Do board connections between product market peers impede competition?*

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Abstract

Using a treated-control matched sample, we find that after a new direct board connection is formed to a product market peer, a firm's gross margin significantly increases by 0.8 p.p. Gross margin also rises after a new indirect board connection is formed to a product market peer through a third intermediate firm. We see consistent results when the new connections are caused by changes on the board of an intermediate firm. Such third-party initiated changes are unlikely to be related to the economic prospects of the focal firm. Consistent with the anti-competitive mechanism, board connections have positive spillover effects on closest rivals, and also the effects are stronger when the newly connected peers are located closer to each other or have more similar businesses and when the firms are in industries with greater potential benefits of collusion.

Keywords: board of directors, social networks, collusion, antitrust

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Director networks can enable the flow of information across firms and help coordinate anti-competitive behavior. Recognizing this, Section 8 of Clayton Antitrust Act of 1914 (Clayton Act) has largely prohibited firms with substantial overlap in their activities from sharing directors and officers on their boards and such practices are actively monitored by the U.S. antitrust authorities.[1] Despite its stringency and effective enforcement, the Clayton Act leaves room for director networks to serve an anti-competitive role. This is partly because of the difficulty of identifying actual product market peers and of policing indirect board connections. In this paper we map the director networks of firms that compete in the product market to explore their anti-competitive role.

We suspect that director networks might be more prevalent than expect for a few reasons. First, in a rapidly changing environment of business strategies and product lines, such as in the technology sector, it may be challenging to determine which firms compete with one another at a given point in time. [2] Second, information can flow across competing firms not necessarily only through a common director but in general through the director network. Since directors routinely sit on multiple firm boards, situations can arise when competing firms A & B are linked through a director network via another firm C, i.e., firms A & C and firms B & C can have common directors. Such situations can also arise inadvertently via corporate restructurings such as mergers or spin-offs (Blaisdell, 2019), or when investors like venture capitalists appoint same directors to their portfolio firms.

We begin by mapping the director network of competing firms to identify instances when such firms are closely linked through the network. We use the sample of all firms in the intersection of Compustat and BoardEx. We employ the Hoberg-Phillips industry

[1] For instance, on June 21, 2021, the Department of Justice (DoJ) announced that two board members of Endeavor Group Holdings Inc. have resigned their positions from the board of Live Nation Entertainment Inc. after the department expressed concerns that the two firms are direct competitors in the entertainment ticketing business and the interlocking directorate has the potential to harm competition (Department of Justice, 2021b). A newly appointed head of the DoJ's antitrust unit Jonathan Kanter highlighted in May, 2022 (Financial Times, 2022) that one area of focus for the agency is "interlocking directorates".

[2] In 2018, DoJ raised concerns about cable operator Comcast appointing executives of its NBC Universal broadcast subsidiary to the board of Hulu, a video streaming service in which Comcast held a 30% stake. As streaming was increasingly seen as competing with cable, DoJ asserted that Comcast's representatives on Hulu's board potentially ran afoul of Clayton Act (Delrahim, 2018).

classification to identify firms that compete in the product market (Hoberg and Phillips, 2010, 2016).

We employ an event study methodology to estimate the effects of director connections between product market peers on firm performance. We identify instances when a firm forms either a direct or an indirect (through an intermediate firm) board connection to a product market peer. During a twenty-year period of 1999-2018 we identify 1,493 instances of new direct connections to a product market peer, and 4,085 instances of a new indirect connection to a product market peer. The fact that we have 1,493 instances of direct board connections between firms that are potentially in the same industry – according to the Hoberg-Phillips classification – indicates the imperfect enforcement of the Clayton Act prohibition on direct board connections between competing firms. A couple of examples of firms that initiate board connections in our sample include Surgical Care Affiliates Inc. with Kindred Healthcare Inc. and Golden Entertainment Inc. with Boyd Gaming Corp.[3]

We find that the firms with board connections have more similar product descriptions – as measured using the cosine similarity score – than a random pair of firms from the same Hoberg-Phillips industry. We also find that the connected firms are more likely to be from the same Standard Industrial Classification (SIC) or Global Industry Classification Standard (GICS) industry as compared to a random pair of firms from the same Hoberg-Phillips industry. These observations indicate that board interlocks do appear between firms that have similar products and that such firms could potentially compete in the product markets.

To motivate our main analysis, we first look at how board connection relates to detected cartel cases. We find that while two directly connected firms have a probability of 0.058% of having an active detected cartel and it is 0.061% for a firm-pairs with one degree of separation, this probability becomes 0.017% for a firm-pairs with two degrees of

[3]In the first example, board interlock arose when Dr. Sharad Mansukani, who had been on the board of Surgical Care Affiliates Inc., was appointed as a director for Kindred Healthcare Inc. in 2015. In the second example, Terry Wright, a long-time independent director of Southwest Gas Holdings Inc., was appointed as an independent director to the board of Golden Entertainment Inc. in 2015. At that time the board of Southwest Gas Holdings Inc. was also directly connected to the board of Boyd Gaming Corp. via Bob Boughner. Thus, an indirect connection appeared between Golden Entertainment Inc. with Boyd Gaming Corp., which are competitors in the gaming and hospitality businesses.

separation and 0.004% for a firm-pairs with three degrees of separation. This strong associative relationship suggests the possibility of the director network's role in facilitating anti-competitive practices.

We conduct our main analysis using a difference-in-differences (DID) model. For every firm that forms a new board connection, we identify a control firm that is from the same year and the same industry as the treated firm and is the closest to the treated firm in terms of total assets, gross margin and Tobin's Q in the year before the event (treatment). We find that the set of treated and control firms are indistinguishable in terms of the matching covariates in the year before treatment. The first difference in our model is between the treated and control firms while the second difference is between the time period before and after treatment. In our empirical model, we include firm fixed effects and within-industry year fixed effects.

We mainly focus on the firm profitability as the outcome variable. We capture profitability in different ways looking at the gross margin, the operating margin, and the return on assets (ROA). We find that firm profitability significantly increases in the three years after which this firm forms a board connection to a product market peer. The increase happens both following a direct and following an indirect connections and is robust to how we measure profitability. Our estimates are economically significant. In the three years following an indirect connection to a product market peer, firm's gross margin, operating margin and ROA increase by 0.4 p.p., 0.8 p.p., and 0.6 p.p. respectively. The estimates are even larger following a direct connection to a product market peer and, respectively, are 0.8 p.p., 1.4 p.p., and 0.9 p.p. These increases represent 1.6%, 11% and 10% of the average corresponding values of the variable in question for the treated firms in the year before treatment.

Changes to the board of directors of a firm could be endogenous to the future prospects of the firm. For example, firms may appoint a director who is an expert in their industry – and consequently connected to a product market peer – if they anticipate an improvement in their performance. Also, the directors of a firm with improving prospects may be more valued in the director labor market and thus more likely to be appointed to the board of a product market peer. These arguments indicate that focusing on new connections not arising from changes to the treated firm's board may identify a more exogenous set of events. Such new connections are more likely to be unrelated to the future prospects of the treated firm. Hence, these events will enable us to better identify the causal effects of board connections to product market peers on firm performance.

To do this we focus on indirect connection events and isolate the subset of events that arise due to changes in the board of an intermediate firm or a product market peer. We deem these events more likely to be exogenous to the treated firm's future prospects. When we implement our DID model within this set of exogenous events we continue to find an increase in profitability following the initiation of an indirect board connection. Our estimates with this subset of events are similar to our estimates in the overall sample.

While our findings are consistent with board connections facilitating anti-competitive practices, the literature on director networks (see, e.g., Bouwman (2011)) suggests that board connections can also improve profitability by propagating governance practices that could enhance the internal efficiency of the firm. We next design tests to distinguish our proposed anti-competitive mechanism from this internal efficiency mechanism.

First, we implement a series of cross-sectional tests. We sort events into top and bottom halves based on a range of characteristics of the events and the treated firms, and then employ a triple-differences model. We find that connections to peers that have more similar businesses according to the cosine similarity score of firms' product descriptions and peers that are closer geographically have stronger effects on firm profitability. We also see stronger effects in industries where potential benefits of collusion are greater. Our estimates more than double when the industry exhibits above-median returns to scale. There is some evidence that more concentrated industries – as measured by the Herfindahl–Hirschman Index (HHI) – exhibit stronger effects, though the differences are not statistically significant.

We also investigate the spillover effects of board connections on the closest common rivals of newly connected firms. Suppose board connections enable anti-competitive practices such as price-fixing. In such a case, we could expect that the closest rivals operating in the same industry are able to follow so-called "umbrella pricing" and also raise prices (Bos and Harrington, 2010). However, if board connections enhance firms' internal efficiency, those rivals not involved in the newly formed network will be put at a relative disadvantage and could see their profitability worsening when faced with more efficient rivals. We find evidence supportive of the former, which is more consistent with the anti-competitive explanation.

As a robustness check, we conduct a placebo analysis. For every event firm-year in our sample, we identify a random set of firms that we refer to as pseudo peers and are from outside the firm's Hoberg-Phillips industry. We ensure that the number of pseudo peers is identical to the number of firms in the firm's Hoberg-Phillips industry. Within this sample of pseudo peers, we identify all direct and indirect board connections. We then implement our DID model within this sample of events and find no significant change in firm profitability following new board connections.

We also discuss that board overlaps might be associated with concurrent increases in within-industry common ownership (Azar, 2021). We indeed document an associative relationship between new board connections and an increase in within-industry common ownership. However, we also show that common ownership cannot fully explain the effect that board connections have on firm profitability. Our results are also robust to a number of changes to our empirical methodology.

This paper provides the very first large-sample evidence that board connections to product market peers have anti-competitive effects. Nili (2021) documents the widespread prevalence of horizontal directors in the U.S. In fact, Fan and Yang (2021) find that board connections in general reduce profit margins. Buch-Hansen (2014) uses a European sample, and finds no correlation between direct or indirect board ties and detected cartels. Westphal and Zhu (2019) surveyed a moderately-sized sample of firms, and learn that a CEO feels less uncertain about the competitive landscape in the product market if she has friends on the board of a competing firm. Geng et al. (2021) find that reducing legal risk of sharing information outside of the board of directors increases the frequency of board overlap, which is then associated with higher sales revenue and profit margins. Barone et al. (2022) show that prohibiting interlocking directorate among banks reduces the interest rates of loans extended by previously interlocked banks. Compared to this literature, we look at a broader firm sample, going beyond detected cartels, and document the pervasiveness of both direct and, importantly, indirect board overlap between firms in the same product space and its positive effects on connected firms' profitability.

This paper also contributes to the literature on the vehicles that facilitate anti-competitive practices. Recent literature has extensively looked at whether sharing common investors between the firms contributes to higher markups (see, e.g., Azar et al. (2018); Anton et al. (2021)). Tacit coordination can also be achieved with financial documents (Bourveau et al., 2020). Moreover, Ha et al. (2021) find that directors exploit their monitoring role and design executive compensation schemes that motivate collusion. We highlight board overlaps as one of the forms how tacit coordination in product markets can be made easier.

In addition, this paper speaks to our understanding of the dual role of directors as both advisors and monitors in a firm (Güner et al., 2008; Adams and Ferreira, 2009; Duchin et al., 2010; Dass et al., 2013; Drobetz et al., 2018; Gopalan et al., 2021) and focuses on how such roles change when the boards of directors can be used to coordinate product market behavior between the competing firms. Relatedly, Campello et al. (2017) show that independent directors suffer personal costs from cartel prosecutions and they take actions to mitigate those costs.

Finally, we add to the literature on social networks and, in particular, the network of directors. At the firm level, prior research has found that the network of directors enhances firm value (Bakke et al., 2021), affects investment decisions (Fracassi and Tate, 2012; Chuluun et al., 2017), disclosure (Intintoli et al., 2018), and governance policies (Coles et al., 2020; Renneboog and Zhao, 2014; Bouwman, 2011), and is associated with better merger outcomes (Cai and Sevilir, 2012; El-Khatib et al., 2015), more intellectual property leakage (Cabezon and Hoberg, 2022), and greater stock price synchronicity (Khanna and Thomas, 2009). This network has also been shown to influence director-level outcomes. Goergen et al. (2019) provide evidence that more connected directors make more profitable insider trades, which corroborates the existence of information exchange via this network, and Intintoli et al. (2018) show that more connected directors have better career prospects.[4] We restrict our attention to connections with competitors,

[4]Prior research also focuses on understanding the incentives behind the formation of the network

and posit that this economically important class of connections are associated with better future firm profitability. We also provide a novel reduced-form identification strategy in the analysis of networks, which bypasses the concern that the formation of new connections could correlate with unobservable future prospects and allows us to identify their treatment effects.

The rest of the paper is organized as the following. We state our hypothesis in Section 1. Section 2 describes the data and sample construction. Section 3 contains results from the difference-in-difference estimation. Section 4 addresses the endogeneity concerns. Section 5 establishes the mechanism with a placebo test and examination of the effects in the cross-section. Section 6 delineates our robustness tests. Section 7 concludes with our discussion over the policy implications of our results.

1 Hypothesis Development

Successful coordination among competitors yields monopolistic profits, which can be divided among these competitors and exceed their respective profits under oligopolistic competition. Such coordination can come in the form of price-fixing schemes, in which two firms competing in the same market agree to fix the price at a higher level, or market allocation, in which competing firms agree to each serve a separate product category, geographic area, or demographic group.

Although the benefits might be substantial to the shareholders of participating firms, successful coordination is hard to achieve for several reasons. First, an equilibrium with successful tacit coordination might be challenging to sustain, as it can be optimal for the participating firms to deviate and engage in predatory behaviors, such as cutting prices and entering into its competitor's market segment (Wiseman, 2017). Second, communication channels among competing firms might be imperfect, so crucial competition-sensitive information such as distribution, marketing, and pricing schemes might not reach or be trusted by the rival decision makers (Kandori and Matsushima, 1998; Genesove and

(Azar, 2021; Gualdani, 2021) and implications for sociology and legal studies (Chu and Davis, 2016; Nili, 2019).

Mullin, 2001; Awaya and Krishna, 2016). Third, explicit collusion is illegal and suspected colluding firms might face legal actions (Department of Justice and Federal Trade Commission, 2000).

