

Interlocking Directorate, Firm Value, and Product Market Collusion

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Abstract

I document a 500% increase in directorate interlocks with product market competitors among U.S. non-financial public firms over the past decade. Utilizing announcements from the U.S. Department of Justice regarding the enforcement of Section 8 of the Clayton Act as an exogenous shock to litigation risk associated with interlocks, I examine the net value of directorate interlocks with product market competitors to a firm. I validate the shock through both a difference-in-differences analysis and a regression discontinuity design, demonstrating that Section 8 enforcement caused firms to lose directorate interlocks in the subsequent months. Then, I show that treated firms experienced a 7.1% decline in firm value within eight weeks following the announcement shock when compared to control firms, while controlling for Fama-French five factors. Moreover, the recent escalation in interlocking directorates was primarily attributed to the subset of firms exhibiting the highest product similarity with their competitors. Both product similarity with competitors and the number of competitors were negatively correlated with stock returns following the shock. These results support the theory that companies collaborate with competitors in the product market via shared board members. However, they are not consistent with the notion that firms lose high-quality directors as a consequence of the shock.

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1. Introduction

Corporate boards have been an active area of research in the field of corporate governance because of their significant influence on firm performance. Two major functions of a corporate board widely studied in the literature are the monitoring of firm managers and the advisory on firm strategy. Substantial evidence has been found that directors provide valuable service to the firm by providing their knowledge and expertise when managers set firm strategies. This unilateral information flow from the outside to the inside of the firm is one of the major motivations behind the setting of corporate boards in the first place. An implicit assumption behind this advisory function is that directors possess some expertise that they acquire outside of the firm, potentially from their professional service, including employment and directorship, at other firms. This assumption naturally gives rise to the possibility of bilateral information flow between the two firms, to which a director provides simultaneous service. Nonetheless, the role of bilateral information flow through shared directors has been underexplored in the literature, especially in the case of information exchange between competing firms, where the impact of such bilateral information flow on firm performance and market outcome may be the most influential.

An important question that remains unresolved in the literature is whether common directors between rival firms benefit shareholders by enabling two-way information exchange and encouraging anti-competitive behavior between the two firms. Despite its economic significance, this question has not been answered, potentially due to the scarcity of empirical data, as the United States has banned interlocking directorate, the appointment of the same director to two firms, between product market rivals since 1914, driven by anti-competitive concerns. In this paper, I document a sharp increase in the prevalence of interlocking directorate between competing firms

in the past decade, suggesting a lack of enforcement of this prohibition in the recent decade. Such a rise in interlocking directorate at competing firms allows researchers to empirically examine the role of directors in the information flow between competing firms. To enable identification, I utilize a regulatory shock that leads to an increased probability of enforcement against interlocking directorate between competitors. Through an event study on this regulatory shock, I illustrate that firms gain benefits from directorate interlocks with their rivals. Subsequently, I demonstrate that these benefits are positively related to the intensity of competition the firm faces in the product market. The observed positive correlation between the intensity of competition and the benefits to firms substantiates the theory that companies coordinate in the product market via bilateral information flow through shared directors. This finding also refutes the alternative theory suggesting that interlocks increase firms' access to high-quality directors.

In 1914, the United States Congress enacted the Clayton Antitrust Act to enhance competition among American businesses. Section 8 of the Clayton Act prohibits competing companies in the product market from having directors in common. Despite being in force for over a hundred years, the enforcement of Section 8 of the Clayton Act has been largely neglected by regulators for a long time until 2022. During the week of April 4th, 2022, Jonathan Kanter, who leads the Antitrust Division at the US Department of Justice, made two public announcements regarding the DOJ's plan to begin enforcing Section 8 of the Clayton Act. This week signified the commencement of Section 8 enforcement by several US regulators. Before April 2022, interlocking directorates among US non-financial public companies had been rapidly increasing since the early 2010s. In figure 1, I document both the decade-long rise in interlocking directorates and the sharp decline following the DOJ announcements. Both panels in figure 1 illustrate this upward trend in directorate interlocks among US firms over

the past decade. In the first panel, I display the average number of interlocked firms, defined as those that share at least one common director with the focal firm, across all US non-financial public firms. This number rose by approximately 20% from the early 2010s low point to the peak in 2022. The second panel shows an enormous 500% rise in the average number of interlocked product market competitors across all firms from the early 2010s to 2022. The annual jumps in the average number of interlocked competitors in the second panel are mechanical due to the use of the annual competitor metric from Hoberg and Phillips (2016). For each month, I use the Hoberg and Phillips (2016) measure from the previous year. This practice is consistent with the analysis in the following sections. If I use the competitor metric in the concurrent year, there is a discrete drop in the average number of interlocked competitors at the beginning of each year. The firms analyzed in this paper are all US non-financial public firms, as the Clayton Act does not apply to the finance industry. While the increase in the number of US non-financial public firms may partially explain the patterns in the first panel, the consistent and substantial rise shown in the second panel cannot be merely attributed to the growth in the number of public firms. Crucially, since the Clayton Act Section 8 aims to prevent interlocks among product market competitors, the significant increase in the second panel highlights the lack of enforcement of Clayton Act Section 8 before 2022. Another key observation from figure 1 is the sharp decline in the number of interlocks starting in April 2022, marked by the vertical dashed lines. This serves as the first piece of evidence for the effectiveness of regulators' efforts to prohibit interlocking directorates following the DOJ announcements.

To address the research questions concerning whether and how firms benefit from interlocking directorates with competitors, I perform empirical analysis using the previously mentioned DOJ announcements as an exogenous shock on the likelihood of

enforcing interlocking directorate prohibitions. My analyses are guided by 2 hypotheses. The first hypothesis is that:

1. The enforcement of prohibition against interlocking directorate will induce firms to sever their interlocking directorate links with other firms, especially their product market competitors.

If the interlocking directorate somehow benefits the interlocked firms, hypothesis 1 will naturally lead to the following second hypothesis:

2. Following the news of Section 8 enforcement, we should see a drop in stock price of firms interlocked with competitors, relative to the price of firms without competitor interlocks.

I design empirical tests for each of the hypotheses mentioned above. To more precisely determine which companies are significantly impacted by Section 8 enforcement, I identify the treated companies based on three criteria: board overlap, product market competition, and book value of equity. These criteria are explicitly stated by the Clayton Act Section 8 to determine if a firm is subject to the prohibition outlined in the Clayton Act Section 8.

After identifying treated firms, I perform an event study examining the number of interlocks around DOJ announcements to test hypothesis 1. At the firm-month level, I determine the number of firms that shared at least one director with the focal firm. By conducting a difference-in-differences analysis, I find that on average, a treated

firm lost 0.203 more interlocked firms compared to a control firm over six months after the announcements. After restricting the dependent variable to include only interlocked firms with a sufficient book value of equity to be targeted by Section 8, the estimated treatment effect expands to -0.377. After narrowing the variable to encompass solely the interlocked competitors in the product market that had substantial equity values, the estimated treatment effect stays at -0.352. The treatment effects estimated for all three variables are not only statistically significant but also economically significant. Prior to the treatment, treated firms had an average of 4.09 interlocked above-threshold competitors. The treatment effect represents nearly 10% of these interlocks. Furthermore, the speed at which the treatment proved effective was noteworthy. Event study graphs show that the majority of the treatment effect took place within two months after the treatment.

To enhance causal interpretation, I employ a sharp regression discontinuity design by leveraging the sharp threshold on firm book value of equity. Section 8 does not apply to a pair of firms if either firm had a book value of equity below the threshold. Regression discontinuity analysis demonstrates that, following DOJ announcements, the firms just above the equity threshold experienced more interlock losses compared to those just below it. The local treatment effect identified in this analysis also grows over time, aligning with the pattern shown in the event study. The findings from the event study and regression discontinuity both validate the policy shock discussed in this paper, and these findings endorse hypothesis 1.

After confirming hypothesis 1, I proceed to test the second hypothesis by performing an event study on stock returns around the announcement events. I observe that in the week after the event, firms that were treated experienced lower stock returns than the control firms. On average, the stock prices of the treated firms underperformed control firms by 7.1%, after accounting for the Fama-French 5 factors, during

the 8 weeks following the announcement events. This underperformance is both statistically and economically significant. These findings indicate that the net value of a directorate interlock with a competitor is significantly positive for the firms in my sample. I demonstrate that the poor performance of the treated firms was not driven by variations in stock performance across different industries or differential exposure to inflation risks.

I perform a regression discontinuity analysis on stock returns to demonstrate the causal relation between announcements on Section 8 enforcement and the ensuing negative reaction in the stock market. Regression discontinuity analysis shows that firms just above the equity threshold underperformed the firms just below the threshold, after accounting for Fama-French 5-factors, on the trading day following the announcement. By leveraging the efficiency of the stock market, I provide evidence that firms enjoy net benefits from having interlocks with competitors after taking all costs and benefits into account. This finding provides insight into the debate on whether interlocking directorate is good or bad for the firms (Geng, Hau, Michaely, and Nguyen, 2024; Gopalan, Li, and Zaldokas, 2024; Cabezon and Hoberg, 2025; Begley, Haslag, and Weagley, 2025; Herrera-Caicedo, Jeffers, and Prager, 2024; Barone, Schivardi, and Sette, 2025; Geng, Hau, Michaely, and Nguyen, 2025).

Lastly, I perform a heterogeneity analysis to distinguish between different channels through which competing firms might benefit from directorate interlocks. First, I demonstrate that the recent rise in interlocking directorates among U.S. public companies is largely due to the subset of firms that have the greatest product similarity with their competitors. Secondly, I conduct a difference-in-differences analysis with triple interaction terms that include the TNIC-4 total similarity score and the number of TNIC-4 level competitors. The results indicate that treated firms with higher total similarity with competitors and a higher number of competitors performed worse than

other treated firms. This indicates that companies with closer competitors benefit more from interlocks with those competitors.

The two findings in the heterogeneity analysis support the theory of collusion in the product market via shared directors. The product market collusion channel can simultaneously explain these findings that firms with more analogous competitors are more likely to have directorate interlocks and that these firms derive greater value from such interlocks. Canonical economic models indicate that when rival companies offer more similar products, they tend to earn lower profits as a result of price competition. Consequently, it would be mutually beneficial for these companies to agree on collusion. In practice, two main factors restrict firms from engaging in collusion, yet these can be alleviated through interlocking directorates. First, if collusive coordination is uncovered, the government will impose penalties. Nonetheless, the private setting of board meetings permits shared directors to secretly coordinate among rival firms without drawing the attention of regulators. Second, mutual trust is essential between colluding firms; without it, each party is tempted to deviate. This trust can be established if a shared director consistently oversees the product market strategies of both firms and acts as a guarantor of their adherence. Thus, the empirical results observed in the heterogeneity analysis are natural consequences of profit-maximizing firms appointing shared directors with competitors to facilitate collusion and achieve greater profits.

The findings from the heterogeneity analysis also reject alternative theories, like the board quality theory, which suggests that the decrease in firm value following the DOJ announcements is due to the possible exit of high-quality directors from affected companies. After announcements by the DOJ, a director can now serve simultaneously at fewer competing firms, reducing the overall pool of qualified directors available. In equilibrium, firms hire the most competent directors. Therefore,

following the enforcement of Section 8, many affected companies will have to replace high-quality directors with lower-quality ones, resulting in diminished monitoring and advisory benefits for the firm. Substituting high-caliber directors with those of lower quality might result in a decrease in company value. The results from the heterogeneity analysis reject this board quality theory, as the expected reduction in monitoring and advisory values following Section 8 enforcement should be consistent across all treated firms. There is no justification for expecting companies facing higher market competition to replace their current directors with less capable ones compared to companies with lower competition levels.

The heterogeneity analysis reveals that a key role of the corporate board is to serve as a discreet means of communication and coordination among rival companies. The significance of this function is on the rise, given that the practice of having directors shared among competing companies has become more prevalent over the past decade. Unlike the typical advisory role, characterized by a one-way flow of information from external sources into the firm, this communication function involves a bilateral exchange of information between rival firms and might have more substantial implications on stakeholder welfare.

The remainder of the paper proceeds as follows. Section 2 discusses relevant literature and my contributions. Section 3 provides background on the Clayton Act Section 8 and details relevant to my analyses. Section 4 introduces data. Section 5 presents empirical analysis. Section 6 concludes the paper.

2. Related Literature and Contributions

This paper contributes to two strands of literature. Firstly, it contributes to the corporate governance literature concerning the role of boards. The literature on

corporate governance has identified two primary roles of corporate boards (Adams and Ferreira, 2007; Adams, Hermalin, and Weisbach, 2010). The first function is monitoring the top management team (Hermalin and Weisbach, 1998; Adams and Ferreira, 2009), and the second function is advising on the firm strategy (Coles, Daniel, and Naveen, 2008; Dass, Kini, Nanda, Onal, and Wang, 2014). Research on board advisory has shown that the industry and professional expertise of outside directors contributes to increasing firm value (Dass et al., 2014; Drobetz, von Meyerinck, Oesch, and Schmid, 2018). However, this body of literature inherently concentrates on the one-way flow of information from outside the firm to inside. I contribute to this body of literature by demonstrating that a board can enhance a firm’s value by serving as a communication channel and enabling bilateral information flow between companies. My focus is on the bilateral information exchanges between competing firms, where such exchanges can be particularly advantageous for the firms involved. I first demonstrate the net positive value generated by competitor interlocks, and then I present evidence supporting the theory that such value enhancement is achieved through bilateral information flow between firms.

