Does common ownership really increase firm coordination? ☆

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A R T I C L E   I N F O

A B S T R A C T

A growing number of studies suggest 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 that some studies have attributed to common ownership are caused by other factors, such as differential responses of firms (or industries) to the 2008 financial crisis. We propose a modification to one of the previously used empirical approaches that 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.

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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&P 500 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 studies conclude that the rise in common ownership has caused cooperation among firms to increase and competition to decrease. Cross-owners' incentives to maxi-

mize 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 antitrust law to assess transactions by institutional investors that increase cross-holdings within the same industry. Posner et al. (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 (2018) point out that these recommendations may be misguided, and that the

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[1] Posner et al. (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.

costs of institutional investors disengaging from corporate governance may outweigh the benefits.

A survey of the literature reveals eight published papers and 11 working papers written since 2016 containing empirical analyses of common ownership.[2] Across these 19 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 et al. (2020) and Lewellen and Lewellen (2021) question the incentives of institutions to encourage anti-competitive practices.[3] Consistent with such concerns, on the empirical side, Kennedy et al. (2017) and Dennis et al. (2020) question the conclusions of Azar et al. (2018a) that common ownership within the airline industry resulted in anti-competitive practices [see also a reply in Azar et al. (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 (e.g., Doidge et al., 2017), and this trend could have caused shifts in profitability or investment. In addition, many studies of common ownership rely disproportionately on the years surrounding the 2008 financial crisis, which further confounds the analysis (as we discuss in more detail below).

Our first objective is to evaluate the empirical approaches 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, the Blackrock/BGI merger, additions to the S&P 500, and reconstitutions of Russell 1000/2000 indices.[4]

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 policymakers cite the recent evidence on coordination between cross-owned firms as an indication of anti-competitive behavior.[5]

Using mergers between financial institutions as a source of identification, as proposed by He and Huang (2017), offers many advantages, and thus we focus first on this approach. 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 (see list in Appendix Table A1). The selection criteria are similar to those in He and Huang (2017): we require that one partner (i.e., financial institution) in the merger holds a block in a firm while the other partner 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 their effects on coordination. Following similar approaches in the literature, we aggregate the cross-ownership stakes across the 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 our 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.[6] 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.

Our findings are striking. First, consistent with prior studies, when we use the full merger sample including the financial crisis years, and a control sample comprised of firms from unaffected (i.e., different) industries, the 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

[2] The list contains studies identified at the time this paper was written. There is much ongoing active research in this area.

[3] Gilje et al. (2020) point out that institutions may lack these incentives if the firms represent only small fractions of their portfolios. Lewellen and Lewellen (2021) show that while dollar cash-flows to institutions from promoting anti-competitive practices can be substantial, they tend to be smaller in the more concentrated industries where the potential for anti-competitive behavior would be greatest.

[4] Several studies have also used mutual fund outflows. See Berger (2019) for an analysis of problems related to this approach.

[5] See, for example, the OECD Directorate for Financial and Enterprise Affairs Competition Committee, 2017, "Common ownership by institutional investors and its impact on competition." December 5-6, 2017.

[6] 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.

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 treatment firms or that are from different industries but are more closely matched to the treatment firms.

Examining the data more closely reveals that the patterns previously attributed to common ownership can be traced to the differential responses of firms in the years immediately following 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 that studies have used to identify exogenous shifts in cross-ownership: S&P 500 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&P 500 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.

The 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 at the institutional level, which disqualifies this setting for studies of institutional cross-ownership. This is related to the fact that Russell reconstitutions affect the aggregate holdings of mutual funds that track the Russell indices but to a lesser extent aggregate holdings 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 despite 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.

The remainder of the paper proceeds as follows. Section 2 provides an overview of the related literature. Section 3 describes our data, and Section 4 details our measurement of cross-ownership. Section 5 examines cross-ownership using financial institution mergers as a source of identification, and Section 6 considers using index reconstitutions as a source of identification. Section 7 concludes.

2. Literature overview

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

Two initial studies examine the effects of common ownership on the prices of airline tickets (Azar et al., 2018a) and banking products (Azar et al., 2019), and both find evidence that common ownership leads to anticompetitive outcomes. However, Kennedy et al. (2017) and Dennis et al. (2020) argue that after accounting for the endogeneity of market shares in the measures of industry concentration, there is no evidence that common ownership increases ticket prices.[7]

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 et al. (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 con-

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[7] O'Brien and Waehrer (2017) discuss 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).

Table 1

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 at the time this paper was written.

Study	Outcome	Identification	Find effect
Antón et al. (2018)	Managerial incentives	Blackrock-BGI merger	Yes
Azar et al. (2019)	Prices of banking products	Banks' ownership by index funds	Yes
Azar et al. (2018)	Airline ticket prices	Blackrock-BGI merger	Yes
Bindal (2019)	Gross margin, R&D	Mergers of financial institutions	Yes
Brooks et al. (2018)	Merger likelihood	Russell reconstitution	Yes
Dennis et al. (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 et al. (2019)	Institutions' votes against management	Mergers of financial institutions	Yes
Kennedy et al. (2017)	Airline ticket prices	Blackrock-BGI merger, Russell reconstitution, structural estimation	No
Kini et al. (2019)	Product market threats from rival firms	Mergers of financial institutions	Yes
Koch et al. (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&P 500 Additions	Yes
Liang (2016)	Relative Performance Evaluation	Blackrock-BGI merger	Yes
Semov (2017)	Cash holdings	Mutual fund flows	Yes
Torshizi and Clapp (2021)	Seed prices	OLS regressions	Yes
Xie and Gerakos (2020)	Patent litigation settlements	Blackrock-BGI merger	Yes

nected 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 et al. (2018) find increased merger likelihood for firms in which cross-owners own large blocks. However, Harford et al. (2011) point out that, in general, cross-owners' holdings are too small to incentivize them to influence merger outcomes. Several studies 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 (2020) 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 et al. (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, 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. Studies to date have considered two potential channels: ==managerial incentives== and "==doing nothing==" (i.e., refraining from pushing for more aggressive competitive strategies).[8] With respect to managerial incentives, Antón et al. (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 dif-

ferent methodologies, arrive at contradictory conclusions. In a different context, Edmans et al. (2019) and He et al. (2019) examine the effects of cross-ownership on corporate governance. Edmans et al. (2019) show theoretically that common ownership can improve price informativeness and, indirectly, strengthen governance. He et al. (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 research are worth noting. First, most empirical studies find support for the hypothesis that common ownership affects firms' behavior. Second, studies have employed a wide variety of methodologies to investigate these effects, and the choices are not uniform across studies. For example, researchers use 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&P 500 and Russell 1000/2000).