We argue that board connections is one way to facilitate anti-competitive practices by alleviating aforementioned hurdles to successful coordination. Board connections might give opportunities for direct communication between the competing firms about their product market strategies or labor and supply chain policies. Moreover, the professional and personal interactions between directors can help build trust among competing firms and make deviation from coordination less likely to occur. In this sense, board connections can be considered as a kind of relational contract as in Baker et al. (2002). Also, even the director interactions on the boards of other unrelated firms could help improve coordination. Observing the rival firms' director voting behavior on third boards could improve understanding how decisions in the rival firms are made, which could help internalize that into more informed reaction functions for firms' strategic interactions.[5]

While the Clayton Act prohibits interlocking directorates among competing firms, it falls unto the burden of regulators to consider whether two firms share the same product market and can be perceived to be competing. In today's overlapping product markets, product market definition is often challenging.[6]

Based on the above discussions, we hypothesize that, holding all else constant, board connections to product market peers lead to a higher gross profit margin. Nonetheless, there are several reasons why it might not be the case. First, one may argue that the role of directors is to monitor the behavior of managers and does not entail interfering with firms' product market strategies or labor and supply chain policies. Second, when the potential gains from collusion are large, firms might have found alternative vehicles to

[5]Directors might also be better aware of other firms' financial policies, and influence them to be less aggressive, in turn making the strategic competition less fierce.

[6]As an example, in its response to the inquiry to the United Kingdom's Competition Market Authority, Facebook said that it saw its market share as the "time captured by Facebook as a percentage of total user time spent on the internet, including social media, dating, news, and search platforms." Similarly, in its reply to the Antitrust Subcommittee's report, Amazon (2020) reported that it "accounts for less than 1% of the $25 trillion global retail market and less than 4% of retail in the U.S.", suggesting that it defines its relevant market as not only online but also offline retail market. In fact, when Antitrust Subcommittee requested Amazon for "a list of the Company's top ten competitors," Amazon identified 1,700 companies, including "a discount surgical supply distributor and a beef jerky company."

facilitate and sustain coordination, so the treatment of board connections would have null effects. Third, board connections might be correlated with busier directors simultaneously sitting on more boards, which might hinder directors' ability to perform their duty well in a single firm (Core et al., 1999; Fich and Shivdasani, 2006). Hence, it remains an empirical question whether board connections to product market peers can lead to easier coordination in product markets and thus superior profitability.

2 Data and Sample Description

2.1 Data

We primarily draw our data from three sources: Compustat, BoardEx, and the Hoberg-Phillips Data Library. We put the following restrictions to identify a sample from the set of firms in the intersection of Compustat and BoardEx: (1) the firm is not in the financial and utilities industry (SIC between 6000 and 6999, or SIC between 4900 and 4999); (2) the firm-year has inflation-adjusted total assets above \$10 million and sales above \$4 million in 2018 dollars; (3) the gross margin and operating margin for the firm-year are both above -50%.

To construct the network of directors, we start from the Individual Profile Employment dataset provided by BoardEx and we keep only those entries where the type of employment is a board position. From this raw data, we construct annual network snapshots, with the nodes as the firms, and the edges as the pairwise direct connections (i.e., interlocking directorates).

We then identify board connections between firms that are product market peers. We classify firms as product market peers using the 10-K Text-based Network Industry Classifications (TNIC) provided in the Hoberg-Phillips Data Library (further: Hoberg-Phillips). According to this classification, a firm's set of competitors is determined by calculating the textual similarity score of the firm's 10-K product descriptions with all other publicly listed firms and retaining those to which the similarity score is above a certain threshold (Hoberg and Phillips, 2010, 2016).

Our main variables are defined as follows. We define assets as the natural logarithm of the firm's total assets in millions of dollars, gross margin as the ratio of gross profit to sales, operating margin as the ratio of operating income before depreciation and amortization to sales, ROA as the ratio of operating income before depreciation and amortization to total assets, sales growth as the percentage change of sales relative to the prior year, and Tobin's Q as the ratio of market value of equity plus book value of debt over total assets.[7] All financial and accounting variables are winsorized at the 1% and 99% percentiles.

2.2 Events of changes in board connections

Our main empirical exercise is an event study of instances when a firm forms a new board connection to a product market peer. We focus on both direct and indirect board connections between product market peers. We define that two firms have a direct board connection if they share a director, and we define that two firms have an indirect board connection if they do not share any board members directly but they have at least one member of their respective boards serve on the board of a third firm. We expect that forming a new *direct* board connection to a product market peer will correspond to a stronger treatment effect on the firm profitability than forming an *indirect* one.

Our treated sample consists of all firms that form a new direct or indirect board connection with a product market peer during the period 1999-2018. We study these firms for the 7-year period around the year when they form the new connection. When identifying these instances of newly formed board connections, we ensure that the firm does not have any prior indirect or direct board connection with their newly connected peers. We further ensure that in the instances of forming an indirect connection the firm does not concurrently form a direct connection with any of its peers.

We study how the firm profitability, sales growth and Tobin's Q change following the formation of the new board connection. To control for the general industry trends in the outcome variables, for each treated firm – i.e., the one that forms a new board connection with a product market peer – we include a control firm that is from the same industry and has similar firm characteristics in the year before treatment.

[7] Please see Table A1 in the Appendix for the definitions of all variables we use in our analysis.

More specifically, for each treated firm-year, we look for one control firm-year, and we match with replacements. The matching takes the following steps. First, following Fracassi and Tate (2012), we require that the control firm is in the same Fama-French 17 industry as the treated firm, and the control firm itself is not treated in the event year. Second, we look for matches in the same quantiles of assets, gross margin and Tobin's Q and have the smallest Mahalanobis distance to the treated firm using these three variables. The matching uses firm characteristics of the year before treatment. Finally, we retain the one candidate control firm with the smallest Mahalanobis distance to the treated firm. That forms each cohort of treated and control firm.

Our sample comprises of a stacked set of these cohorts of treated and control firms for the treatment year, the three-year period before, and the three-year period after treatment, i.e., from year -3 to year +3 where year 0 refers to the treatment year.

2.3 Sample description

Our final sample consists of 1,493 events of new direct connections to product market peers, and 4,085 events of new indirect connections to product market peers via an intermediate firm. Table 1 reports the distribution of the treated firms' industries. The five industries with most events are Business Services (accounting for 20.6% of all events), Electronic Equipment (13.0%), Pharmaceutical Products (12.3%), Medical Equipment (8.5%), and Computers (7.7%).

Table 2 reports the summary statistics of the treated and control firms in the year prior to the treatment (i.e., the year for which firm characteristics are used in the matching procedure). As can be seen from this table, the treated firms and control firms are balanced in terms of the variables used in matching. In particular, this table shows that the treated and control firms do not have a pre-existing difference in terms of our main outcome variable, gross margin. In terms of the other co-variates, we find that the treated firms have lower average operating margin, lower average ROA and higher average sales growth as compared to the control firms in the year before treatment. Thus if anything, the treated firms appear to under-perform the control firms in terms of the profitability and sales growth.[8]

In Figure 3, we compare the product market similarity of the pairs of newly connected firms to that of all firm pairs in an Hoberg-Phillips industry. Panel A studies direct connections and Panel B studies indirect connections. In both Panels, the green line plots the distribution of the cosine similarity of the newly connected pairs of firms while the orange line plots the distribution of the cosine similarity of all firm-pairs in an Hoberg-Phillips industry. We find that not only does the green line lie to the right of the orange line but its peak is also to the right of the peak of the orange line. This observation highlights that the newly connected pairs of firms on average tend to have a higher cosine similarity score than a random set of Hoberg-Phillips industry peers. Thus the products of the newly connected pairs of firms appear more similar – at least in their descriptions – as compared to a random set of Hoberg-Phillips industry peers.

In Table 3, we check to see that the likelihood that the newly connected pairs of firms are in the same SIC or GICS industry. We also compare that likelihood to the probability a random set of Hoberg-Phillips industry peers are in the same SIC or GICS industry. Our comparison indicates that the newly connected pairs of firms are more likely to be in the same SIC and GICS industry as compared to an average pair of product market peers and this holds for both direct and indirect connections. If anything, the fractions are even slightly larger for the indirect new connections.

In sum, the evidence in both Figure 3 and Table 3 indicates that the new board connections are between pairs of firms that have more similar products than an average pair of Hoberg-Phillips product market peers.

2.4 Convicted cartels

To motivate our main analysis, we first look at how board connection relates to actual detected cartel cases. We acknowledge the caveats of studying detected cartels, as only

[8] As we will discuss in Section 3.2, we do not see differential pre-trends in outcome variables between treated and control firms, which makes it unlikely that any residual unbalancedness in firm characteristics is driving our main results. Nevertheless, in robustness tests that we describe in Section 6.2.3 we repeat our analysis after including these variables one at a time in our list of matching co-variates and find our results to be robust.

about 10% to 30% of all cartel conspiracies are discovered (Connor, 2014), and those detected ones may not be the most economically important ones. Hence, the analysis in this section is only suggestive.

We obtain information on convicted cartels from the Private International Cartels database (Connor, 2020). We restrict the sample to firms headquartered in the US only, and hand-match those firms to the universe of firms we described in Section 2.2. Equipped with these cartels cases, we construct a firm-pair-year level indicator of whether two firms are in an active detected cartel in a certain year. We also construct the minimum distance of each firm-pair in the network described in Section 2, which is the minimum number of edges (i.e., interlocking directorates) between two nodes (i.e., firms) that can connect these two nodes together. We excluded firm-pairs that are unconnected in the director network or connected but with a minimum distance above five.

In Figure 1 we plot the probability of a firm-pair having an active cartel in a certain year, conditional on the distance of these two firms in the director network. We find that while two directly connected firms (distance of one) have a probability of 0.058% of having an active detected cartel and it is 0.061% for a firm-pairs with one degree of separation (distance of two), this probability becomes 0.017% for a firm-pairs with two degrees of separation (distance of three) and 0.004% for a firm-pairs with three degrees of separation (distance of four). This strong associative relationship suggests the possibility of the director network's role in facilitating anti-competitive practices.

We defer a more detailed description of analysis using detected cartel cases to Section IA.3 in the Internet Appendix.

3 Main Results

3.1 Difference-in-differences regression

Our objective is to estimate the impact of new board connections to product market peers on firm performance and value. To do this, we estimate a difference-in-differences model within our sample of treated and control firms. The first difference is taken between the time period before and the period after the treatment while the second difference is between the treated and control firms. Our empirical model can be represented as:

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times DirectTreated_{i,c} + \alpha_3 \times IndirectTreated_{i,c} \\ &+ \beta_1 \times DirectTreated_{i,c} \times Post_{c,t} + \beta_2 \times IndirectTreated_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t}, \end{aligned} \tag{1}$$

where i is the index for each firm, j is the index for each industry, c is the index for each cohort which consists of all observations of a treated firm and its matched control, and t is the index for each calendar year. $Y_{i,j,c,t}$ is one of our outcome variables: gross margin, operating margin, ROA, sales growth, and Tobin's Q. $Post_{c,t}$ is a dummy variable that takes a value of 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year[9]. $DirectTreated_{i,c}$ is a dummy variable equal to one for the treated firms that experience a direct board connection to a product market peer while $IndirectTreated_{i,c}$ is a dummy variable equal to one for the treated firms that experience a new indirect board connection to a product market peer[10]. Our coefficients of interest are β_1 and β_2. They identify the change in the outcome variable for treated firms that respectively form a direct and an indirect board connection with a product market peer.

We include two sets of fixed effects. First, we include firm fixed effects θ_i. [11] Next, although our control firms are from the same Fama-French 17-industry classification as the treated firms, we take a conservative approach and also include industry-times-calendar year fixed effects, $\theta_{j,t}$ in our regressions to further control for industry specific shocks, for which we use the Fama-French 48-industry classification to identify a firm's industry.[12]

Throughout the paper, we cluster standard errors at the firm level to avoid serial

[9] $Post_{c,t}$ is not absorbed by industry-times-calendar year fixed effects because treatments occur in different years for different cohorts.

[10] $DirectTreated_{i,c}$ and $IndirectTreated_{i,c}$ are not absorbed by firm fixed effects only because a firm might be treated with a direct new connection in one year and act as a control or be treated with an indirect new connection in another year.

[11] Section 6.3 shows that our results are unaffected if instead we use firm-cohort fixed effects as in Gormley and Matsa (2011).

[12] Section 6.3 shows that our results are unaffected if instead we use SIC-3 or FIC-200 classification, where the latter is a version of the Hoberg-Phillips 10-K Text-based Fixed Industry Classifications.

correlation within a firm affecting our statistical inference (Bertrand et al., 2004). Our methodology of pooling cohorts of treated and control observations together and estimating a difference-in-differences model bears resemblance to Gormley and Matsa (2011), Deshpande and Li (2019), and Cengiz et al. (2019).

Table 4 reports the results from estimating the above regression in our sample. From the first three columns we see that profitability uniformly increases in the three years after the firm forms a new board connection with its product market peer. There is also some weak evidence that the increase in profitability is greater following a direct connection as compared to that following an indirect connection.

Our estimates are economically meaningful. From column (1) we see that the gross margin increases by 0.8 p.p. for a firm that forms a direct board connection with a product market peer. This is a 1.6% of the mean gross margin of the treated firms in the pre-treatment year. As reported in columns (2)-(3), our estimates of the increase in operating margin and ROA for a firm that forms a direct connection constitute a much larger 11% and 10% of their respective mean values in the pre-treatment year.

In column (4), we see that sales growth decreases by 2.3 p.p. after a firm forms a new direct connection with an industry peer. We find no statistically significant effect on sales growth after a firm forms an indirect connection with a product market peer. Overall, these results are consistent with firms limiting their output while increasing their profitability following new board connections to their product market peers.

From column (5), we find that there is no statistically significant effect of new board connections on Tobin's Q. This is consistent with the results in Larcker et al. (2013), who show that firms with more connected boards earn superior risk-adjusted stock returns, suggesting that the market may not adequately incorporate the potential benefits of board network connections in the stock price.

3.2 Dynamic specification

We next document the dynamics of the change in performance around new board connections. These tests should also allow us study if there are any differential pre-trends in the outcome variables between the treated and control firms. To do this, we estimate the following regression within our sample:

$$\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times DirectTreated_{i,c} + \alpha_2 \times IndirectTreated_{i,c} \\
&+ \sum_{s=-3}^{-2} \beta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \beta_s \times \mathbb{1}(s=\tau)_{c,t} \\
&+ DirectTreated_{i,c} \times (\sum_{s=-3}^{-2} \gamma_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \gamma_s \times \mathbb{1}(s=\tau)_{c,t}) \\
&+ IndirectTreated_{i,c} \times (\sum_{s=-3}^{-2} \delta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \delta_s \times \mathbb{1}(s=\tau)_{c,t}) \\
&+ \theta_i + \theta_{j,t} + e_{i,j,c,t}.
\end{aligned} \tag{2}$$

where t represents the calendar year amd τ represents the year relative to the treatment. $\mathbb{1}(s=\tau)_{c,t}$ is a dummy variable that turns on if the observation is s years before the treatment (for $s=-3,-2$) or if the observation is s years after the treatment (for $s=0,1,2,3$). We omit the dummy variables for the year prior to the event, i.e., $\tau=-1$, which forms the baseline year. Thus all the effects we document are relative to this year. The estimates of γ_s and δ_s capture the difference in outcome variables between treated and control firms of year s relative to their differences in the baseline year.