I also provide insights to the most relevant contemporaneous literature on direct board interlocks between product market competitors. This strand of literature has provided evidence that direct interlocks between competitors could be both beneficial and costly to the firm. Such interlocks could benefit the firm through higher product prices (Barone et al., 2025; Gopalan et al., 2024), lower expenses Geng et al. (2024), and more segmented markets (Poberejsky, 2024). At the same time, competitor interlocks could lead to innovation herding, which harms competitive edge and long-term profits Cabezon and Hoberg (2025). In this paper, I estimate the aggregate firm value, after taking all potential channels into account, created by competitor interlocks by examining stock prices. I find that directorate interlocks positively

affect the value of a firm. This finding informs the current debate on the impact of competitor interlocks on firm value.

3. Institutional Background

Among the US antitrust statutes, Clayton Antitrust Act has been one of the primary building blocks since it was first enacted in 1914¹. In 1914, the United States Congress passed the Clayton Antitrust Act that aimed at modifying and complementing the then-existing federal antitrust laws to further facilitate competition among US businesses. The Clayton Act targets many business practices that may harm competition. Of particular interest for this paper, Section 8 of the Clayton Act prohibited “interlocking directorates” among competing non-financial firms, except for firms that have less than 1 million dollars in “capital, surplus, and undivided profits,” which are legal terms that map to components of firm equity². Importantly, the Clayton Section 8 prohibition is *per se*, meaning that the act of having an interlocking directorate, by itself, is a violation of antitrust law, no matter whether it has an ultimate impact on the competition between the interlocked firms³.

The interlocking directorate ban by the Clayton Act was not uncontroversial, especially around the end of the 20th century. The advocates for a reform of the Clayton Act claimed that Section 8 prevented firms from hiring qualified directors, even when such hiring would not lessen competition. As a result, the US Congress amended the Clayton Act in 1990. The major modifications to the Clayton Act include extending the scope of Section 8 from directors to both directors and officers, adding several

¹Refer to “<https://www.govinfo.gov/content/pkg/COMPS-3049/pdf/COMPS-3049.pdf>” for the latest version of the Clayton Act

²https://www.law.cornell.edu/supremecourt/text/275/215/USSC_PRO_275_215_156

³<https://www.justice.gov/opa/pr/justice-department-s-ongoing-section-8-enforcement-prevents-more-potentially-illegal>

safe harbor exceptions to Section 8, modifying the equity threshold, and charging the Federal Trade Commission to adjust this threshold annually based on Gross National Product⁴. Among these modifications, the most relevant to this paper is that the FTC is demanded by Congress to publish an updated equity threshold each year. As of 2022, the focal year of this paper, the latest equity threshold was \$41,034,000⁵. This means that in 2022, all firms with a book value of equity greater than this amount would be subject to the interlocking directorate ban of the Clayton Act Section 8, while all firms below this threshold were exempted. The amendment also added safe harbor exemptions for firm pairs that are not significant “competitors”⁶. The amendment stipulates that a firm pair is exempted from Section 8, as long as any one of the three conditions listed below is satisfied. The first condition is that the competitive sales of either firm in the pair are below a sales threshold. The second condition is that the competitive sales of either firm in the pair are below 2% of its total sales. The third condition is that the competitive sales of each firm in the pair are below 4% of its total sales, respectively.

Since the 1990 reform of the Clayton Act, the enforcement of Section 8 had been slack, justified by the rare violations of Section 8 before 2014⁷. However, the interlocking directorate between product market competitors started to rise since 2014, as shown in figure 1. Despite such a sharp increase in board interlocks between competitors, the enforcement of Section 8 did not keep up with the rise in interlock. Then, this situation of under-enforcement changed in the week of April 4th, 2022, when two significant regulatory events took place. At the 2022 Spring Enforcers Summit on

⁴<https://www.congress.gov/bill/101st-congress/house-bill/29>

⁵<https://www.ftc.gov/news-events/news/press-releases/2022/01/ftc-announces-annual-update-size-transaction-thresholds-premerger-notification-filings-interlocking>

⁶<https://www.congress.gov/bill/101st-congress/house-bill/29>

⁷<https://www.justice.gov/archives/opa/speech/remarks-assistant-attorney-general-bill-baer-american-bar-association-clayton-act-100th>

April 4th, 2022, the assistant attorney general Jonathan Kanter of the US Department of Justice Antitrust Division delivered a speech in which he announced a step-up in the Clayton Act Section 8 enforcement⁸. Then on April 8th, 2022, Friday of the same week, Kanter attended the roundtable event at the American Bar Association Antitrust Law Section spring meeting and reiterated DOJ’s intent to step up Section 8 enforcement⁹. These announcements were surprises to the public, and they were the first high-profile indications of DOJ’s intent to enforce Section 8. While the Spring Enforcers Summit was mainly attended by government officials around the world and may not attract much attention from the industry and the market, the ABA meeting was targeting all practicing lawyers in the United States. Therefore, it’s reasonable that these two announcements together would affect both firm management teams and the stock market. Since this week of announcements, DOJ has taken intense regulatory and legal actions against interlocking directorate. In the following months, numerous firms severed their director links with each other¹⁰. DOJ was later joined by other regulators, such as the Federal Trade Commission, in the effort to enforce Section 8¹¹. It’s obvious that the current wave of Section 8 enforcement has made huge impacts on US firms, and all of these started with the two announcements by Kanter in the week of April 4th, 2022. Figure 1 validates this claim by showing that interlocking directorate among US public firms started to decrease since April 2022, reversing the decade-long trends. Given that the Clayton Act was passed but rarely enforced for decades, these announcements were largely unanticipated by the market. Because these two significant events took place in the same week, I use this week as

⁸<https://www.justice.gov/opa/speech/assistant-attorney-general-jonathan-kanter-delivers-opening-remarks-2022-spring-enforcers>

⁹<https://www.justice.gov/archives/opa/video/assistant-attorney-general-jonathan-kanter-virtually-participated-enforcers-roundtable-aba>

¹⁰<https://www.justice.gov/opa/pr/directors-resign-boards-five-companies-response-justice-department-concerns-about-potentially>

¹¹<https://www.ft.com/content/33f0d653-c53b-4844-a044-3f72507a518b>

the event week in my following analysis.

The Department of Justice’s announcements in April 2022 were a component of the Biden administration’s broader initiative to enhance competition in the United States. This competition-promoting initiative commenced with an executive order in July 2021¹². In the following months, multiple government agencies intensified antitrust enforcement in traditional areas, such as merger and acquisition and labor market competition. In April 2022, antitrust enforcement was broadened to include interlocking directorates, an area that had previously seen limited enforcement. This antitrust initiative stemmed from Biden’s narrow victory in the 2020 election, suggesting that the government’s antitrust efforts were likely exogenous to the interlocking directorates among U.S. firms. Additionally, due to the fact that the enforcement announcements for Clayton Act Section 8 followed years of inadequate enforcement of this particular section, it is probable that neither companies nor investors anticipated this enforcement against interlocking directorates, even in light of Biden’s antitrust efforts.

There are two nuances in the Clayton Act that are worth mentioning. First, the 1990 amendment of the Clayton Act extended the scope of interlocks from only directors to both directors and senior officers. Therefore, in this paper, my definition of interlocking directorate includes all directors on the board and C-suite executives. However, the inclusion of senior executives makes a tiny difference, as detailed in the next section, in the main variables and the treatment classification, and all results would remain qualitatively similar if I exclude senior executives from consideration. This shows that interlocked non-director executives are very rare among US non-financial public firms. Second, simultaneous board positions at two firms by different

¹²<https://bidenwhitehouse.archives.gov/briefing-room/presidential-actions/2021/07/09/executive-order-on-promoting-competition-in-the-american-economy/>

natural persons representing the same investment institutions are also prohibited¹³. I cannot empirically account for this phenomenon because I do not have access to affiliations of directors to investment institutions. However, this should not be concerning, because institutional investors rarely hold simultaneous boards in competing public firms (Geng et al., 2025).

4. Data

The data used in this paper come from 4 sources. First, I get data on board of directors and C-suite executives (including CEO, CFO, and COO) from BoardEx for all US public firms. Only CEO, CFO, and COO are considered because the Clayton Act Section 8 defines an "officer" to be any executive who is appointed by the board of directors. BoardEx specifies the basic information for each board member and executive, and it assigns a director ID to each individual who has ever appeared in the dataset. With this information, I could identify the directors and C-suite executives who have served simultaneously at different companies. I include C-suite executives in the data because the 1990 Amendment of the Clayton Act Section 8 extends the scope from directors to both directors and officers, which are typically defined as board-selected senior executives in legal interpretations. Nonetheless, the incidence of non-director senior executives simultaneously sitting at another public firm is extremely rare. Including senior executives would result in a 3% increase in average numbers of both interlocked firms and interlocked competitors and a 2% increase in the number of treated firms. These differences are small, and repeating all analyses using director-only interlocks makes no significant difference in the results. For this reason, I refer to the interlocks of both directors and senior executives as

¹³<https://www.ftc.gov/enforcement/competition-matters/2017/01/have-plan-comply-bar-horizontal-interlocks>

director interlocks in this paper. Second, I get the stock price and returns for all US-listed firms from CRSP. All stock returns reported in this paper are in decimal. Third, I obtain quarterly financial data for the firms from Compustat. Then I link the firms in the BoardEx dataset with those in CRSP and Compustat datasets, using the linking tool provided by Wharton Research Data Services. I obtain SIC codes from CRSP and then augment them with Compustat data. Since the Clayton Act Section 8 only targets non-financial firms, I exclude financial firms from all following analyses. Following Cabezón and Hoberg (2025), I identify financial firms as those with SIC codes from 6000 to 6999. I compute equity as $cstkq + capsq + req$, closely corresponding to the terms "capital, surplus, and undivided profits". Lastly, I use Hoberg and Phillips (2016, 2010) text-based product competition data from Hoberg and Phillips Data Library¹⁴. This data provides an annual metric that measures the pairwise product market competition between all US public firms, based on a textual analysis of their annual financial reports. For all firm pairs, I use the TNIC scores from fiscal year 2021, which are computed from firms' annual reports for fiscal year 2021. Because the fiscal years of the majority of US public firms conclude around the end of the calendar year, the fiscal year 2021 annual reports for most of the firms are released in the early months of 2022. Given that the DOJ announcement events took place in April, the most up-to-date similarity metrics the market could observe in the event week are for fiscal year 2021.

5. Empirical Analysis

The empirical strategy of this paper consists of three parts. First, I establish a relation between the enforcement of the Clayton Act Section 8 and the reduction

¹⁴<http://hobergphillips.tuck.dartmouth.edu>

in firms’ interlocking directorate connections. To do so, I deploy a difference-in-differences analysis to investigate whether firms reduced their directorate interlocks following the DOJ announcements and whether there was a significant trend before the announcement events. This exercise tests the first hypothesis discussed earlier. Second, I investigate whether firms affected by the Clayton Act Section 8 suffered from lower stock returns than other firms following the DOJ announcements by conducting an event study around the week of the announcements. This provides an estimate of the overall firm value attributed to directorate interlocks with product market competitors. Third, I conduct a heterogeneity analysis on the interlocking directorates and stock returns to shed light on the channel through which directorate interlocks could contribute to the firm value.

5.1. Definition of Firm-Level Treatment

In this paper, I conduct two event studies to investigate the differences in both interlocking directorate and stock returns between firms affected by the Clayton Act Section 8 and firms that were not affected. Before running difference-in-differences analysis, I first need to define the treated and control firms. To define the treatment group, I rely on three criteria adapted from the Clayton Act Section 8. The Clayton Act Section 8 specifies that interlocking directorate prohibition is applicable to firm pairs that are both above the equity threshold, share a common director, and compete in the product market simultaneously. Therefore, I begin by identifying all firm pairs that satisfy all three criteria. First, I identify firm pairs as product market competitors if their annual TNIC score (Hoberg and Phillips, 2016, 2010) was in the top 1% among all firm pairs in 2021. This definition of product market competitor is identical to that in Cabezón and Hoberg (2025), who find that firm pairs within the top 1%

of TNIC score are direct competitors in the product market and that interlocking directorate between these firm pairs results in innovation herding. According to these authors, the 1% threshold yields a competitor classification that is as coarse as the 4-digit standard industrial classification code. They show that this granularity better captures competition than does TNIC-3 level classification, which is likely to include complementary firms in the product market.

