It's Not Who You Know—It's Who Knows You: Employee Social Capital and Firm Performance[†]

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Abstract

We show that the social capital embedded in employees' networks contributes to firm performance. Using novel, individual-level network data, we measure a firm's social capital derived from employees' connections with external stakeholders. Our directed network data allow for differentiating those connections that know the employee and those that the employee knows. Results show that firms with more employee social capital perform better; the positive effect stems primarily from employees being known by others. We provide causal evidence exploiting the enactment of a government regulation that imparted a negative shock to networking with specific sectors and provide evidence on the mechanisms.

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1. Introduction

The role of physical capital, human capital, and intellectual capital in corporations is well studied. Yet, another type of capital, perhaps equally important, has received much less attention: a firm's social capital, consisting of the relationships that a firm and its employees have built with economically related agents outside the firm. Social capital is a broad concept that can be understood as the norms of reciprocity and trust within social networks (Putnam, 2000). The social capital of individuals—such as the size of their Rolodex—is shown to provide benefits and access to resources (Bourdieu, 1986; Coleman, 1988; Lin, 2002; Glaeser et al., 2002).[1] At the firm level, an individual's social capital is important since employees, including both management and rank and file, interact directly with business partners, clients, and other stakeholders. Yet, due to the latent nature of social networks, how the social capital embodied in employees' connections contributes to firm value and performance remains an open question.[2]

In this paper, we aim to establish a causal link between the social capital embedded in employee networks and firm performance. To this end, we construct a novel firm-level measure of employee social capital using professional connections that a firm's employees, across all job levels, have built with business contacts outside the firm.[3] We identify the types of employee connections that are valuable to firms and discover the economic benefits that firms obtain from these connections, thus contributing to a more granular understanding of social capital in corporations.

To measure employee social capital, we exploit a unique cultural practice in Asia: the exchange of business cards when people make connections. We obtain full access to novel data from the professional networking app "Remember," to which users upload business cards they have collected from others. Remember has a near-monopoly of business card management in

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[1] A complementary approach measures social capital at the country or regional level using metrics such as the civic engagement of the population or civic norms and trust. These studies find that regions with more social capital have better economic outcomes (e.g., Knack and Keefer, 1997; La Porta et al., 1997; Guiso et al., 2004, 2008) and that firms in these regions suffer less from agency problems (Hasan et al., 2017; Hoi et al., 2019). Another complimentary approach uses direct observation of behavior in laboratory experiments to measure social capital (e.g., Glaeser et al., 2000; Karlan, 2005).

[2] Limited by data availability on networks, the literature that uses the network approach focuses almost exclusively on benefits firms obtain from their well-connected executives and board members (e.g., Cai and Sevilir, 2012; Engelberg et al., 2012; Larcker et al., 2013).

[3] Our construction of employee social capital distinguishes it from relationships *within* the firm (see, e.g., Jeffers and Lee, 2019) or norms and values that are shared within the firm, also referred to as corporate culture (see, e.g., Guiso et al., 2015; Popadak, 2016; Graham et al., 2018; Grennan, 2022; Graham et al., 2022; Gorton et al., 2022; Grennan and Li, 2022).

Korea. The comprehensive data on the card collections of every user allow us to directly identify the professional networks of individual employees and quantify the connections each employee has built with people outside their firm. We further map the connections of public firm employees to the financial variables of their employers to obtain a matched employer-employee dataset.

Several aspects of our data are novel and noteworthy. First, our final sample consists of 2.4 million employees, with 12.4 million professional connections between them. The data's broad coverage of employees across ranks allows us to quantify employee social capital at the firm level. Second, because in Asian culture business cards are typically exchanged in face-to-face meetings (it is not the norm to pass on cards on behalf of others), our data depict real-world professional connections more reliably than those from online networking platforms, such as LinkedIn where people can connect even though they have never met. Third, while card exchanges are mutual between the two parties, uploading cards to the app is not necessarily mutual because users are more likely to upload the cards of contacts that they want to remember (apropos the name of the app). Using language from the network literature, we refer to the network as *directed*: each connection is directed from the employee who uploads the card to the employee whose card is uploaded.

We calculate several connection measures at the individual employee level—*In-degree* (the number of others who have uploaded the employee as a contact), *Out-degree* (the number of business contacts uploaded by the employee), and *Total degree* (the sum of *In-degree* and *Out-degree).* In other words, *In-degree* counts the people who remember the employee by uploading the employee's card on the app, which we refer to as "who knows you"; *Out-degree* counts the contacts the employee remembers by uploading their cards, which we refer to as "who you know." [4] As we discuss below, the directed nature of our network data enables us to move beyond "who knows who" and analyze the extent to which social capital—as distinguished by "who knows you" versus "who you know"—matters for the firm.

We begin by constructing firm-level measures of employee social capital (ESC) based on the employee-level degree measures (*In-degree*, *Out-degree, Total degree*) within a firm. Drawn

[4] Although none are perfect descriptors, we use "who knows you" and "who remembers you" interchangeably throughout the paper to describe an employee's *In-degree* connections. Similar descriptors are used to describe *Out-degree* connections. In a sense, *In-degree* captures the extent to which the employee is in the rolodex (or among the list of business contacts) of others whereas *Out-degree* measure the size of the employee's rolodex. A reciprocal connection where both parties upload each other's cards ("know each other") counts toward both the *In-degree* and *Out-degree* for each party.

from a comprehensive sample of Korean public firms in the OSIRIS Industrials database from 2014 to 2018, our initial analysis examines the average *Total degree* of a firm's employees without regard to the direction of connections; baseline regressions show that firms with more employee social capital have significantly higher profitability and sales growth in the following year.

We then investigate whether the direction of connections matters in the relation between employee social capital and firm performance. We re-estimate the model when firm-level employee social capital takes the value of *ESC in-degree* and *ESC out-degree*. Results show that the positive association with future performance arises mainly from *ESC in-degree*, which captures the extent to which a firm's employees are remembered or known by their external contacts. In sharp contrast, the coefficient estimates on *ESC out-degree* are largely insignificant. While the social capital literature argues that networks provide benefits for individuals, our findings suggest that the extent to which employees can mobilize these benefits for their employers depends on whether their business contacts remember them. In this sense, having a broad network of business contacts who know you appears more valuable to your employer than having a broad network of contacts whom you know.[5] Finally, we leverage the data's coverage of employees across job levels to study employee social capital beyond the executive team—an aspect less explored in the literature. Our results emphasize the unique value of social capital embodied in non-executive employees.

We perform a range of robustness checks to allay concerns with omitted variable bias, measurement error, and selection bias. A firm's employee social capital may proxy for other variables that relate to firm performance. For example, sales personnel who serve as customer touchpoints are, by nature, active in exchanging cards, such that the observed relation between employee connections and sales growth might simply reflect firms' sales activities. Our results, however, are robust to excluding the connections of a firm's customer-facing employees or excluding the connections with external contacts working in the customer industries. Another possibility is that firms with well-connected employees might also have high employee technical skills or high employee satisfaction, both associated with superior firm performance. Following the strategy in Cohen et al. (2010), we exclude subsamples of firms that are popular employers

[5] Although appearing less useful to employers, "who you know" can be an asset for employees themselves. To the extent that employees uploading contacts from other firms—as measured by *ESC out-degree*—expands outside job opportunities, as shown by Gortmaker et al. (2020) using data from LinkedIn, the resources mobilized through these connections do not necessarily accrue to their employer.

among skilled employees and find the results continue to hold. Finally, we conduct a battery of tests to show the robustness of our results against potential measurement error and selection bias in constructing firm-level employee social capital caused by differential app usage among a firm's employees.