We plot the coefficient estimates of γ_s and δ_s in Figure 4. Panels A1 and A2 report that there is no significant difference in the gross margin between the treated and control firms in the years before treatment for both direct and indirect connection events. This confirms the lack of pre-trends. Focusing on Panel A1, which reports the results for the direct connections, we find that the gross margin of treated firms increases significantly starting from the second year following the formation of a new connection. Furthermore, the magnitude of the increase gets larger in the third year. This is consistent with the new director taking some time to get used to understanding the firm(s) and the board(s) before having an impact on performance. In Panel A2, we report the results for the indirect connections, showing a similar pattern, albeit smaller in economic magnitude. Also, from Panels B1, B2, C1, and C2, we find that similar patterns are present for operating margin and ROA.

4 Third-Party Initiated Board Connection Changes

New board connections to a product market peer can arise either from changes to a firm's board or from changes to the board of a connected firm. Indeed, new connections arising from changes to a firm's board could be endogenous. For example, firms may appoint a director who is an expert in their industry – and consequently connected to a product market peer – if they anticipate an improvement in their performance. Also, the directors of a firm whose prospects are improving may be more valued in the director labor market and thus more likely to be appointed to the board of a product market peer. These arguments indicate that focusing on new connections arising from changes to a non-treated firm's board may identify a more exogenous set of events. This may enable us to better identify the causal effect of board connections to product market peers on firm performance.

To identify new board connections initiated due to changes on the board of a third firm, we focus on indirect connections. That is, we look at the indirect board connections to product market peers that are initiated due to the changes in the board of either the intermediate firm or the product market peer. In these events, the treated firm is already directly connected with the intermediate firm prior to the treatment year. In the treatment year, there is a change in the board of either the intermediate firm or the product market peer. That is we focus on instances when the (1) intermediate firm appoints a new director who is also on the board of a product market peer of the treated firm, or (2) a product market peer appoints a director who is also on the board of the intermediate firm. Consequently, the treated firm forms a new indirect connection to a product market peer via the intermediate firm. At the same time, the treated firm does not form any direct or indirect connection to its product market peers during the event year that results from changes on its own board. Thus, in these events, the new connection between the treated firm and its peer is not due to any changes to the board composition of the treated firm. To this extent we expect these connections to be exogenous to the future prospects of the treated firm.

Figure 2 illustrates an example to fix ideas. In this example, Firm 3 is a product market peer of Firm 1. In the year prior to the event, Firm 1 and an intermediate firm, Firm 2, are directly connected through Director A who serves on the boards of both firms. Firm 1 does not have any direct or indirect connection to Firm 3. In the event year, Firm 2 forms a new direct connection with Firm 3 via Director B. This can be either due to Firm 2 newly appointing Director B, who is also on the board of Firm 3, or due to Director B, who has always been on the board of Firm 2, additionally taking on a board position in Firm 3. Either way, Firm 1 now gets to have an indirect connection with Firm 3, and this new connection is purely a result of change in the composition of the board of directors of either the intermediate firm or a product market peer. It is not due to any change in the composition of Firm 1's board or due to change in the directorships of any of its directors.

Out of the 4,085 events of indirect board connections in our overall sample, we find that 2,114 are initiated due to changes on the board of a firm other than the treated firm. Within this subsample we estimate a regression (4) and present the results in Table 5.

Consistent with our prior evidence, in column (1) we see that gross margin of treated firm increases by 0.7 p.p. in the three years following the initiation of an indirect board connection to a product market peer. As a comparison, our baseline results that include both exogenous and potentially endogenous board connections reveal that gross margin increases by 0.4 p.p. following a new indirect board connection to a product market peer. This suggests that, if anything, the endogenous nature of changes to the board composition of a treated firm appears to bias our baseline estimates downward. From columns (2) and (3), we find that consistent with our baseline estimates our results are not sensitive to how we measure firm profitability.

Our results are economically significant. The increase in operating margin and ROA are 8.5% and 10% of the mean values for the treated firms in the year before treatment. In unreported tests, we find that the exogenous board connections to product market peers do not have a significant effect on Sales Growth or Tobin's Q.

In Figure 5, we present the corresponding results of estimating the dynamic model presented in equation (7) on the subsample of exogenous board connections. We find that across the measures of profitability there is no pre-existing difference between the treated and control firms. We also show that the profitability significantly increases in the three years following the initiation of a new indirect board connection, independent of the measures employed.

5 Mechanism

Our results are consistent with the interpretation that board connections to product market peers may facilitate anti-competitive practices among competing firms.

While anti-competitive practices enabled by board connections can come in a wide variety of forms, strategies, and markets, we acknowledge that board connections can improve a firm's profitability without anti-competitive coordination. For instance, Bouwman (2011) shows that good corporate governance practices can propagate across firms via the network of directors. The newly appointed board members might also have connections to regulators and thus be of high demand among industry peers and help improve their independent performance through "revolving doors" (Emery and Faccio, 2021). If these practices that spread through board connections can enhance a firm's internal efficiency, board connections can have positive effects on firm profitability, but such an effect would not necessarily be of a concern for antitrust policymakers.

We have already documented that the sales growth decreases after the new connections, which is unlikely to be consistent with the efficiency-enhancing explanation. To further disentangle between the anti-competitive and the internal efficiency mechanisms, we first examine the effects of new board connections using a range of cross-sectional tests. Next, we investigate the spillover effects of new board connections to the closest rivals of those newly connected firms.

5.1 Cross-sectional heterogeneities

In this section we perform four cross-sectional tests to further investigate the mechanism behind our main results. Specifically, we examine how the effects of board connections vary with: (1) the similarity in businesses between the treated firm and its new connections; (2) the geographical distance between the treated firm and its new connections; (3) the

HHI of the industry that the treated firm is in; (4) the returns to scale of the industry that the treated firm is in.

It is reasonable to expect firms with more similar businesses or that are geographically closer to be closer in terms of their product markets, so board connection can yield greater product market coordination benefits for them. Such benefits are also likely to be greater when the industry is more concentrated or exhibits greater returns to scale. Hence we expect our results to be stronger among peers which are closer in terms of their products, geographic location and in industries with a larger HHI and greater returns to scale.

To test our predictions, we estimate the following triple-differences model:

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times Treated_{i,c} + \alpha_3 \times EventCharacteristic_c \\ &+ \alpha_4 \times Post_{c,t} \times EventCharacteristic_c + \alpha_5 \times Treated_{i,c} \times EventCharacteristic_c \\ &+ \beta_1 \times Treated_{i,c} \times Post_{c,t} + +\beta_2 \times EventCharacteristic_c \times Treated_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t}, \end{aligned} \tag{3}$$

where $EventCharacteristic_c$ is a sorting variable in the cross-section. It equals to one for the time series of both the treated firm and its control firm if the new connections are in the top or bottom half in terms of a certain characteristic of the new connections.

5.1.1 By similarity in businesses between new connections

To test if the degree of similarity in businesses between the connected firms matters, we first sort the events based on the cosine similarity scores between the treated firm and its new connections. We then define a dummy variable, $TopSimilarity$, which equals to one for the treated and control firms involved in events in which the new connections have a cosine similarity score above the sample median and are in the same SIC-3 industry. It equals to zero otherwise. By pooling information about business similarity from both text-based scores and SIC, we obtain a rather sharp sorting variable in the cross-section. We then implement the triple-differences regression model (3).

Panel A of Table 6 reports results from our triple-difference regressions. For brevity, we pool events of new direct and indirect connections together and we only report the coefficients on the double-difference terms and the triple-difference terms. Consistent with the results in Section 3, the coefficients on the double-difference terms are positive for all three outcome variables and significant for operating margin and ROA, supporting that board connections to product market peers improve profitability. The triple-difference terms are always positive and are significant for operating margin and ROA. New board connections lead to a 0.3 p.p. (0.6 p.p. / 0.5 p.p.) increase in gross margin (operating margin / ROA) for the subset of events where $TopSimilarity$ is equal to zero, and these effects become 1.0 p.p. (2.0 p.p. / 1.3 p.p.) when $TopSimilarity$ is equal to one. There is strong evidence that effects of new board connections are stronger when the new connections are between peers that are closer in terms of their businesses.

5.1.2 By geographical distance between new connections

A firm is more likely to share the product market with its peers that are geographically closer. Such peers are also more likely to be competitors with the treated firm in the raw material and labor markets. Hence, we examine whether the effects are stronger when the new connections are geographically closer. We obtain the geographical distance between the ZIP codes (*addzip*) of the treated firm and its newly connected peer. We sort events in each year based on this distance. Then we define an indicator variable, $BottomDistance$, which equals to one for the time series of both the treated firm and its control if the events are in the bottom half in terms of the geographical distance between the treated firm and its new connections, and zero otherwise. Using this sorting variable, we estimate the triple-difference regression model (3).

Panel B of Table 6 reports results from these triple-difference regressions. The sign and magnitude of the double-difference terms are consistent with the results in Section 3. Moreover, the triple-difference terms are all positive and are significant for operating margin and ROA. The effects are about twice larger when the new board connections are between firms that are closer together. Overall, we find supportive evidence that the effects of new board connections are stronger between geographically closer peers.

5.1.3 By HHI of the treated firm's industry

Firms in more concentrated industries find it more beneficial to collude (Motta, 2004; Huck et al., 2004). To test if our results are stronger in more concentrated industries, we sort firms based on the HHI developed in Hoberg and Phillips (2016) and provided in the Hoberg-Phillips Data Library and develop an indicator $TopHHI$, which equals 1 if the firm is in the top half in terms of the HHI of its industry.

Panel C of Table 6 reports results from triple-difference regressions (3). We see that the effects are indeed stronger in more concentrated industries, but the differential effects are not statistically significant. Hence, we do not find conclusive evidence over how the effects of new board connections vary with industry concentration.

A potential reason for the lack of significance could be that firms in more concentrated industries may have other alternate, effective mechanisms to coordinate their behavior.

5.1.4 By returns to scale of the treated firm's industry

In addition, we recognize that the measures of HHI estimated based on solely publicly listed firm data might not reflect the actual degree of concentration in the industries (Ali et al. (2008)). As an alternative, we use the returns to scale in an industry as a proxy for the extent of competition. An industry is more likely to be oligopolistic if it exhibits increasing returns to scale. Following Dong et al. (2019), we estimate a two-factor Cobb-Douglas production function for each two-digit industry using data of the year 1999.[13] We classify the firms according to whether the industry in which they operate is experiencing above median returns to scale and estimate triple-difference regressions.

Panel D of Table 6 reports results from these regressions. We find that the effects of new board connections are stronger in industries that exhibit greater returns to scale. In industries with top half $ReturnstoScale$, forming new board connections is followed by an increase in gross margin (operating margin / ROA) of 0.8 p.p. (1.6 p.p. / 1.1 p.p.), while the number is 0.4 p.p. (0.5 p.p. / 0.4 p.p.) in industries with bottom half $ReturntoScale$.

[13] Please see Table A1 or Dong et al. (2019) for the detailed definitions of this measure.

5.2 Spillover effects

To distinguish between the anti-competitive and efficiency-enhancing mechanisms, we also investigate the effects of new board connections on the common closest rivals of the newly connected firms. Suppose board connections enable anti-competitive practices such as price-fixing or suppressing labor or raw material prices, we could expect that the closest rivals of the newly connected firms are also able to benefit from colluding rivals. For example, they could also raise the prices of the product to the same level as that of the product sold by newly connected firms being "under the umbrella of the cartel" (Bos and Harrington, 2010). Even if they are not aware that their rivals became connected, they could simply follow the upward pricing trend in the market caused by the connected firms and benefit as free-riders (Deneckere and Davidson, 1985). However, if board connections enhance the internal efficiency of newly connected firms, their closest rivals could be put at a disadvantage, which can translate into worse firm profitability when faced with more efficient rivals. Hence, the direction of these spillover effects can help us establish the mechanism that drives increase in profitability of treated firms.

We identify firms subject to spillover based on the events we identified in Section 2.2. Specifically, we define common rivals to be firms that are among the ten closest Hoberg-Phillips peers of both sides of the new board connections and are not treated with new board connections themselves in the event year. Next, we repeat the matching procedure as in Section 2.2 with the sole difference that we additionally require that the control firms are not subject to spillover in the event year. Our sample includes 686 unique firm-year's subject to spillover from new direct board connections, and 2,478 unique firm-year's subject to spillover from new indirect board connections.

Equipped with this matched sample, we estimate the following regression:

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times DirectSpillover_{i,c} + \alpha_3 \times IndirectSpillover_{i,c} \\ &+ \beta_1 \times DirectSpillover_{i,c} \times Post_{c,t} + \beta_2 \times IndirectSpillover_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t}. \end{aligned} \tag{4}$$

We report the results in Table 7. We find some evidence supportive of positive spillover effects. While gross margin of firms subject to spillover from direct board connections does not evolve differently relative to control after the event, their operating margin and ROA significantly increase with magnitudes of 1.0 p.p. and 0.6 p.p. When we look at the firms subject to spillovers from indirect board connections, the estimated effects of the spillovers are positive but insignificant with magnitudes around 0.2 p.p. to 0.3 p.p.. We also plot the coefficient estimates from a dynamic specification in Figure 6. Firms affected by spillover do not trend differently relative to the control firms prior to the event but consistent with Table 7 we see statistically significant and positive coefficients on the operating margin and ROA after the events when we focus on the firms subject to the spillovers from the direct board connections. Overall, these evidence corroborates the anti-competitive explanation of our main results.

6 Robustness Tests

In this section, we first conduct a placebo test using a pseudo industry classification. We also show that our main results are robust to alternative choices in the matching procedure and the specification of fixed effects. Lastly, we confirm the robustness of our main results to controlling for common ownership.

6.1 A placebo test using pseudo product market peers

In this section we design and conduct a placebo test where we replace the product market peers with a randomly chosen set of firms to see how new board connections to such firms affect firm performance.

For every firm-year in the Compustat-BoardEx merged data set, we start by generating a random group of firms that we designate as the pseudo industry corresponding to that firm-year. We use this pseudo industry in place of the Hoberg-Phillips industry to identify direct and indirect board connections. We ensure that none of the firms in the pseudo industry are actually in the Hoberg-Phillips industry and keep the size of the pseudo industry to be the same as the Hoberg-Phillips industry.

Note that some pseudo-treated firms also form an actual direct or indirect connection to a product market peer in the same year they form a pseudo connection. In the tests that follow, we control for such instances with our main independent variables *DirectTreated* $\times$ *Post* and *IndirectTreated* $\times$ *Post*.[14] We identify 67 cases of a firm forming a new direct connection to a pseudo peer and 760 cases of a firm forming a new indirect connection to a pseudo peer.

