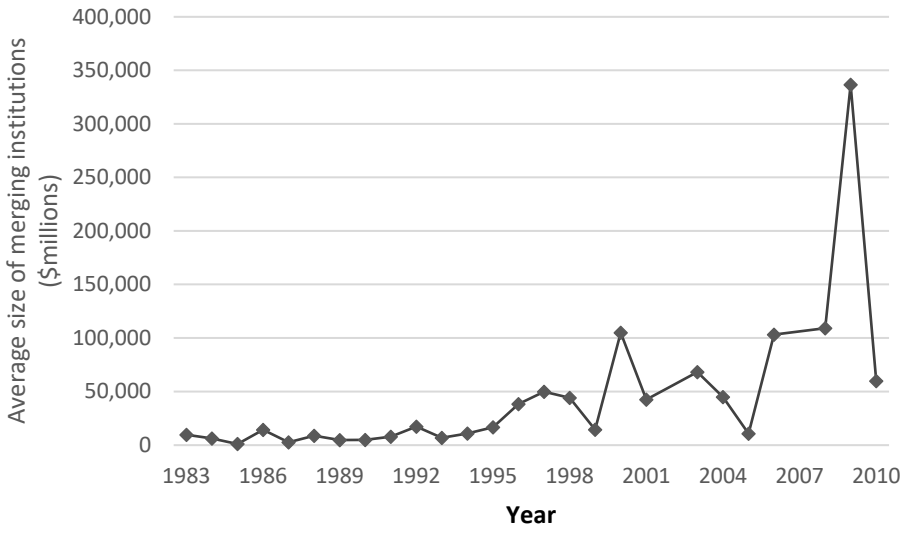
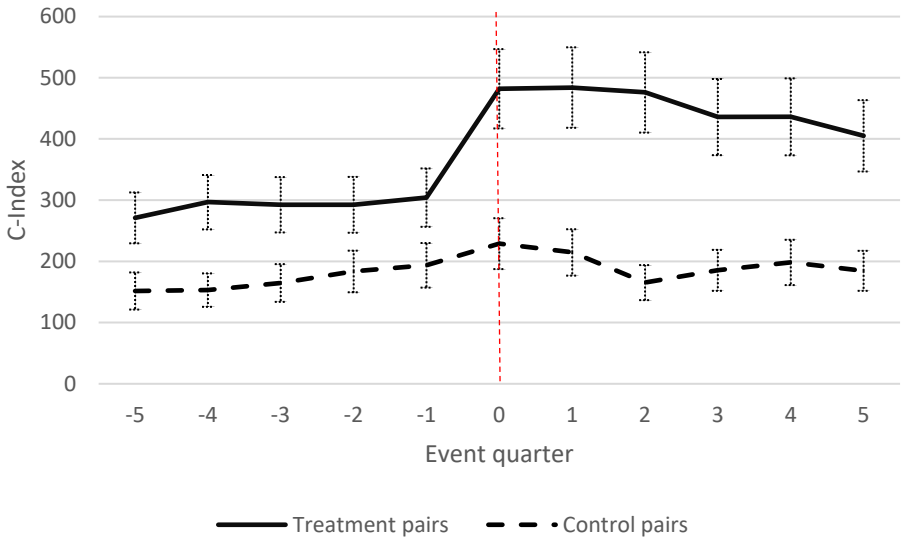


Figure 2: Pair-level Cross-ownership Index for treatment and control pairs around mergers of financial institutions. Treatment and Control Pairs are described in Section 5.1. Event quarters are quarter -5 to 5 around the quarter of the financial-institution merger effective date. Cross-ownership Index (C-Index) for a pair of firms (j,k) is constructed by summing up products of each common owner's (i) ownership stakes in the two firms: $\sum_i^N \mu_{ij} * \mu_{ik}$ (details are in Section 4). The products are multiplied by 10,000. The figures include the 95% confidence intervals. In the top figure, all ownership stakes of common owners are counted in the construction of the index. In the bottom panel, only 5% blocks are counted and cross-ownership involving smaller stakes is set to zero.

Panel A: Pair-level C-Index, computed using all shareholdings



Panel B: Pair-level C-Index, computed using block holdings

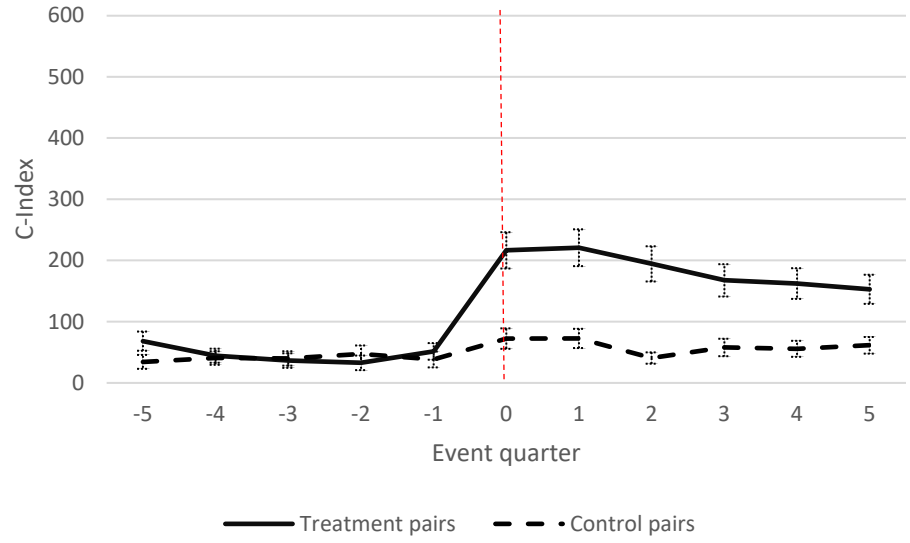
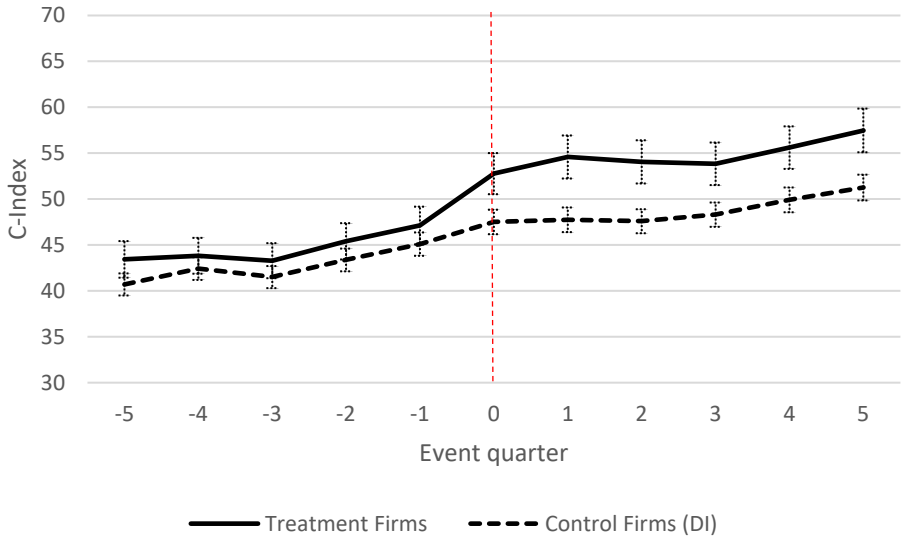