Establishing a causal link between employee social capital and firm performance requires a careful account of the endogeneity of networks. Despite our extensive robustness tests, concerns remain, such as reverse causality whereby better firm performance leads to the formation of connections. To address the endogeneity of employee social capital and reinforce its causal effect on firm performance, we exploit the 2016 enactment of the Kim Young-ran Act (the Act) as a plausibly exogenous shock to professional networking in Korea. The Act makes it illegal for media professionals (such as journalists) and public sector employees (such as public servants, lawmakers, and teachers), and their spouses to accept gifts or meals exceeding a specified limit, regardless of whether they are in exchange for favors. The Act is a suitable identification tool because of the uncertainty in the legislative process and its aggressive enforcement. Evidence suggests that the Act caused significant precautions among businesses, creating a chilling effect on social events and meetings with contacts in the media and the public sector. By limiting employees' ability to extract benefits from their existing connections to these affected sectors, the Act constituted a negative shock to a firm's employee social capital.

We use a difference-in-differences framework surrounding the enactment of the Act. The treatment intensity is the fraction of a firm's preexisting employee social capital derived from its employees' connections with the media and the public sector. Since some firms have employees more connected to these two sectors, we can estimate differences in performance before and after the Act between firms with differential exposure. We find that firms with employees more connected to these two sectors experience a significant decline in performance after the Act relative to those less connected. For instance, a one standard deviation increase in treatment intensity yields an increase in *Tobin's q* of 17.5% relative to the sample mean before the Act, but only by 4.4% after. This differential effect does not appear in pre-treatment years and persists over the subsequent years. Our results are robust to matching treatment to control firms based on industry and observable firm characteristics and to excluding firms that are economically linked to the two sectors directly affected by the Act, such as customers and suppliers of the media and the public sector.

Using an event study approach, we examine stock price reactions around the court ruling date of the Act. Consistent with the value of firms' employee social capital being destroyed by the limits on social interactions imposed by the Act, we find a significantly negative cumulative abnormal return of −0.61% (p-value = 0.017) for firms with employees more connected to the media and the public sector over the [−3, 3] event window, and a differential cumulative abnormal return of −1.02% (p-value = 0.019) relative to firms that are less connected.

To shed light on the mechanisms through which employee social capital contributes to firm value, we consider the benefits that firms can derive from their employees' connections with the sectors affected by the Act—the media and the public sector. Motivated by the literature on media coverage and firm value (e.g., Ahern and Sosyura, 2014), we predict that employees' media connections will foster reciprocity and information sharing with journalists, which in turn promotes media coverage of the firm. Indeed, we find that firms with more employee media connections have substantially more news articles and a greater fraction of news articles with a positive tone. Moreover, the positive effects diminish after the enactment of the Act, reinforcing our causal inference.

We then turn to the benefits of employee connections with the public sector. Drawing on evidence that public officers allocate more procurement contracts to firms with a connected CEO, we expect that employees with public sector connections may also help their firms secure procurement contracts. Our evidence is consistent with this prediction. For example, a one standard deviation increase in the fraction of employee social capital accumulated from public sector connections leads to a 6.8% increase in the number of newly signed contracts before the Act and only a 3.4% increase after.[6]

Our study adds to the burgeoning literature on the role of social capital in corporations. Because relationships of a firm are difficult to observe and measure, existing metrics for firm social capital largely rely on corporate social responsibility efforts or norms and social interactions in local areas surrounding corporate headquarters, such as voter turnout, census response rate, density of sports clubs, and friendship links on Facebook. This literature finds that firms that entered a financial crisis with more social capital perform better (Lins et al., 2017; Servaes and Tamayo,

[6] A possible underlying channel is that employees' media and public sector connections facilitate favor exchanges with journalists and public officials (which may include bribery). Although bribery reflects a dark side from a societal perspective, it represents a favor exchange facilitated through employee networks that benefits the firm. We elaborate on this point in Section 4.5

2017) and that firms operating in regions with higher social capital have better access to finance (Hasan et al., 2017; Kuchler et al., 2022), suffer less from agency problems (Hoi et al., 2019), and have earnings news more rapidly incorporated into stock prices (Hirshleifer et al., 2021). Our contribution to this literature is to develop a novel measure of a firm's social capital using the professional connections of its employees, and show that otherwise similar firms with more employee social capital perform better, thus shedding light on the drivers of firm productivity (Syverson, 2011).

Our study also complements prior work that identifies the benefits of managerial networks, such as high announcement returns in mergers and acquisitions (Cai and Sevilir, 2012), better firm performance (Larcker et al., 2013; Cai and Szeidl, 2017; Dass et al., 2014), favorable lending terms (Engelberg et al., 2012; Haselmann et al., 2018; Karolyi, 2018), and survival during a financial crisis (Acemoglu et al., 2016).[7] Adding to this literature, we present novel evidence that executives are not the only group that possesses beneficial connections for their firms; employee connections across all job ranks matter for firm outcomes. More importantly, by exploiting the directed feature of our data, we uniquely show that the value of employee social capital to a firm comes mainly from employees being remembered by their external contacts.

Finally, our study leverages the Asian cultural practice of exchanging business cards, which provides a unique institutional setting for identifying interpersonal networks. Although our evidence draws from Korean firms, the effects of social ties on business outcomes have been documented in diverse business cultures, such as the US (Hochberg et al., 2007; Shue, 2013), China (Cai and Szeidl, 2017), Germany (Haselmann et al., 2018), the UK (Rossi et al., 2018), and the global setting (Houston et al., 2018), suggesting that the insights are general and broadly contribute to our understanding of social capital.

This paper proceeds as follows. Section 2 describes the data and the construction of firm-level employee social capital. Section 3 examines the relation between employee social capital and firm performance. In Section 4, we provide causal evidence using the enactment of the Kim Young-ran Act as a quasi-natural experiment, and Section 5 concludes.

[7] Other studies point out potential downsides to the firm with well networked executives: connections could weaken effective monitoring of board members, increase the entrenchment of CEOs, and lead to rent-seeking coalitions (Hwang and Kim, 2009; Fracassi and Tate, 2012; Ishii and Xuan, 2014; Khanna et al., 2015; Gompers et al., 2016).

2. Data and summary statistics

2.1. Remember, a professional networking app

We exploit a unique dataset extracted from a professional networking app, Remember, which was developed by the Korean mobile and web service provider Drama & Company. Since its launch in January 2014, Remember has become the single most popular professional business card management app in Korea, with virtually no domestic competitors.[8] As of December 2018, the total number of users was around 2.5 million, which is approximately 18.1% of the total number of full-time employees in Korea.

To keep a record of their professional network, users of the app upload the business cards they have collected in face-to-face meetings. Professional typists hired by the app developer hand-type the scanned cards into the database, which renders the network data virtually free of automatic recognition errors. The app allows users to keep track of their professional networks, to use search criteria to connect to calls, texts, emails, and addresses, and to add updates about promotions or new job titles. Unlike online networking platforms (e.g., LinkedIn, Facebook, or Twitter), the network of a user is not visible to others.