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

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[8] See Elhauge (2021) and Hemphill and Kahan (2020) for a broader discussion of channels by which common ownership potentially influences firm outcomes.

Suite – 13F Holdings dataset for the June 2013 to 2015 period.[9]

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.[10] 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 et al. (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 around the time of financial institution mergers (described in Section 5.1), we manually verify holding data during these periods.[11]

In addition to ownership data, we obtain information on mergers, joint ventures, and strategic alliances from the Securities Data Company (SDC) database of Refinitiv. Stock return data and information on S&P 500 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:

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

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 $\mu_{i,k}$, which is the extent to which a shareholder owns a similar stake in both firms [this approach is similar to Gilje et al. (2020) and Lewellen and Lewellen (2021)].[12] 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:

$$\text{Firm-level C-Index}_{j} = \sum_{k=1}^{K} \sum_{i=1}^{N} w_{k} * \mu_{i,j} * \mu_{i,k} \qquad (2)$$

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.[13] 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:

$$\text{Industry-level C-Index} = \sum_{j=1}^{J} \sum_{k \neq j}^{K} \sum_{i=1}^{N} w_{j} * w_{k} * \mu_{i,j} * \mu_{i,k} \qquad (3)$$

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 et al. (2018a).[14]

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

Intuitively, the denominator in Eq. (4) measures ownership concentration across all shareholders of firm j,

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[9] 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/ .

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

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

[12] 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 between the two firms. Gilje et al. (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 (2021) except that they focus on industry-level measures. In Section 5.3, we report tests based on the arithmetic-average measures.

[13] 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.

[14] 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 $MHHI = HHI + MHHI\ Delta$.

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 3306 (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	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

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 and 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 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).[15] 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.[16] This list consists of 7100 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 2492 firm pairs (1246 firm combinations) formed from 1048 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 1894 pairs (947 firm combinations), across 934 firms.

We construct control samples at the firm level for the 934 treatment firms and at the firm-pair level for the 1894 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 3249 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

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[15] Kini et al. (2019) use the same procedure, and many other studies (see Table 1) use related methodologies.

[16] 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.

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 1956 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 et al. (2018a). 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 is 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).[17] However, as shown in Panel B, the number of treatment 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.[18] 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.[19] 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.

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 Treatment Firms. The table highlights a challenge with selecting the control sample this way: while Control Firms[DI] have similar market capitalization to 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 the Control Firms[DI] sample 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 treatment (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. 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, although it declines to 153 by quarter five. To put these magnitudes in perspective, two crossholders, 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).[20] For the median

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[17] While our process of identifying financial institution mergers covers the 1980-2015 period, we find no events that satisfy our criteria after 2010.

[18] 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).

[19] 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 et al. (2018) and Lewellen and Lewellen (2021).

[20] In Internet 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. Internet Ap-

Panel A: The number of financial institution mergers
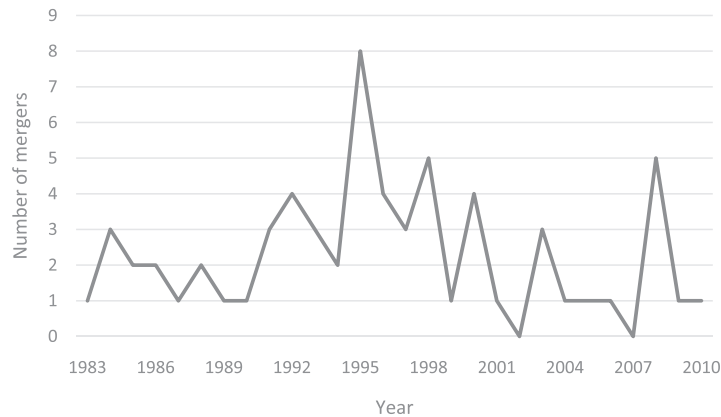
Panel B: The number of treatment firms and firm-pairs associated with the financial institution mergers
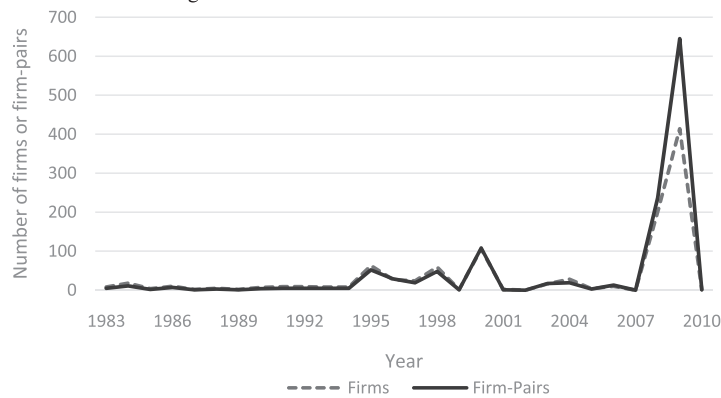
Panel C: Average size of merging institutions

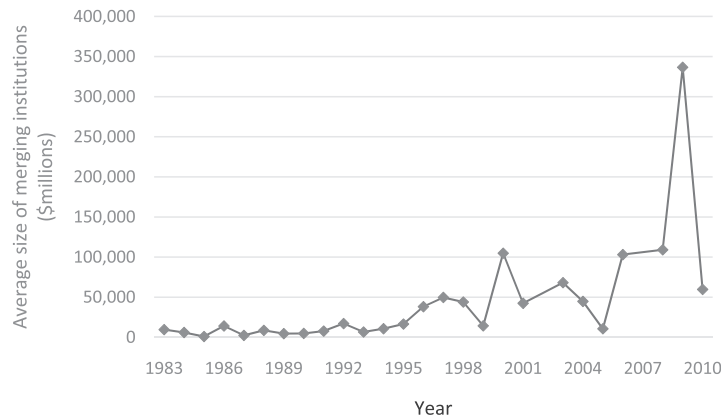**Fig. 1.** The frequency and size of financial institution mergers and the number of Treatment Firms by quarter. Panel A includes the 64 financial institution mergers that satisfy our criteria for sample selection described in Section 5.1. Panel B includes the 1048 treatment firms and the 1246 treatment firm pairs. Panel C includes the same 64 financial institution mergers as Panel A, and it shows 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.

firm, the increase is smaller and corresponds to an additional cross-holder with two 6% stakes. The smaller magni-

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pendix Fig. A3 shows analogous patterns for the firm-level C-Index, discussed below.

tudes 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 the results for a series of regressions that test the statistical significance of the increases in the pair-level C-Index around the merger events.