Figure 3: Firm-level Cross-ownership index for treatment and control firms around mergers of financial institutions. Treatment Firms and Control Firms[DI] are described in Section 5.1. Event quarters are quarter -5 to 5 around the quarter of the financial-institution merger effective date. Cross-ownership Index (C-Index) for a firm (j) is constructed by averaging the pair-level indices across all of firm j's competitors (k), either equal weighting or value-weighting the pair-level indices: $\sum_i^N \sum_k^K w_k * \mu_{ij} * \mu_{ik}$. Value-weighting is done using the competitor's market capitalization. The indices are multiplied by 10,000. The figures include the 95% confidence intervals.

Panel A: Equal-weighted Firm-level C-Index



Panel B: Value-weighted Firm-level C-Index

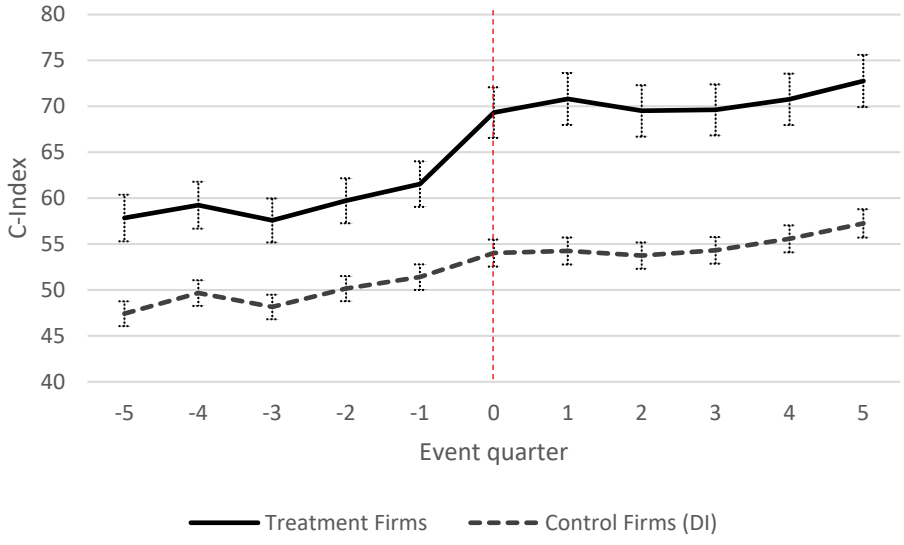
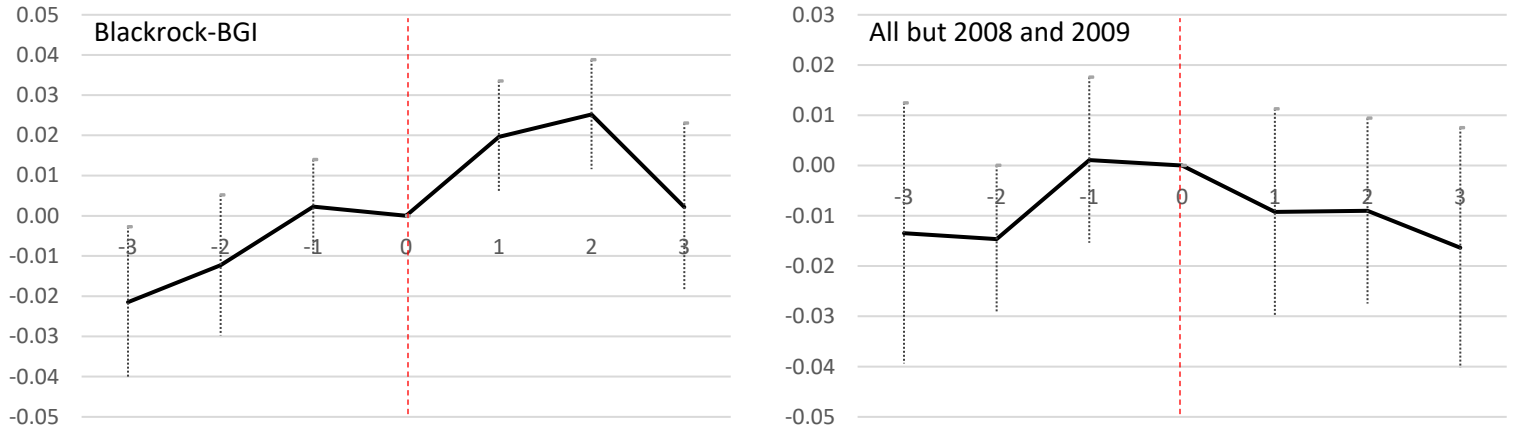


Figure 4: Firm performance around financial-institution mergers. Panel A shows interaction coefficients on *Treat×Event Year* from *ROA* regressions similar to those in Table 5, columns 3 and 5, except that the dummy variable *Treat* is interacted with indicators for event years (*Event Year*). *ROA* is measured as operating income scaled by lagged assets. Panel B shows average *ROA* for Treatment Firms and Control Firms^{DI} during fiscal years -3 to 3 around the year of the financial-institution merger. In the left panel, the sample is restricted to the Blackrock-BGI merger; in the right panel, the sample includes all financial-institution mergers outside of the 2008-2009 period. The top panels include the 95% confidence intervals.