While the 1990 amendment of the Clayton Act Section 8 exempts non-competing firm pairs based on competitive sales revenues, these numbers are not easily accessible, so I follow recent literature (Cabezón and Hoberg, 2025; Hoberg and Phillips, 2016; Gopalan et al., 2024; Poberejsky, 2024; Begley et al., 2025) to use TNIC scores to identify product market competitors. It's reasonable to use this annually-updated text-based industry classification to proximate the threshold used by regulators because both competitive revenue and TNIC scores are closely related to the actual sales of products each firm had in the relevant year. By legal requirements, the products that constitute larger shares of revenue for a firm are more likely to be mentioned in the product description section in the annual report, from which the TNIC score is constructed each year. Nonetheless, it's important to keep in mind that the TNIC measure for competition is not likely to coincide with the measure used by enforcement agencies and that SIC 4-digit granularity is also likely to deviate from that adopted by the government.

Then, I further identify firm pairs with at least 1 overlapping director on March 31st, 2022, using BoardEx data. Lastly, I examine whether the latest book value of equity of both firms in the firm pair was greater than the equity threshold as of event week. If a firm pair satisfies all three criteria simultaneously, I identify this pair as a firm pair treated by the enforcement of the Clayton Act Section 8. To convert the firm pair-level treatment to firm-level treatment, I require a firm to be

in at least one treated firm pair for the firm to be classified as a treated firm. This treatment classification is used in both event studies on interlocking directorate and stock returns. The summary statistics on main variables for both treated and control firms are presented in table 1.

5.2. *Event Study on Number of Interlocks*

After defining the firm-level treatment, I conduct the monthly level event study and difference-in-differences analysis on the number of directorate interlocks. I include all non-financial firms with non-missing book value of equity observations as of the end of March 2022 from Compustat. I require non-missing book value of equity because this value is necessary to conduct treatment classification.

5.2.1. *Event Study Plots on Number of Interlocks*

I first visualize the evolution of the differences in the number of interlocks between treated and control firms. I look at 3 dependent variables on the number of interlocks in this event study exercise. The first dependent variable is the number of non-financial firms that a firm shared any common director with. Note that not all common directors are prohibited by Section 8, as discussed earlier. Therefore, to further refine the measure, I also compute the number of interlocked firms that had the latest book value of equity greater than the 2022 threshold. This variable takes into account the equity value of the interlocked firm, and thus the interlocks included in this variable are more affected by Section 8 enforcement than the first variable. The third dependent variable, which is the most related to 3 criteria of Section 8, is the number of above-threshold non-financial competitors that a firm shared any common director with. This variable takes all three criteria of Section 8 into account.

I plot the coefficients from the following regression equation:

$$Interlocks_{it} = \sum_{r=-6}^6 \beta_r \times \mathbf{1}[Treated_i = 1] \times \mathbf{1}[t = r] \times \mathbf{1}[r \neq -1] + \delta_t + \lambda_i + \epsilon_{it} \quad (1)$$

where $Interlocks_{it}$ is the number of interlocks, as defined above, for firm i at the end of month t , which is relative to the event month of April 2022. $Treated_i$ is the treated dummy for firm i as defined earlier. The point estimates and standard errors for β_r are plotted in figure 2. We can see that following April 2022, treated firms experienced larger drops in all three measures of directorate interlocks. These results are consistent with hypothesis 1 that treated firms reduced directorate interlocks to lower the Section 8 enforcement risk. Furthermore, there were limited pre-trends for all three variables.

5.2.2. Difference-in-Differences on Number of Interlocks

After visualizing the change in the number of interlocks, I further conduct a difference-in-differences analysis to quantify the relation. The regression equation is as follows:

$$Interlocks_{it} = \alpha + \beta_1 \times Post_t \times Treated_i + \delta_t + \lambda_i + \epsilon_{it} \quad (2)$$

where $Interlocks_{it}$ is one of the three interlock variables defined above for firm i in week t . Treated dummy for firm i is the same as defined in section 5.1. I define post dummy as equal to 1 if the observation month is equal to or later than April 2022. I include firm and month fixed effects and double-cluster standard errors at

firm and month level.

The results from this regression are presented in table 2. One can see that the coefficients of interaction terms are significantly negative across all three dependent variables. This suggests that treated firms experienced a larger reduction in interlocking directorates than control firms. The treated firms lost 0.203 more interlocked firms than control firms. This number includes interlocks with both firms that were targeted by Section 8 and firms that were too small to be targeted. When we restrict our interlock to those with firms that were large enough to be targeted by Section 8, the treatment effect magnifies to -0.377, which is also more statistically significant. The third column shows that treated firms reduce the number of interlocked above-threshold competitors by 0.352 more than control firms. The statistical significance of this estimate is also at 1%. Although the estimated value is slightly smaller for interlocked above-threshold competitors than for interlocked above-threshold firms, it represents a much larger percentage decrease in the number of interlocked above-threshold competitors, as the pre-event number of interlocked above-threshold competitors is more than 30% smaller than the pre-event number of interlocked above-threshold firms, as shown in table 1. These patterns suggest that firms severed Section 8-eligible interlocks more than Section 8-ineligible interlocks, supporting the hypothesis that firms severed those interlocks to lower the enforcement risk related to the Clayton Act Section 8. This event study on the number of interlocks suggests that regulators' efforts since the DOJ announcements in April 2022 are effective in reducing interlocking directorate among targeted firms, further validating the policy shock.

5.2.3. *Sharp Regression Discontinuity on Number of Interlocks*

To enhance the causal interpretation, I use a sharp regression discontinuity design to answer the question of whether Section 8 enforcement caused firms to reduce their directorate interlocks. To achieve this, I leverage the sharp equity threshold in Section 8. The Clayton Act Section 8 exempts all firm pairs from interlocking directorate prohibition if either firm has “capital, surplus, and undivided profits” smaller than an annually updated threshold. I obtain the latest quarterly book value of equity ($cstkq + capsq + req$) as of the event week from Compustat. The sharp equity threshold was \$41,034,000 for the year 2022, as published by the FTC in January 2022.

A complication in the identification is that not every firm exceeding the threshold had an interlocking directorate with other companies. Consequently, some firms above the threshold were not impacted by Section 8 because they didn’t have a qualified interlock. This suggests that the treatment probability does not jump from 0 to 100% at the equity threshold. In order to apply sharp regression discontinuity design, which requires a discrete jump in treatment probability from 0 to 100%, I restrict the sample of the firms in the regression discontinuity analysis such that all sample firms above the equity threshold were involved in at least one firm pair that was targeted by Section 8. In this section, I adopt two different subsamples because the exact competition measure and granularity adopted by enforcement agencies are not observable. In the first specification, I restrict my sample to the firms that had at least one interlocked firm that had an equity value above the threshold as of the last day in March 2022. In this specification, I do not consider the competition metric. In the second specification, I restrict my sample to the firms that had at least one interlocked competitor that had an equity value above the threshold. In this

specification, I am conservative by incorporating the competition metric at TNIC-4 level. For each subsample, I estimate the following regression:

$$Y_i = \alpha + \beta_1 \times D_i + \beta_2 \times Equity_i + \beta_3 \times D_i \times Equity_i + \epsilon_i \quad (3)$$

where D_i is a dummy equal to 1 for firm i if its latest book value of equity was greater than \$41,034,000 as of the event week of April 4th, 2022. To facilitate interpretation, $Equity_i$ here is the latest book value of equity of firm i minus the equity threshold, in millions of dollars. The local treatment effect of interest will be captured by the estimate for β_1 . Two dependent variables corresponding to two different subsamples are used. The first dependent variable is the change in the number of interlocked above-threshold firms. The second dependent variable is the change in the number of interlocked above-threshold competitors at TNIC-4 level. I compute the dependent variables at two horizons to capture the change over time. First, I compute the change from March 31st, 2022 to April 30th, 2022. This measures the change that happened within one month after the announcements. Second, I also compute the change from March 31st, 2022 to May 31st, 2022. This measures the change over the two months following the shock. Since appointing and removing directors requires time-consuming administrative processes, one may expect the change in interlocking directorate to gradually happen in a few months, as demonstrated in figure 2.

Note that the construction of this regression discontinuity analysis is not cross-sectional, as typical in regression discontinuity designs. It's similar to the "first-difference estimator" (Lemieux and Milligan, 2008), which is derived from taking the difference between two cross-sectional regressions. The intuition behind this construc-

tion is that the correct measure for the treatment effect is the difference in the outcome variable before and after the treatment. Lemieux and Milligan (2008) argue that this "first-difference estimator" is more stringent than cross-sectional regression discontinuity because it further controls for individual-specific fixed effects. This relaxes the assumption of cross-sectional regression discontinuity that individuals around the threshold are similar by comparing the changes within each individual at different times. These authors also address the potential selection bias concern in subsample regression discontinuity by proving that such selection bias will not affect the validity of this test as long as the selection bias is a smooth function of the running variable. There is no evidence for the violation of this condition in my sample.

I estimate the above regression with a bias-corrected estimator and the corresponding robust standard error Calonico, Cattaneo, and Titiunik (2014). I adopt a data-driven bandwidth selector to optimize the mean squared error (Calonico et al., 2014). Before applying the bandwidth selector, I manually drop firms with book values of equity that were either greater than 101 times the equity threshold or smaller than negative 99 times the equity threshold. This manual exclusion is very generous and unlikely to affect the estimation of treatment effects. Such manual exclusion on both sides is necessary for the above estimator to work. I report the bias-corrected point estimates for the treated effect and the corresponding robust standard errors in table 3.

From table 3, the first observation we can make is that the estimated local treatment effect is negative across all four specifications. By comparing results at different horizons in table 3, we can see that the treatment effect is increasing over time. This finding is consistent with the expectation that the change in directors should happen over a few months. Out of four specifications, the estimated local treatment effect is negative and significant in three specifications. The results in table 3 indicate that the

DOJ announcements on Section 8 enforcement caused firms to sever their directorate interlocks. Overall, the results from difference-in-differences and regression discontinuity exercises in this section validate the DOJ announcements in April 2022 as a policy shock to the enforcement probability on interlocking directorate among US firms by showing that firms lost directorate interlocks following the announcements.

5.3. Event Study on Stock Returns

Having confirmed hypothesis 1 by showing the negative impact of the DOJ announcements on the number of director interlocks, I continue to test hypothesis 2 by investigating whether shareholders experienced a loss in stock values following the enforcement announcements. Academic literature (Barone et al., 2025; Geng et al., 2024; Gopalan et al., 2024; Herrera-Caicedo et al., 2024; Begley et al., 2025) has shown that reducing interlocking directorate benefits consumers and employees. However, it's crucial to understand the net effect of interlocking directorate prohibition on firm value to comprehensively understand the value of directorate interlocks with competitors. In the remainder of this section, I examine how stock prices evolved around the announcement events.

In this section, I first conduct an event study to investigate the stock market reaction around the events of DOJ announcements in the week of April 4th, 2022, and then I perform a heterogeneity analysis to investigate what firm characteristics are related to such reactions. As described earlier, the assistant attorney general Jonathan Kanter of the US Department of Justice Antitrust Division made two public announcements, indicating DOJ's interest in enforcing the Clayton Act Section 8, at both the 2022 Spring Enforcers Summit on April 4th, 2022, and the American Bar Association spring meeting on April 8th, 2022. While the Spring Enforcers Summit

was mainly attended by regulators around the globe, the ABA spring meeting was targeting all practicing lawyers in the US. These pieces of news were largely unanticipated by the market because they were the first, in the recent decades, high-profile indication of DOJ’s intent to enforce Section 8 and to crack down on interlocking directorate. It signaled a shift in the emphasis of the DOJ Antitrust Division, and then more US regulators followed suit. Since both announcements were made in the same week, I use this week as the event week and conduct the event study at the weekly level.

5.3.1. *Weekly Event Study Plot on Stock Returns*

To capture the difference in the stock returns between treatment and control groups, I run the following two-way fixed effect regression:

$$R_{it} = \sum_{r=-8}^8 \beta_r \times \mathbf{1}[Treated_i = 1] \times \mathbf{1}[t = r] \times \mathbf{1}[r \neq -1] + \delta_t + \lambda_i + \epsilon_{it} \quad (4)$$

where t is the week relative to the event week, which is the week of April 4th, 2022 in this case. $\mathbf{1}[Treated_i = 1]$ is an indicator variable that is equal to 1 if the firm i is treated by Section 8 enforcement, as defined in previous sections. I set both the pre-event window and post-event window to 8 weeks. Thus, r will range from -8 to 8. The dependent variable, R_{it} , is the week-end Fama-French 5-factor and 3-factor cumulative abnormal return, starting from the beginning of my event window. To compute cumulative abnormal return, I first estimate the Fama-French 5-factor model on daily stock returns of each stock using all daily observations in 2021, the year before DOJ announcements. Then I compute the daily Fama-French

5-factor and 3-factor abnormal returns for each stock on each day in my event window, and then aggregate daily abnormal returns to weekly abnormal returns. Finally, I use these weekly abnormal returns to compute weekly cumulative abnormal returns from the beginning of the event window. Fama-French abnormal returns are used to control for the potential confounding factors of different market exposure, size, growth perspective, profitability, and investment aggressiveness between treated and control firms. Coefficients of interest are β_r , which denotes the difference in return between treated firms and control firms in week r . β_{-1} is not estimated to avoid the multicollinearity problem. Standard errors are double clustered at the firm and week level, and fixed effects on firm and week are included. The point estimates and 95% confidence intervals for β_r in the above regression are plotted in figure 3.