2.2. Business card data and individual employee-level connections

The cultural background of Korea strongly supports the notion that tracking business card exchanges is a useful way to identify employees' professional networks. As in most other Asian countries, in Korea, exchanging business cards in face-to-face meetings is more than an exchange of personal details; it is a ritual for building professional connections. It is widely believed that, besides being an ice breaker, the exchange of business cards can help establish a positive first impression and boost professional credibility. Business cards are also a physical reminder that one has met the contact rather than simply googled them. In addition, exchanging cards helps the two parties bond and build trust by encouraging follow-up social events.[9]

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[8] The Remember app won the Google Play Awards in 2015 and 2016 and received the Brand of the Year Korea for four consecutive years, from 2015 through 2018. The app is accessible at rememberapp.co.kr, and is available free of charge from Google Play and the App Store. Figure IA.1 in the Internet Appendix illustrates how the app appears in the App Store, the app's user interface, and how to upload business cards.

[9] As discussed extensively in the *Economist* (May 2015), "business cards are doubly useful. They can be a quick way of establishing connections, particularly in Asia, where they are something of an obsession . . . exchanging business cards still seems to be an excellent way to initiate a lasting relationship. The ritual swapping of paper rectangles may be old-fashioned but on it will go." Also see "Why Business Cards Still Matter," *BBC*, September 2016, www.bbc.com/worklife/article/20160914-how-a-small-yet-mighty-bit-of-paper-can-still-get-you-a-job.

Tracing the exchange of business cards using our dataset is thus a feasible and reasonable way to identify Koreans' professional networks. From each card uploaded by each app-user by December 2018, we obtain detailed information about the business contact, including an individual identifier (uniquely defined by a coded name and coded mobile phone number to comply with user privacy laws), email domain, firm name, job position, and timestamp of card upload. The unit of observation is the *connection pair* consisting of the app-user who uploads the card and the business contact whose card is uploaded. Since our goal is to count connections among employees, we exclude connections that involve individuals who do not have a firm name on their card, whose email domain is inconsistent with their firm, or whose firm does not have a Korea Investors Service (KIS) identifier (a corporate registration number for listed and unlisted firms). To focus on interfirm connections, we select connections between employees with different KIS identifiers, so that each connection involves employees of different firms. The Internet Appendix provides more details on our data and an illustrative example.

In general, cards are mutually exchanged between two parties, but the uploading of cards is not necessarily mutual. For example, after Aaron and Bob meet and exchange cards, Aaron uploads Bob's card, but Bob does not upload Aaron's card. Borrowing terminology from the network literature (e.g., Jackson, 2008; Newman, 2010), this feature implies our connection-level data are directed. More specifically, in social networks, individuals (nodes) form connections (links) to other individuals; the nodes and links constitute the network. If the links have a specified direction and are not necessarily mutual, we say the network is directed. The literature visualizes directed networks by drawing links as arrows to indicate the direction. Thus, there can be links pointing inward to and outward from each node. The number of links pointing inward to each node is the in-degree, and the number of links pointing outward is the out-degree. The total degree of a node is the sum of its in- and out-degree.

Applying these concepts to our data, each connection is a link directed from the user who uploads the card to the contact whose card is uploaded. The example of Aaron uploading Bob's card counts as an *out-degree* for Aaron, and an *in-degree* for Bob. Because users are most likely to remember those business contacts whose cards they uploaded—as suggested by the name of the app—Bob is more likely to be remembered when others upload his card as opposed to when Bob uploads others' cards. To capture this distinction, we define the degree measures at the employee-year level as follows. *In-degree* is the number of employees of other firms who have uploaded the employee as a business contact by a given year ("who knows you"). *Out-degree* is the number of external business contacts uploaded by the employee by a given year ("who you know"). *Total degree* is the sum of *In-degree* and *Out-degree.* A reciprocal relationship, which occurs when both parties upload each other's cards, counts toward both the *In-degree* and *Out-degree* for each party, thereby increasing the *Total degree* of each party by two.

Since our interest is in the performance of publicly listed firms, we keep only the connections in which at least one of the two individuals is a public firm employee. This network consists of 12.4 million connections between 2.4 million employees. Among these employees, 17.4% are app-users and 43.0% work for public firms. There are 126,987 firms with KIS identifiers; among them, 1,866 are public firms. To analyze the performance of Korean public firms, we use the OSIRIS Industrials database compiled by Bureau van Dijk, which contains financial information on publicly listed industrial firms worldwide. Our data cover firms in a wide array of sectors, as shown in Table IA.1 in the Internet Appendix.

Panel A of Table 1 presents summary statistics of employee-level connections as of December 2018 for the public firm employees in our sample. We begin by summarizing the connections of the 119,423 app-user employees. An average app-user employee has been uploaded as a contact by 26 app-users outside the firm (*In-degree*) and has uploaded 57 contacts from other firms (*Out-degree*). The sum of the two degrees, *Total degree*, has a mean of 83. All degree measures have a median much lower than the mean, suggesting that the distributions are highly right skewed. In the network, there are 896,600 non-app-users working for public firms. Non-app-users enter the network when their cards are uploaded by app-users and thus, by definition, only have links pointing inward.[10] On average, a non-app-user, whose *In-degree* (which also equals *Total degree*) is around five, is uploaded as a contact by five app-users outside the firm. Pooling the app-users and non-app-users together, an average public firm employee in the network is uploaded by seven others as a business contact and has a total degree of 14.

[Table 1 about here]

Our data have several advantages in identifying employees' professional networks. First, the data's broad coverage of individual employees (including management and rank and file)

[10] We discuss potential measurement error and selection bias caused by not observing the *Out-degree* of non-app-users in Section 3.3.2.

allows us to map employee-level connections to their employers to construct a matched employer-employee dataset. This feature overcomes a limitation of the literature that has focused primarily on managerial networks. Second, because business cards are typically exchanged in a face-to-face meeting, our data depict real-world professional relationships more reliably than online professional networks such as LinkedIn. An uploaded card is a physical imprint that the two people indeed met rather than simply connected via an online invitation. Third, since the connections of an employee are not publicly visible, one's *In-degree* and *Out-degree* are unlikely to strategically influence each other. Fourth, the directed nature of the data allows us to move beyond "who knows who" and analyze the extent to which social capital—as distinguished by "who knows you" versus "who you know"—matters for firm outcomes.

2.3. Firm-level employee social capital (ESC)

To examine the extent to which resources inherent in an employee's professional connections contribute to the employer's performance, we construct measures of firm-level employee social capital (ESC) based on the employee-level degree measures. Our strategy is to average across the employee-level degrees to obtain a proxy for the connectedness of the representative employee of each firm. We utilize the direction of connections to decompose firm-level employee social capital into *ESC in-degree* and *ESC out-degree*. *ESC in-degree* is the average *In-degree* across a firm's employees in the network; it quantifies the number of times a firm's employees have been uploaded as business contacts. As noted earlier, non-app-users enter the network when their cards are uploaded by others and thus, only have *In-degree*. Accordingly, *ESC out-degree* is the average *Out-degree* across the app-user employees of a firm; it quantifies the number of external business contacts that a firm's app-user employees have uploaded. Finally, *ESC total degree* is the average *Total degree* across a firm's employees in the network.[11]

2.4. Sample construction and summary statistics

To construct our sample, we start with Korean public firms from the annual OSIRIS Industrials database from 2014 through 2018. We match the 1,866 public firms in the network data with OSIRIS Industrials using firm names. We use three measures for firm performance: *Tobin's*

[11] To reduce measurement error when taking averages, we restrict our sample to firm-year observations with at least ten employees observed in the network. Our results are robust to using alternative thresholds for the minimum number of employees who appear in the network; see further discussions in Section 3.3.2 on potential measurement error.

q is the market value of assets divided by the book value of assets; *ROA* (return on assets) is earnings before interest, tax, depreciation, and amortization (EBITDA) divided by the lagged total assets;[12] *Sales Growth* is the annual log growth rate of sales. The definitions of all variables are provided in Internet Appendix II. We drop firm-year observations with missing data for the main variables in the baseline regressions. To reduce the effects of outliers, we winsorize all potentially unbounded variables at the 1st and 99th percentiles of the distribution. The final sample consists of 5,340 firm-year observations and covers 1,553 unique firms.