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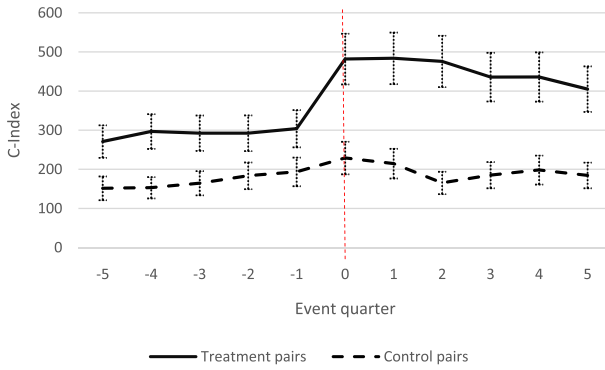
Panel B: Pair-level C-Index, computed using block holdings

Fig. 2. Pair-level Cross-ownership Index (C-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. 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 firm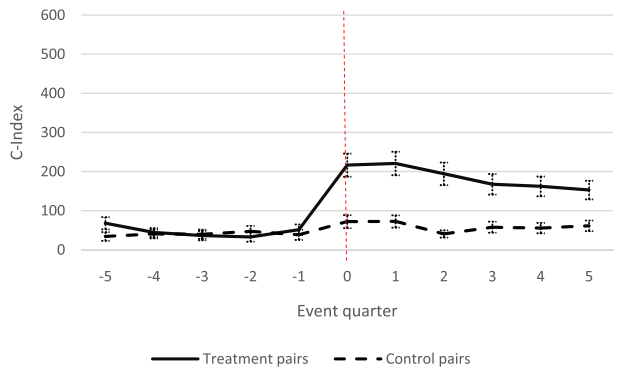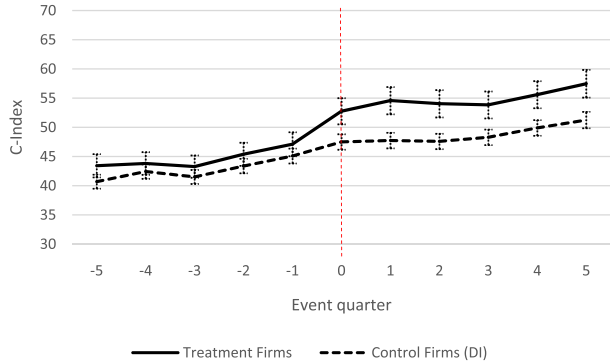s: $\sum_i^N \mu_{ij} * \mu_{ik}$ (details are in Section 4). The products are multiplied by 10,000. The figures include the 95% confidence intervals. In Panel A, all ownership stakes of common owners are counted in the construction of the index. In Panel B, only 5% blocks are counted and cross-ownership involving smaller stakes is set to zero.

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

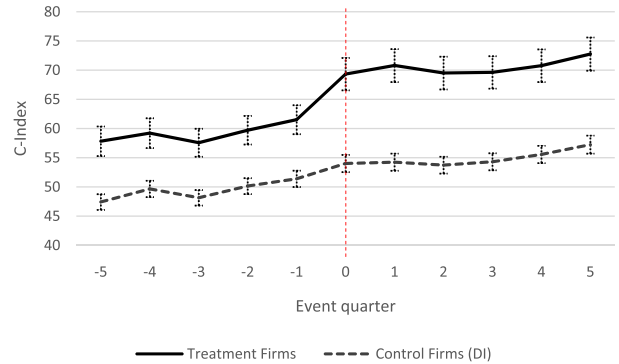Panel B: Value-weighted firm-level C-Index

Fig. 3. Firm-level Cross-ownership index (C-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. 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.

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 differences-in-differences 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 show the "Blackrock-BGI" sub-sample, and the right-hand columns show the "All but 2008 and 2009" sub-sample. The results indicate that the 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 Treatment Firms and for 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 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, regression results 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 results show similar effects, with three of the four coefficients being sta-

Table 3

Descriptive statistics for the Cross-ownership Index (C-Index): treatment and control samples for the financial institution merger analysis. The table shows descriptive statistics for the 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
Equal-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

Differences-in-differences regressions of Cross-ownership Index (C-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 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	5573	5573
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

tistically significant.[21] In Tables A1 and A2 in the Internet Appendix, we show that the results are similar when

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[21] 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 differences-in-differences framework used here). In Panel A of Table 4, the 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.

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 in the Internet Appendix show that the results are also similar using a tighter winsorization level, or using a trimming instead of winsorizing.

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

Table 5

Differences-in-differences regressions of ROA and R&D around financial institutions mergers: baseline control sample. The table shows the results of 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	6140	6167	9523	9786
Fiscal Year FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y

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

Studies have identified multiple channels through which cross-ownership could affect firm choices, and thus financial performance (see overview in Section 2). To explore these effects, in Table 5 we examine 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. In Section 5.4.4, we discuss other measures. Descriptive statistics on the full panel dataset used in the regressions are provided in Panel A of Table A4 in the Internet Appendix. All variables are winsorized at 1%.