In figure 3, differences in weekly Fama-French 5-factor and 3-factor cumulative abnormal returns between treated and control groups are plotted. Figure 3 shows that differences in both Fama-French 5-factor and 3-factor cumulative abnormal returns show very similar patterns around the announcement week. There are roughly parallel trends in the returns of treated and control firms in the pre-event window. Immediately after the event week, treated firms started to underperform control firms and continued to underperform in the following weeks. Starting from week 3 relative to the event week, the difference between treated and control firms in both Fama-French 5-factor and 3-factor cumulative abnormal returns started to stabilize between -5% and -10%. This pattern suggests that the DOJ's announcements on interlocking directorate prohibition enforcement are followed by a 5% to 10% disproportionate drop in the stock values of affected firms. Nonetheless, one noticeable pattern in figure 3 is that the drop in stock price did not start in the event week. This may be explained by the fact that the more public-facing announcement at the ABA meeting took place on Friday, and thus the market did not fully react to the news in

the event week. The in-person format of the ABA meeting might have also slightly slowed down the diffusion of this news. In fact, at the daily level, the largest drop in stock returns of treated firms happened on Monday of the following week, which suggests that the stock market digested most of the information in the announcement at the ABA meeting over the weekend, and this information was immediately reflected in the stock price on the first trading day following the announcement. The fact that the treatment effect on stock returns gradually took place over a few weeks may be explained by the market’s initial uncertainty about the scale and intensity of the enforcement on Section 8. Because Section 8 has not been enforced for decades, the market couldn’t fully appraise the full impact of such enforcement until additional details on enforcement were gradually released in the weeks after the initial announcements.

5.3.2. *Difference-in-Differences Regression on Stock Returns*

Having visually shown that stock returns of treated firms underperformed control firms following the DOJ announcement, I attempt to numerically quantify the amount of loss in shareholder value by adopting the following difference-in-differences regression at the stock-weekly level:

$$R_{it} = \alpha + \beta_1 \times Treated_i \times Post_t + \delta_t + \lambda_i + \epsilon_{it} \quad (5)$$

where R_{it} includes 3 cumulative return measures. The first one is the Fama-French 5-factor cumulative abnormal return for stock i from the beginning of the pre-event window to the end of week t . The second one is the Fama-French 3-factor cumulative abnormal return for stock i from the beginning of the pre-event window to the end of

week t . The third one is the raw cumulative return for stock i from the beginning of the pre-event window to the end of week t . $Treated_i$ is a dummy equal to 1 for stock i if it were treated by the Clayton Act Section 8, as determined by the three criteria discussed earlier. $Post_t$ is a dummy equal to 1 if week t is equal to or later than the week of April 4th, 2022. I include firm and week fixed effects, and use standard errors double-clustered at the stock and week level. In the above regression, the estimate for β_1 will represent the treatment effect of the DOJ announcements on stock return. The results of this regression are presented in table 4.

The main observation from table 4 is that the treatment effect is significantly negative across all specifications. From the first column, we can see that the average treatment effect on stock returns is -7.1%, even after controlling for Fama-French 5 factors. While the second column shows a smaller treatment effect on Fama-French 3-factor cumulative abnormal returns, the statistical significance of the estimate is still at the 5% level. The third column shows that if we don't control for any asset pricing factor, the treatment effect on stock returns is -10.6%. The -7.1% drop in stock value after controlling for Fama-French five factors may seem very large at plain sight, but it is a reasonable amount if we relate it to other estimates for director values in the literature. Nguyen and Nielsen (2010) estimate the value of an independent director to be around 0.85% of the firm value by using incidences of sudden death of independent directors. Burt, Hrdlicka, and Harford (2020) show that a director contributes to firm value by 1% every year after accounting for Fama-French 5 factors. These estimates are for an average director and an average independent director, respectively. As literature (Geng et al., 2024; Barone et al., 2025; Cabezón and Hoberg, 2025) has shown numerous ways a director interlock could benefit firms, the value of an interlocking director with a product market competitor should be much higher than the 1% estimate for an average director in Burt et al. (2020).

We should notice that the 7.1% short-term drop in stock price is likely a lower bound for the cost of the Clayton Act Section 8 enforcement to shareholders for the following two reasons. First, some acts of collusion are not easily observable by outside investors, and thus the potential termination of collusion is not fully reflected in short-term stock price, even if investors know the firm is going to lose directorate interlocks. If acts of collusion are easily observable by outside investors, they should also be observed and thus already stopped by regulators, as per antitrust laws other than the Clayton Act Section 8, before the enforcement of Section 8. Therefore, even if the market knows a firm pair is going to lose an overlapping director due to the DOJ enforcement, investors will underestimate its impact on firm fundamentals until it's directly reflected in the deteriorating accounting performance. This argument suggests that the stock return of treated firms may suffer more in the long run than in the short run, due to the difficulty of observing acts of collusion through interlocking directorate. Second, although DOJ Assistant Attorney General Jonathan Kanter announced that the DOJ would start to enforce Section 8, the market might not be certain regarding the speed, scope, and intensity of the eventual enforcement. Therefore, the market response immediately after the announcement may not reflect the full impact of Section 8 enforcement on interlocking directorate among public firms. Instead, the market may gradually find out that DOJ and other regulators are very ambitious about Section 8 enforcement, especially after repeated announcements by multiple regulators and news about the actual removal of many interlocking directorates¹⁵¹⁶.

¹⁵<https://www.justice.gov/archives/opa/pr/directors-resign-boards-five-companies-response-justice-department-concerns-about-potentially>

¹⁶<https://www.justice.gov/archives/opa/pr/justice-department-s-ongoing-section-8-enforcement-prevents-more-potentially-illegal>

5.3.3. *Sharp Regression Discontinuity on Stock Returns*

I use a sharp regression discontinuity design to enhance the causal interpretation of the relation between DOJ announcements and stock returns. I estimate the following regression equation:

$$R_i = \alpha + \beta_1 \times D_i + \beta_2 \times Equity_i + \beta_3 \times D_i \times Equity_i + \epsilon_i \quad (6)$$

where D_i is a dummy equal to 1 for firm i if its latest book value of equity was greater than \$41,034,000 as of the event week of April 4th, 2022. $Equity_i$ is the latest book value of equity of firm i minus the equity threshold, in millions of dollars. The local treatment effect of interest will be captured by the estimate for β_1 . The dependent variable is the daily Fama-French 5-factor abnormal return on April 11th, 2022, the trading day immediately after the DOJ announcement on April 8th, 2022. I choose to use the daily return for the next trading day because April 11th, 2022, marks the date with the greatest disparity in stock returns between the treated and control groups. Considering the Department of Justice made its announcement at the American Bar Association’s spring meeting around noon, it is plausible for the stock market to respond on the subsequent day, particularly since the American Bar Association meeting is not usually an event that garners substantial market attention.

I use the same subsamples as in table 3 to run this regression. I estimate the above regression with a bias-corrected estimator and the corresponding robust standard error Calonico et al. (2014). I adopt a data-driven bandwidth selector to optimize the mean squared error (Calonico et al., 2014). Before applying the bandwidth selector, I manually drop firms with book values of equity that were either greater than 101 times the equity threshold or smaller than negative 99 times the equity threshold. I

report the bias-corrected point estimates for the treated effect and the corresponding robust standard errors in table 5.

From table 5, one can observe that the estimated local treatment effect on stock returns is negative in both columns. For the subsample of firms that had at least one interlock with an above-threshold firm, the estimated treatment effect is -1.1% Fama-French 5-factor abnormal return. Although this point estimate is much smaller than the estimated average treatment effect in table 4, the statistical significance supports the causal relation between the Section 8 enforcement and the drop in firm value. This implies that investors, at least some of them, not only efficiently process information from various sources but also can identify the equity threshold mentioned in Section 8 and factor it into their investment decisions. Given that the equity threshold is not easily noticeable to people unfamiliar with Section 8, it's surprising to observe this significant result in the regression discontinuity analysis on stock returns.

The local treatment effect on stock returns estimated in the regression discontinuity exercise is smaller than the average treatment effect estimated in the difference-in-differences analysis in table 4. There are three possible reasons for this difference. First, the regression discontinuity exercise relies on a discrete jump in stock return for firms around the threshold. Finding out the equity threshold for Section 8 is not a trivial task for investors, as it requires reading through long legal texts. It's plausible that only a small number of investors can incorporate this information on the equity threshold into their investment decisions in a short period of time, and their impact on the stock price may be limited. Second, the time horizons of stock returns are different in the difference-in-differences analysis and this regression discontinuity exercise. Stock returns considered in this regression discontinuity are only for one day after the announcement, while I consider the stock returns over two months following the events in the difference-in-differences analysis. There may be a difference in short-

term and long-term stock market reactions if the market kept finding new information regarding the Section 8 enforcement, as very likely in reality. Third, regression discontinuity measures the local treatment effect, but difference-in-differences estimates the average treatment effect across all treated firms, many of which are far from the equity threshold. One may expect that firms of different sizes were differentially affected by the enforcement of Section 8.

5.3.4. Robustness Tests For Stock Performance

There may be concerns that the difference in stock returns between treated firms and control firms may be attributed to differences between industries, because treated firms are clustered in a few industries. To address this concern, I conduct a difference-in-differences analysis on stock returns, while controlling for different industries. I do so by including industry-week fixed effects in the difference-in-differences regression above. The dependent variable in this regression is the cumulative raw return since the start of the pre-event window, which starts eight weeks from the event week. I include both firm fixed effects and week-industry fixed effects in the regression. I do not include week fixed effects because the interaction between week and industry subsumes week fixed effects. Including industry-week fixed effects is equivalent to demeaning the cumulative stock returns by industry. This practice removes the impact of industry average returns on the estimated treatment effects. I also adopt two different ways to cluster the standard errors. I double-cluster the standard errors at the firm and week level and the industry and week level, respectively. To identify the industry of a firm, I use the SIC code as of the event week.

The results of this regression are presented in table 6. The first thing to note in table 6 is that the number of observations declines after including more granular industry controls because the singleton observations at the industry-week level are

excluded, suggesting that there are more industries with a single firm when we define industries at a more granular scale. We can observe that after controlling for industry average returns at the 3-digit SIC level, the magnitude of the treatment effect reduces to -5.6%. This suggests that part of the treatment effect we observed in table 4 is attributed to industry-level variation in stock returns. However, the treatment effect is still negative and significant at the 1% level after controlling for industry-level differences. When I control for the industry average returns at the more granular 4-digit SIC code level, the point estimate for the treatment effect further reduces to -4.4%, and the statistical significance is at the 5% level. The results in table 6 suggest that the estimated treatment effect of DOJ announcements on firm value is not due to differential stock performance among different industries.

Another potential confounding factor behind the underperformance of treated firms is inflation news. In the morning of April 12th, 2022, the United States Bureau of Labor Statistics announced an 8.5% increase in the consumer price index year-over-year, the sharpest increase in decades, and the S&P 500 index dropped by 2.1% in this week. The week of this inflation announcement coincided with the first week after the DOJ announcements when we saw a large underperformance in the stock returns of treated firms. To investigate whether the underperformance of treated firms was due to their high exposure to inflation risk, I conduct placebo tests for three later dates in 2022 when the inflation numbers deviated from market consensus. The three placebo dates are June 10th, 2022, September 13th, 2022, and November 10th, 2022. On June 10th, 2022, the U.S. BLS reported another record-breaking 8.6% year-over-year increase in the consumer price index, and the S&P 500 index fell by 5.1% in that week. On September 13th, 2022, the consumer price index rose 8.3% year-over-year, and the S&P 500 index dropped by 4.8% in that week. Finally, on November 10th, 2022, the U.S. inflation cooled down to a 7.7% increase year-over-year. This

lower-than-expected inflation number was accompanied by a 5.9% rise in the S&P 500 index in the same week. I conduct event study around these three placebo weeks using equation 4 and plot the corresponding coefficients in figure 4. The dependent variable is Fama-French 5-factor cumulative abnormal return.

From figure 4, one can see that starting from the week of June 10th, 2022 when the surprise in inflation number was in the same direction as in April 2022, treated firms outperformed control firms by almost 20%, opposite to the direction we observed after the week of April 4th, 2022. Following the weeks of September 13th, 2022 and November 10th, 2022, the treated firms and control firms had similar stock returns. The results from these three placebo tests suggest that the underperformance in the stock returns of treated firms following the announcement week was not due to inflation risks.