Panel A: Interaction coefficients on Treat×Event Year from ROA regressions



Panel B: Average ROA around merger events

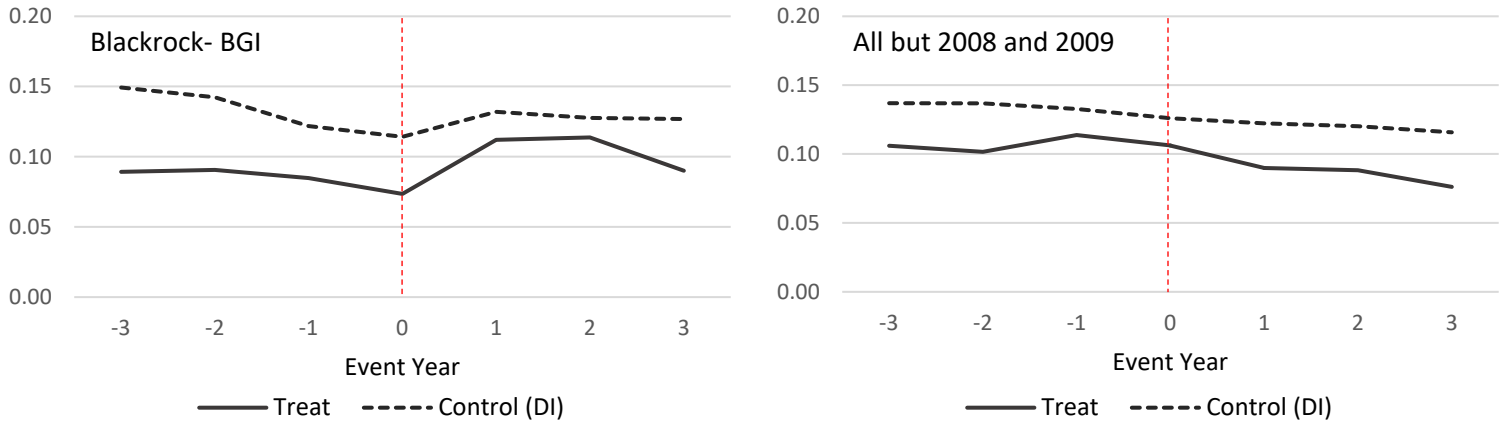
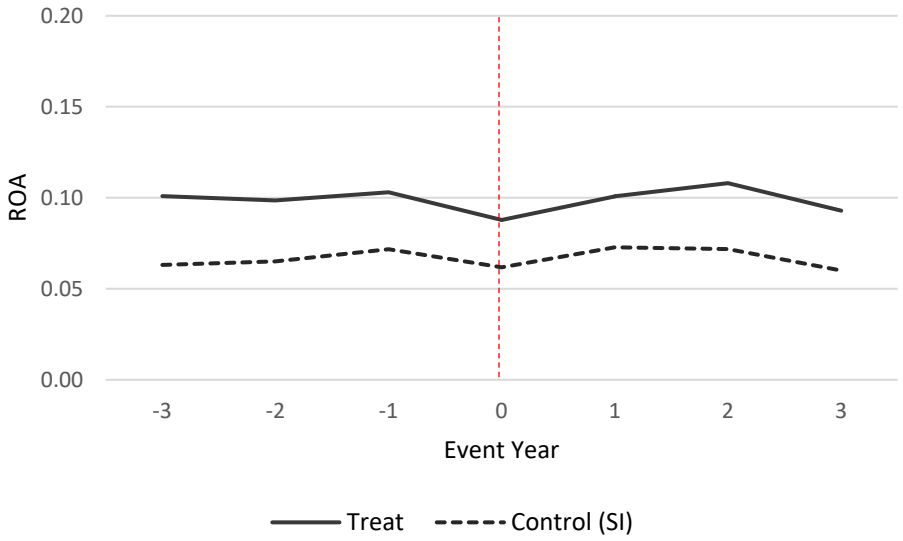


Figure 5: Average ROA in years around Blackrock-BGI merger for firms representative of the treatment and the control samples. We compare the 423 firms with positive R&D (similar to the Treatment sample) and the 743 firms with zero R&D (similar to the Control Firms^{DI} sample). Both samples are constructed from Control Firms^{DI}, with Treatment Firms excluded. The solid lines show the sample average ROA for each year; the dashed lines show sample ROA averaged across the three years before and after 2009.

Figure 6: Firm performance around financial-institution mergers: alternative control samples. Panel A shows *ROA*, measured as operating income scaled by lagged assets, for Treatment Firms and Control Firms^{SI} during fiscal years -3 to 3 around the year of the financial-institution merger. Panel B is analogous but uses Treatment Firms and Control Firms^{DIM}. Control Firms^{SI} are matched with the Treatment Firms on 3-digit SIC industry and size, and have at least one institutional blockholder. Control Firms^{DIM} come from different 3-digit SIC industries than Treatment Firms, are matched on size and R&D, and have at least one institutional blockholder. The details of sample construction are in Section 5.4.3.