Panel B of Table 1 reports summary statistics for our firm-year sample. *ESC in-degree* has a mean of 3.7 and a median of 3.1; *ESC total degree* has a mean of 6.8 and a median of 5.3. These numbers show that employees of a firm, on average, have 6.8 connections with employees of other firms and that in 3.7 of those connections, they are uploaded as a business contact by others. In comparison, *ESC out-degree* has a mean of 31.0 and a median of 24.2 among users, suggesting that app-user employees of a firm, on average, upload 31.0 business contacts from other firms; *ESC out-degree* is larger in magnitude than *ESC total degree* because we observe a more complete picture of connections by app-user employees of a firm, as reported in Panel A of Table 1.[13] The financial variables are comparable in magnitude to those of US firms during the same period; Korean firms have less skewed *Tobin's q,* larger *ROA,* smaller *Sales Growth*, and lower *Book Leverage.* Summary statistics of firm-level ESC measures by sector are reported in Table IA.1.

3. Employee social capital and firm performance: baseline analysis

This section provides baseline estimates of the relation between employee social capital, as variously measured by employee professional connections, and firm performance. In Section 3.1, we examine *ESC total degree,* without accounting for the direction of connections. In Section 3.2, we exploit the directed nature of our network data, considering both *ESC in-degree* and *ESC out-degree* to determine whether the direction of connections matters. Section 3.3 provides a variety of robustness tests to address concerns with omitted variable bias, measurement error, and selection bias. Section 3.4 evaluates employee social capital across executives and non-executive employees.

[12] Using EBIT instead of EBITDA to measure *ROA* does not change our results.

[13] The number of observations of *ESC out-degree* is slightly smaller than that of the other main variables; this is because some firm-year observations do not have app-user employees and thus are missing *ESC out-degree*.

3.1. Employee social capital measured by total degree

The social capital literature suggests that social ties are associated with valuable resources (Bourdieu, 1986; Coleman, 1988; Putnam, 2000; Lin, 2002; Glaeser et al., 2002; Granovetter, 2005). For instance, Bourdieu (1986) considers social capital as "the actual or potential resources which are linked to possession of a durable network"; Putnam (2000) notes that social connections lead to reciprocity, trust, and better sharing of information; and Lin (2002) defines social capital as resources that can be accessed or mobilized through ties in the networks. Motivated by this literature, we examine the relation between employee social capital and future firm performance by estimating the following specification:

$$Y_{i,t} = \beta_0 + \beta_1 \times \ln\left(1{+}ESC_{i,t\text{-}1}\right) + \gamma' X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ is one of the performance measures (*Tobin's q*, *ROA*, or *Sales Growth*), $ESC_{i,t\text{-}1}$ is the one-year lagged firm-level employee social capital, $X_{i,t\text{-}1}$ is a set of one-year lagged time-varying firm-specific control variables (R&D, book leverage, total assets, stock return volatility, firm age, and number of employees) commonly included in the literature (see, e.g., Anderson and Reeb, 2003), and $\alpha_{j,t}$ is a full set of two-digit Standard Industrial Classification (SIC) industry-by-year fixed effects. As our data have a short time span, much of the variation in firm-level ESC is in the cross section; hence, we include industry-by-year fixed effects to control for unobserved time-varying heterogeneity across industries in, for example, business performance, professional connectivity, or employee app usage. Since our ESC measures are right skewed, we take the log transformation to reduce the effects of outliers; our results are qualitatively robust to using ln(*ESC*) and also robust to not taking the log transformation.

[Table 2 about here]

The estimation results when $ESC_{i,t\text{-}1}$ takes the value of *ESC total degree* (the average *Total degree* measured at year *t*-1 across employees of firm *i* who are in the network) are presented in columns (1)–(3) of Panel A of Table 2. The coefficient estimates on ln(1+*ESC*) are positive across all firm performance measures. The estimated effect is statistically significant for *ROA* and *Sales Growth*. The coefficient estimates in columns (2)–(3) imply that a one standard deviation increase in *ESC* from its mean is associated with an increase in *ROA* of 0.4 percentage points (=0.008×(ln(1+6.836+5.844)−ln(1+6.836))) and *Sales Growth* of 2.1 percentage points. The effects are significant, given the mean *ROA* of 4.3 percentage points and the mean *Sales Growth* of 4.1 percentage points over the sample period.[14] These results suggest a positive relation between a firm's future performance and its employee social capital based on employees' total number of connections.

3.2. Does direction of employee connections matter? In-degree versus out-degree

To shed more light on the economic value of employees' professional connections, we exploit the directed nature of our data which allows us to separately account for the business contacts that remember the employee and the business contacts that the employee remembers. More specifically, by using our decomposition of employee social capital into *ESC in-degree,* which measures "who knows you," and *ESC out-degree*, which measures "who you know," we consider whether the direction of connections matters.

Columns (4)–(9) of Panel A report the results of re-estimating equation (1) separately for *ESC in-degree* and *ESC out-degree.* The results provide strong evidence that the direction of connections plays a role in firm performance. All coefficient estimates on *ESC in-degree,* reported in columns (4)–(6), are positive and statistically significant at the 1% level. The estimated effects are economically meaningful: a firm with one standard deviation more *ESC in-degree* has a 9.4% higher *Tobin's q* relative to the sample mean, a 0.9 percentage points higher *ROA*, and a 4.0 percentage points higher *Sales Growth*. By contrast, the coefficient estimates on *ESC out-degree* in columns (7)–(9) are insignificant or borderline significant. The estimated coefficients for *ESC out-degree* and economic significance are an order of magnitude smaller than those for *ESC in-degree,* which is also confirmed by the one-tailed tests (p-value < 1% for all three columns). For example, relative to the 9.4% increase in *Tobin's q* for *ESC in-degree* noted above, the same increase in *ESC out-degree* from its mean is associated with only a 1.8% increase in *Tobin's q*.[15]

These findings suggest that the positive relation between employee social capital and firm performance comes mainly from employees' connections with external contacts who remember

[14] Since *ROA* and *Sales Growth* have negative values in the distribution, we do not compute the percentage increase relative to the sample mean when evaluating the economic magnitudes.