For each of the pseudo treated firm, we identify a control firm using our matching procedure. Then we estimate the regression (4) and report the results in Table 8. We fail to find a significant increase in profitability following the establishment of a board connection to a non-product market peer firm. These results confirm that the effects we document are not a mechanical effect of board connections but arise due to board connection to product market peers.

6.2 Robustness to alternative matching schemes

6.2.1 Retain two or three matches instead of one

In the tests we describe in Section 2.3, we employ one control firm for each treated firm. The choice of how many control firms to use involves a trade-off between the efficiency and bias of our estimators. By retaining more control firms, we obtain more precise estimates at the cost of potentially greater bias due to the treated and control firms being less similar.

In this section, we repeat the matching process with two or three control firms for each treated firm. As before, we require an exact match on Fama-French 17 industry and quantiles of the matching co-variates. This results in us not finding more than one control firm for some treated firms. For our 5,578 treated firms, we find 9,677 controls when we retain the two closest matches and 12,809 controls when we retain the three closest matches. As expected, we find that the matching co-variates are no longer balanced statistically in the year prior to the event. Interestingly, we find that the estimated effects

[14] In robustness tests in Table IA2 in the Internet Appendix, we repeat our tests excluding the pseudo events that coincide with a new connection to a product market peer and obtain results consistent with those reported.

of new board connections presented in Panels A and B of Table 9 are virtually identical to those in Table 4.

6.2.2 Match additionally on number of new appointments during the event year

We next address the concern that the appointment of new directors to the board by itself might have positive effects on profitability. For instance, new directors might have the incentive to put extra effort at the beginning of their tenure, possibly due to career concerns. Although this is not a concern in the tests that focus on an exogenous set of indirect connections, this is a potential concern for our baseline tests. In our main sample, treated firms appoint on average one new director during the event year, while control firms appoint an average of 0.64.

To address this concern, we refine the matching procedure and incrementally require that treated firm and its control have exactly the same number of newly appointed directors during the event year. For 706 events of new direct connections and 2,224 events of new indirect connections, an exact match can be found and this constitutes our sample in Panel C of Table 9. We see that our main results are unaffected.

6.2.3 Match additionally on other co-variates

Table 2 shows that treated and control firms are not significantly different in terms of co-variates used in matching. However, imbalance remains for other co-variates. As we do not see pre-trends in Figure 4, it is highly unlikely that any residual imbalance in firm characteristics is driving our results. Nevertheless, we conduct a robustness test to address such concerns. We additionally include one co-variate in our matching procedure each time, and report results from the new matched samples in Panels D, E, and F in Table 9, respectively for operating margin, sales growth, and ROA. Our main results hold under the new matching schemes.

6.2.4 Require that the control firm is never treated during [-3,3]

In the matching process, we require that the control firm is not treated in the event year. Nonetheless, it is possible that it might be treated during the three years prior to the event or the three years post the event. To avoid such treatment affecting our estimates, we additionally require that the control firm is never treated during the [-3, 3] window around the event year. For 1,183 events of new direct connections and 3,067 events of new indirect connections, a match can be found, which constitute our sample in Panel G of Table 9. Our main results are unaffected by this alternative matching choice.

6.3 Robustness to alternative specification of fixed effects

In all regressions we estimate, we include industry-times-year fixed effects and firm-specific fixed effects. With the industry-times-year fixed effects we can further rule out the possibility that some industry-level trends coincide with our events of new connections. Panels A and B of Table 10 show that our results are robust to using alternative industry definitions such as SIC-3 and FIC-200 to define industry-times-year fixed effects. This gives us confidence that our estimates are not capturing some industry-level common trends.

Second, we always include firm fixed effects, which specify the baseline level of the outcome variables, and so we are comparing differences in the post-to-prior changes between treated firms and controls. Panels C of Table 10 shows estimation results when we control for firm-cohort fixed effects. The sample is divided into cohorts consisting of observations for a treated firm and its matched control. Thus we have a fixed effect for each time a firm appears as a treated or a control firm in our sample. These fixed effects are more granular than firm fixed effects. Our results hold irrespective of the type of firm-specific fixed effects we use.

6.4 Board connections or common ownership?

An active ongoing debate studies the role of common ownership in firm's anti-competitive behavior (Azar et al., 2018, 2021; Nain and Wang, 2018; Koch et al., 2021). For instance, Azar et al. (2018) find a positive correlation between common ownership concentration and flight ticket prices and discuss various potential mechanisms how common ownership can affect firm behavior. Indeed, one such mechanism can be shared board connections. An increase in common ownership between the treated firm and its product market peers can be accompanied by the establishment of new board connections. For example, an investor may appoint the same directors to its portfolio firms in the same industry. Even when a common investor appoints different directors to different firms, these directors might still belong to the same network and be more likely than a random director to simultaneously sit on a third intermediate firm. As a consequence, the indirect board connections that we study in this paper can also arise. Indeed, Azar (2021) has shown a substantial overlap between firm common ownership and board interlock networks.

We further study the possibility that the profitability-enhancing effects of board connections we discover purely overlap with the common ownership. We thus conduct the following robustness test. We use the firm-pair level measures of common ownership developed in Gilje et al. (2020), i.e., GGL_{linear}, GGL_{fitted}, and GGL_{full_attn}.[15] We first examine whether new board connections are associated with an increase in common ownership. We then estimate the double-differences regressions (4) by additionally controlling for concurrent changes in within-industry common ownership.

We find that, around the treatment year, treated firms experience a larger increase in common ownership with its product market peers than control firms. The post-minus-prior increase in the mean GGL_{linear} ($GGL_{fitted}/GGL_{full\_attn}$) between treated firms and their product market peers is 1.42 (35.85/1554.59) on average. For control firms, it is 1.12 (33.85/1558.01). A two-sample t-test yields a t-statistic of 2.20 (1.42/0.09). Hence, there is an associative relationship between new board connections and concurrent increase in within-industry common ownership. We also find that, consistent with Azar (2021), the establishment of new board connections is associated with a higher *level* of common ownership.

[15]Please see Table A1 in the Appendix or Gilje et al. (2020) for detailed descriptions of these measures.

Next, we estimate the following regression,

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times Treated_{c,t} + \beta_1 \times Treated_{i,c} \times Post_{c,t} \\ &+ \gamma_1 \times \Delta(CommonOwnership)_{i,c} \times Post_{c,t} + \theta_i + \theta_{j,t} + e_{i,j,c,t}. \end{aligned} \tag{5}$$

In this regression we pool events of new direct and indirect connections together. $\Delta(CommonOwnership)_{i,c}$ is the change in the mean common ownership between a firm and all its Hoberg-Phillips product market peers from $\tau = -3, -2, -1$ to $\tau = 0, 1, 2, 3$. We scale it by its sample standard deviation. It is a constant for each time series of length 7 (from τ = -3 to +3). We report results in Table 11.

We find that, holding concurrent changes in within-industry common ownership constant, treated firms experience significantly higher growth in profitability compared to control firms. The coefficient estimates are similar to our main results in Table 4. This suggests that the profitability-enhancing effects of board connections we discover are not fully capturing effects of potential concurrent increase in within-industry common ownership.

We also find a strong positive associative relationship between changes in within-industry common ownership and the changes in profit margin. When using GGL_{linear} as the measure, a one standard deviation increase in the post-minus-prior change in within-industry common ownership is associated with 0.3 p.p. increase in the change of gross margin. While these coefficients do not necessarily bear a causal interpretation, their signs are consistent with anti-competitive effects of common ownership.

6.5 Does the type of directors matter?

Competition-sensitive information is more likely to flow across firms when directors involved in the board connections are also executives in the same firm, as such directors are likely more engaged in firms' product market decisions than an average director. Therefore, we expect stronger effects when directors linking competing firms are also executives.

We classify a director in a firm-year as a non-executive director or a director who is also an executive based on the Non-Executive Director indicator in BoardEx. Among firms in our sample, the average fraction of non-executive directors is 80.1%. We say an event of new direct connection involves executives if the director linking the newly connected firms is either an executive in the treated firm or an executive in its newly connected peer. For events of new indirect connections, suppose peers A and C are linked via intermediate firm B, if director X who connects firms A and B is an executive in A, or director Y who connects firms B and C is an executive in C, we classify this event as one that involves executives. Out of all 1,493 events of new direct connections, 1,098 only involve non-executive directors while 395 are initiated by directors who are also executives. For the 4,085 events of new indirect connections, 2,886 involve non-executive directors only and 1,199 also involve executives.

Next, we pool the four kinds of events identified above together and estimate double-differences regressions. Results are reported in Table IA3 in the Internet Appendix. We find that the effects of board connections that involve executives are consistently stronger than those that only involve non-executive directors. Moreover, we still find positive and mostly significant effects when looking at events that only involve non-executive directors, suggesting that the anti-competitive effects are not limited to executives.

6.6 Robustness to customer-supplier connections

One might argue that the Hoberg-Phillips industry classification can capture customer-supplier relationships instead of product market rivalry, as a customer firm and its supplier can also have similar languages in their business descriptions. Such connections may have positive effects on firm's profitability, for reasons outside the scope of anti-competitive practices. To address this concern, we check if any of the new board connections we identify are between customer and supplier firms. We define a firm-pair as customer-supplier if one side is a principal customer of the other, i.e., accounting for more than 10% of the total revenue of a firm[16].

We find that out of all events used in Table 4 and during 1998-2013, only 50 of them are between pairs of customer-supplier firms. We then estimate the double-differences regressions (4) by additionally controlling for such events. We report results in Table

[16] Please see Cen et al. (2016) for details on the construction of corporate customer-supplier data.

IA4 in the Internet Appendix. Our main results hold if we focus on board connections between firms that are not customer-supplier pairs. We find some weak evidence that board connections have stronger effects on profitability when the two firms are not only Hoberg-Phillips peers but also a pair of customer-supplier firms.

7 Conclusion

Taking advantage of the networks formed by interlocking directorates and the text-based Hoberg-Phillips industry classification, we find that board connections to product market peers have positive profitability implications. Specifically, a firm's gross margin rises by an average of 0.8 p.p. after forming new direct connections to product market peers and by 0.4 p.p. after forming new indirect connections to product market peers via an intermediate firm. We address endogeneity concerns by exploring the network structure and focusing on new connections that are unlikely to be correlated with future firm prospects.

It is worth noting that we remain agnostic over the specific market, strategy, and format of anti-competitive practices that board connections facilitate between peer firms. Connected firms might engage in market segmentation and target separate product category, demographic groups, or geographic areas, and wield market power in their respective market segments, or they might sell in the same market and fix the price at a high level. The coordination can come via pure information exchange, or alternatively the social network could bring trust among competing firms and make market segmentation or price-fixing more sustainable. Moreover, while we study board connections between product market peers, these peers are also likely to be peers in the raw material and labor markets due to similarity in their business models. Board connections can facilitate anti-competitive practices in these markets as well, for example, if connected firms coordinate to suppress the price of raw ingredients or wages.[17] In this paper, we acknowledge the

[17]For example, consider two competing firms sharing a supplier that produces key raw ingredients for both firms. In the absence of coordination, firms will compete by bidding up the price of the ingredients. However, with coordination, firms are able to suppress the ingredients price by agreeing not to pay any level higher. While collusive agreements among employers to not hire each other's employees or to suppress wages are deemed unlawful under the Sherman Antitrust Act and potentially a criminal offense (Department of Justice and Federal Trade Commission, 2016), legal cases (Department of Justice, 2010, 2021a) and recent research (Krueger and Ashenfelter, 2018; Krueger and Posner, 2018) suggest

possibility of a wide variety of anti-competitive practices, and we interpret our results as an aggregation of the effects of all such practices made possible by new board connections between peer firms.

We also want to point out that we cannot speak to the full extent of the board's role in anti-competitive practices or its economic consequences for firms, as we identify the effects of incremental board connections to product market peers rather than the stock of board connections. We provide robust inferences for the effects of the first, but we are unable to identify the effects of the latter due to the lack of valid (natural) experiments. Thus, it would be more valuable to view our results in a qualitative rather than quantitative way. We are also in no way quantifying the economic impacts of anti-competitive behavior among firms in general or broader welfare implications beyond consumer-welfare standard.

Still, with that said this paper has several important regulatory implications. First, our results indicate the role of directors in anti-competitive practices and provide support for the current ban of interlocking directorates between competing firms. Second, the results suggest that text-based analyses are powerful in identifying competitors in the market place and can have the potential to aid the execution of antitrust regulations. In addition, we find that indirect connections via an intermediate firm also have positive effects on profitability even though their economic magnitudes are smaller than those of direct connections. This argues for going beyond interlocking directorates and putting restraints on indirect board connections between competitors as well, especially in cases where the detrimental effects of anti-competitive practices on consumer welfare are substantial.

that such anti-competitive practices do exist. For example, on July 15, 2021, DaVita and Surgical Care Affiliates LLC, two competing healthcare providers, were accused of conspiring not to hire each other's key employees by the Department of Justice (Department of Justice, 2021a).

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Tables

Table 1: Industry distribution of events

Fama-French industry code (48 industries)	No.	%
Agriculture	3	0.1%
Food Products	16	0.3%
Candy & Soda	5	0.1%
Beer & Liquor	7	0.1%
Recreation	1	0.0%
Entertainment	45	0.8%
Printing and Publishing	22	0.4%
Consumer Goods	9	0.2%
Apparel	29	0.5%
Healthcare	206	3.7%
Medical Equipment	472	8.5%
Pharmaceutical Products	687	12.3%
Chemicals	63	1.1%
Rubber and Plastic Products	2	0.0%
Construction Materials	22	0.4%
Construction	35	0.6%
Steel Works Etc	29	0.5%
Machinery	188	3.4%
Electrical Equipment	31	0.6%
Automobiles and Trucks	35	0.6%
Aircraft	28	0.5%
Shipbuilding, Railroad Equipment	9	0.2%
Defense	5	0.1%
Non-Metallic and Industrial Metal Mining	8	0.1%
Coal	8	0.1%
Petroleum and Natural Gas	425	7.6%
Communication	2	0.0%
Personal Services	42	0.8%
Business Services	1,147	20.6%
Computers	431	7.7%
Electronic Equipment	723	13.0%
Measuring and Control Equipment	186	3.3%
Business Supplies	19	0.3%
Shipping Containers	2	0.0%
Wholesale	118	2.1%
Retail	368	6.6%
Restaraunts, Hotels, Motels	100	1.8%
Banking	4	0.1%
Insurance	2	0.0%
Trading	4	0.1%
Almost Nothing	40	0.7%
Total	5,578	100.0%

Note: This table reports the distribution of events based on the Fama-French 48-industry classification of the treated firm.