Panel A: All mergers – Control Firms^{SI}



Panel B: All mergers – Control Firms^{DIM}

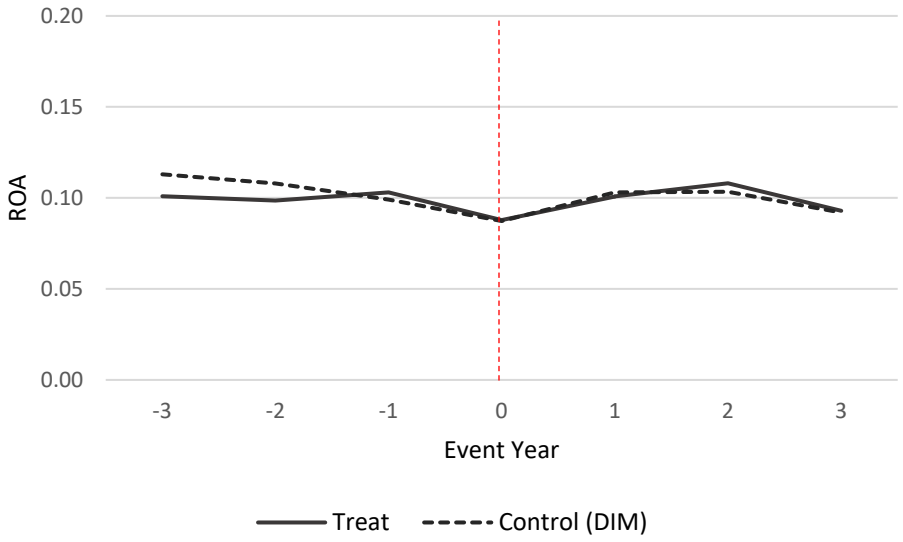


Figure 7: Firm-level Cross-ownership Index and institutional ownership around S&P500 additions. Entering Firms and Matched Firms are described in Section 6.1. Event quarters are quarters -5 to 5 around the entry quarter. Cross-ownership Index (C-Index) for a firm (j) is constructed by averaging the pair-level indices across all of firm j's competitors (k), either equal weighting or value-weighting the pair-level indices: $\sum_i^N \sum_k^K w_k * \mu_{ij} * \mu_{ik}$. Value-weighting is done using the competitor's market capitalization. The indices are multiplied by 10,000. See details in Section 4. Institutional ownership and Block ownership are expressed as a fraction of market capitalization.

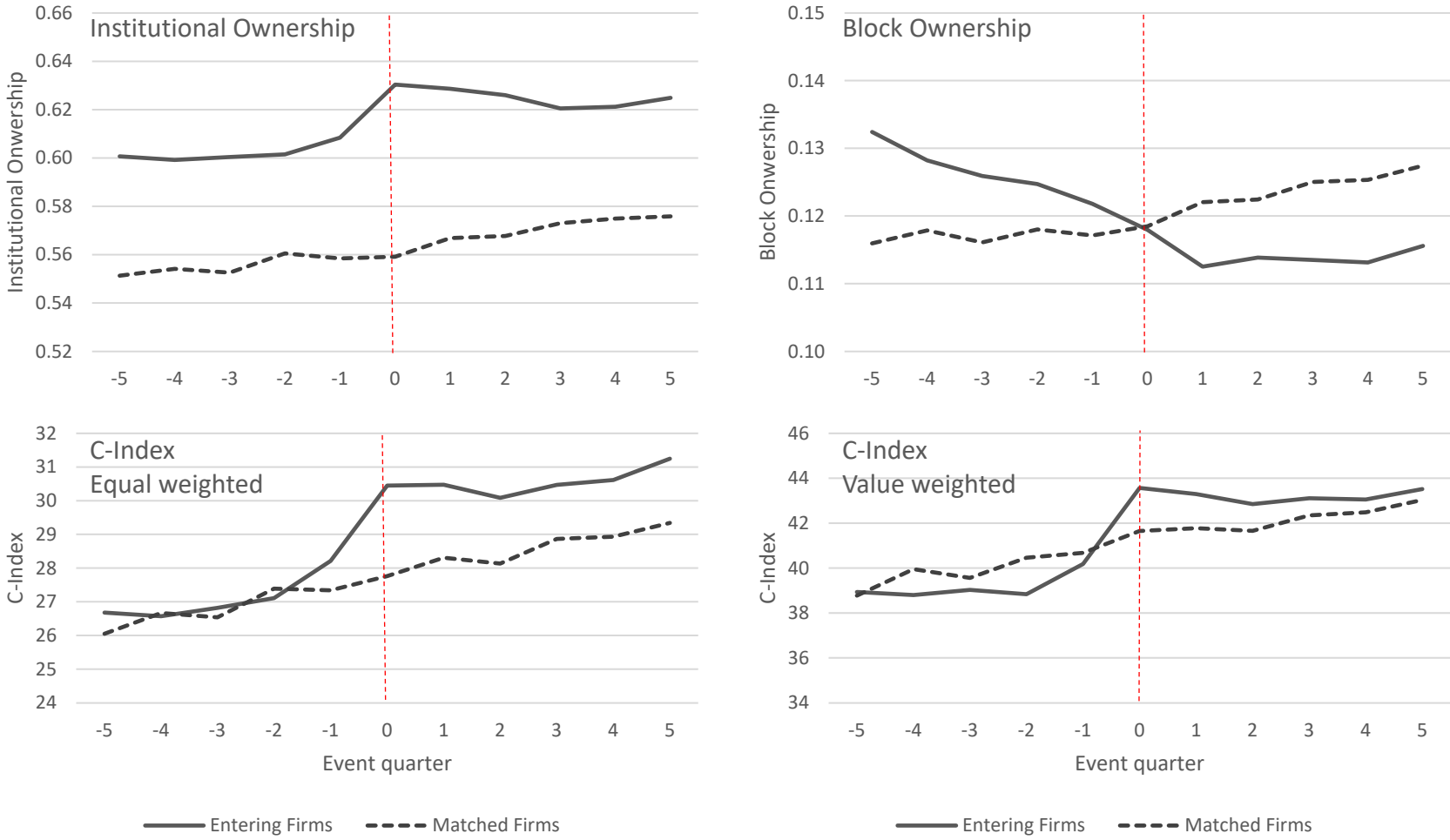


Figure 8: Firm-level Cross-ownership Index and institutional ownership around firms' entry into Russell2000 from Russell1000. Entering Firms and Matched Firms are described in Section 6.2. Event quarters are quarters -5 to 5 around the entry quarter. Cross-ownership Index (C-Index) for a firm (j) is constructed by averaging the pair-level indices across all of firm j's competitors (k), either equal weighting or value-weighting the pair-level indices: $\sum_i^N \sum_k^K w_k * \mu_{ij} * \mu_{ik}$. Value-weighting is done using the competitor's market capitalization. The indices are multiplied by 10,000. See details in Section 4. Institutional ownership and Block ownership are expressed as a fraction of market capitalization.