The regression results in the left panel of Table 5 verify the findings from prior studies. 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 columns of Table 5 contain 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 despite a smaller sample size. 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 treatment 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 above, 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 2008–2009 period. Consistent with the conjecture that the financial 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 the results in column 5, we can reject with 95% confidence that the mergers increased ROA by more than 1.4 percentage points, that is, by a magnitude similar to the point estimate in column 1 (1.2 percentage points). Similarly, the results in column 6 indicate 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 the results are not driven by outliers, we re-estimate our main regressions using a tighter winsorization level or using trimming instead of winsorizing. The results, which are shown in Table A3, Panel C in the Internet Appendix, 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 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 oc-

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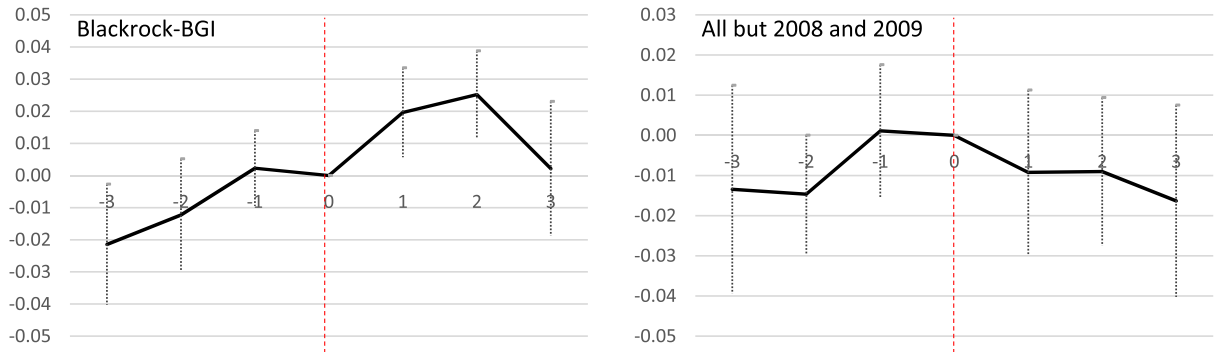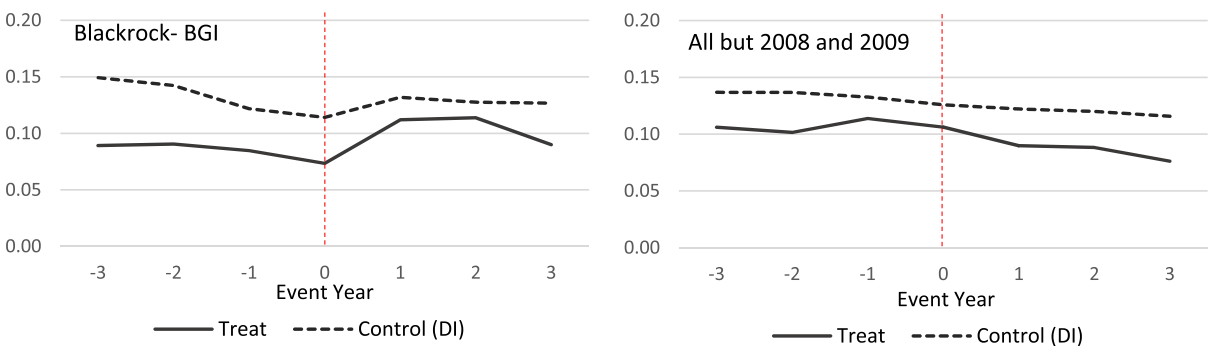

Panel B: Average ROA around merger events

Fig. 4. Firm performance around financial institution mergers. Panel A: Interaction coefficients on Treat×Event Year from ROA regressions. Panel B: Average ROA around merger events. 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.

curred in 2009, these patterns could reflect the differential effects of the financial crisis across the two samples.[22] To control for this, the figure in the right panel show results using 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. The profitability of the treatment firms increased relative to control firms following the Blackrock-BGI merger in 2009. However, after excluding mergers that occurred around the financial crisis, we find no evidence of a post-merger improvement.

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

In this subsection, we take 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 commonly 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 Internet Appendix Table

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[22] 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 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.
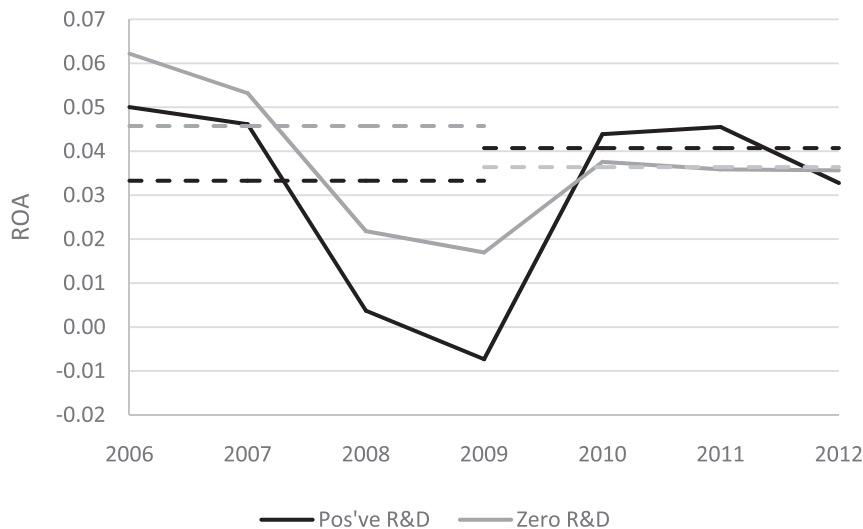
Fig. 5. Average ROA in years around Blackrock-BGI merger for firms representative of the treatment and the control samples. We compare the 423 firms with positive R&D (similar to the Treatment sample) and the 743 firms with zero R&D (similar to the Control Firms[DI] sample). Both samples are constructed 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. .

A6). Treatment Firms come disproportionally from high-growth industries such as drugs (SIC 283, representing 15.0% of the 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).[23]

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 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.

Fig. 5 shows that the two sets of firms experienced 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 the 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 employs 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 (see Appendix Fig. A1 for 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, the Control Firms[SI] sample consists of 941 firms. Table A7 in the Internet Appendix shows that Control Firms[SI] are similar to Treatment Firms based on most characteristics (e.g., 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]. The 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 the results in column 1, we can reject with 95% confidence that common ownership increased ROA by more than 0.9 percentage points.[24]

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[23] 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.