Table 2: Comparison of treated and matched control firms

	Treated				Control					
	Mean	Median	SD	N	Mean	Median	SD	N	Dif	T-stat
Variables used in matching										
Assets	6.71	6.59	1.85	5,578	6.68	6.56	1.86	5,578	0.04	1.0
Gross Margin	0.51	0.53	0.26	5,578	0.51	0.51	0.24	5,578	0.00	0.9
Tobin's Q	2.67	1.98	2.03	5,578	2.62	1.96	1.88	5,578	0.06	1.6
Variables not used in matching										
Operating Margin	0.13	0.13	0.20	5,571	0.17	0.15	0.18	5,558	–0.04	–12.0***
ROA	0.09	0.11	0.12	5,571	0.13	0.13	0.11	5,558	–0.03	–15.2***
Sales Growth	0.22	0.11	0.50	5,253	0.17	0.10	0.38	5,291	0.05	5.5***

Note: This table reports the summary statistics of the treated and control firms. All statistics are based on data of the year prior to the event, which year's data we also used in matching.

Table 3: Proportion of newly connected peers that are in the same SIC/GICS industry

	Direct new connections		**Indirect new connections**	
	Newly connected peers	All H-P peers	Newly connected peers	All H-P peers
Using SIC industry classification				
Is in the same SIC-2 industry	62.0%	50.4%	65.3%	51.1%
Is in the same SIC-3 industry	55.2%	44.7%	58.5%	45.2%
Is in the same SIC-4 industry	34.4%	26.4%	40.3%	26.5%
Using GICS industry classification				
Is in the same GGROUP industry	75.2%	61.7%	76.1%	61.6%
Is in the same GIND industry	58.6%	45.8%	61.6%	45.2%
Is in the same GSUBIND industry	44.2%	34.2%	48.9%	33.5%

Note: Columns (1) and (3) report the proportion of newly connected H-P peers that are in the same SIC/GICS industry as the treated firms. Columns (2) and (4) report the proportion of all Hoberg-Phillips peers that are in the same SIC/GICS industry as the treated firm, averaged across all the treated firms. A two-sample T-test between columns (1) and (2) or between columns (3) and (4) shows a difference significant at the 1% level.

Table 4: Double-difference regressions

VARIABLES	(1) Gross Margin	(2) Operating Margin	(3) ROA	(4) Sales Growth	(5) Tobin's Q
Post	-0.007***	-0.008***	-0.007***	-0.020***	-0.077***
	(-4.27)	(-4.77)	(-6.12)	(-5.23)	(-4.07)
DirectTreated X Post	0.008**	0.014***	0.009***	-0.023***	-0.011
	(2.45)	(3.86)	(3.52)	(-2.60)	(-0.26)
IndirectTreated X Post	0.004*	0.008***	0.007***	-0.004	-0.033
	(1.88)	(3.59)	(4.01)	(-0.75)	(-1.19)
Observations	68,690	68,534	68,602	67,033	67,904
Firm FE	Yes	Yes	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm
# of Matched Controls	1	1	1	1	1
Within R-squared	0.001	0.001	0.002	0.002	0.002
P-value from a test of the equality of effects	0.24	0.12	0.42	0.04	0.57

Note: This table reports results from the following regression using the sample of all events,

$$
\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times DirectTreated_{i,c} + \alpha_3 \times IndirectTreated_{i,c} \\
&+ \beta_1 \times DirectTreated_{i,c} \times Post_{c,t} + \beta_2 \times IndirectTreated_{i,c} \times Post_{c,t} + \theta_i + \theta_{j,t} + e_{i,j,c,t}.
\end{aligned}
$$

Here i is the index for each firm, j is the index for each industry, c is the index for each cohort which consists of all observations of a treated firm and its matched control, and t is the index for each calendar year. $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. $DirectTreated_{i,c}$ is a dummy variable equal to one for the treated firms that experience a direct board connection to a product market peer while $IndirectTreated_{i,c}$ is a dummy variable equal to one for the treated firms that experience a new indirect board connection to a product market peer. $DirectTreated_{i,c} \times Post_{c,t}$ and $IndirectTreated_{i,c} \times Post_{c,t}$ are the double-difference terms, the coefficient estimates of which are the estimated effects of new board connections with product market peers. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. We omitted coefficients of $DirectTreated_{i,c}$ and $IndirectTreated_{i,c}$ from the table. Table 10 shows that results in this table are robust to alternative specification of fixed effects. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The p-value from a test under the null hypothesis that effects of a direct connection equal the effects of an indirect board connection is also reported.

Table 5: Double-difference regressions, using the exogenous subset of events

	(1) Gross Margin	(2) Operating Margin	(3) ROA	(4) Sales Growth	(5) Tobin's Q
Post	-0.009***	-0.011***	-0.010***	-0.023***	-0.101***
	(-3.34)	(-4.23)	(-5.17)	(-3.79)	(-3.28)
ExogenousTreated X Post	0.007**	0.011***	0.009***	-0.008	-0.006
	(2.25)	(2.99)	(3.63)	(-0.95)	(-0.15)
Observations	26,065	26,016	26,032	25,473	25,777
Firm FE	Yes	Yes	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes	Yes	Yes
Clustering	Firm	Firm	Firm	Firm	Firm
# of Matched Controls	1	1	1	1	1
Within R-squared	0.001	0.002	0.002	0.002	0.002

Note: This table reports results from the following regression using the subset of events that are deemed to be exogenous,

$$
\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times ExogenousTreated_{i,c} + \\
&+ \beta_1 \times ExogenousTreated_{i,c} \times Post_{c,t} + \theta_i + \theta_{j,t} + e_{i,j,c,t}.
\end{aligned}
$$

Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. $TreatedExogenous_{i,c} * Post_{c,t}$ is the double difference term, the coefficient of which is the estimated effects of exogenous new board connection with peer firms. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. T-stats are in the parentheses. We omitted coefficients of $TreatedExogenous_{i,c}$ from the table. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effects of new connections in the cross-section

	Gross Margin	Operating Margin	ROA
Panel A: By similarity in businesses between newly connected peers			
Treated X Post	0.003	0.006**	0.005***
	(1.41)	(2.45)	(2.80)
Treated X Post X Top Score	0.007	0.014***	0.008**
	(1.44)	(2.85)	(2.52)
Observations	68,690	68,534	68,602
Panel B: By geographical distance between newly connected peers			
	Gross Margin	Operating Margin	ROA
Treated X Post	0.004	0.006**	0.005**
	(1.60)	(2.20)	(2.28)
Treated X Post X Bottom Distance	0.001	0.007*	0.005*
	(0.39)	(1.65)	(1.67)
Observations	63,289	63,136	63,203
Panel C: By HHI of the treated firm's industry			
	Gross Margin	Operating Margin	ROA
Treated X Post	0.003	0.008**	0.006***
	(1.22)	(2.58)	(2.89)
Treated X Post X Top HHI	0.004	0.004	0.002
	(0.98)	(1.02)	(0.80)
Observations	68,532	68,376	68,431
Panel D: By returns to scale of the treated firm's industry			
	Gross Margin	Operating Margin	ROA
Treated X Post	0.004	0.005*	0.004*
	(1.63)	(1.67)	(1.77)
Treated X Post X Top Return to Scale	0.004	0.011**	0.007**
	(1.02)	(2.49)	(2.01)
Observations	65,006	64,868	64,908
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
# of Matched Controls	1	1	1

Note: This table reports results from the following regression,

$$
\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times Treated_{i,c} + \alpha_3 \times EventCharacteristic_c \\
&+ \alpha_4 \times Post_{c,t} \times EventCharacteristic_c + \alpha_5 \times Treated_{i,c} \times EventCharacteristic_c \\
&+ \beta_1 \times Treated_{i,c} \times Post_{c,t} + +\beta_2 \times EventCharacteristic_c \times Treated_{i,c} \times Post_{c,t} \\
&+ \theta_i + \theta_{j,t} + e_{i,j,c,t},
\end{aligned}
$$

Here $Treated_{i,c}$ equals 1 for the time series of a firm experiencing new direct or indirect connections to product market peers, and 0 otherwise. In this regression we pool events of new direct and indirect connections together. $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. $EventCharacteristic_c$ is a sorting variable in the cross-section. It equals 1 for the time series of both the treated firm and its control if the new connections are in the top or bottom half in terms of a certain characteristic of the new connections. $Treated_{i,c} \times Post_{c,t}$ is the double-difference term, the coefficient of which is the estimated effects of new connections in the subsample where $EventCharacteristic_c$ takes the value of 0. $EventCharacteristic_c \times Treated_{i,c} \times Post_{c,t}$ is the triple-difference term, the coefficient of which is the estimated incremental effects of new connections where $EventCharacteristic_c$ takes the value of 1 relative to where $EventCharacteristic_c$ takes the value of 0. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. For brevity, only coefficients of the double-difference and triple-difference terms are reported in the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Spillover effects of board connections to closest rivals

VARIABLES	(1) Gross Margin	(2) Operating Margin	(3) ROA
Post	-0.002	-0.005***	-0.005***
	(-1.48)	(-2.61)	(-3.81)
DirectSpillover X Post	-0.002	0.010**	0.006**
	(-0.57)	(2.29)	(2.06)
IndirectSpillover X Post	0.002	0.003	0.002
	(0.93)	(1.05)	(0.86)
Observations	40,885	38,361	38,381
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
# of Matched Controls	1	1	1
Within R-squared	0.000	0.001	0.001

Note: This table reports results from the following regression using the sample of firms subject to spillover effects from new board connections,

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times DirectSpillover_{i,c} + \alpha_3 \times IndirectSpillover_{i,c} \\ &+ \beta_1 \times DirectSpillover_{i,c} \times Post_{c,t} + \beta_2 \times IndirectSpillover_{i,c} \times Post_{c,t} + \theta_i + \theta_{j,t} + e_{i,j,c,t}. \end{aligned}$$

Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. $DirectSpillover_{i,c}$ equals 1 for the time series of a firm affected by spillover from new direct connections. $IndirectSpillover_{i,c}$ equals 1 for the time series of a firm affected by spillover from new indirect connections. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. We omitted coefficients of $DirectSpillover_{i,c}$ and $IndirectSpillover_{i,c}$ from the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Double-difference regressions, using pseudo events

	(1) Gross Margin	(2) Operating Margin	(3) ROA
Post	-0.003	-0.006**	-0.006***
	(-1.33)	(-2.50)	(-3.00)
PseudoDirectTreated X Post	0.015	0.003	0.001
	(0.91)	(0.24)	(0.09)
PseudoIndirectTreated X Post	-0.000	0.002	0.004
	(-0.00)	(0.45)	(1.36)
Observations	27,337	27,271	27,281
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
# of Matched Controls	1	1	1
Within R-squared	0.000	0.001	0.001

Note: This table reports results from the following regression using the sample of events of connections to pseudo peers,

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times DirectTreated_{i,c} + \alpha_3 \times IndirectTreated_{i,c} \\ &+ \alpha_4 \times PseudoDirectTreated_{i,c} + \alpha_5 \times PseudoIndirectTreated_{i,c} \\ &+ \beta_1 \times DirectTreated_{i,c} \times Post_{c,t} + \beta_2 \times IndirectTreated_{i,c} \times Post_{c,t} \\ &+ \gamma_1 \times PseudoDirectTreated_{i,c} \times Post_{c,t} + \gamma_2 \times PseudoIndirectTreated_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t}. \end{aligned}$$

Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. $PseudoDirectTreated_{i,c}$ equals 1 for the time series of a firm experiencing direct connections to pseudo peers. $PseudoIndirectTreated_{i,c}$ equals 1 for the time series of a firm experiencing indirect connections to pseudo peers. Note that pseudo events have overlaps with events of connections to actual peers, so we control for them. $DirectTreated_{i,c}$ equals 1 for the time series of a firm experiencing direct connections to actual peers. $IndirectTreated_{i,c}$ equals 1 for the time series of a firm experiencing indirect connections to actual peers. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. We omitted coefficients of $DirectTreated_{i,c}$, $IndirectTreated_{i,c}$, $PseudoDirectTreated_{i,c}$, $PseudoIndirectTreated_{i,c}$, $DirectTreated_{i,c} \times Post_{c,t}$ and $IndirectTreated_{i,c} \times Post_{c,t}$ from the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Robustness to alternative matching schemes

	Gross Margin	Operating Margin	ROA
Panel A: Choose two controls for each event			
DirectTreated X Post	0.008***	0.014***	0.010***
	(2.65)	(4.02)	(3.91)
IndirectTreated X Post	0.004**	0.009***	0.008***
	(2.10)	(4.00)	(4.65)
Observations	94,120	93,938	94,026
Panel B: Choose three controls for each event			
DirectTreated X Post	0.008***	0.013***	0.009***
	(2.63)	(3.76)	(3.57)
IndirectTreated X Post	0.004**	0.008***	0.007***
	(2.03)	(3.66)	(4.25)
Observations	11,3475	113,225	113,319
Panel C: Match additionally on number of new appointments during the event year			
DirectTreated X Post	0.012***	0.014***	0.008**
	(2.59)	(2.86)	(2.33)
IndirectTreated X Post	0.004	0.010***	0.009***
	(1.28)	(3.28)	(3.41)
Observations	36,715	36,666	36,696
Panel D: Match additionally on Operating Margin			
DirectTreated X Post	0.007**	0.010***	0.005*
	(2.07)	(2.61)	(1.90)
IndirectTreated X Post	0.004*	0.006**	0.005**
	(1.92)	(2.35)	(2.46)
Observations	60,648	60,596	60,653
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes

Table 8: Robustness to alternative matching schemes (continued)

	Gross Margin	Operating Margin	ROA
Panel E: Match additionally on Sales Growth			
DirectTreated X Post	0.009**	0.014***	0.009***
	(2.48)	(3.48)	(3.05)
IndirectTreated X Post	0.004	0.008***	0.007***
	(1.61)	(3.11)	(3.81)
Observations	57,882	57,772	57,820
Panel F: Match additionally on ROA			
DirectTreated X Post	0.009**	0.013***	0.006**
	(2.52)	(3.37)	(2.30)
IndirectTreated X Post	0.006**	0.008***	0.005***
	(2.43)	(3.25)	(2.89)
Observations	60,604	60,552	60,618
Panel G: Require that the control firm is never treated during [–3,3]			
DirectTreated X Post	0.011***	0.015***	0.010***
	(2.68)	(3.47)	(3.12)
IndirectTreated X Post	0.005*	0.007**	0.006**
	(1.87)	(2.14)	(2.31)
Observations	52,361	52,198	52,251
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes

Note: This table report coefficients for the regression in Table 4 if alternative matching schemes are used. Panel A (B) uses a matching scheme that retains two (three) controls for each treated event. Panel C uses a matching scheme that additionally requires an exact match on the number of new appointments to the board during the event year. Panel D (E/F) uses a matching scheme that additionally matches on operating margin (sales growth/ROA). Panel G uses a matching scheme that additionally requires the control firm being never treated from τ = -3 to +3.