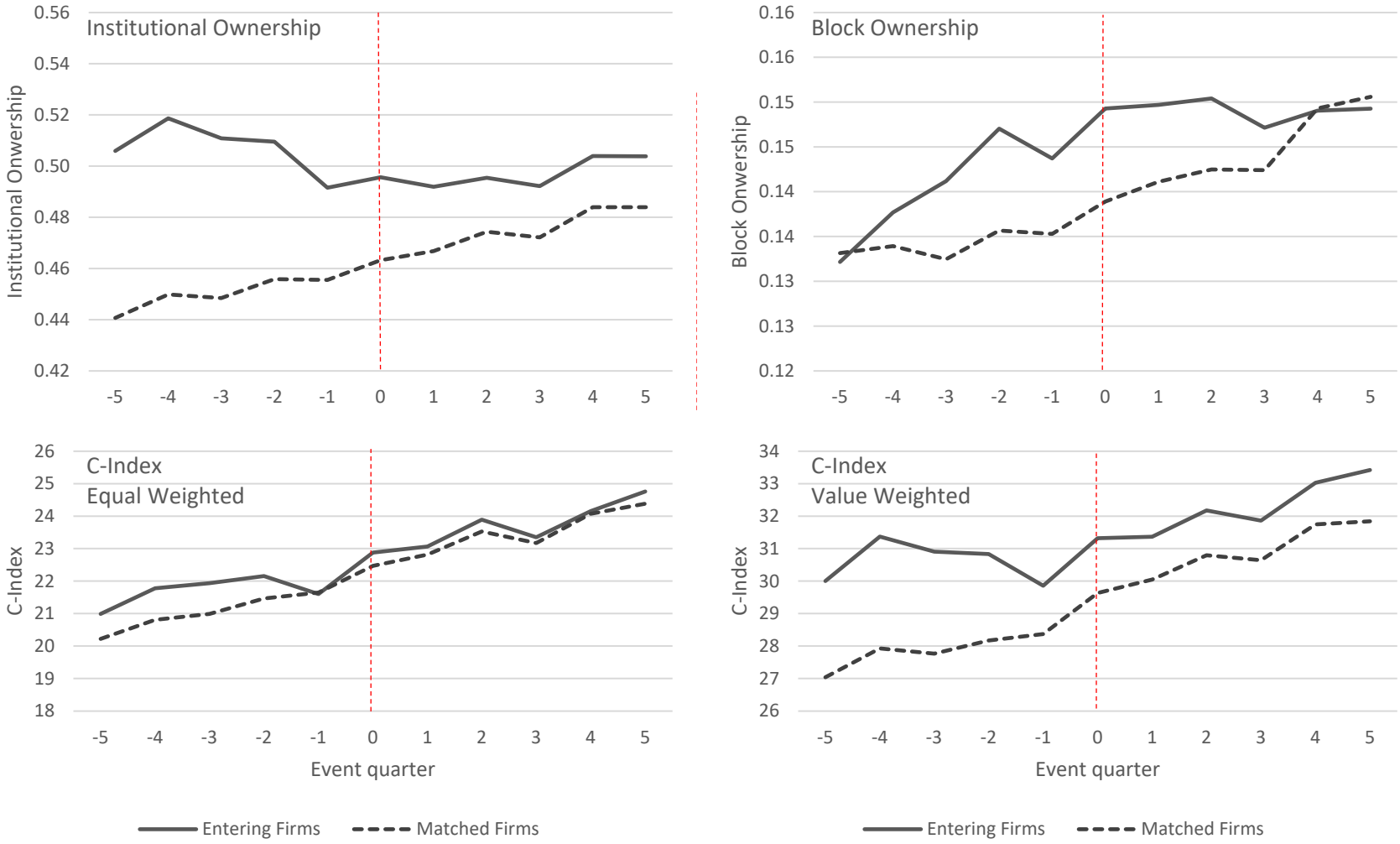


Table 1: Recent empirical studies of the effects of common ownership. This table lists papers on the effects of common ownership written since 2017 (earlier versions of these papers may have been circulated before 2017). The list was compiled based on a journal and SSRN search.

Study	Outcome	Identification	Find effect
Antón, Ederer, Giné, and Schmalz (2018)	Managerial incentives	Blackrock-BGI merger	Yes
Azar, Raina, and Schmalz (2019)	Prices of banking products	Banks' ownership by index funds	Yes
Azar, Schmalz, and Tecu (2018)	Airline ticket prices	Blackrock-BGI merger	Yes
Bindal (2019)	Gross margin, R&D	Mergers of financial institutions	Yes
Brooks, Chen, and Zeng (2018)	Merger likelihood	Russell reconstitution	Yes
Dennis, Gerardi, and Schenone (2020)	Airline ticket prices	OLS regressions	No
Freeman (2019)	Customer-supplier relationships	Mutual fund flows	Yes
Gutiérrez and Philippon (2017)	Investment	OLS regressions	Yes
He and Huang (2017)	Performance, mergers, joint ventures, strategic alliances	Mergers of financial institutions	Yes
He, Huang, and Zhao (2019)	Institutions' votes against management	Mergers of financial institutions	Yes
Kennedy, O'Brien, Song, and Waehrer (2017)	Airline ticket prices	Blackrock-BGI merger, Russell reconstitution, structural estimation	No
Kini, Lee, and Shen (2019)	Product market threats from rival firms	Mergers of financial institutions	Yes
Koch, Panayides and Thomas (2020)	Investment, SGA, advertising expenses	OLS, mergers of financial institutions	No
Kostovetsky and Manconi (2020)	Patent citations	Russell reconstitution	Yes
Kwon (2017)	Relative Performance Evaluation	S&P500 Additions	Yes
Liang (2016)	Relative Performance Evaluation	Blackrock-BGI merger	Yes
Semov (2017)	Cash holdings	Mutual fund flows	Yes
Torshizi and Clapp (2019)	Seed prices	OLS regressions	Yes
Xie and Gerakos (2018)	Patent litigation settlements	Blackrock-BGI merger	Yes

Table 2: Financial characteristics of the treatment and control samples for the financial-institution merger analysis. The table shows descriptive statistics for the Treatment Firms and Control Firms^{DI} (as described in Section 5.1) for the analysis of the financial-institution mergers in Tables 4 and 5. All variables are for the fiscal year of the effective date of the merger. The number of observations with non-missing market capitalization data are 936 (Treatment) and 3,306 (ControlFirms^{DI}). *B/M* is the book-to-market ratio. *R&D* is the ratio of R&D expenditures to total assets with R&D set to zero wherever missing. *PPE* is the ratio of property, plant, and equipment plus inventory to total assets. *Leverage* is the ratio of long-term and short-term debt to total assets. *ROA* is the ratio of operating income to lagged assets. *Market Share* is computed based on the firm's industry sales. *Institutional Own* is the fraction of institutional ownership to total market capitalization. *Block Own* is the fraction of institutional block ownership to total market capitalization, with blocks defined as ownership stakes of at least 5% of equity. All variables are winsorized at 1%.