[24] Table A8 in the Internet Appendix presents a different way of controlling for industry effects. We re-estimate the Table 5 regressions (where we draw control firms from different industries than treatment firms), but include industry-year fixed effects. As shown in Panel A, we again

Table 6

Differences-in-differences 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	8908	9041	3808	3842	3215	3304
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	9534	9672	4240	4275	3346	3436
Fiscal Year FE	Y	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y	Y

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

The results for Controls Firms[DIM] are presented in Panel B of Table 6. They are qualitatively similar to those for Control Firms[SI], as reported in Panel A. We continue to find no evidence that common ownership caused the profitability to increase or the R&D investments to decrease. The lack of effects for both control samples is illustrated in Fig. 6. The figure shows average ROA around the merger events using all mergers. Panel A shows Treatment Firms and Control Firms[SI], and Panel B shows Treatment Firms and Control

Firms[DIM]. In each panel, ROA has similar patterns around merger events for both 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, 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. In this section, we consider alternative financial outcomes that have been used in broad studies of cross-ownership, and for which researchers find some evidence of a response to cross-ownership. Table 7 reports results for regressions using operating margin, change in market share, R&D plus capital expenditures scaled by 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.[26] In addition, the table includes results using an alternative

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find that the coefficients on *Treat×After* switch signs relative to those in Table 5 and become statistically insignificant. As shown in Panel B, we then cluster standard errors by industry and year (instead of firm and year) and obtain somewhat weaker significance compared to Table 5.

[25] 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 chosen as it is close to the sample median for Compustat firms with positive R&D.

[26] To minimize the influence of outliers, we exclude firms in industries that 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.
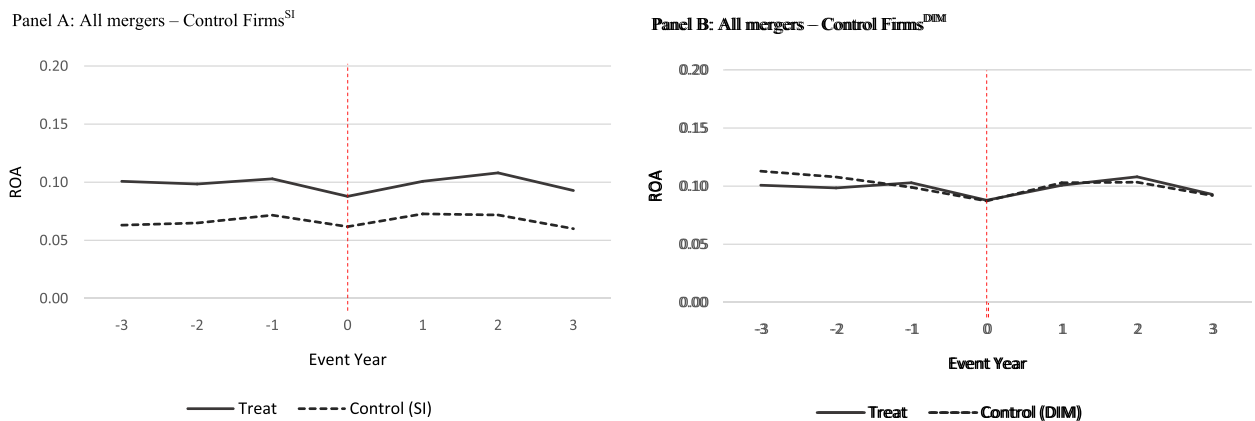
Panel A: All mergers – Control Firms[SI]

Panel B: All mergers – Control Firms[DIM]

Fig. 6. Firm performance around financial institution mergers: alternative control samples. Panel A: All mergers – Control Firms[SI]. Panel B: All mergers – Control Firms[DIM]. 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.

measure of ROA based on operating income after depreciation.[27]

Panels A-C show our baseline results for regressions using Control Firms[DI], analogous to those in Table 5. Panel A, which presents results for the full sample of mergers, shows significant increases in *ROA* (*After Depr.*), *Margin*, and *△Market 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, for example as shown in Table 5, many of these effects appear to be influenced by the Blackrock-BGI merger sub-sample (Panel B of Table 7). When mergers occurring during 2008–2009 are excluded (Panel C of Table 7), 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 *△Market 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.

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 *△Market 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?

In previous analysis, we construct 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 treatment firms 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.

Our 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. In fact, for mergers outside the 2008–2009 period, we observe small but statistically significant declines in cross-ownership relative to control firms. 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, which means that 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%

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[27] These variables have been used, for example, in Gutierrez and Philippon (2017), He and Huang (2017), Semov (2017), and Bindal (2019). Similar effects have been tested in industry-specific settings, for example, in Azar et al. (2018), Azar et al. (2019), and Torshizi and Clapp (2021). 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 et al. (2018). Finally, we examine asset growth, stock returns, and industry-adjusted profitability and fail 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.

Table 7

Differences-in-differences 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 coefficients on *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. *△Market 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	△Market 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	9658	9197	9714	9780	5643
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	9658	9197	9714	9780	8280
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	8956	8589	8869	9036	8479
Fiscal Year FE	Y	Y	Y	Y	Y
Firm-Merger FE	Y	Y	Y	Y	Y

Table 8

Differences-in-differences 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. *△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	△Market Share	ROA	R&D	Margin	△Market Share	ROA	R&D	Margin	△Market 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	3685	3762	3666	3457	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	2969	3004	2972	2655	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

minimum threshold in our main sample. The number of events for the 1%−2% ownership cutoff is 103, while 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 the results for regressions similar to those in Tables 5 and 7, except that we are now using the samples of 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 Δ*Market Share* around the Blackrock-BGI event (middle columns). 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 Δ*Market Share* when we use the 0.5–1% cutoff for cross-ownership).

Because the cross-ownership increases we examine 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 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

In this section, we examine the possibility that the effects of cross-ownership can be better observed at the firm-pair level. As 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, strategic alliances, and 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 acquire data on all mergers, joint ventures, and strategic alliances between publicly traded firms, for 1980 to 2015, from the SDC Mergers and Acquisition database. This represents a total of 6609 mergers and 13,721 joint ventures and strategic alliances. To include the broadest sample of firms, we relax the requirement that firms have Compustat data, thereby increasing our treatment sample to 1048 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 1048 treatment firms, 28% engage in one of these events over the three years following the financial institution merger, and this fraction drops to 15% when we consider only events involving same-industry firms. For the event to be driven by cooperation between the newly cross-owned firms, these firms must be owned by the merging financial institution. This decreases the frequency further, as shown in Panel B.