Table 10: Robustness to alternative fixed effects

	Gross Margin	Operating Margin	ROA
Panel A: SIC-3 X Year FE and Firm FE			
DirectTreated X Post	0.008**	0.015***	0.010***
	(2.54)	(4.03)	(4.08)
IndirectTreated X Post	0.004*	0.008***	0.008***
	(1.67)	(3.32)	(4.31)
Panel B: FIC-200 X Year FE and Firm FE			
DirectTreated X Post	0.008***	0.013***	0.008***
	(2.58)	(3.77)	(3.16)
IndirectTreated X Post	0.003	0.006***	0.006***
	(1.11)	(2.79)	(3.45)
Panel C: FF-48 X Year FE and Firm-Cohort FE			
DirectTreated X Post	0.008**	0.014***	0.009***
	(2.29)	(3.84)	(3.41)
IndirectTreated X Post	0.004*	0.008***	0.006***
	(1.81)	(3.38)	(3.74)

Note: This table report coefficients for the regression in Table 4 if alternative fixed effects are used. Regressions in Panel A use SIC 3-digit industry times year fixed effects and firm fixed effects. Regressions in Panel B use FIC-200 industry times year fixed effects and firm fixed effects. FIC-200 industry classification is provided in the Hoberg-Phillips Data Library. Regressions in Panel C use Fama-French 48-industry times year fixed effects and firm-cohort fixed effects.

Table 11: Robustness to controlling for concurrent changes in common ownership

	Gross Margin	Operating Margin	ROA
Panel A: Using GGL_{linear}			
Treated X Post	0.004	0.009***	0.008***
	(1.39)	(3.22)	(3.57)
Δ(Common Ownership) X Post	0.003**	0.005***	0.004***
	(2.49)	(3.93)	(3.71)
Panel B: Using GGL_{fitted}			
Treated X Post	0.004	0.009***	0.008***
	(1.41)	(3.25)	(3.59)
Δ(Common Ownership) X Post	0.005***	0.008***	0.004***
	(4.68)	(5.98)	(3.58)
Panel C: Using GGL_{full_attn}			
Treated X Post	0.004	0.009***	0.008***
	(1.39)	(3.22)	(3.58)
Δ(Common Ownership) X Post	0.003***	0.002*	0.000
	(2.71)	(1.86)	(0.02)
Observations	40,408	40,302	40,343
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
# of Matched Controls	1	1	1

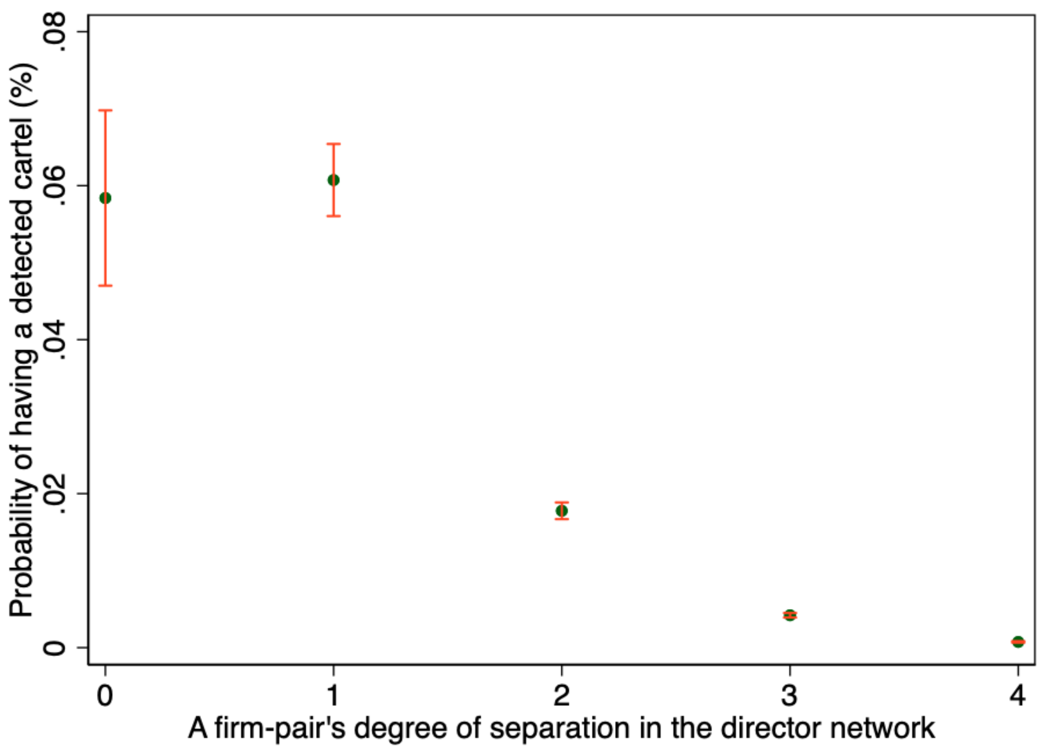
Note: This table reports results from the following regression,

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times Treated_{c,t} + \beta_1 \times Treated_{i,c} \times Post_{c,t} \\ &+ \gamma_1 \times \Delta(CommonOwnership)_{i,c} \times Post_{c,t} + \theta_i + \theta_{j,t} + e_{i,j,c,t}. \end{aligned}$$

Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. $Treated_{i,c}$ equals 1 for the time series of a firm experiencing new direct or indirect connections to product market peers, and 0 otherwise. In this regression we pool events of new direct and indirect connections together. $\Delta(CommonOwnership)_{i,c}$ is the change in the mean common ownership between a firm and all its Hoberg-Phillips product market peers from $\tau = -3, -2, -1$ to $\tau = 0, 1, 2, 3$. It is a constant for each time series of length 7 (from τ = -3 to +3). We use three firm-pair level measures of common ownership as constructed in Gilje et al. (2020), which are GGL_{linear}, GGL_{fitted}, and GGL_{full_attn}. The sample size shrinks relative to Table 4 as the measures of common ownership are only available for the years 2000-2012. For brevity, only coefficients of $Treated_{i,c} \times Post_{c,t}$ and $\Delta(CommonOwnership)_{i,c} \times Post_{c,t}$ are reported in the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

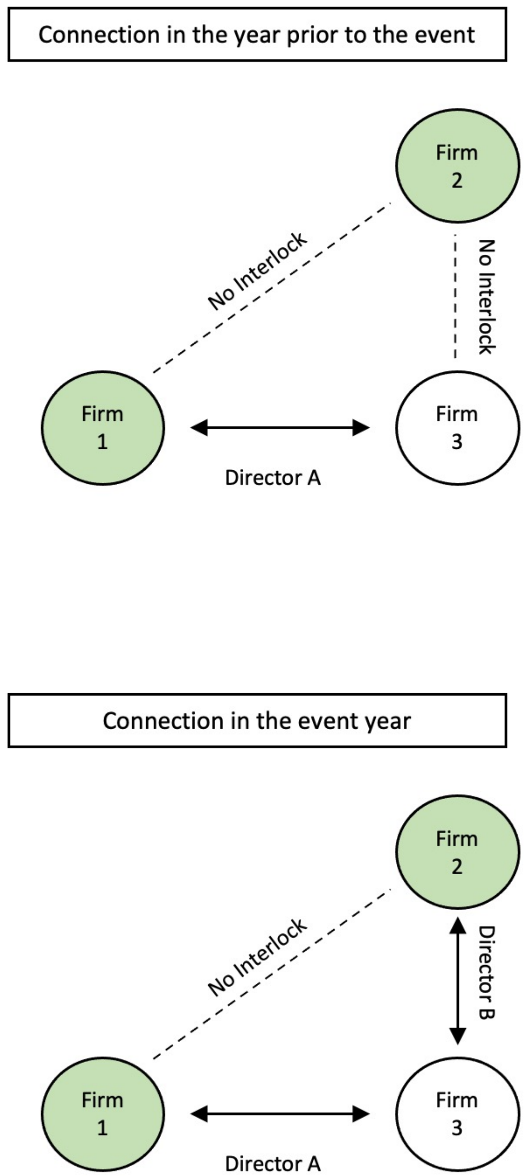
Figures

Figure 1: Director network and detected cartel cases



Note: This figure plots the probability of a firm-pair in a certain year being in an active detected cartel case, conditional on each level of minimum distance between the firm-pair in the director network.

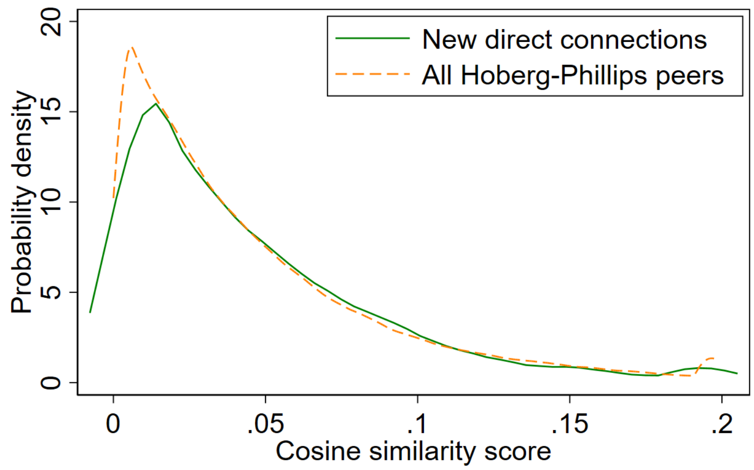
Figure 2: Illustration of third-party initiated board connection changes



Note: This figure presents the empirical distribution of the cosine similarity score between the event firms and their newly connected Hoberg-Phillips peers, along with the cosine similarity score between the event firms and all their Hoberg-Phillips peers. The scores are winsorized at the 99% percentile.

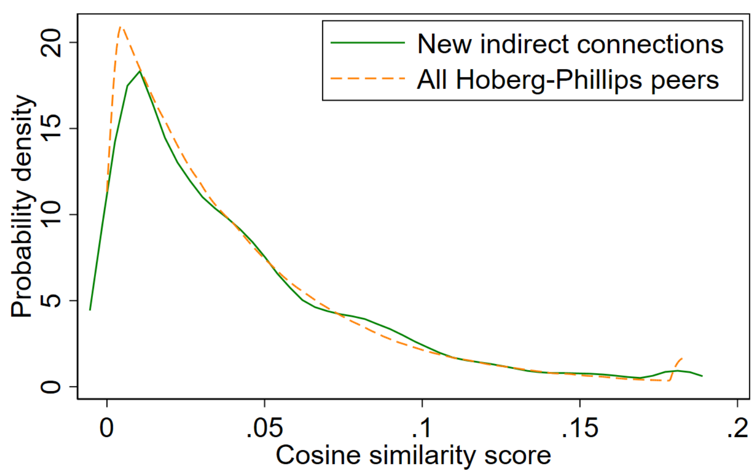
Figure 3: Distribution of cosine similarity score

Panel A: Similarity between new direct connections



Note: The average score is 0.046 for new direct connections and 0.046 for all Hoberg-Phillips peers. The p-value is 0.91 in a two-sample T-test and 0.43 in a Kolmogorov–Smirnov test.

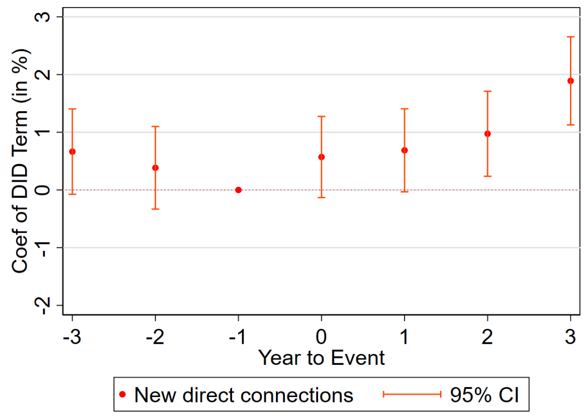
Panel B: Similarity between new indirect connections



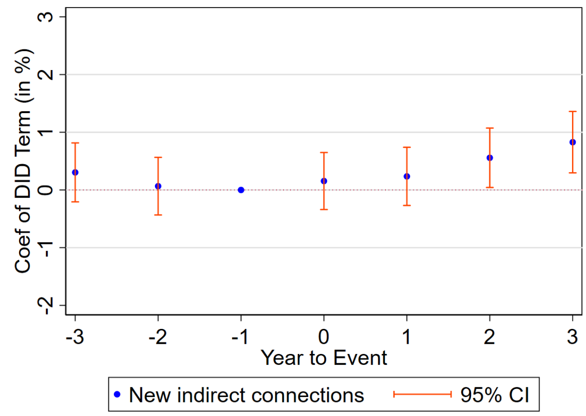
Note: The average score is 0.042 for new indirect connections and 0.041 for all Hoberg-Phillips peers. The p-value is 0.03 in a two-sample T-test and 0.01 in a Kolmogorov–Smirnov test.