	Treatment Firms		Control Firms^{DI}	
	Mean	Median	Mean	Median
Total Assets ($mil.)	3541.48	530.72	4633.88	748.63
Market Cap. ($mil.)	2672.13	614.32	2257.22	589.81
B/M	0.66	0.56	0.81	0.69
R&D	0.07	0.02	0.02	0.00
PPE	0.30	0.23	0.39	0.40
Leverage	0.26	0.22	0.34	0.33
ROA(Operating)	0.08	0.11	0.11	0.11
Market Share	0.09	0.01	0.22	0.05
Institutional Own	0.69	0.75	0.66	0.71
Block Own	0.25	0.24	0.25	0.23

Table 3: Descriptive statistics for the Cross-ownership Index: treatment and control samples for the financial-institution merger analysis. The table shows descriptive statistics for Cross-ownership Index (C-Index) for the treatment and control samples used in the analysis of financial-institution mergers in the quarter before the effective date of the merger. Panel A shows pair-level C-Index for the Treatment and Control Pairs samples, and Panel B shows firm-level C-Index for the Treatment and Control Firms[DI]. C- Index for a pair of firms (j,k) is constructed by summing up products of each common owner's (i) ownership stakes in the two firms: $\sum_i^N \mu_{ij} * \mu_{ik}$. The products are multiplied by 10,000. C-Index for a firm (j) is constructed by averaging the pair-level indices across all of firm j's competitors (k), either equal weighting or value-weighting the competitor's indices: $\sum_i^N \sum_k^K w_k * \mu_{ij} * \mu_{ik}$. Value-weighting is done using the competitor's market capitalization. All variables are winsorized at 1%. See details in Section 4.

Panel A. Pair-level C-Index

	Mean	Median	Std Dev	Min	P25	P75	Max	N
All ownership stakes								
Treatment Pairs	297.1	56.7	687.3	0.0	12.0	250.6	4337.1	947
Control Pairs	194.5	31.5	535.2	0.0	5.9	135.9	4337.1	978
Block ownership stakes								
Treatment Pairs	48.6	0.0	191.7	0.0	0.0	0.5	1700.0	947
Control Pairs	35.3	0.0	182.4	0.0	0.0	0.0	1700.0	978

Panel B: Firm-level C-Index

	Mean	Median	Std Dev	Min	P25	P75	Max	N
Equally weighted C-Index								
Treatment Firms	45.99	46.51	30.20	0.01	17.62	67.09	127.14	934
Control Firms[DI]	45.25	38.67	34.59	0.00	15.24	67.76	127.14	3249
Value-weighted C-Index								
Treatment Firms	60.17	60.51	36.75	0.01	26.79	86.70	140.63	934
Control Firms[DI]	51.82	46.89	38.34	0.00	18.15	77.01	140.63	3302

Table 4: Difference-in-difference regressions of Cross-ownership Index around mergers of financial institutions. The sample in Panel A consists of Treatment and Control Pairs (as described in Section 5.1) in quarters -5 to 5 around the quarter of the financial-institution merger effective date. The dependent variable is the pair-level Cross-ownership Index (C-Index), constructed using either all ownership stakes or using blocks of 5% or more (see details in Section 4). *Treat* equals one for Treatment Pairs and zero for Control Pairs. *After* is an indicator for quarters 0 to 5. The regressions include firm-merger fixed effects, quarter fixed effects, and the *After* dummy. The sample in Panel B includes Treatment and Control Firms^{DI}, (as described in Section 5.1) in quarters -5 to 5 around the merger effective date. The dependent variable is a firm-level C- index, constructed either equal-weighting or value-weighting the competitor firms (see details in Section 4). *Treat* equals one for treatment and zero for control firms. *After* is an indicator for quarters 0 to 5. The regressions include firm-merger fixed effects, quarter fixed effects and the *After* dummy. Standard errors are clustered on the firm level. Standard errors are in parentheses. ***, **, * indicate p-values of less than 0.01, 0.05, and 0.1.

	All Mergers		Blackrock-BGI merger		All but 2008 and 2009	
Panel A: Pair-level regressions						
	All Stakes	Blocks	All Stakes	Blocks	All Stakes	Blocks
Treat × After	130.651***	113.073***	128.033***	131.965***	180.516***	96.803***
	(19.043)	(11.338)	(18.160)	(14.433)	(53.935)	(23.546)
N	20,370	20,370	11,705	11,705	5,573	5,573
Quarter FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y
Panel B: Firm-level regressions – Control Firms^{DI}						
	EW	VW	EW	VW	EW	VW
Treat × After	2.330***	3.923***	4.735***	5.075***	0.443	3.061***
	(0.460)	(0.620)	(0.914)	(1.134)	(0.536)	(0.888)
N	45,138	45,707	11,290	11,484	22,595	22,780
Quarter FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y

Table 5: Difference-in-difference regressions of ROA and R&D around financial-institutions mergers: baseline control sample. The table shows regressions of *ROA* and *R&D* in fiscal years -3 to 3 around financial-institution mergers. The sample consists of Treatment Firms and Control Firms^{DI} (as described in Section 5.1). Control Firms^{DI} are block-held by the merging institutions before the merger but come from different industries than Treatment Firms. *Treat* equals one for treatment firms and zero for control firms. *After* is an indicator for fiscal years 1 to 3. The table shows separately regressions based on all mergers, the Blackrock-BGI merger, and all mergers except those in 2008 and 2009. *ROA* is operating income scaled by lagged assets. *R&D* is R&D expenditure scaled by total assets with missing values set to zero. The variables are winsorized at 1%. The regressions include firm-merger fixed effects, fiscal year fixed effects and the *After* dummy. Standard errors are clustered on the firm level and the year level. Standard errors are in parentheses. ***, **, * indicate p-values of less than 0.01, 0.05, and 0.1.