In Panel B, we report the frequencies of mergers and joint ventures or strategic alliances 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 2448 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 (0.08%) for the Treatment Pairs.[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

In this section, we explore two alternative settings used in the literature to identify exogenous changes in cross-ownership: additions to the S&P 500 index and Russell index reconstitutions. To 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&P 500

To form the sample of index additions, we obtain from CRSP the date on which any firm either joined or exited the S&P 500 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

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[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.

[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').

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	1048	
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	2492	2448
# 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&P 500 and for the firms entering the Russell2000 from the 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&P 500 and 1972 firms entering Russell2000 with non-missing market capitalization data. The corresponding numbers for benchmark firms are 776 and 1933. *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&P 500 additions				
Total Assets ($mil.)	12,039.75	3484.76	12,174.27	3317.40
Market Cap. ($mil.)	10,152.46	6768.36	7841.09	5113.11
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	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 ($mil.)	2022.17	826.55	1201.14	466.12
Market Cap. ($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	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

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&P 500 additions as an identifying event is apparent from Table 10, Panel A, which shows the descriptive statistics for 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 the 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&P 500 are not perfectly observed.[32]

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[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&P 500 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,
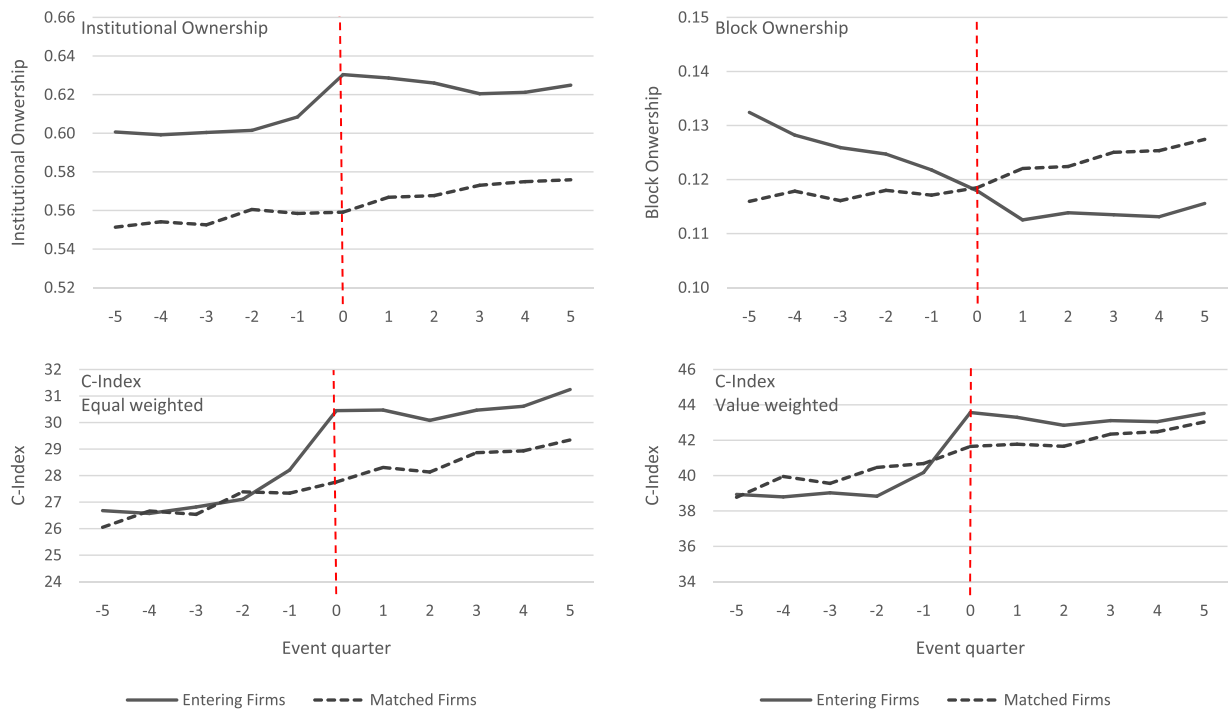
Fig. 7. Firm-level Cross-ownership Index and institutional ownership around S&P 500 additions. Entering Firms and Matched Firms are described in Section 6.1. Event quarters are quarters -5 to 5 around the entry quarter. Cross-ownership Index (C-Index) for a firm (j) is constructed by averaging the pair-level 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.

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&P 500 additions are not a useful setting for studies of cross-ownership.

6.2. Russell reconstitutions

The annual reconstitution of the Russell indices similarly influences firm ownership, so it is worthwhile to explore it as a potential instrument for cross-ownership research. Compared to entry into the S&P 500, 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 case of S&P 500 additions.

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 the Russell indices is that a firm's weight in the overall index increases discretely as the firm crosses the boundary from the Russell 1000 to 2000.[34] We obtain data from FactSet

[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 percentage. See Chang et al. (2015) for more details.