5.4. Channel and Heterogeneity Analysis

After showing the underperformance in stock returns for treated firms following the announcements of Section 8 enforcement, I continue to investigate the channels through which the firm could benefit from interlocking directorate with competitors. Literature has shown that product market competitors could benefit from director interlocks through coordination in the product market (Barone et al., 2025; Gopalan et al., 2024), lower R&D and investment costs (Geng et al., 2024), and market segmentation (Poberejsky, 2024). At the same time, directorate interlocks are also shown to reduce firm profitability due to lower innovation and less product differentiation and competitiveness (Cabazon and Hoberg, 2025). In this section, I inform the academic debate by doing heterogeneity analyses on both the number of interlocks and stock returns to investigate which firm characteristics are related to firms' increasing direc-

torate interlocks and to the firm value creation by competitor interlocks, as assessed by the stock market investors.

5.4.1. Increasing Interlocks and Firm Competition

In this section, I conduct the first analysis on heterogeneity. I investigate what firm characteristics are related to increasing interlocking directorate. As shown previously in figure 1, there have been increases in both the number of interlocked firms and the number of interlocked competitors among US non-financial public firms since the early 2010s. To shed light on why firms chose to increase the number of their directorate interlocks, I plot both the number of interlocked firms and interlocked competitors again in figures 5 and 6, but this time, I separate the whole sample of firms into five groups, based on the concurrent value of total product similarities they have with all of the competitors. Different from all the analyses before, where I define competitors as firm pairs with TNIC score in the top 1%, I use the sum of TNIC scores with all competitors at TNIC-3 level, which is the top 2.05% of all firm pairs (Hoberg and Phillips, 2016). As discussed earlier, TNIC-4 level competition classification captures competition better than TNIC-3 level, but I choose to use TNIC-3 level competition classification here simply because there is a sizable portion of firms with no TNIC-4 level competitors. Therefore, if I divide firms into five groups based on total product similarity with TNIC-4 competitors, more than 20% of the firms will have zero value, resulting in unstable sorts and plots, especially when plotting the average number of interlocked firms. This problem does not exist when I use the sum of TNIC scores with TNIC-3 level competitors, because there are sufficiently many competitor pairs under this specification. However, even if I group the stocks by total similarity scores with TNIC-4 level competitors, the major patterns will be similar to those exhibited in figures 5 and 6, except for irregularities in the two subplots with the lowest similarity

scores. The values on the y-axes of the two figures are computed at the end of each calendar month. Thus, I perform grouping of stocks for each month separately, based on the most up-to-date TNIC scores from the previous fiscal year.

Different from the increasing trend in the number of interlocks for the whole firm sample, as demonstrated in the top panel of figure 1, figure 5 shows that the average number of interlocked firms for the 4 groups with lower TNIC-3 total similarity remains stable between 3 and 4 throughout the years. However, the average number of interlocked firms for the top 20% of firms with the highest TNIC-3 total similarity increased by a wide margin from around 4 to 6 between 2013 and 2022. It's clear that the overall increase in the average number of interlocked firms is mainly attributed to the subset of firms that have the most homogeneous products with their competitors. Figure 6 exemplifies a much starker contrast between the top 20% of firms in total similarity and the remainder of the firms. The average number of interlocked competitors has stayed around 0 throughout the years for the bottom 80% of firms in total similarity, whereas the 20% of the firms with the highest total product similarity have experienced a sixfold increase in the number of interlocked competitors, again accounting for almost all the increase we observe in the full sample. One may argue that since both the sorting variable and the dependent variable are positively related to the number of interlocked competitors, there might be a mechanical relation that underlines the patterns in figure 6. I confirm that even when I switch the sorting variable to the average value, instead of the sum, of TNIC scores with TNIC-3 level competitors, the exact same pattern persists. Moreover, there is no straightforward mechanism where the number of interlocked firms and total similarity with competitors should be correlated, but the pattern that the top 20% of firms in total similarity accounts for the majority of the increase in the number of interlocks is also prominent in figure 5.

Figures 5 and 6 both show that the subset of firms with the most homogeneous products with competitors account for most of the increase in interlocking directorate we observe in recent years. If this increase in interlocking directorate was proactively chosen by this subset of firms, then there must be some benefit to this subset of firms by doing so. This suggests that having interlocking directorate is beneficial to the firms, and the size of this benefit is positively related to the product market closeness a firm has with its competitors.

5.4.2. *Stock Performance and Firm Competition*

After finding that firms with closer product market competitors contribute to most of the recent increase in interlocking directorate, I conduct a heterogeneity analysis on the magnitude of stock underperformance among treated firms. As demonstrated in the previous section, product similarity with competitors is potentially related to firms' decisions on whether to have directorate interlocks. I investigate whether the same characteristic is considered influential to firm value by the stock market investors.

To perform the heterogeneity analysis on stock returns, I adopt the similar setup of difference-in-differences as in section 5.3.2, except that I add a triple interaction term with the variables on the firm characteristics of interest. The regression equation is as follows:

$$\begin{aligned}
 R_{it} = & \alpha + \beta_1 \times Treated_i \times Post_t \\
 & + \beta_2 \times Treated_i \times Post_t \times Competition_i + \delta_t + \lambda_i + \epsilon_{it}
 \end{aligned}
 \tag{7}$$

where R_{it} includes 3 cumulative return measures, including Fama-French 5-factor

cumulative abnormal return, Fama-French 3-factor cumulative abnormal return, and the raw cumulative return for stock i from the beginning of the pre-event window to the end of week t . $Treated_i$ is a dummy equal to 1 for stock i if it was treated by the Clayton Act Section 8, as determined by the three criteria discussed in section 5.1. $Post_t$ is a dummy equal to 1 if week t is equal to or later than the week of April 4th, 2022. $Competition_i$ is one of the two variables: the sum of TNIC scores with all TNIC-4 level competitors or the number of TNIC-4 level competitors. The competition variables are from fiscal year 2021, which were up to date as of April 2022. I include firm and week fixed effects, and use standard errors double-clustered at the stock and week level. The competition variables are not included as a standalone term in the regression equation because they would have been absorbed by the firm fixed effect. The results of this regression are presented in table 7.

From table 7, we can observe that the triple interaction terms with the two competition variables have negative coefficients in all six columns for all three measures of stock returns. The negative coefficients for the triple interaction with the sum of product similarity scores with all TNIC-4 competitors mean that among the treated firms, the underperformance of the stock returns following the announcement events is negatively related to the total product similarity with competitors. It shows that the treated firms with more homogeneous products with competitors had a lower stock return than other treated firms following the shock. The coefficients for the triple interaction term with the number of competitors are also negative across three return specifications, meaning that a treated firm had a lower stock return after the announcements if it operates in product markets with more competitors. Importantly, these negative coefficients of triple interaction terms should be interpreted as the heterogeneity in treatment effect among treated firms, instead of the difference between treated firms and control firms. Moreover, we observe that after including the triple

interaction terms in the regressions, the coefficients of the double interaction terms become positive, opposite to the negative signs in table 4. This suggests that the negative treatment effect we observe in table 4 is fully explained by product market competition.

There may be concerns that the treatment classification could be correlated with the two competition variables, because the total similarity is potentially related to the interlocking directorate among competitors, as shown in figure 6, making estimations in table 7 unreliable. To address this concern, I perform cross-sectional regressions on post-event stock returns with the subset of treated firms only. I run the following cross-sectional regression:

$$R_i = \alpha + \beta_1 \times Competition_i + \beta_2 \times Controls_i \quad (8)$$

where R_i is the Fama-French 5-factor cumulative abnormal return for stock i from the end of treatment week to the end of the third week after the treatment. $Competition_i$ is one of the two variables on the product market competition faced by firm i . The two competition variables are the same as defined in the difference-in-differences regression in table 4. Control variables include market capitalization, book-to-market ratio, debt-to-equity ratio, volatility, and Amihud liquidity ratio. Market capitalization, book-to-market ratio, and debt-to-equity ratio are the latest as of the beginning of the treatment week. Volatility and Amihud illiquidity ratio are computed from stock returns and trading volumes in the 21-trading day period immediately before the treatment week. All firms included in this regression are treated firms, as defined in section 5.1.

I choose to compute the cross-sectional return from the end of treatment week

to the end of the third week after treatment, because this is the period where the treatment effect on stock returns is the most prominent, as shown in figure 3. Figure 3 shows that the difference in stock returns between treatment and control groups stabilized after the end of week 3. At the daily level, the largest movement in stock price took place on the first trading day of week 1, which is also included in the sample period for this cross-sectional regression. This large movement on the first trading day following the ABA meeting announcement fits well with the policy shock I propose in this paper, because the ABA meeting on Friday was more public-facing than the Spring Enforcer meeting on Monday, which targeted regulators around the globe. It's reasonable for the market to digest the policy shock and identify the affected firms over the weekend and then react on the following trading day. Therefore, the sample period from the end of treatment week to the end of the third week after treatment is likely to capture the majority of the treatment effect on the stock price.

The results from the cross-sectional regression above are presented in table 8. The first observation from table 8 is that both the total similarity with competitors and the number of TNIC-4 level competitors are negatively correlated with the post-event stock returns of treated firms. This is consistent with the observation in difference-in-differences regressions in table 7, suggesting that the results in table 7 are not driven by potential correlation between treated dummy and competition variables. Moreover, the inclusion of control variables does not affect the estimates for the main competition variables. It's not surprising that the coefficients for the control variables are not significant, because most of the factors affecting stock returns are already controlled for in the computation of Fama-French 5-factor abnormal returns. The results from this cross-sectional regression corroborate with the results from the previous difference-in-differences regression and confirm the finding that among treated firms, those with more homogeneous competing products and more competitors had lower

stock returns following the announcements on Section 8 enforcement.

Overall, there are two major findings from heterogeneity analysis. First, firms that had the most homogeneous products with competitors were the main driving force behind the rapidly increasing interlocking directorate among US non-financial public firms in the recent decade. Second, among the firms affected by the Section 8 enforcement, those with more homogeneous competing products and more competitors were the ones experiencing larger drops in firm value. These results are consistent with the theory that competing firms collude in the product market through sharing common directors. As suggested by canonical economic models on competition, it's more beneficial for firms to collude if they operate in more similar markets. However, coordinating and sustaining such collusion requires a communication channel and a monitoring channel. Without a covert communication channel, it might be hard for competitors to coordinate on collusion without attracting attention from regulators. Without a reliable monitoring channel, it's hard to detect deviation, and thus sustaining collusion could be dependent on many restrictive conditions (Harrington and Skrzypacz, 2011). Interlocking directorate could serve as both a private communication channel and an effective monitoring channel because directors could both privately interact with management teams and reliably monitor firm strategies and product market actions (Antón, Ederer, Giné, and Schmalz, 2023; Barone et al., 2025). Hence, the product market collusion channel could explain the findings in this section that the coexistence of common directors and product homogeneity brings value to the firm. This suggests that an increasingly important way a director could provide value to the firm is by acting as a channel for covert two-way information exchange between rival firms.

Moreover, the findings in the heterogeneity analysis reject the alternative theory of lower board quality. As the enforcement of Section 8 reduced the number of firms

a director can simultaneously work for, the total supply of qualified directors would decrease as a result. Since in equilibrium, a firm will hire the best quality directors who can provide the highest monitoring and advisory value to it, the lower director supply will result in many firms replacing current high-quality directors with lower-quality ones. However, this expected reduction in monitoring and advisory value should not vary with the level of product market competition faced by each firm, contradicting the findings in the heterogeneity analysis.

6. Conclusion

In this paper, I propose a novel policy shock to the enforcement probability regarding board interlocks between product market competitors. In the first part of the paper, I validate this shock by confirming its negative impact on the number of interlocks among U.S. non-financial public firms through both an event study and a regression discontinuity exercise. Then I evaluate the stock market reaction to this policy shock and confirm that this policy shock negatively affected the value of the treated firms. Lastly, I find that the recent rise in interlocking directorate is attributed to the firms facing the most intense competition in the product market and that the value of such interlocks is higher for firms facing higher levels of competition. These findings support the theory that competing firms collude in the product market by sharing directors.

I make two major contributions to the literature. First, I add to the corporate governance literature by showing that an important function of the corporate board could be enabling covert two-way information flow between firms, especially those competing in the product market. Second, I provide insight to the contemporaneous literature on interlocking directorate between competitors by measuring the overall

value of such interlocks to the firms through stock prices.

Appendix A. Tables

Table 1: Summary Statistics for Treated and Control Group in Event Study

This table presents the summary statistics on basic firm characteristics and interlocking directorate measures for the firms in my sample. The sample is divided into two groups: treated group and control group. Treated group and control group are defined as in section 5.1. The beginning number of interlocking directorates are cross-sectional as of March 31st, 2022. The change in the number of interlocking directorate measures the change from March 31st, 2022 to December 31st, 2022. The last column presents the summary statistics for all firms in my whole sample. Mean values and standard deviations (in parentheses) are reported, except in the first row.