	Full Sample		Blackrock – BGI merger		All but 2008 and 2009	
Dependent Var.:	ROA	R&D	ROA	R&D	ROA	R&D
Treat × After	0.012*	-0.004**	0.024**	-0.006*	-0.005	-0.000
	(0.006)	(0.002)	(0.008)	(0.003)	(0.010)	(0.002)
N	21,542	21,879	6,140	6,167	9,523	9,786
Fiscal Year FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y

Table 6: Difference-in-difference regressions of ROA and R&D around financial-institutions mergers: alternative control samples. The table shows regressions of *ROA* and *R&D* in fiscal years -3 to 3 around financial-institution mergers. In Panel A, the sample consists of Treatment Firms and Control Firms^{SI}. Control Firms^{SI} are matched with the Treatment firms on 3-digit SIC industry and size, and have at least one institutional blockholder. In Panel B, the sample consists of Treatment Firms and Control Firms^{DIM}. Control Firms^{DIM} come from different 3-digit SIC industries than Treatment Firms, are matched with Treatment Firms on size and R&D, and have at least one institutional blockholder. The details of sample construction are in Section 5.4.3. Each panel tabulates *Treat*×*After*, where *Treat* equals one for treatment firms and zero for control firms, and *After* is an indicator for fiscal years 1 to 3. The table shows separately regressions based on all mergers, the Blackrock-BGI merger, and all mergers except those in 2008 and 2009. *ROA* is operating income scaled by lagged assets. *R&D* is R&D expenditure scaled by total assets with missing values set to zero. The variables are winsorized at 1%. The regressions also include firm-merger fixed effects, fiscal year fixed effects and the *After* dummy. Standard errors are clustered on the firm level and the year level. Standard errors are in parentheses. ***, **, * indicate p-values of less than 0.01, 0.05, and 0.1.

	Full Sample		Blackrock – BGI merger		All but 2008 and 2009	
Dependent Var.:	ROA	R&D	ROA	R&D	ROA	R&D
Panel A: Control firms from the same industries: Control Firms^{SI}						
Treat × After	-0.004	0.000	-0.008	-0.000	-0.005	0.002
	(0.007)	(0.003)	(0.011)	(0.004)	(0.010)	(0.004)
N	8,908	9,041	3,808	3,842	3,215	3,304
Fiscal Year FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y
Panel B: Control firms from different industries, matched on R&D: Control Firms^{DIM}						
Treat × After	0.002	0.001	0.003	0.000	0.006	0.002
	(0.006)	(0.002)	(0.009)	(0.004)	(0.011)	(0.003)
N	9,534	9,672	4,240	4,275	3,346	3,436
Fiscal Year FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y

Table 7: Difference-in-difference regressions of other outcome measures, around financial institution mergers. This table presents regressions of alternative outcome variables in fiscal years -3 to 3 around financial-institution mergers (similar to Tables 5 and 6). The sample in Panels A - C consists of Treatment Firms and Control Firms^{DI}, where Control Firms^{DI} are block-held by the merging institutions before the merger but come from different industries than Treatment Firms (as described in Section 5.1). The sample in Panel D consists of Treatment Firms and Control Firms^{SI}. Control Firms^{SI} are matched with the Treatment firms on 3-digit SIC industry and size, and have at least one institutional blockholder (as described in Section 5.4.3). Each panel tabulates *Treat*×*After*, where *Treat* equals one for treatment firms and zero for control firms, and *After* is an indicator for fiscal years 1 to 3. *ROA (After Depr.)* is operating income after depreciation scaled by lagged assets. *R&D + CapEx*, is the sum of R&D and capital expenditures scaled by total assets, where missing values of R&D are set to zero. *Margin* is the ratio of operating income after depreciation to sales, where sales are required to be at least 1% of assets. *Cash is* cash and marketable securities scaled by assets. *ΔMarket Share* is change in market share, where market share equals sales as a fraction of total industry sales. All variables are winsorized at 1%. The regressions include firm-merger fixed effects, fiscal year fixed effects, and the *After* dummy. Standard errors are clustered on the firm level and the year level. Standard errors are in parentheses. ***, **, * indicate p-values of less than 0.01, 0.05, and 0.1.

Dependent Variable:	ROA (After Depr.)	R&D + CapEx	Margin	Cash	ΔMarket Share
Panel A: Baseline control sample (Control Firms^{DI}), with full sample of mergers					
Treat × After	0.013**	-0.004	0.034*	-0.010*	0.001*
	(0.006)	(0.003)	(0.019)	(0.005)	(0.001)
N	21,710	21,274	21,754	21,872	19,410
Fiscal Year FE	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y
Panel B: Baseline control sample (Control Firms^{DI}) - Blackrock-BGI merger					
Treat × After	0.024**	-0.006	0.050	-0.026**	0.002
	(0.009)	(0.004)	(0.030)	(0.009)	(0.001)
N	9,658	9,197	9,714	9,780	5,643
Fiscal Year FE	Y	Y	Y	Y	Y
Firm- Merger FE	Y	Y	Y	Y	Y
Panel C: Baseline control sample (Control Firms^{DI}) - all mergers but 2008 and 2009					
Treat × After	-0.003	-0.000	-0.005	0.006	0.000
	(0.009)	(0.003)	(0.023)	(0.007)	(0.001)
N	9,658	9,197	9,714	9,780	8,280
Fiscal Year FE	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y
Panel D: Alternative control sample (Control Firms^{SI}) – with full sample of mergers					
Treat × After	-0.004	0.002	-0.042	-0.006	0.000
	(0.007)	(0.004)	(0.029)	(0.007)	(0.001)
N	8,956	8,589	8,869	9,036	8,479
Fiscal Year FE	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y