[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
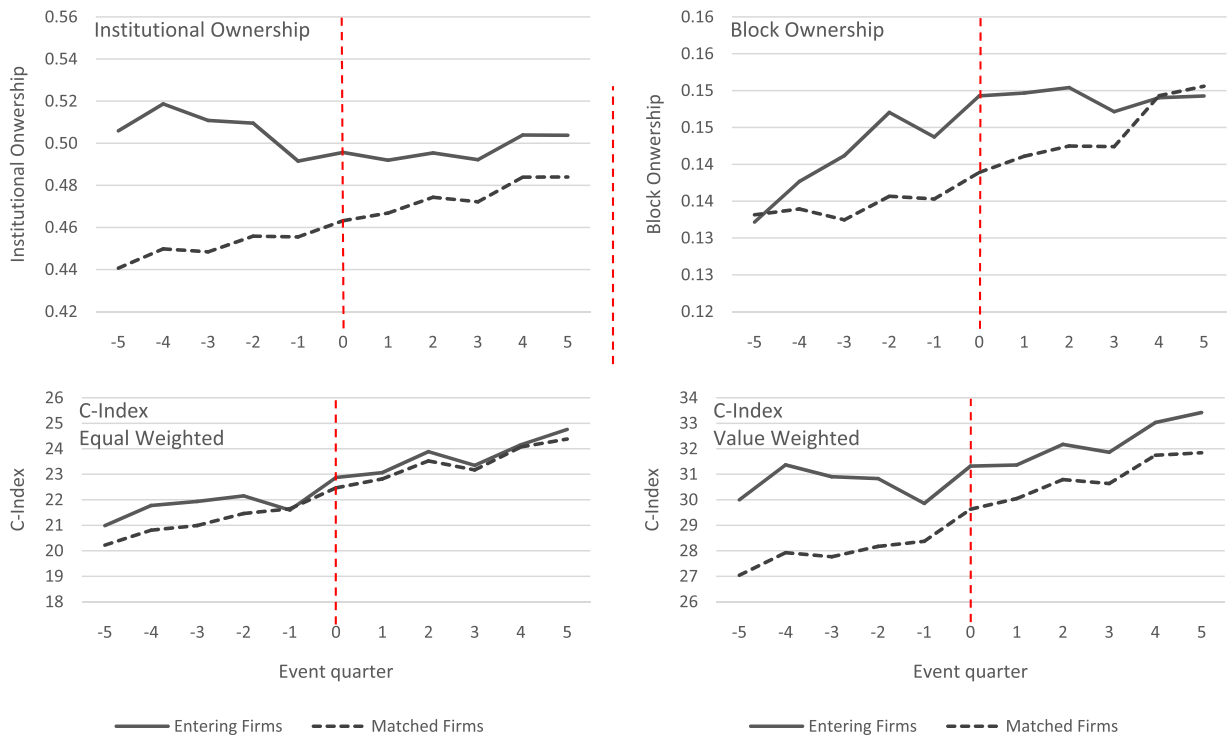
Fig. 8. Firm-level Cross-ownership Index and institutional ownership around firms' entry into Russell2000 from Russell1000. Entering Firms and Matched Firms are described in Section 6.2. Event quarters are quarters −5 to 5 around the entry quarter. Cross-ownership Index (C-Index) for a firm (*j*) is 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.

for a sample of 2081 firms that cross the 1000 boundary during the 1980 to 2015 period (we refer to these events as entries to the Russell 2000 from the Russell 1000). After requiring that 13F data and Compustat data are available, we have 1972 firm-years. Following the approach used in the S&P 500 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, the increase for broader categories of index funds or 13F institutions is less pronounced. It is therefore not surprising to see that cross-ownership by institutions is not significantly affected by these events (as with S&P 500 additions, the changes in institutional ownership and cross-ownership are mechanically 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. Conclusion

The increasing propensity of institutional investors to own shares in rival firms has led both academics and policymakers 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 are large investors that provide valuable services to shareholders, for example by pressuring firms toward better governance practices or, in the case of index funds, offering low-fees investment options to individual

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30th of each year, and they become effective on June 30th. Details are in Chang et al. (2015) and Appel et al. (2016).

[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.

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. This includes methods that rely on the Blackrock-BGI merger, the S&P additions, or the reconstitutions of Russell indices. For example, we show that tests that rely on the Blackrock-BGI merger to identify shifts in common ownership can be contaminated by the firms' differential responses to the 2008 financial crisis.

We propose two sources of identification that are less sensitive to these issues: financial institutions mergers out-

side of the 2008–2009 period, or a more complete sample of mergers with close matching of treatment and control firms. Using either of these approaches, 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	25,610	8650
10/27/2003	4/1/2004	Bank of America Corp	FleetBoston Financial Corp,MA	62,890	38,260
9/14/2008	1/1/2009	Bank of America Corp	Merrill Lynch & Co Inc	62,890	56,780
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	15,345	15,230
11/5/1984	2/1/1985	Equitable Life Assurance	Donaldson Lufkin & Jenrette	25,610	23,375
3/6/1994	6/30/1994	First Union Corp,Charlotte,NC	Evergreen Asset Management Crp	37,700	26,100
6/19/1995	1/2/1996	First Union Corp,Charlotte,NC	First Fidelity Bancorp,NJ	37,700	29,580
3/14/1999	10/1/1999	Fleet Financial Group Inc,MA	BankBoston Corp,Boston,MA	38,260	6000
8/1/1985	3/17/1986	Fleet Finl Group,Providence,RI	First Connecticut Bancorp	38,260	29,445
10/25/2000	4/10/2001	Franklin Templeton Investments	Fiduciary Trust Co Intl	39,300	28,060
4/14/2003	4/30/2003	Goldman Sachs Group Inc	Ayco Co LP	41,260	5500
4/6/2010	4/6/2010	Goldman Sachs Group Inc	Level Global Investors LP	41,260	10,194
4/5/1995	4/5/1995	Heine Securities Corp	Chase Manhattan Corp	44,438	15,230
1/14/2004	7/1/2004	JPMorgan Chase & Co	Bank One Corp,Chicago,IL	58,835	5955
4/23/2008	9/30/2008	Lehman Brothers Holdings Inc	David J Greene & Co LLC	50,200	42,120
12/6/1984	7/9/1985	Marshall & Ilsley,Milwaukee,WI	Heritage Wisconsin Corp	54,280	44,465
9/14/1992	5/21/1993	Mellon Bank Corp,Pittsburgh,PA	Boston Co	55,390	9750
1/20/1997	5/20/1997	Mellon Bank Corp,Pittsburgh,PA	Ganz Capital Management Inc	55,390	39,800
12/11/1997	4/1/1998	Mellon Bank Corp,Pittsburgh,PA	Founders Asset Management Inc	55,390	38,870
6/28/1995	1/3/1996	Morgan Stanley Asset Mgmt	Miller Anderson & Sherrerd	58,950	57,980
10/31/2006	12/4/2006	Morgan Stanley	FrontPoint Partners LLC	58,950	7759
8/30/1996	1/6/1997	NationsBank Corp,Charlotte,NC	Boatmen's Bancshares,St Louis	62,890	9480
4/13/1998	9/30/1998	NationsBank Corp,Charlotte,NC	BankAmerica Corp	62,890	5980
3/18/1992	10/15/1992	NBD Bancorp,Detroit,Michigan	INB Financial Corp	59,800	45,800
1/21/1997	11/5/1997	PIMCO Advisors LP	Oppenheimer Capital LP	70,470	67,463
11/5/1997	12/1/1997	PIMCO Advisors LP	Oppenheimer Capital LP	70,470	67,463
6/30/1986	2/27/1987	PNC Financial,Pittsburgh,PA	Citizens Fidelity, Louisville	67,600	16,575
9/22/1988	3/31/1989	PNC Financial,Pittsburgh,PA	Bank of Delaware, Wilmington	67,600	6500
9/16/1991	7/23/1992	PNC Financial,Pittsburgh,PA	First National Pennsylvania	67,600	34,640
6/23/1995	9/29/1995	New England Investment Cos LP	Harris Associates LP	63,120	43,485
9/24/1984	9/3/1985	Society Corp	Centran Corp	79,295	15,015
5/15/1991	3/16/1992	Society Corp	AmeriTrust Corp,Cleveland,OH	79,295	3700
4/27/1987	11/1/1987	Sovran Financial,Norfolk,VA	Commerce Union Corp,Nashville	80,190	19,600
9/26/1989	9/1/1990	Sovran Financial,Norfolk,VA	Citizens & Southern Georgia	80,190	16,910
7/20/1998	12/31/1998	SunTrust Banks Inc,Atlanta,GA	Crestar Finl Corp,Richmond,VA	82,355	21,650
4/6/1998	10/8/1998	Travelers Group Inc	Citicorp	84,900	16,260
10/3/2008	12/31/2008	Wells Fargo & Co	Wachovia Corp,Charlotte,NC	65,850	37,700
10/18/1995	4/1/1996	Wells Fargo Capital C	First Interstate Bancorp,CA	92,035	29,800
7/14/1986	7/15/1986	First Interstate Bancorp,CA	First Natl Bk & Tr,OK City,OK	29,800	36,140
9/17/1985	3/31/1986	Citizens and Southern GA Corp	Citizens and Southern Corp,SC	16,910	16,915
5/4/1988	12/26/1988	Boatmen's Bancshares,St Louis	Centerre Bancorp,St Louis,	9480	13,000
12/21/1983	7/1/1984	Chase Manhattan Corp	Lincoln 1st Banks Inc	15,230	51,220
7/10/1990	4/19/1991	Norwest Corp,Minneapolis,MN	United Banks of Colorado	65,800	87,815
4/14/1992	3/31/1993	BANC ONE Corp,Columbus,Ohio	Valley National Corp,Phoenix	5955	89,960
9/9/1992	7/13/1993	Bank of Boston Corp,Boston,MA	Multibank Financial Corp	6000	59,400