Figure 4: Plots of the dynamics of the difference between treated and control firms

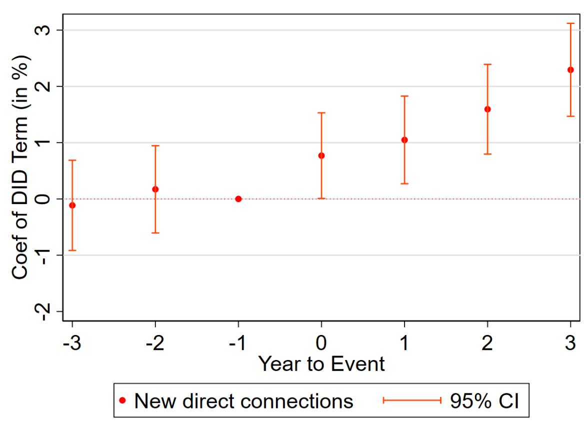
Panel A1: Gross margin, direct



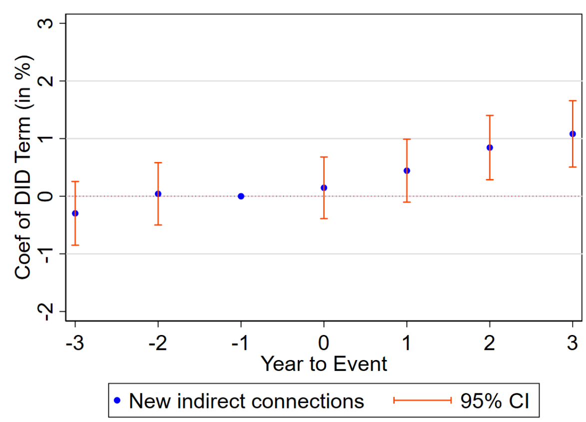
Panel A2: Gross margin, indirect



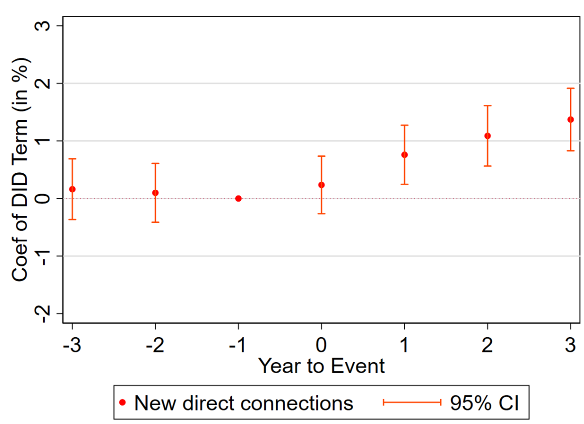
Panel B1: Operating margin, direct



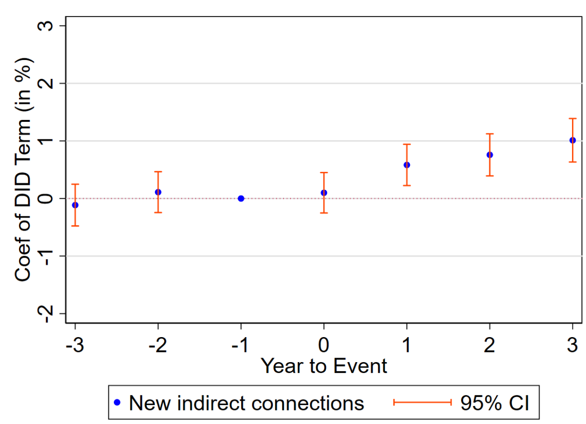
Panel B2: Operating margin, indirect



Panel C1: ROA, direct



Panel C2: ROA, indirect



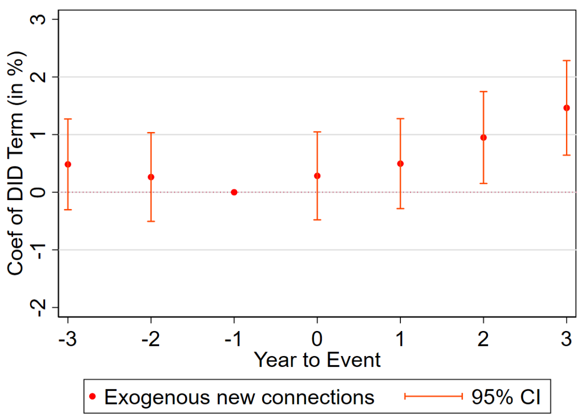
Note: This figure plots coefficients from the following regression,

$$
\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times DirectTreated_{i,c} + \alpha_2 \times IndirectTreated_{i,c} \\
&+ \sum_{s=-3}^{-2} \beta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \beta_s \times \mathbb{1}(s=\tau)_{c,t} \\
&+ DirectTreated_{i,c} \times (\sum_{s=-3}^{-2} \gamma_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \gamma_s \times \mathbb{1}(s=\tau)_{c,t}) \\
&+ IndirectTreated_{i,c} \times (\sum_{s=-3}^{-2} \delta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \delta_s \times \mathbb{1}(s=\tau)_{c,t}) \\
&+ \theta_i + \theta_{j,t} + e_{i,j,c,t}.
\end{aligned}
$$

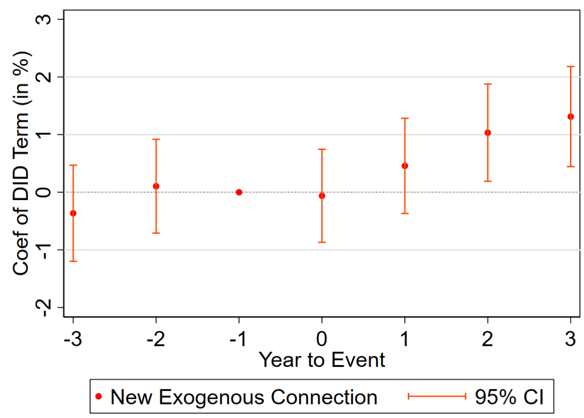
Here t represents the calendar year amd τ represents the year relative to the treatment. $\mathbb{1}(s=\tau)_{c,t}$ is a dummy variable that turns on if the observation is s years before the treatment (for $s=-3,-2$) or if the observation is s years after the treatment (for $s=0,1,2,3$). We omit the dummy variables for the year prior to the event, i.e., $\tau=-1$, which forms the baseline year. Thus all the effects we document are relative to this year. The estimates of γ_s and δ_s capture the difference in outcome variables between treated and control firms of year s relative to their differences in the baseline year. Standard errors are clustered at the firm level.

Figure 5: Plots of the dynamics of the difference between treated and control firms, using the exogenous subset of events

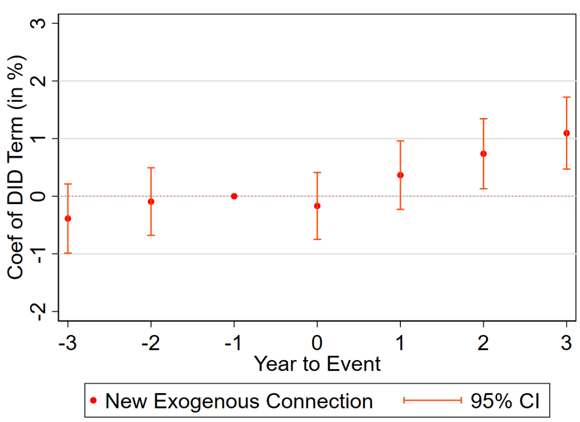
Panel A: Gross margin



Panel B: Operating margin



Panel C: ROA

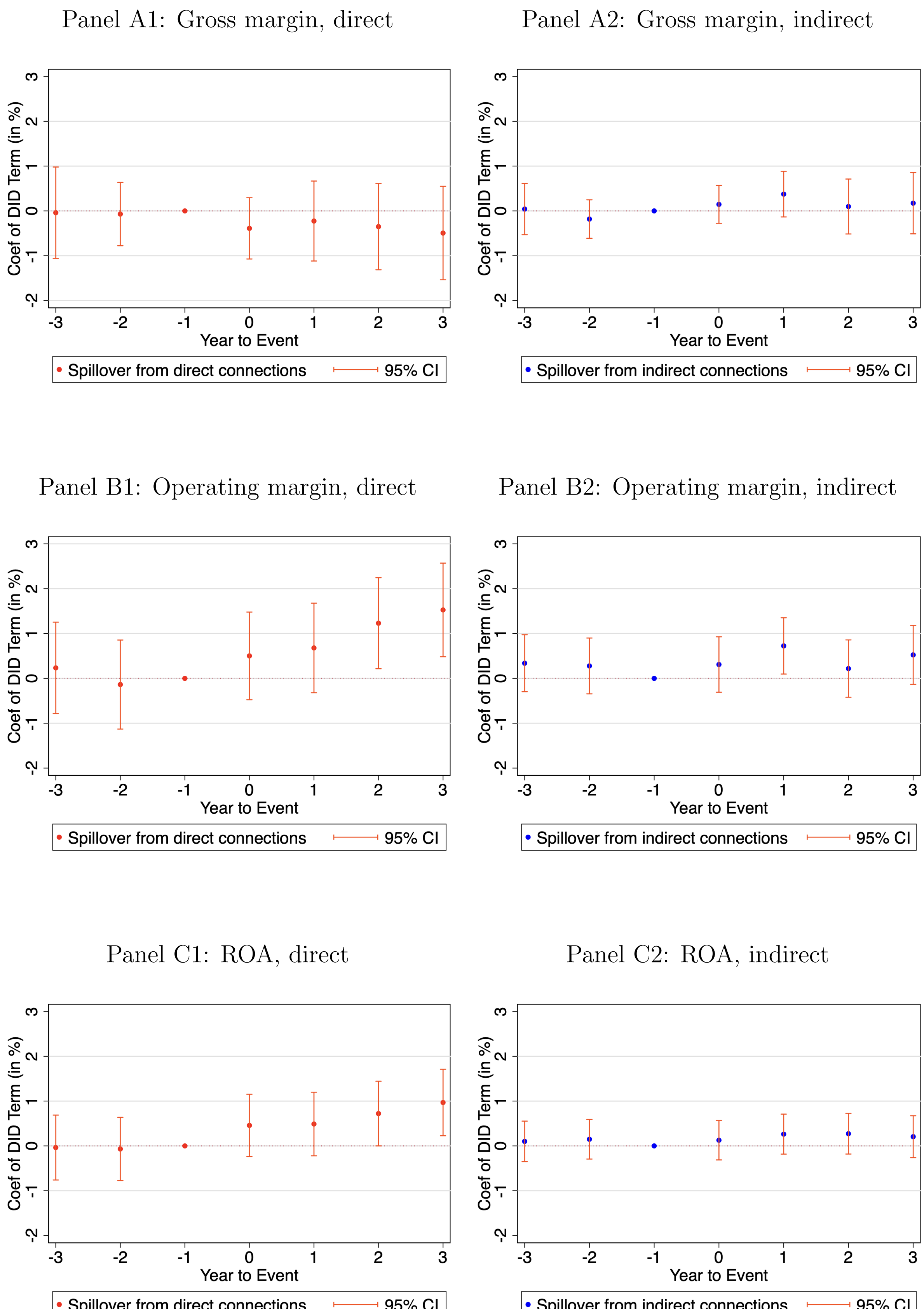


Note: This figure plots coefficients from the following regression,

$$
\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times ExogenousTreated_{i,c} \\
&+ \sum_{s=-3}^{-2} \beta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \beta_s \times \mathbb{1}(s=\tau)_{c,t} \\
&+ ExogenousTreated_{i,c} \times \Big(\sum_{s=-3}^{-2} \gamma_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \gamma_s \times \mathbb{1}(s=\tau)_{c,t} \Big) \\
&+ \theta_i + \theta_{j,t} + e_{i,j,c,t}.
\end{aligned}
$$

Here t represents the calendar year amd τ represents the year relative to the treatment. $\mathbb{1}(s = \tau)_{c,t}$ is a dummy variable that turns on if the observation is s years before the treatment (for $s = -3, -2$) or if the observation is s years after the treatment (for $s = 0, 1, 2, 3$). We omit the dummy variables for the year prior to the event, i.e., $\tau = -1$, which forms the baseline year. Thus all the effects we document are relative to this year. The estimates of γ_s capture the difference in outcome variables between treated and control firms of year s relative to their differences in the baseline year. Standard errors are clustered at the firm level.

Figure 6: Plots of the dynamics of the difference between firms subject to spillover effects and control firms



Note: This figure plots coefficients from the following regression,

$$\begin{aligned}
Y_{i,j,c,t} &= \alpha_1 \times DirectSpillover_{i,c} + \alpha_2 \times IndirectSpillover_{i,c} \\
&+ \sum_{s=-3}^{-2} \beta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \beta_s \times \mathbb{1}(s=\tau)_{c,t} \\
&+ DirectSpillover_{i,c} \times (\sum_{s=-3}^{-2} \gamma_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \gamma_s \times \mathbb{1}(s=\tau)_{c,t}) \\
&+ IndirectSpillover_{i,c} \times (\sum_{s=-3}^{-2} \delta_s \times \mathbb{1}(s=\tau)_{c,t} + \sum_{s=0}^{3} \delta_s \times \mathbb{1}(s=\tau)_{c,t}) \\
&+ \theta_i + \theta_{j,t} + e_{i,j,c,t}.
\end{aligned}$$

Here t represents the calendar year amd τ represents the year relative to the treatment. $\mathbb{1}(s=\tau)_{c,t}$ is a dummy variable that turns on if the observation is s years before the treatment (for $s=-3,-2$) or if the observation is s years after the treatment (for $s=0,1,2,3$). We omit the dummy variables for the year prior to the event, i.e., $\tau=-1$, which forms the baseline year. Thus all the effects we document are relative to this year. The estimates of γ_s and δ_s capture the difference in outcome variables between firms subject to spillover effects and control firms of year s relative to their differences in the baseline year. Standard errors are clustered at the firm level.

Appendix

Table A1: List of variables

Panel A: Financial and accounting variables	
Assets	The natural logarithm of the firm's total assets (*log(at)*)
Gross Margin	The ratio of gross profit to sales (*gp / sale*)
Operating Margin	The ratio of operating income before depreciation and amortization to sales (*oibdp / sale*)
ROA	The ratio of operating income before depreciation and amortization to total assets (*oibdp / at*)
Sales Growth	The percentage change of sales relative to the prior year (*(sale - l.sale) / l.sale*)
Tobin's Q	The ratio of market value of equity plus book value of debt over total assets (*(at + csho × prcc f - ceq) / at*)
Panel B: Indicator variables used in regressions	
DirectTreated	A dummy that equals 1 for the time series of a firm experiencing new direct connections to product market peers
IndirectTreated	A dummy that equals 1 for the time series of a firm experiencing new indirect connections to product market peers
Post	It equals 0 for τ = -3, -2, -1 and 0, and is 1 for τ = 1, 2, and 3
ExogenousTreated	A dummy that equals 1 for the time series of a firm experiencing new indirect connections to product market peers that occur due to changes on the board of another firm rather than itself
PseudoDirectTreated	A dummy that equals 1 for the time series of a firm experiencing direct connections to pseudo peers
PseudoIndirectTreated	A dummy that equals 1 for the time series of a firm experiencing indirect connections to pseudo peers

Table A1: List of variables (continued)

Panel C: Sorting variables	
Similarity Score	The cosine similarity scores between the treated firm and its new connections. If an event involves a new connection between a treated firm and multiple product market peers, it takes the value of the largest cosine similarity score. This score is developed in Hoberg and Phillips (2010, 2016) and provided in the Hoberg-Phillips Data Library
Geographical Distance	Geographical distance between the ZIP codes (*addzip*) of the treated firm and its newly connected peer. If a treated firm is incrementally connected to multiple peer firms, it takes the value of the smallest distance
HHI	The Herfindahl-Hirschman of the industry that the treated firm is in. This measure is developed in Hoberg and Phillips (2016) and provided in the Hoberg-Phillips Data Library
Returns to Scale	The estimated returns to scale of the industry that the treated firm is in. Following Dong et al. (2019), we estimate a two-factor Cobb-Douglas production function for each SIC 2-digit industry using data of the year 1999 and OLS regressions. We proxy for the firm's output by its sales (*sale*), for the firm's labor by the number of its employees (*emp*), and for the firm's capital by the firm's property, plant, and equipment (*ppent*). We then add the coefficients for the proxies for labor and capital, which is our measure of an industry's returns to scale
Top Similarity	A dummy that equals 1 for the time series of both the treated firm and its control if the new connections are in the top half in terms of the cosine similarity score between the treated firm and its new connections, and some new connections are in the same SIC-3 industry as the treated firm
Bottom Distance	A dummy that equals 1 for the time series of both the treated firm and its control if the new connections are in the bottom half in terms of the geographical distance between the treated firm and its new connections
Top HHI	A dummy that equals 1 for the time series of both the treated firm and its control if the treated firm is in the top half in terms of HHI among all treated firms treated in the same year
Top Returns to Scale	A dummy that equals 1 for the time series of both the treated firm and its control if the industry of the treated firm is in the top half in terms of Returns to Scale