	Control	Treated	Total
N	2,751 (89.2%)	334 (10.8%)	3,085 (100.0%)
Book value of equity (in million dollars)	3639.64 (15946.17)	532.12 (1368.22)	3299.00 (15084.61)
Debt-to-equity ratio	1.22 (13.85)	0.62 (1.05)	1.16 (13.08)
Book-to-market ratio	0.49 (0.77)	0.48 (0.42)	0.49 (0.74)
Beginning number of directors	9.49 (2.79)	9.15 (1.74)	9.46 (2.70)
Beginning number of common directors	2.97 (2.41)	4.93 (1.94)	3.19 (2.44)
Beginning number of interlocked firms	3.88 (3.48)	7.87 (3.89)	4.32 (3.73)
Beginning number of interlocked above-threshold firms	3.43 (3.28)	6.75 (3.75)	3.79 (3.49)
Beginning number of interlocked above-threshold competitors	0.07 (0.56)	4.09 (3.15)	0.50 (1.71)
Change in number of directors	0.01 (1.22)	0.00 (0.96)	0.01 (1.20)
Change in number of common directors	-0.07 (0.89)	-0.13 (0.99)	-0.08 (0.90)
Change in number of interlocked firms	-0.16 (1.28)	-0.68 (1.60)	-0.22 (1.33)
Change in number of interlocked above-threshold firms	-0.23 (1.23)	-0.92 (1.68)	-0.31 (1.31)
Change in number of interlocked above-threshold competitors	-0.01 (0.29)	-0.62 (1.31)	-0.08 (0.55)

Table 2: Monthly Difference-in-Differences on Interlocking Directorate

This table shows the change in the interlocking directorate among US public firms around April 2022, when Department of Justice made a series of announcements on stepping up the enforcement of the Clayton Act Section 8. The regression is at firm-monthly level. The sample period for this regression is from October 2021 (6 months before event month) to October 2022 (6 months after event month). The firm sample includes all non-financial public firms with non-missing equity observations from Compustat. The dependent variables reported in the table include: (1) number of linked firms through interlocking directorate, (2) number of interlocked firms that had the latest book value of equity greater than the equity threshold, (3) number of interlocked competitors, as defined by TNIC-4 similarity measure, that had the latest book value of equity greater than the equity threshold. These variables are computed for the end of each calendar month. Post dummy is equal to 1 if the month is later than or equal to April 2022, and equal to 0 if earlier than April 2022. Treated dummy is equal to 1 if this firm is involved in any firm pair that simultaneously satisfies the following 3 conditions: (1) both firms have equity value above threshold; (2) two firms have at least 1 common director; (3) two firms are identified as product market competitors by TNIC-4 measure. Firm and month fixed effects are included. Standard errors are double-clustered at firm and month level. *, **, *** denote 10%, 5%, 1% significance respectively.

Dependent Variable:	(1)	(2)	(3)
Post dummy $\times$ Treated dummy	-0.203** (0.085)	-0.377*** (0.105)	-0.352*** (0.080)
Constant	4.227*** (0.005)	3.708*** (0.006)	0.513*** (0.005)
Firm FE	YES	YES	YES
Month FE	YES	YES	YES
R-squared	0.96	0.96	0.98
N	41,730	41,730	41,730

Table 3: Regression Discontinuity on Change in Interlocking Directorate

This table presents results from sharp regression discontinuity analysis on the change in interlocking directorate. Two subsamples of firms are used. First subsample is the firms with at least one interlocked firm that was above the equity threshold, as of March 31st, 2022. Second subsample is the firms with at least one interlocked product market competitor, as identified by TNIC-4 measure, that was above the equity threshold, as of March 31st, 2022. First dependent variable is the change in the number of interlocked firms that was above the equity threshold. Second dependent variable is the change in the number of interlocked competitors that was above the equity threshold. Changes are measured in two horizons. First is the change from March 31st, 2022 to April 30th, 2022. Second is the change from March 31st, 2022 to May 31st, 2022. Treatment effect is estimated with bias correction and the corresponding robust standard errors (Calonico et al., 2014). Mean squared error-optimal bandwidth selection (Calonico et al., 2014) is applied after manually excluding firms with equity greater than 101 times the equity threshold or less than negative 99 times the equity threshold. *, **, *** denote 10%, 5%, 1% significance respectively.

Subsample:	Linked Above-Threshold Firm > 0		Linked Above-Threshold Competitor > 0	
Dependent variable:	Δ Linked Above-Threshold Firm		Δ Linked Above-Threshold Competitor	
Horizon:	April	May	April	May
Treatment effect	-0.056 (0.043)	-0.149** (0.061)	-0.108* (0.059)	-0.273* (0.163)
N	1,273	1,382	251	248

Table 4: Underperformance of Stock Returns for Treated Firms After DOJ Announcements

This table quantifies the difference in stock returns after the DOJ announcements between firms that were treated by the Clayton Act Section 8 and firms that were not treated. I adopt a difference-in-differences design. The regression is at firm-week level. The sample period is from 8 weeks before event week to 8 weeks after event week. Event week is the week of 4th April, 2022. The dependent variables are Fama-French 5-factor cumulative abnormal return, Fama-French 3-factor cumulative abnormal return, and cumulative raw return. All cumulative returns are computed from the beginning of week -8 to the end of the week of observation. Fama-French factor loadings are computed for each stock using all daily observations in 2021, the year before DOJ announcements. Treated dummy is equal to 1 if the stock was treated, as defined in section 5.1, by the Clayton Act Section 8. Post dummy is equal to 1 if the observation week is equal to or after the week of April 4th, 2022, and equal to 0 otherwise. Firm and week fixed effects are included. Standard errors are double clustered at firm and week level. *, **, *** denote 10%, 5%, 1% significance respectively.

Dependent Variable:	FF5 CAR	FF3 CAR	Cumulative Return
Treated dummy $\times$ Post dummy	-0.071*** (0.022)	-0.053** (0.020)	-0.106*** (0.023)
Constant	-0.027*** (0.001)	0.004*** (0.001)	-0.038*** (0.001)
Firm FE	YES	YES	YES
Week FE	YES	YES	YES
R-squared	0.75	0.73	0.71
N	51,725	51,725	52,392

Table 5: Regression Discontinuity on Stock Returns

This table presents results from sharp regression discontinuity analysis on the stock return. Two subsample of firms are used: (1) firms with at least one interlocked firm that was above the equity threshold, as of March 31st, 2022; (2) firms with at least one interlocked product market competitor, as identified by TNIC-4 measure, that was above the equity threshold, as of March 31st, 2022. The dependent variable is Fama-French 5-factor abnormal return on April 11th, 2022, the trading day immediately following the DOJ announcement on April 8th, 2022. Treatment effect is estimated with bias correction and the corresponding robust standard errors (Calonico et al., 2014). Mean squared error-optimal bandwidth selection (Calonico et al., 2014) is applied after manually excluding firms with equity greater than 101 times the equity threshold or less than negative 99 times the equity threshold. *, **, *** denote 10%, 5%, 1% significance respectively.

Dependent variable:	Fama-French 5-Factor Abnormal Return	
Subsample:	(1)	(2)
Treatment effect	-0.011* (0.006)	-0.005 (0.014)
N	1,551	272

Table 6: Underperformance of Stock Returns After Controlling For Industry

This table quantifies the difference in stock returns after the DOJ announcements between treated and control firms, after controlling for average returns in different industries. I adopt a difference-in-differences design. The regression is at firm-week level. The sample period is from 8 weeks before event week to 8 weeks after event week. Event week is the week of 4th April, 2022. The dependent variable is cumulative raw return from the beginning of week -8 to the end of the week of observation. Treated dummy is equal to 1 if the stock was treated, as defined in section 5.1, by the Clayton Act Section 8. Post dummy is equal to 1 if the observation week is equal to or after the week of April 4th, 2022, and equal to 0 otherwise. Both firm and industry-week fixed effects are included. The industry of a firm is classified by its 3-digit or 4-digit SIC codes as of the event week. Standard errors are either double clustered at firm and week level or double clustered at industry and week level. *, **, *** denote 10%, 5%, 1% significance respectively.

Dependent Variable:	Cumulative Return			
Industry classification:	SIC-3		SIC-4	
Treated dummy $\times$ Post dummy	-0.056*** (0.018)	-0.056*** (0.018)	-0.044** (0.018)	-0.044** (0.019)
Constant	-0.042*** (0.001)	-0.042*** (0.001)	-0.046*** (0.001)	-0.046*** (0.001)
Firm FE	YES	YES	YES	YES
Week-SIC FE	YES	YES	YES	YES
Clustering	Firm-Week	SIC-Week	Firm-Week	SIC-Week
R-squared	0.76	0.76	0.76	0.76
N	50,898	50,898	47,818	47,818

Table 7: Product Market Competition and Stock Returns After DOJ Announcements

This table demonstrates the relation between product market competition and stock underperformance following the announcement events. I adopt a difference-in-differences design. The regression is at firm-week level. The sample period is from 8 weeks before event week to 8 weeks after event week. Event week is the week of 4th April, 2022. The dependent variables are Fama-French 5-factor cumulative abnormal return, Fama-French 3-factor cumulative abnormal return, and cumulative raw return. All cumulative returns are computed from the beginning of week -8 to the end of the week of observation. Fama-French factor loadings are computed for each stock using all daily observations in 2021, the year before DOJ announcements. Treated dummy is equal to 1 if the stock was treated, as defined in section 5.1, by the Clayton Act Section 8. Post dummy is equal to 1 if the observation week is equal to or after the week of April 4th, 2022, and equal to 0 otherwise. Firm-level competitor total similarity is the sum of TNIC scores for each firm with all of its TNIC-4 level competitors in fiscal year 2021. Firm-level number of competitors is the number of its TNIC-4 level competitors in fiscal year 2021. Firm and week fixed effects are included. Standard errors are double clustered at firm and week level. *, **, *** denote 10%, 5%, 1% significance respectively.

Dependent Variable:	FF5 CAR		FF3 CAR		Cumulative Return	
Treated $\times$ Post	0.080**	0.084**	0.086***	0.089***	0.059**	0.064**
	(0.030)	(0.031)	(0.027)	(0.027)	(0.026)	(0.027)
Treated $\times$ Post $\times$ Competitor Total Similarity	-0.002***		-0.002***		-0.002***	
	(0.000)		(0.000)		(0.000)	
Treated $\times$ Post $\times$ Number of Competitors		-0.001***		-0.000***		-0.001***
		(0.000)		(0.000)		(0.000)
Constant	-0.027***	-0.027***	0.004***	0.004***	-0.038***	-0.038***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm FE	YES	YES	YES	YES	YES	YES
Week FE	YES	YES	YES	YES	YES	YES
R-squared	0.75	0.75	0.73	0.73	0.71	0.71
N	51,725	51,725	51,725	51,725	52,392	52,392

Table 8: Return Heterogeneity Among Treated Firms

This table presents results from cross-sectional regressions of stock returns after DOJ announcements for the subsample of firms treated by the announcements. The dependent variable is the Fama-French 5-factor cumulative abnormal return from the end of the treatment week to the end of the third week after the treatment week. Fama-French factor loadings are computed for each stock using all daily observations in 2021, the year before DOJ announcements. The main independent variables include the sum of product similarity scores with all TNIC-4 level competitors and the number of TNIC-4 level competitors. TNIC scores are from fiscal year 2021. The control variables include log of firm market capitalization, book-to-market ratio, debt-to-equity ratio, volatility, and Amihud illiquidity measure (Amihud, 2002). Both volatility and Amihud illiquidity measure are computed using daily data from the period of 21 trading days immediately before treatment week. All other control variables are from the latest data as of the beginning of treatment week. All firms included in this regression are treated by DOJ announcements, as defined in section 5.1. *, **, *** denote 10%, 5%, 1% significance respectively.

Dependent Variable:	FF5 CAR in weeks 1 to 3			
Competitor Total Similarity	-0.001*** (0.000)	-0.001*** (0.000)		
Number of Competitors			-0.000*** (0.000)	-0.000*** (0.000)
Log(Market Cap)		0.003 (0.011)		0.002 (0.011)
Book-to-Market		0.046 (0.030)		0.044 (0.030)
Debt-to-Equity		-0.011 (0.011)		-0.011 (0.011)
Amihid Illiquidity		-0.000 (0.000)		-0.000 (0.000)
Volatility		-0.052 (0.497)		-0.056 (0.497)
Constant	-0.004 (0.023)	-0.035 (0.100)	-0.001 (0.023)	-0.031 (0.101)
R-squared	0.07	0.09	0.07	0.08
N	334	334	334	334

Appendix B. Figures

Fig. 1. Monthly Evolution of Interlocking Directorate Among US Public Firms
This figure shows the monthly time-series plots on the interlocking directorate among all US non-financial public firms. The first panel shows the average number of interlocked non-financial firms for each US non-financial public firm at the end of each month. The second panel shows the average number of interlocked non-financial competitors for each US non-financial public firm at the end of each calendar month. Competitors are defined at TNIC-4 level. The vertical dashed line denotes April 2022, when 2 DOJ announcements on the Clayton Act Section 8 enforcement took place.