[15] In Table IA.2 in the Internet Appendix, we perform a propensity score matching analysis to mitigate the potential effects of heterogeneous selection by matching each above-median ESC firm with a below-median firm on year, industry, and the controls in our baseline regression. Results confirm that firms with above-median *ESC in-degree* experience significantly better performance than their matched firms, whereas no significant difference is found for firms with different *ESC out-degree*. In addition, to evaluate whether the effects of *ESC in-degree* are evident for both firms with high performance and firms with low performance, we run quantile regressions and find that the estimated effect is equally strong among firms in different deciles of the performance distribution (shown in Figure IA.2).

the firm's employees. While social ties can provide benefits, the extent to which employees can leverage these benefits for their employers depends on whether their business contacts remember them. Although our results show that out-degree connections are less useful to their employers, individuals may still derive personal benefits from these connections. For example, studies show that social networks are useful for individuals seeking outside job opportunities (e.g., Lin et al., 1981; Granovetter, 1973, 1995; Hacamo and Kleiner, 2021). If employees uploading contacts from other firms—as measured by *ESC out-degree*—reflects employees' desire and efforts to switch employers,[16] the resources mobilized through these connections do not accrue to their employer. Overall, our baseline regressions show that firms with more employee social capital have significantly better performance in the next year; however, compared with the rolodex that an employee possesses, being on others' rolodex is a more robust indicator of employee social capital that can benefit the firm.

3.3. Robustness tests

3.3.1. Omitted variables

A concern is that omitted variables that are correlated with both employee social capital and firm performance may be driving our findings. Although including industry-by-year fixed effects mitigates such concerns by controlling for unobservable industry-specific trends, we perform tests in Panel B of Table 2 to further address this issue.

One possibility is that the observed relation between *ESC in-degree* and sales growth might merely reflect a firm's sales activities. Sales employees serve as customer touchpoints and are particularly active in exchanging business cards, such that firms with more sales employees may mechanically have greater sales as well as more employee connections. To alleviate this concern, we calculate *ESC: Excl. Sales* by excluding the connections of a firm's customer-facing employees who perform sales functions.[17] In addition, while connections with customer industries are clearly important to firms, to provide further evidence that our results are not a byproduct of sales

[16] This mechanism is consistent with the evidence in . They analyze micro-level data from LinkedIn and find that, after learning about their firms' credit deterioration, workers start initiating connections on LinkedIn more frequently; this is followed by an increased likelihood of a job change afterward.

[17] The employees who perform sales functions are identified by job title and department information extracted from their business cards. Examples of job titles related to sales include sales representative, manufacturer's representative, financial advisor, loan consultant; examples of departments involving sales include customer service, sales strategy, dealership, marketing communication, retail advisory, and marketing. Our method identifies 98,404 public firm employees as sales personnel.

activities, we also calculate *ESC: Excl. Customers* by excluding a firm's employee connections with individuals working in its customer industries.[18] As shown in Panel B of Table 2, the coefficients on *ESC in-degree* continue to be positive and statistically significant for both alternative measures, while those for *ESC out-degree* are not.

Another possibility is that firms with well-connected employees might also have high employee technical skills or high employee satisfaction, and it is the employees' skill or job satisfaction rather than their connections that drives superior firm performance. To alleviate this concern, we use a similar strategy as Cohen et al. (2010) and conduct subsample analyses. We first exclude firms that ranked at least once in the "top 20 most wanted employers by university students" during 2015–2018 according to the Job Korea Survey, such as Samsung Electronics and Hyundai Motor, because these firms tend to show high employee satisfaction and attract some of the most talented university graduates. We then drop financial firms (SIC codes 61, 62, 65, 67) and firms that are in the top three percentile of the asset size distribution, both of which are competitive in the market for talented employees. The results, in Panel B of Table 2, show that *ESC in-degree* remains significantly related to firm performance, whereas the coefficient estimates of *ESC out-degree* largely remain insignificant, indicating that our results are not an artifact of a selected sample of employees with good technical skills or job satisfaction that drive firm performance.

3.3.2. Measurement error and selection bias

Although our network data cover employees in a wide array of firms and industries, we do not observe the universe of employee connections. Thus, we investigate the robustness of our results against potential measurement error and selection bias caused by (i) differential app usage among a firm's employees, (ii) potential differences between app-users and non-app-users, and (iii) our aggregation approach to measuring firm-level employee social capital.

First, the fact that our network data are based on the business card collections of app-users might introduce measurement error and selection bias. As discussed in Section 2, *ESC in-degree* likely underestimates "who knows you" because it does not reflect external employees that

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[18] To identify customer industries, we follow Frésard et al. (2020) and measure vertical relatedness using detailed Make-and-Use tables obtained from the Bank of Korea Economic Statistics System. Specifically, we use the 2014 Make-and-Use tables to construct a 328-by-328 industry flow matrix in which each cell indicates the dollar flows from an upstream industry to a downstream industry. We define industry *j* as a customer industry of industry *i* if the fraction of industry *i*'s total production used by industry *j* exceeds a threshold of 3%.

remember the firm's employees but do not use the app. To the extent that measurement error biases our estimates toward zero, partially observing employees' *In-degree* biases against finding a significant effect of *ESC in-degree*. On the other hand, because we do not observe the *Out-degree* of non-app-user employees, *ESC out-degree* might also contain noise as it is measured on a smaller sample than *ESC in-degree*. To address this issue, we randomly assign *Out-degree* to non-app-users by drawing from the *Out-degree* distribution of app-users in the same firm with replacement; we then construct a bootstrapped *ESC out-degree* using the actual *Out-degree* of app-users and the bootstrapped *Out-degree* of non-app-users. Results based on the bootstrapped data show that the coefficient estimate of *ESC out-degree* is robustly small in magnitude and insignificant (see Figure IA.3 in the Internet Appendix), suggesting that the insignificance of *ESC out-degree* to firm performance is unlikely an outcome of measurement error.[19]

Second, app-users, by nature, are more likely to be tech-savvy and socially active than non-app-users. Since *In-degree* is observed for both app- and non-app-users, whereas *Out-degree* is observed only for app-users, a concern is that our decomposition of employee social capital by the direction of connections may pick up these or other differences between app- and non-app-users. To address this concern, in Panel C of Table 2, we examine *ESC in-degree* of non-app-user employees to compare with our baseline estimates for *ESC in-degree* (measured for both app- and non-app-user employees). If app-user employees drive our results, we should expect *ESC in-degree* of non-app-user employees not to be significant; however, the coefficient estimates on *ESC in-degree* continue to be positive and statistically significant. Similarly, we examine *ESC out-degree* to only those external contacts who are app-users to compare with our baseline estimates for *ESC out-degree* (to external contacts including app- and non-app-users), and still find similar results. Moreover, to directly compare the effects of *ESC in-degree* and *ESC out-degree*, we include both measures in the same regression; and, since we observe a more complete picture of connections by app-users, we also run the same regressions when we construct both measures using only app-user employees of a firm. Our findings are robust in both cases. These tests suggest that our findings concerning the direction of connections are not an artifact of the asymmetry between app- and non-app-users.

[19] We repeat this procedure 500 times and find that none of the coefficient estimates based on the bootstrapped data are significant at the 5% level. Results are similar when we multiply the bootstrapped *Out-degree* of non-app-users with a scaler from 0.5 to 1.5 to account for potential differences between app-users and non-app-users.

Third, errors could arise in measuring firm-level ESC since we average across the individual-level degree measures among the employees that are in the network. To reduce error when taking averages, we restrict our sample to observations with at least ten employees observed in the network. Panel B of Table IA.2 in the Internet Appendix shows that our results are unchanged when we apply alternative thresholds for the minimum number or percentage of firm employees who are in the network. Relatedly, employees' connections might collectively contribute to firm performance; hence, in lieu of averaging across employees, we calculate *ESC: Sum* as the sum of *In-degree* (or *Out-degree*) aggregated across the firm's employees and find qualitatively similar results. These tests suggest that our results are robust to alternative sample selection and aggregation methods at the firm level.