Table A1: List of variables (continued)

Panel D: Other control variables	
GGL_{linear}	The firm-pair-year level measure of common ownership developed in Gilje et al. (2020), which is defined as $$GGL_{linear}(A,B) = \sum_{i=1}^{I} \alpha_{i,A} g(\beta_{i,A}) \alpha_{i,B},$$ where $\alpha_{i,A}$ is the fraction of firm A's shares held by investor i, $\alpha_{i,B}$ is the fraction of firm B's shares held by investor i, and $\beta_{i,A}$ is the weight of firm A in investor i's portfolio. Function g describes how the likelihood of an investor being attentive is increasing in how important a stock is in this investor's portfolio. It is assumed to take a linear form
GGL_{fitted}	A version of common ownership measure developed in Gilje et al. (2020) that uses a non-parametric fitted attention function estimated with voting data
GGL_{full_attn}	A version of common ownership measure developed in Gilje et al. (2020) that assumes full attention, i.e., $g = 1$
$\Delta(CommonOwnership)_{linear}$	The change in the within-industry common ownership, i.e, the mean common ownership between a firm and all its Hoberg-Phillips product market peers from $\tau = -3, -2, -1$ to $\tau = 0, 1, 2, 3$. For each treated firm, we calculate the average GGL_{linear} between it and all of its Hoberg-Phillips product market peers. We do it separately for each year from $\tau = -3$ to $\tau = 3$. This is our firm-year level measure of within-industry common ownership. Then we take a prior-event average using $\tau = -3, -2, -1$, and a post-event average using $\tau = 0, 1, 2, 3$, and take the post-minus-prior difference. We arrive at a constant for each time series of length 7 (from τ = -3 to +3). We do the same calculation for each control firm around the treatment year of the treated firm it is matched to. Finanlly, we scale this measure by its sample standard deviation
$\Delta(CommonOwnership)_{fitted}$	A version of $\Delta(CommonOwnership)$ that uses GGL_{fitted}
$\Delta(CommonOwnership)_{full\_attn}$	A version of $\Delta(CommonOwnership)$ that uses GGL_{full_attn}

Internet Appendix

IA.1 Supplemental Tables and Figures

Table IA1: Director network and detected cartel cases

	(1) Prob. of active detected cartel (%)	(2) Prob. of active detected cartel (%)	(3) Prob. of active detected cartel (%)
Degree of separation = 0	0.058***	0.043***	0.025***
	(33.38)	(24.70)	(11.62)
Degree of separation = 1	0.060***	0.053***	0.005***
	(84.82)	(74.30)	(7.32)
Degree of separation = 2	0.017***	0.014***	–0.000
	(52.28)	(41.54)	(–1.00)
Degree of separation = 3	0.003***	0.002***	–0.000
	(16.23)	(11.07)	(–0.81)
Cosine similarity score		0.285***	0.074***
		(72.05)	(12.18)
Is H-P peer		0.036***	–0.007***
		(41.31)	(–6.86)
Observations	53,893,933	53,893,933	53,893,933
Year FE	No	Yes	Yes
Firm-pair FE	No	No	Yes

Note: This table reports results from the following regression

$$\begin{aligned} Prob(\text{Having an active detected cartel})_{i,j,t} &= \sum_{m=0}^{4} \beta_m \times \mathbb{1}(\text{Degree of separation is } m)_{i,j,t} \\ &+ Control_{i,j,t} + \theta_t + \theta_{i,j} + e_{i,j,t}. \end{aligned} \quad (6)$$

using the sample of firm-pair's with the degree of separation in the director network less than or equal to 4. Firm-pair's with a degree of separation of 4 serve as the omitted category.

Table IA2: Double-difference regressions, using pseudo events and excluding actual events

	(1) Gross Margin	(2) Operating Margin	(3) ROA
Post	0.000	-0.000	-0.004
	(0.08)	(-0.10)	(-1.30)
PseudoDirectTreated X Post	0.012	-0.004	-0.001
	(0.67)	(-0.28)	(-0.06)
PseudoIndirectTreated X Post	0.001	-0.001	0.004
	(0.17)	(-0.12)	(1.09)
Observations	10,329	10,302	10,303
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
# of Matched Controls	1	1	1
Within R-squared	0.000	0.000	0.000

Note: This table reports results from the following regression

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times PseudoDirectTreated_{i,c} + \alpha_3 \times PseudoIndirectTreated_{i,c} \\ &+ \beta_1 \times PseudoDirectTreated_{i,c} \times Post_{c,t} + \beta_2 \times PseudoIndirectTreated_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t} \end{aligned}$$

using the sample of events of connections to pseudo peers and excluding the overlap of pseudo events with actual events. Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. $PseudoDirectTreated_{i,c}$ equals 1 for the time series of a firm experiencing direct connections to pseudo peers. $PseudoIndirectTreated_{i,c}$ equals 1 for the time series of a firm experiencing indirect connections to pseudo peers. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. We omitted coefficients of $PseudoDirectTreated_{i,c}$ and $PseudoIndirectTreated_{i,c}$ from the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table IA3: Double-difference regressions, differentiating by whether executives are involved

VARIABLES	(1) Gross Margin	(2) Operating Margin	(3) ROA
Post	-0.007***	-0.008***	-0.007***
	(-4.26)	(-4.78)	(-6.12)
DirectTreated, Is Not Exec. X Post	0.007*	0.013***	0.009***
	(1.83)	(3.35)	(3.13)
DirectTreated, Is Exec. X Post	0.011**	0.015**	0.010**
	(2.29)	(2.37)	(2.14)
IndirectTreated, Is Not Exec. X Post	0.002	0.006**	0.007***
	(0.88)	(2.25)	(3.42)
IndirectTreated, Is Exec. X Post	0.009**	0.014***	0.008***
	(2.54)	(3.90)	(3.02)
Observations	68,690	68,534	68,602
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
# of Matched Controls	1	1	1
Within R-squared	0.001	0.002	0.002

Note: This table reports results from the following regression

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times DirectTreatedIsNotExec_{i,c} + \alpha_3 \times DirectTreatedIsExec_{i,c} \\ &+ \alpha_4 \times IndirectTreatedIsNotExec_{i,c} + \alpha_5 \times IndirectTreatedIsNotExec_{i,c} \\ &+ \beta_1 \times DirectTreatedIsNotExec_{i,c} \times Post_{c,t} + \beta_2 \times DirectTreatedIsExec_{i,c} \times Post_{c,t} \\ &+ \beta_3 \times IndirectTreatedIsNotExec_{i,c} \times Post_{c,t} + \beta_4 \times IndirectTreatedIsExec_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t} \end{aligned}$$

using the sample of events of connections to product market peers. Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. $DirectTreatedIsNotExec_{i,c}$ equals 1 for the time series of a firm experiencing direct connections to product market peers and only non-executive directors are involved. $DirectTreatedIsExec_{i,c}$ equals 1 for the time series of a firm experiencing direct connections to product market peers and directors who are also executives are involved. $IndirectTreatedIsNotExec_{i,c}$ equals 1 for the time series of a firm experiencing indirect connections to product market peers and only non-executive directors are involved. $IndirectTreatedIsExec_{i,c}$ equals 1 for the time series of a firm experiencing indirect connections to product market peers and directors who are also executives are involved. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. We omitted coefficients of $DirectTreatedIsNotExec_{i,c}$, $DirectTreatedIsExec_{i,c}$, $IndirectTreatedIsNotExec_{i,c}$ and $IndirectTreatedIsNotExec_{i,c}$ from the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table IA4: Robustness to controlling for connections between customer-supplier firms

	(1)	(2)	(3)
	Gross Margin	Operating Margin	ROA
NonCusSupTreated X Post	0.005**	0.010***	0.008***
	(2.07)	(3.83)	(4.27)
CusSupTreated X Post	0.005	0.016	0.017**
	(0.56)	(1.01)	(1.98)
Observations	53,144	52,999	53,048
Firm FE	Yes	Yes	Yes
FF48 X Year FE	Yes	Yes	Yes
Clustering	Firm	Firm	Firm
Number of Matched Controls	1	1	1

Note: This table reports results from the following regression

$$\begin{aligned} Y_{i,j,c,t} &= \alpha_1 \times Post_{c,t} + \alpha_2 \times NonCusSupTreated_{i,c} + \alpha_3 \times CusSupTreated_{i,c} \\ &+ \beta_1 \times NonCusSupTreated_{i,c} \times Post_{c,t} + \beta_2 \times CusSupTreated_{i,c} \times Post_{c,t} \\ &+ \theta_i + \theta_{j,t} + e_{i,j,c,t}. \end{aligned}$$

Here $Post_{c,t}$ is 1 for both treated and control firms for the years $\tau = 0$, 1, 2, and 3, where $\tau = 0$ is the treatment year. Coefficient on $Post_{c,t}$ is the estimated difference between prior and post for the control firm. In this regression we pool events of new direct and indirect connections together. $NonCusSupTreated_{i,c}$ equals 1 for the time series of a firm experiencing new direct or indirect connections to a product market peer that is not its customer or supplier firm. $CusSupTreated_{i,c}$ equals 1 for the time series of a firm experiencing new direct or indirect connections to a product market peer that is a customer or supplier firm of its. θ_i are firm fixed effects. $\theta_{j,t}$ are industry times year fixed effects, which use the Fama-French 48-industry classification. We omitted coefficients of $Post_{c,t}$, $NonCusSupTreated_{i,c}$ and $CusSupTreated_{i,c}$ from the table. T-stats are in the parentheses. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

IA.2 Definition of the exogenous subset of events

In this section, we define the exogenous subset of events in greater details and describe how we operationalize our definition. These are events of new board connections to peer firms that occur solely due to changes on the board of a third firm. To identify this exogenous subset of events, we first identify the complement set of it, which are events that occur at least partly due to changes on the board of the treated firm itself.

Note that, in our paper, the changes refer to the scenarios of both a firm appointing new directors, and existing directors taking on new roles at other firms. Both kinds of changes on the board of the treated firm could be related to certain future prospects of this treated firm. Hence, we want to avoid both in the exogenous subset of events we identify. Also note that, by our definition, the exogenous events are a subset of events of new indirect connections, as events of new direct connections must take place because of changes of either kind on the board of the treated firm.

We check the events one by one. We say that an event is a *not* exogenous one, if either of the following conditions is true.

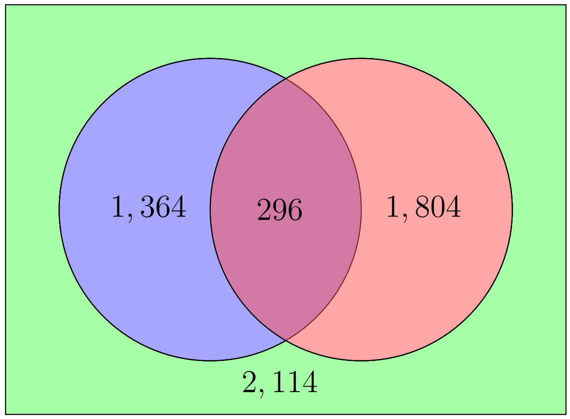
1. The appointment of new directors to the treated firm is causing new connections between the treated firm and its product market peers.

2. New board positions taken by existing directors in the treated firm is causing new connections between the treated firm and its product market peers.

Taking out these *not* exogenous events from the set of all events, we get the subset of events that we deem to be exogenous. These events occur solely due to changes on the board of a third intermediate firm or a product market peer, and not due to any changes on the board of the treated firm.

The logic can be represented using the following Venn diagram.

Figure IA1: Definition of the exogenous subset of events



	All events of new connections	5,578
■ (blue)	Some new connections result from new appointment of directors to the treated firm	1,364 + 296 = 1,660
■ (red)	Some new connections result from existing directors on the treated firm's board taking on directorship elsewhere	1,804 + 296 = 2,100
■ (green)	New connections purely result from changes on boards of other firms (The exogenous subset of events)	2,114

When operationalizing our definition, we call the treated firm i, and the set of its newly connected peer firms $NCP(i)$. Then we take a union of all the firms that firms in $NCP(i)$ are connected to, which forms the set $NCP(i)'$. We exclude firm i itself from $NCP(i)'$.

Condition 1 can be expressed as, firm i has newly appointed directors in the event year, and at least one of these newly appointed directors also sits on a firm in $NCP(i)$ or $NCP'(i)$. That is, these new appointments to the board of the treated firm are causing new direct or indirect connections of the treated firm to its peers.

Condition 2 can be expressed as, in the event year, some existing directors in firm i take on new directorships in a firm in $NCP(i)$ or $NCP'(i)$. That is, these new roles that existing directors of the treated firm take on are causing new direct or indirect connections of the treated firm to its peers.

IA.3 Director network and detected cartels

We obtain information on convicted cartels from the Private International Cartels database (Connor, 2020). We restrict the sample to firms headquartered in the US only, and hand-match those firms to the universe of firms we described in Section 2. Equipped with these cartels cases, we construct a firm-pair-year level indicator of whether two firms are in an active detected cartel in a certain year. We also construct the degree of separation of each firm-pair in the network described in Section 2, which is the minimum number of intermediate firms that can connect these two firms together. Hence, two directly connected firms have a degree of separation of 0 and two indirectly connected firms have a degree of separation of 1. We exclude firm-pairs that are unconnected in the director network or connected but with a degree of separation above 4.

We first plot the probability of a firm-pair having an active detected cartel in a certain year, conditional on the degree of separation of these two firms in the director network. We find that while this probability is around 0.06% for firm-pairs with a degree of separation of 0 or 1, it becomes 0.017% for firm-pairs with a degree of separation of 2, and diminishes to near zero as the degree of separation further increases.

Next, we estimate the following probit model on this sample:

$$\begin{aligned} Prob(\text{Having an active detected cartel})_{i,j,t} &= \sum_{m=0}^{4} \beta_m \times \mathbb{1}(\text{Degree of separation is } m)_{i,j,t} \quad (7) \\ &+ Control_{i,j,t} + \theta_t + \theta_{i,j} + e_{i,j,t}. \end{aligned}$$

and report the results in Table IA1. Column (1) reports the probability of having an active detected cartel, with firm-pair's of degree of separation 4 being the baseline group. As firms closer in the director network might have more similar businesses, which can be a confounding factor that affects the tendency for anti-competitive practices, in Column (2) we also control for the cosine similarity of two firms' business description as well as an indicator for whether two firms are in the same Hoberg-Phillips industry. In Column (3) we additionally include firm-pair fixed effects. Across all these specifications, the associative relationship between degree of separation in the director network and probability of having an active cartel all holds and remains statistically significant.