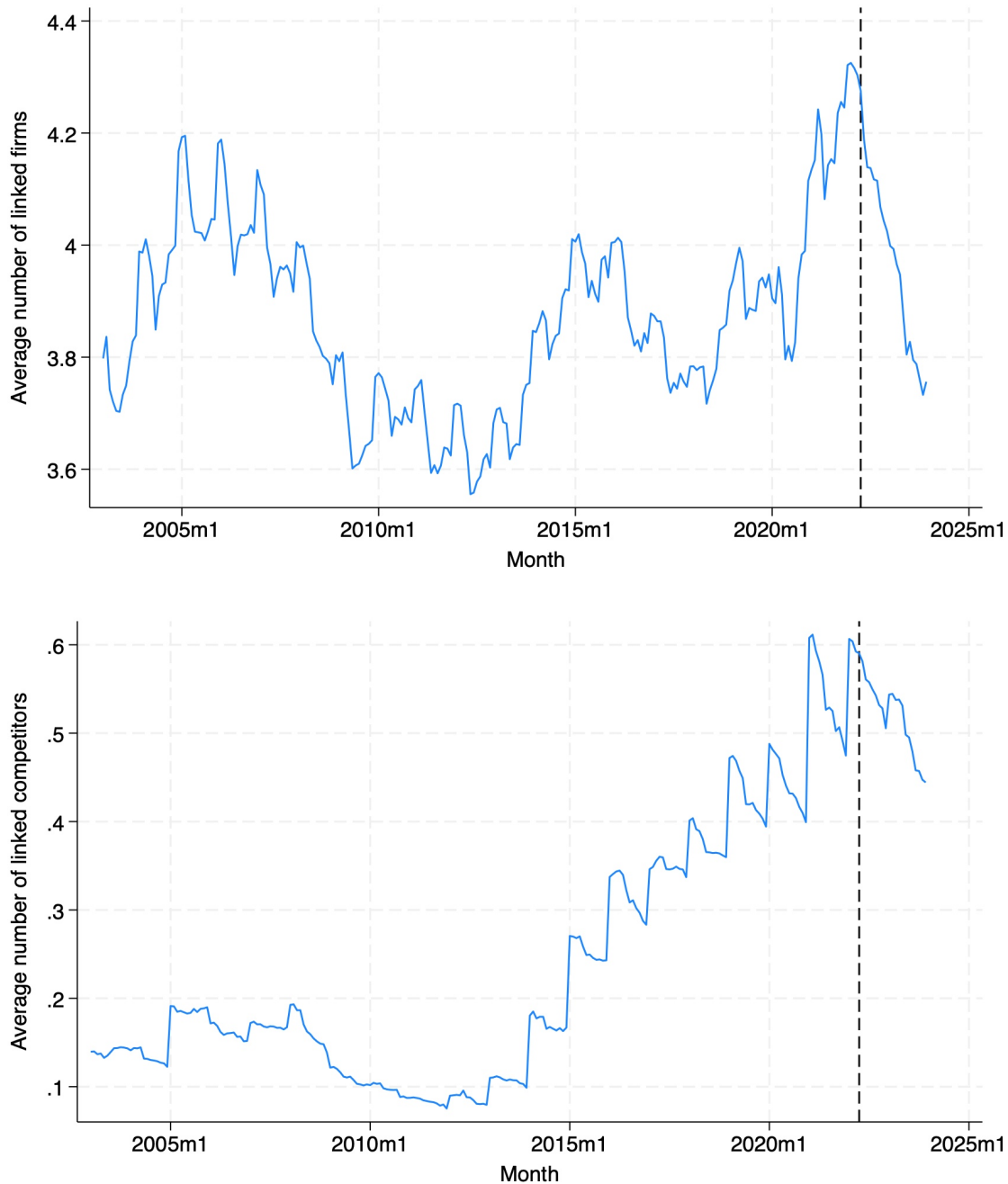


Fig. 2. Monthly Event Study Plot for Interlocking Directorate Around Announcements

This event study plot draws the evolution of differences in interlocking directorate connections between stocks treated by the Clayton Act Section 8 and stocks not treated. The event study regression is at firm-month level. The sample period for this regression is from October 2021 (6 months before event month) to October 2022 (6 months after event month). The firm sample includes all non-financial public firms with non-missing book value of equity observations from Compustat. Treatment classification is derived from 3 criteria: book value of equity, interlocking directorate, and product market competition, as described in section 5.1. The x-axis is the month relative to the month of event, where April 2022 is labeled as 0. The first panel shows the evolution of differences between treated and control firms in the number of firms interlocked through common directors. The second panel shows the evolution of differences between treated and control firms in the number of above-threshold firms linked through common directors. The third panel shows the evolution of differences between treated and control firms in the number of interlocked above-threshold product market competitors, as identified by TNIC-4 measure. The vertical dashed line denotes the month of event, April 2022. Standard errors are double-clustered at firm and month level. The confidence intervals are at 95% level.

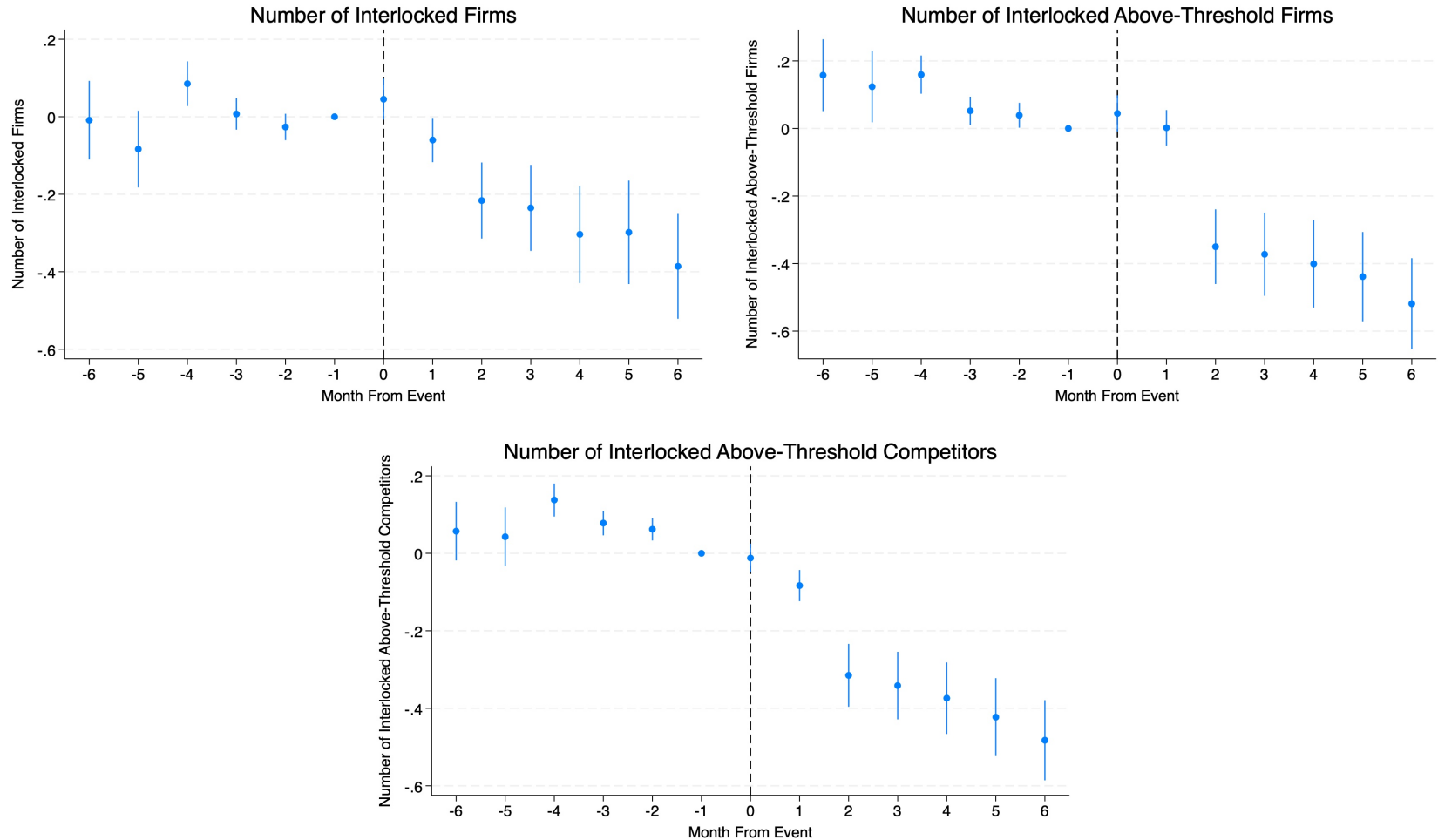


Fig. 3. Weekly Event Study Plot for Stock Return Around DOJ Announcements

This event study plot draws the evolution of differences in Fama-French 5-factor and 3-factor cumulative abnormal returns between stocks treated by the Clayton Act Section 8 and stocks not treated. The regression is at firm-week level. The sample period is from 8 weeks before event week to 8 weeks after event week. The firm sample includes all non-financial public firms with latest non-missing book value of equity observations from Compustat. Treatment classification is derived from 3 criteria: book value of equity, interlocking directorate, and product market competition. The y-axis is the Fama-French cumulative abnormal return since the beginning of the pre-window in decimal. The x-axis is the relative week from the beginning of the window. The vertical dashed line denotes the event week, the week of April 4th, 2022. The coefficients plotted represent the difference in FF5 and FF3 CAR between treated and control groups in corresponding weeks. The confidence intervals are at 95% level. Standard errors are double clustered at firm and week level.

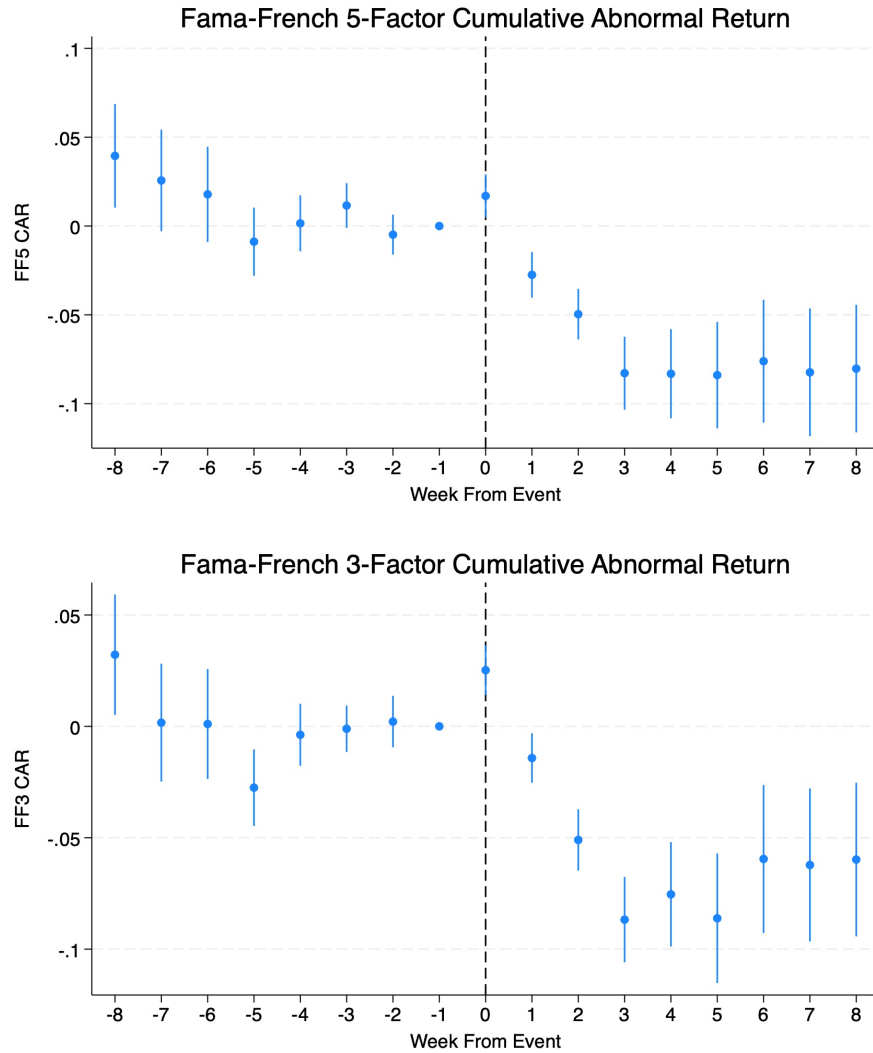


Fig. 4. Weekly Event Study Plot for Stock Return Around Inflation News Events

This event study plot draws the evolution of differences in Fama-French 5-factor cumulative abnormal returns between stocks treated by the Clayton Act Section 8 and stocks not treated around three inflation news events in 2022. The regression is at firm-week level. The sample period is from 8 weeks before event week to 8 weeks after event week. The firm sample includes all non-financial public firms with latest non-missing book value of equity observations from Compustat. Treatment classification is derived from 3 criteria: book value of equity, interlocking directorate, and product market competition. The y-axis is the Fama-French cumulative abnormal return since the beginning of the pre-window in decimal. The x-axis is the relative week from the beginning of the window. The weeks of three inflation report events are used. The dates of inflation news events are June 10th, 2022, September 13th, 2022, and November 10th, 2022. The vertical dashed lines denote the event weeks. The coefficients plotted represent the difference in FF5 CAR between treated and control groups in corresponding weeks. The confidence intervals are at 95% level. Standard errors are double clustered at firm and week level.