3.4. Does employee job level matter? Executives versus non-executive employees

Finally, we investigate the value of employee social capital by job level. While executives make the firm's major strategic decisions, non-executive employees—including middle managers and rank-and-file employees—constitute most of a firm's workforce and often closely interact with business partners, clients, and other key stakeholders. Understanding the social capital embodied in non-executive employees is important since decision-making and information processing within a firm are often decentralized by a hierarchical structure (Radner, 1992). A key advantage of our data is the broad coverage of employees across ranks, which allows us to study the social capital embodied in employees beyond the executive team, an aspect scarcely examined in prior literature largely due to data limitations.

Panel D of Table 2 presents results on the effects of employee social capital on firm performance across executives and non-executive employees.[20] Results show that *ESC in-degree* is positively associated with all firm performance measures for both executives and non-executives. For example, a one standard deviation increase in *ESC in-degree* of executives is associated with an increase in *ROA* of 0.7 percentage points; and that for non-executive employees is associated with an increase in *ROA* of 1.3 percentage points.[21] While our findings echo existing studies on the value of executive networks based on undirected network data (e.g., Cai and Sevilir,

[20] Job levels classified as executives include chairman, vice chairman, president, deputy president, executive vice president, and senior vice president; about 9.7% of the observed employees are executives. Non-executive employees include all other employees.

[21] The number of observations varies slightly across regressions because a small number of firm-years do not have executives. Results are similar when we run the regressions on the same set of observations.

2012; Engelberg et al., 2012; Larcker et al., 2013), they also uniquely suggest that non-executive employees have beneficial connections that contribute to their employers' performance.

4. Causal evidence from the 2016 Kim Young-ran Act

Although we conduct a battery of tests to mitigate concerns with omitted variable bias and measurement error (and to some extent reverse causality by using lagged ESC measures), the results of our analysis may still be subject to endogeneity concerns. To establish a causal relation between employee connections and firm performance, it is important to identify exogenous variation in employee social capital. In this section, we provide causal evidence by exploiting a quasi-natural experiment that imparted a negative shock to professional networking in Korea.

4.1. Exogenous shock to employee social capital: the 2016 Kim Young-ran Act

We exploit the enactment of the Kim Young-ran Act (the Act) in September 2016 as an exogenous shock to social interactions with employees in specific sectors. Named after the former head of the Anticorruption and Civil Rights Commission, the Act makes it illegal for media professionals (such as journalists) and public sector employees (such as civil servants, lawmakers, and teachers), and their spouses to accept gifts of more than 50,000 Korean won (about 45 USD) or 100,000 won at events such as weddings and funerals; it also limits meal expenditures to 30,000 won per person.[22] Violations of the Act are subject to severe penalties, including imprisonment.[23]

Although the Act was intended to prevent corruption, the gift and meal limits also resulted in fewer social events and meetings with contacts employed in the media and the public sector, thereby restricting firms' ability to leverage their employee social capital with these sectors. As a culturally ingrained business practice in Korea, corporate employees would regularly treat clients, business partners, and public employees to dinners, drinks, and other entertainment as part of normal networking activity (Choi and Storr, 2019). Through engagement in these networking activities, professionals invest in their social capital, enhance trust, and share information. However, anecdotal evidence suggests that the Act has caused significant precautions among businesses in their interactions with the media and the public sector due to the severity of its

[22] The upper limits were adjusted in January 2018 to 100,000 won for non-cash gifts and to 50,000 won for cash gifts.
[23] The Act imposes a punishment of imprisonment for up to three years, or a fine of up to 30 million Korean won on persons convicted of accepting money or goods valued at more than one million won from one person in one installment, regardless of whether such compensation was in exchange for favors or related to the recipient's work. If the money or goods are worth less than one million won, a fine of up to five times the gift's value is imposed.

penalties, its aggressive enforcement, as well as its somewhat abstract and vague provisions and the lack of precedents.[24] For example, companies say "they are concerned about how to maintain business relationships they have built with government officials and the media over the years. The law's definition of those related to work is ambiguous…as it excludes socializing as part of business formality." This concern by firms is consistent with the observations that "reservation rates of restaurants in Seoul's financial and legal districts and those near government complexes in Sejong and Daejeon, have rapidly dropped" and that Korean reporters were intentionally left off the invitation list in a launch event for Apple's iPhone X.

To provide more systematic evidence that the Act resulted in an exogenous shock to employee social capital with the media and the public sector, we examine changes in the formation of connections with these sectors around the Act. Specifically, we examine the fraction of a firm's employee social capital (*ESC in-degree*) that is derived from connections with employees in the industries affected by the Act ($\textit{ESC in-degree}^{Act}$), as identified using industry codes listed in Internet Appendix II.[25] Our estimation results in Table IA.3 further show that the fraction dropped by 7.8% (= −0.266/3.414) after the enactment relative to the sample mean. Hence, the evidence is consistent with the Act discouraging the formation of new connections with personnel in the media and the public sector.

Another aspect that makes the Act a useful identification tool is the uncertainty around whether the Act would be ruled constitutional. Right after bipartisan approval of the Act in 2015, the Korean Bar Association and the Korean Journalists Association filed a court petition questioning the law's constitutionality on the grounds that it threatened freedom of speech. The Constitutional Court upheld the law on July 28, 2016, rejecting the petition. This series of unforeseen events supports our identifying assumption of orthogonality between the enactment and unobservables that affect firm performance.

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[24] See, for example, "Corporate Korea Braces for Change over Anti-Graft Law," *Korea Herald*, September 27, 2016, www.koreaherald.com/view.php?ud=20160927000851; "Companies Still Need to be Cautious of Kim Young-ran Act," *Korea Herald*, September 24, 2017, www.koreaherald.com/view.php?ud=20170922000818.

[25] Our results in Section 3 show that the economic value of employee social capital to a firm comes mainly from its employees being remembered (uploaded) by others rather than the other way around. Hence, we focus on a firm's *ESC in-degree* for this and the remaining tests.

4.2. Evidence for causality

We assess the causal effect of employee social capital on firm performance using a difference-in-differences framework surrounding the enactment of the Kim Young-ran Act. Since some firms have more of their employee social capital derived from connections to the media and the public sector (thus have employee social capital more exposed to the Act) than others, we can estimate differences in performance between firms with differential exposure to the Act. The restrictions of the Act impair the ability of employees to access the resources embedded in their existing connections to the media and the public sector; hence, we hypothesize that firms with greater exposure experienced a bigger reduction in the value of their employee social capital.

We test the predictions of our hypothesis by estimating the following regression model:

$$Y_{i,t} = \beta_0 + \beta_1 \times Act\ Exposure_i + \beta_2 \times Act\ Exposure_i \times Post_t + \gamma' X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t}, \qquad (2)$$

where $Y_{i,t}$ measures firm performance and $Act\ Exposure_i$, the treatment intensity, is calculated as the ratio $ESC\ in\text{-}degree^{Act}_{i,2015}/ESC\ in\text{-}degree_{i,2015}$, where $ESC\ in\text{-}degree^{Act}_{i,2015}$ is *ESC in-degree* in 2015 that is due to connections to employees in industries subject to the Act.[26] We measure the treatment intensity in 2015, before the enactment, to isolate it from the dynamic response of a firm's employee social capital to the Act. The summary statistics of *Act Exposure* are shown in Panel B of Table IA.4 in the Internet Appendix. *Post* is a dummy variable for the years during and after the enactment (2016–2018). *X* is the same set of lagged control variables as in Table 2; $\alpha_{j,t}$ is a full set of industry-by-year fixed effects. We are interested in β_2, the coefficient of the interaction term, *Act Exposure*×*Post*. If employee social capital indeed has a causal effect on firm performance, we expect firms with ESC more exposed to the Act to derive less value from their employee social capital after the Act than firms that are less exposed, i.e., we expect β_2 to be negative.