Table 8: Difference-in-difference regressions of the effects of financial-institutions mergers using alternative ownership thresholds. The table shows regressions of firm-level outcome variables in fiscal years -3 to 3 around financial-institution mergers. The sample consists of Treatment Firms and Control Firms^{DI} constructed using alternative thresholds for cross-ownership, as described in Section 5.4.5. The thresholds are 1%-2% in Panel A and 0.5%-1% in Panel B. Control Firms^{DI} are block-held by the merging institutions before the merger but come from different industries than Treatment Firms. *Treat* equals one for treatment firms and zero for control firms. *After* is an indicator for fiscal years 1 to 3. The table shows separately regressions based on all mergers, the Blackrock-BGI merger, and all mergers except those in 2008 and 2009. *ROA* is operating income scaled by lagged assets. *R&D* is R&D expenditure scaled by total assets with missing values set to zero. *Margin* is the ratio of operating income after depreciation to sales, where sales are required to be at least 1% of assets. Δ*Market Share* is the change in the fraction of the firm's sales on total industry sales. All variables are winsorized at 1%. The regressions include firm-merger fixed effects, fiscal year fixed effects and the *After* dummy. Standard errors are clustered on the firm level and the year level. Standard errors are in parentheses. ***, **, * indicate p-values of less than 0.01, 0.05, and 0.1.

	Full Sample				Blackrock-BGI Merger				All but 2008 and 2009			
Dependent Var.:	ROA	R&D	Margin	ΔMarket Share	ROA	R&D	Margin	ΔMarket Share	ROA	R&D	Margin	ΔMarket Share
Panel A: Cross-owners' stakes are 1% to 2%												
Treat × After	-0.001	-0.001	0.005	0.001*	0.019*	-0.003	0.068*	0.007**	-0.004	-0.001	0.002	0.000
	(0.003)	(0.001)	(0.008)	(0.001)	(0.009)	(0.002)	(0.035)	(0.003)	(0.003)	(0.001)	(0.007)	(0.001)
N	84,788	86,247	85,707	75,090	3,685	3,762	3,666	3,457	73,058	74,347	74,021	64,327
Fiscal Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Panel B: Cross-owners' stakes are 0.5% to 1%												
Treat × After	0.001	-0.001*	0.008	0.001***	0.014*	-0.004	0.041*	0.006**	-0.001	-0.001	0.002	0.001***
	(0.002)	(0.001)	(0.005)	(0.000)	(0.007)	(0.002)	(0.019)	(0.002)	(0.002)	(0.001)	(0.004)	(0.000)
	131,144	133,399	132,813	114,723	2,969	3,004	2,972	2,655	118,432	120,561	120,065	103,243
Fiscal Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Table 9: The analysis of mergers, joint ventures, and strategic alliances following financial-institution mergers. Panel A shows the overall frequency of mergers, joint ventures (JVs), or strategic alliances (SAs) for the Treatment Firms (with any partner or with an industry peer) within the three years following the financial institution merger. Panel B shows the frequency of these events for the Treatment Pairs (left column) and Control Pairs (right column).

Panel A: Descriptive statistics for the frequency of mergers, JVs, and SAs for Treatment Firms

	# Firms	*Percent*
# Treatment Firms in year -1	1,048	
Firms involved in a merger, JV, or SA in following 3 years	298	28.40%
Firms involved in a merger, JV or SA within same industry, in following 3 years	162	15.5%
JV and SA cases	62	5.9%
Merger cases	109	10.4%

Panel B: Pair-level analysis of mergers, JVs and SAs

	Treatment Pairs	Control Pairs
# pairs in year -1	2,492	2,448
# pairs involved in the event in years 1 to 3	2	2
Percent of pairs	0.08%	0.08%
Percent of all events involving Treatment Firm in years 1 to 3	0.67%	0.70%

Table 10: Descriptive statistics for the firms entering the S&P500 and the Russell2000 from Russell1000 from 1980 to 2015. All variables are for the fiscal year of entry. Benchmark firms are firms matched with the entering firms in the quarter prior to entry on their 3-digit SIC code and market capitalization. There are 804 firms entering S&P500 and 1,972 firms entering Russell2000 with non-missing market capitalization data. The corresponding numbers for benchmark firms are 776 and 1,933. *R&D* is the ratio of R&D expenditures to total assets with R&D set to zero wherever missing. *PPE* is the ratio of property, plant, and equipment plus inventory to total assets. *Leverage* is the ratio of long-term and short-term debt to total assets. *ROA* is the ratio of operating income to lagged assets. *Stock return* is the sum of monthly returns over the fiscal year. *Institutional Own* is the fraction of institutional ownership to total market capitalization. *Block Own* is the fraction of institutional block ownership to total market capitalization, with blocks defined as ownership stakes of at least 5% of equity. All variables are winsorized at 1%.

	Entering Firms		Benchmark Firms	
	Mean	Median	Mean	Median
Panel A: S&P500 additions				
Total Assets ($mil.)	12,039.75	3,484.76	12,174.27	3,317.40
Market Cap. ($mil.)	10,152.46	6,768.36	7,841.09	5,113.11
B/M	0.49	0.39	0.58	0.48
R&D	0.02	0.00	0.02	0.00
PPE	0.41	0.39	0.42	0.43
Leverage	0.32	0.29	0.34	0.32
ROA(Operating)	0.21	0.19	0.17	0.16
Stock return	0.22	0.19	0.18	0.18
Institutional Own	0.62	0.63	0.56	0.59
Block Own	0.11	0.09	0.12	0.08
Panel B: Entries into Russell2000 from Russell1000				
Total Assets ($mil.)	2,022.17	826.55	1,201.14	466.12
Market Cap. ($mil.)	985.18	804.52	865.19	680.40
B/M	0.84	0.67	0.67	0.54
R&D	0.03	0.00	0.03	0.00
PPE	0.45	0.47	0.43	0.44
Leverage	0.35	0.34	0.31	0.30
ROA(Operating)	0.10	0.10	0.15	0.14
Stock return	-0.06	-0.02	0.13	0.13
Institutional Own	0.50	0.49	0.46	0.45
Block Own	0.15	0.12	0.14	0.11