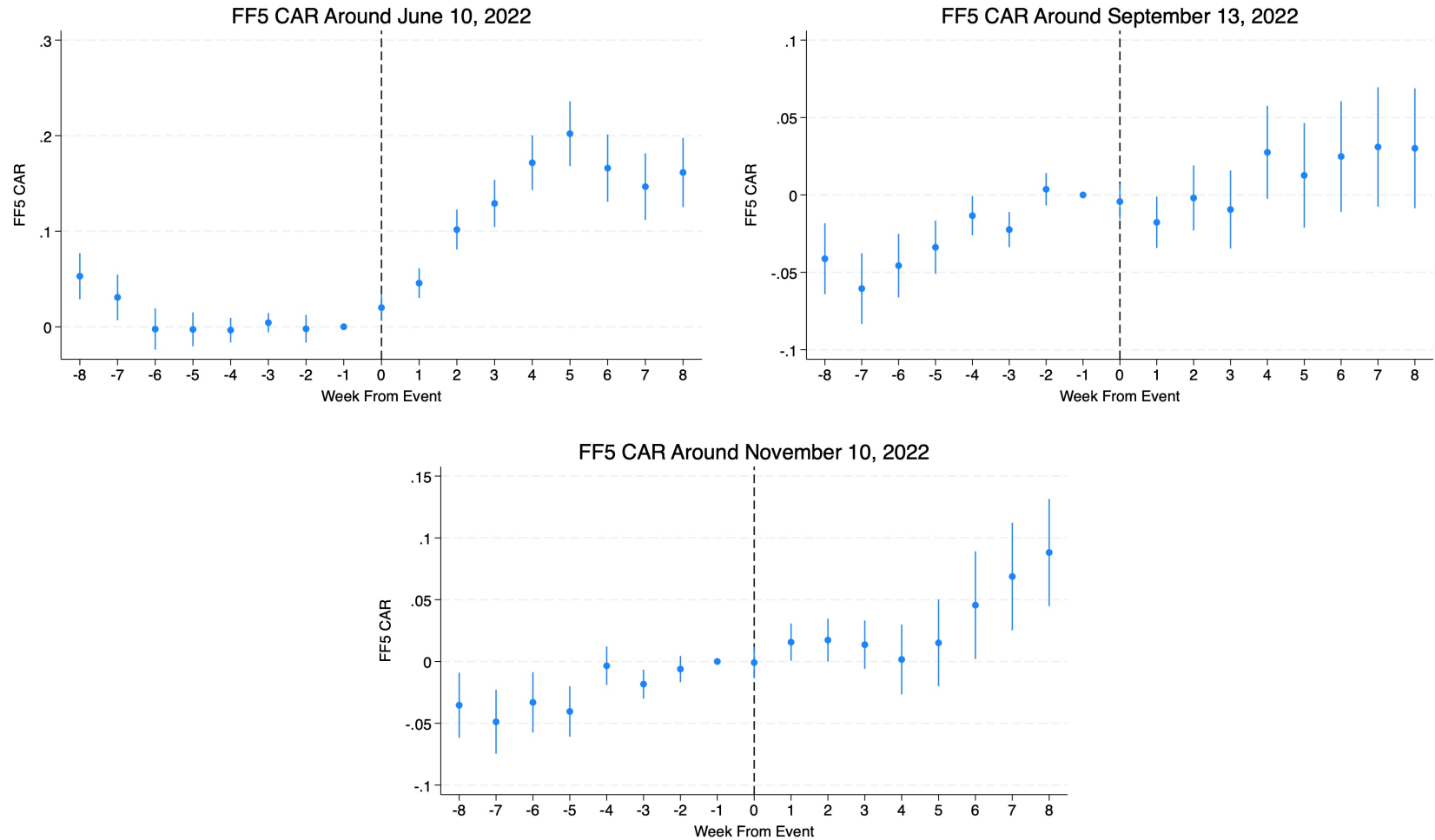


Fig. 5. Evolution of Interlocked Firms Grouped by TNIC-3 Total Similarity

This plot shows different trends in the number of interlocked firms among groups of stocks with different product similarity with competitors. For each month, I separate all non-financial US public firms into 5 groups based on the firm-month level sum of product similarity scores between the firm and all of its TNIC-3 competitors. Then for each group in each month, I compute the average number of interlocked non-financial firms across all firms in the group. The y-axis in the plot is the average number of interlocked firms. The x-axis is the calendar month. The number above each subplot represents the corresponding quintile for each subplot, with 1 denoting the group with the lowest product similarity with competitors and 5 denoting the group with the highest product similarity. Vertical dashed line denotes April 2022, the month of event.

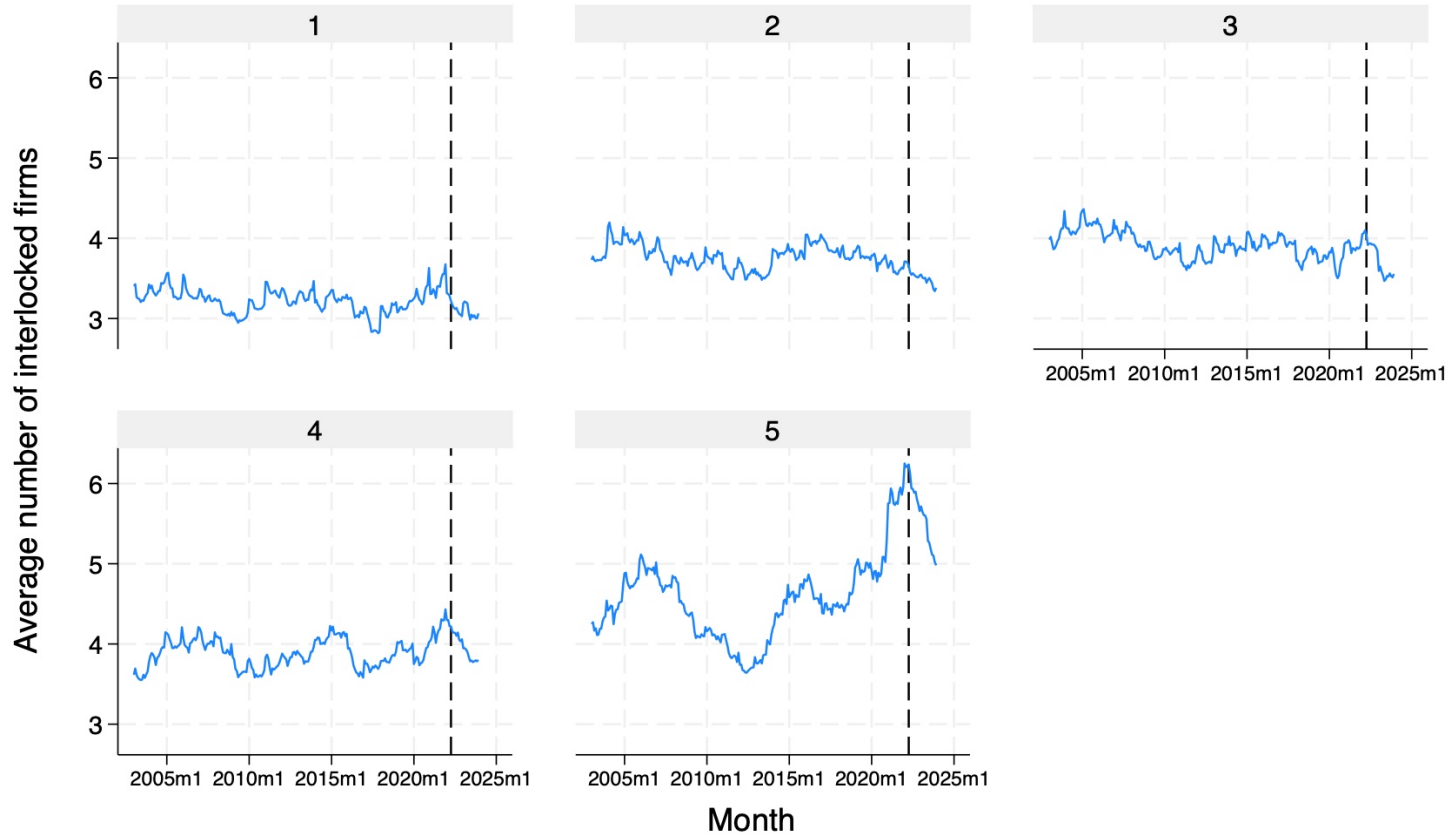
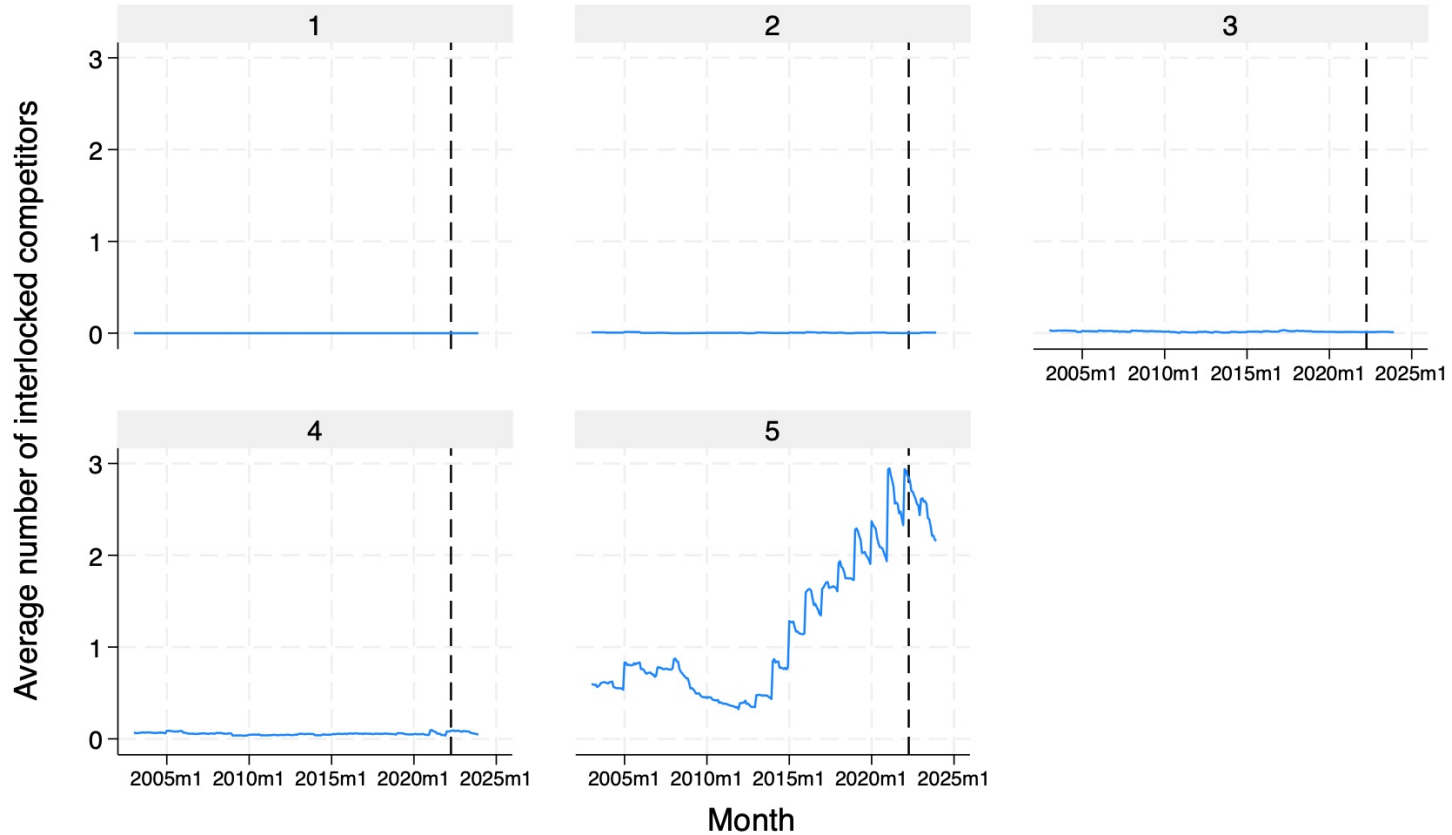


Fig. 6. Evolution of Interlocked Competitors Grouped by TNIC-3 Total Similarity

This plot shows different trends in the number of interlocked product market competitors among groups of stocks with different product similarity with competitors. For each month, I separate all non-financial US public firms into 5 groups based on the firm-month level sum of product similarity scores between the firm and all of its TNIC-3 competitors. Then for each group in each month, I compute the average number of interlocked non-financial competitors across all firms in the group. The y-axis in the plot is the average number of interlocked competitors. The x-axis is the calendar month. The number above each subplot represents the corresponding quintile for each subplot, with 1 denoting the group with the lowest product similarity with competitors and 5 denoting the group with the highest product similarity. Vertical dashed line denotes April 2022, the month of event.



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