[Table 3 about here]

Table 3 summarizes the results of estimating equation (2). The regression in column (1) excludes observations during the enactment year because the Act only became effective in the

[26] We focus on *Tobin's q* as our measure of firm performance in testing for causality since, as shown in Table IA.4 in the Internet Appendix, connections to industries affected by the Act have a significant and positive impact on firm performance, with the effect concentrated in *Tobin's q*.

latter half of 2016. Consistent with our prediction, the estimate of β_2 is negative and significant at the 1% level. Based on the positive and significant β_1 estimate, employee connections to the media and the public sector contribute positively to a firm's *Tobin's q* before the Act; however, the negative β_2 estimate shows that the positive impact declines substantially after the Act. For instance, a one standard deviation increase in *Act Exposure* (0.038) leads to an increase in *Tobin's q* by 17.5% (=0.038×6.578/1.432) relative to the sample mean before the Act, but only by 4.4% after. Our estimate is little changed when we control for *Act Exposure* measured by *ESC out-degree* in the regressions (see Panel A of Table IA.6 in the Internet Appendix); this robustness result reinforces our earlier finding on the value of "who knows you" to firms as opposed to "who you know." Panel A of Table IA.6 also shows that the results are robust to alternative thresholds for the minimum number of employees or a minimum percentage of firm employees who appear in the network. Finally, we include observations in 2016 in column (2) of Table 3 and find little change in the magnitude and significance of our β_2 estimate.

To test for the presence of pre-trends, in columns (3)–(4) we estimate an augmented version of equation (2) where we interact *Act Exposure* with an indicator variable for each year.[27] The finding is visualized in Figure IA.4 in the Internet Appendix. Consistent with *Act Exposure* capturing an adverse shock to employee social capital, the decline in firm performance does not occur prior to the enactment. Starting from the enactment in 2016, the estimate becomes negative and remains negative and significant at the 1% level. Our results suggest no preexisting trend in firm performance before the enactment, reinforcing that the Act negatively affects firm performance by reducing employee social capital.

To further assess the reliability of our identification strategy, we perform a placebo test. We randomly assign a *Pseudo Exposure* to each firm while maintaining the true distribution of *Act Exposure* and re-estimate column (1) in Table 3. By randomizing *Act Exposure* while holding all other variables fixed, we break the true link between employee social capital and firm performance, thereby imposing the null hypothesis on the data. We repeat this procedure 1,000 times and obtain the empirical distribution of the coefficient estimate on the interaction term. The true coefficient estimate (−4.930) falls well below the 1% threshold of this distribution, as reported in Table IA.5

[27] In column (3), we set 2015 as the baseline year and omit the 2015 interaction term (the outcome variable in year 2014 is dropped in our baseline analysis because we lag all control variables by one year). To highlight the insignificance of the pre-treatment interaction terms, in column (4) we extend our pre-treatment sample to include year 2014 and set 2014 as the baseline year, omitting the 2014 interaction term.

in the Internet Appendix. This placebo test gives confidence that the negative estimate of β_2 is not a statistical artifact.

The exposure of a firm's employee social capital to the Act is not randomly assigned. Firms with ESC more exposed to the Act tend to be larger in asset size and number of employees. It is likely they also had more frequent business interactions with the media and the public sector by 2015. We perform two robustness checks to address the issue of covariate balance. First, we use propensity score matching to generate a group of control firms similar to the treated firms and conduct the tests using this matched sample. We use a probit model to estimate the probability of being a treated firm (those with above-median *Act Exposure* in 2015). Then we match each treated firm to a control firm with replacement, using nearest neighbor matching with a maximum difference of 0.01. Panel A of Table 4 shows that the treated and control firms in the matched sample display indistinguishable differences. In Panel B, we estimate the same specifications as in Table 3 on the matched sample and find consistent results. Second, we use the full sample and interact firm-level control variables with the *Post* dummy to control for any observable differences in characteristics related to the treatment that could lead to differences in performance around the enactment. We find the results continue to hold, as reported in Panel B of Table IA.6.

[Table 4 about here]

To alleviate concerns that adverse sectoral shocks to the industries directly affected by the Act (media and public sector) could spill over to treated firms through economic linkages rather than employee connections, we conduct subsample analyses in Panel C. Firms in the media and the public sector may be highly connected among themselves, thereby mechanically having a high *Act Exposure*; therefore, we drop firms that belong to the industries directly affected by the Act (26 firms) in column (1) and also drop firms that more broadly belong to the media and the publishing activities sectors (KSIC 58, 59) in column (2). In column (3), we further drop firms in the supplier and customer industries of the media and the public sector.[28] To examine whether our results are driven by firms that have no employee connections to the affected industries, in column (4), we focus on the subsample with positive exposure of employee social capital to the Act. Across

[28] We use the same method described in footnote 18 to identify the customer industries and a similar method to identify the supplier industries. Examples of supplier industries include manufacturers of newsprint, printing and reproduction of recorded media, infrastructure suppliers, and restaurants; examples of customer industries include the wholesale and retail sectors and sellers of motor vehicles and parts (with significant advertising expenses).

all these subsamples, the coefficient estimates on the interaction term remain negative and significant at the 1% level. These tests help rule out alternative explanations due to potential differences between the treated and control firms and economic spillovers.

4.3. Stock market reaction to the court ruling on the Kim Young-ran Act

To reinforce a causal interpretation of our findings, we conduct an event study analysis of the stock market response to the Act. We focus on event days surrounding the date the court ruled that the Act was constitutional. After bipartisan approval, the Act faced a lengthy petition challenging its scope and constitutionality. The Korean Bar Association and the Korean Journalists Association argued that applying the law to journalists and private school teachers (and their spouses) infringed on freedom of the press and on the rights of private schools. However, the petition was eventually rejected at 2pm on July 28, 2016 when seven out of the nine Constitutional Court justices ruled that the Act was constitutional. We examine stock price reactions around the court ruling for firms differentially exposed to the Act. A negative market reaction for firms with ESC more exposed to the Act would buttress support for the causal effect of employee social capital on firm performance.

[Table 5 about here]

We divide firms into above-median and below-median subgroups based on *Act Exposure* ($\textit{ESC in-degree}^{Act}_{i,2015}/\textit{ESC in-degree}_{i,2015}$). We calculate average cumulative abnormal returns for each subgroup, both CAPM-adjusted and size-adjusted, for various windows around the court ruling date. As reported in Table 5, we find evidence of a negative market reaction to firms with ESC more exposed to the Act. For example, the average cumulative abnormal return over the [−3, 3] event window is −0.61% (p-value = 0.017) for firms with ESC more exposed to the Act and 0.41% for firms with ESC that is less exposed. The difference between the two groups is statistically significant with a p-value of 0.019.[29] We also examine the cross-sectional pairwise correlation between *Act Exposure* and the cumulative abnormal returns and find that greater exposure to the Act is significantly associated with more negative stock price reactions. Taken

[29] The observation that the return differentials are not significant for the [−1, 1] event window and are increasing with the length of the event windows suggests that firms' social capital exposed to the Act might not be immediately known to the market as employee connections are latent.

together, the event study evidence supports the notion that employee social capital positively contributes to firm value.