Table A1 (*continued*)

Announcement Date	Completion Date	Acquiror Name	Target Name	Acquirer Mgrno	Target Mgrno
3/9/1993	7/30/1993	Primerica Corp	Shearson Lehman Brothers Inc	71,300	78,685
10/18/1993	7/1/1994	First Union Corp,Charlotte,NC	Lieber & Co	37,700	50,800
11/3/1993	8/15/1994	BANC ONE Corp,Columbus,Ohio	Liberty National Bancorp	5955	50,680
11/28/1994	4/12/1995	KeyCorp,Cleveland,Ohio	Spears,Benzak,Salomon & Farrel	49,240	80,200
2/21/1995	11/30/1995	Fleet Financial Group Inc,MA	Shawmut National Corp	38,260	78,660
5/8/1995	12/27/1995	US Bancorp,Portland,Oregon	West One Bancorp,Boise,Idaho	88,855	92,150
11/20/1995	11/15/1995	Donaldson Lufkin & Jenrette	First Interstate Bancorp,CA	25,610	29,800
9/6/1996	12/12/1996	First Union Corp,Charlotte,NC	Keystone Investments Inc	37,700	49,250
6/9/1997	10/1/1997	BankAmerica Corp	Robertson Stephens & Co	5980	74,535
2/15/1999	7/6/1999	Credit Suisse Asset Management	Warburg Pincus Asset Mgmt	5720	91,450
4/30/1999	9/20/1999	Firstar Corp,Milwaukee,WI	Mercantile Bancorp,St Louis,MO	38,230	55,510
9/13/2000	12/31/2000	Chase Manhattan Corp,NY	JP Morgan & Co Inc	15,345	58,835
10/18/2000	2/14/2001	Allianz AG	Nicholas-Applegate Capt Mgmt	1275	64,240
4/16/2001	9/4/2001	First Union Corp,Charlotte,NC	Wachovia Corp,Winston-Salem,NC	37,700	91,000
5/26/2004	1/3/2005	Wells Fargo & Co	Strong Financial-Fund Asts	65,850	82,100
5/19/2005	8/4/2005	Transamerica Investment Mgmt	Westcap Investors LLC	84,750	92,160
7/7/2008	11/7/2008	RiverSource Investments LLC	J&W Seligman & Co	45,639	78,400
9/16/2008	9/22/2008	Barclays PLC	Lehman-Invest Bkg Bus	7900	50,200
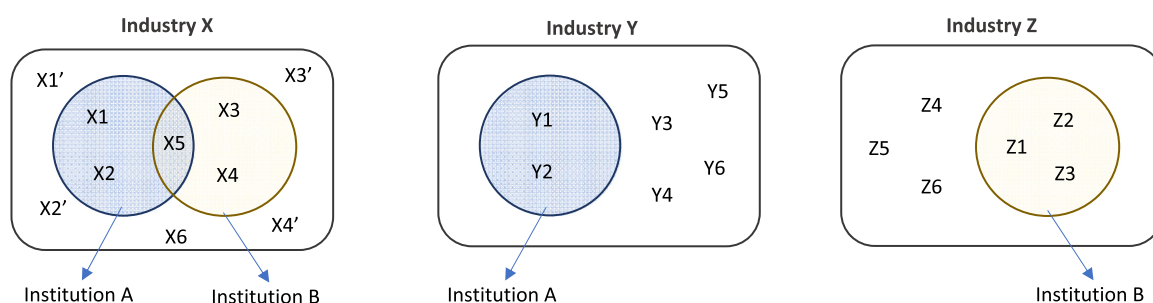
FOR PAIR-LEVEL ANALYSES:

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'

Fig. 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 at the bottom-left of the figure.

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