4.4. Mechanisms: benefits of employee connections with the media and the public sector

To shed light on the economic mechanisms through which employee social capital contributes to firm value, we proceed to identify benefits that a firm can extract from its employee connections to the sectors affected by the Act—the media and the public sector.

We start by showing that the negative effect of the Act on the value of employee social capital demonstrated in Table 3 (where *Act Exposure* is measured using the *sum* of the connections to both affected sectors) is also observed *separately* for each of the affected sectors. $\textit{Act Exposure}^{Media}$ is the fraction of *ESC in-degree* in 2015 that is due to connections to media employees ($\textit{ESC in-degree}_{2015}^{Media}/\textit{ESC in-degree}_{2015}$) ; $\textit{Act Exposure}^{Public}$ is defined similarly. Panel B of Table IA.4 presents summary statistics of these two variables. As shown in Panel A of Table 6, when we re-estimate equation (2) by setting the treatment intensity separately as $\textit{Act Exposure}^{Media}$ and $\textit{Act Exposure}^{Public}$, we find results similar to what we find for the combined effect as captured by *Act Exposure*. Before the Act, employee connections to both the media and the public sector have a significant positive impact on firm *Tobin's q*, and the impact declines for both sectors after the Act.

[Table 6 about here]

Given the positive value of employee social capital tied to each sector, we can now consider some specific benefits that firms can derive from their employee connections with these sectors. With respect to media connections, a large body of literature suggests that media coverage influences stock returns (Tetlock et al., 2008; Dougal et al., 2012; Gurun and Butler, 2012; Ahern and Sosyura, 2014). Gurun and Butler (2012) document that local media tend to display a "positive slant" toward local firms by using fewer negative words in news articles and that the positive slant strongly relates to firms' equity value. Relatedly, Ahern and Sosyura (2014) find that firms actively manage media coverage to influence their stock prices. Like the positive slant observed when media covers local firms, media connections of a firm's employees may lead to a positive slant in news coverage and a resulting positive effect on firm value. For instance, reporters who are well connected to a firm's employees may have developed trust in those employees and therefore be more likely to report positive news about the firm. Media connections might also facilitate active media management by allowing firms to influence the timing and content of media coverage. We thus expect that all else equal, employee connections with the media foster more news coverage of the firm, and more news stories with a positive tone; moreover, if employee social capital is driving this relationship, we expect a decline in the positive impact of media connections after the Act.

To test these predictions, we examine the effect of a firm's employee social capital—derived from connections with the media—on media coverage of the firm before and after the Act; the results are reported in columns (1)–(2) in Panel B of Table 6. The dependent variable in column (1) is the log of the weighted number of news articles from RavenPack News Analytics covering a firm in a given year. To measure positive slant by media, we calculate the fraction of news articles covering a firm each year that are associated with a positive sentiment according to RavenPack's sentiment series and use this measure as the dependent variable in column (2).[30]

Consistent with the notion that media connections promote news coverage, we obtain a significant and positive coefficient on $\textit{Act Exposure}^{\textit{Media}}$. Moreover, consistent with the idea that reduced social interactions due to the Act undermine the benefits of media connections, the estimated coefficient for $\textit{Act Exposure}^{\textit{Media}} \times \textit{Post}$ is significantly negative for both the number and the tone of news articles. For example, a one standard deviation increase in $\textit{Act Exposure}^{\textit{Media}}$ increases the number of news articles by 13.0% (=0.029×4.495) and positive media coverage by 49.1% before the Act, but only increases news articles by 4.3% and positive media coverage by 14.8% after the Act. Taken together, these findings suggest that media connections lead to more favorable media coverage, enhancing firm performance. After the Act, the positive impact of media coverage declines substantially, consistent with the diminished contribution to *Tobin's q* in Panel A as well as the event study results showing negative valuation effects.

We now turn to investigating the benefits of employee social capital due to connections with the public sector. A nontrivial responsibility of public sector employees is public procurement, which accounts for 10–20% of GDP in developed countries (OECD, 2015). Schoenherr (2019) documents that Korean public officers who control the distribution of government contracts allocate significantly more procurement contracts to firms with connected CEOs. Similarly, we expect that firms with employees (including non-executive employees) who

[30] We report results excluding observations in the enactment year of 2016 because the outcome variables reflect the cumulative outcomes throughout the year. Results are robust if we also include observations from 2016.

are better connected with the public sector may obtain more government contracts, thereby resulting in superior performance.

To assess this prediction, we examine the effect of a firm's employee connections with the public sector on public procurement contracting outcomes using data from the Korea online e-Procurement Service. Consistent with our prediction, findings in columns (3)–(5) in Panel B of Table 6 show that firms highly connected to public sector employees obtain more public procurement contracts, in terms of the number of newly signed contracts, their value in Korean won, and their value scaled by firm assets, respectively. The estimated effect is reduced by about half after the Act. For example, column (3) shows that a one standard deviation increase in $\textit{Act Exposure}^{\textit{Public}}$ leads to a 6.8% increase in the number of newly signed contracts before the Act and only 3.4% after.

We conduct a falsification test to ensure our results are not driven by unobserved firm characteristics that are correlated with exposure to the Act. Specifically, we swap the Act exposure variables and instead regress the media coverage outcomes on $\textit{Act Exposure}^{\textit{Public}}$ and regress the procurement contracting outcomes on $\textit{Act Exposure}^{\textit{Media}}$. If our findings in Panel B indeed reflect a causal effect of media connections in promoting media coverage and of public sector connections in obtaining procurement contracts, we should not expect significant effects in this falsification test. The results reported in Panel C of Table 6 confirm this prediction, thus supporting a causal interpretation of the mechanism in Panel B.

In sum, Tables 3–6 provide causal evidence that a firm's employee social capital tied to the media or the public sector contributes to its performance by promoting favorable media coverage of the firm or by enhancing its ability to obtain public procurement contracts.

4.5. Discussion

Given the policy intention of the Act, a natural question is to what extent our results are due to the Act's success in reducing the ability of firms to obtain resources (favorable news coverage and procurement contracts) by bribing their connections in the media and the public sector. Several points are worth discussing in this context. First, the social capital literature (e.g., Bourdieu, 1986) highlights favor exchanges and reciprocity as important channels through which social relations increase the ability of individuals to advance their economic interests. Despite the negative connotation (and potential negative welfare effects), the literature recognizes bribery for resources as an example of a favor exchange that is more easily achieved for individuals with greater social capital.[31] For example, it is difficult to offer bribes to people who do not know or trust you. Hence, to the extent that results in Table 6 are driven by employees' connections with journalists or public officials facilitating bribery for resources, this bribery channel is still consistent with the notion that employee social capital improves firm outcomes (although not necessarily social welfare).

Second, our evidence suggests that a reduction in bribery is unlikely the only channel driving our results in Table 6. While bribery is not directly observable, a firm's entertainment expenses are shown to include a significant bribe component (Cai et al., 2011; Kang et al., 2020). Using a firm's entertainment expenses scaled by total assets as a proxy for bribery activities, we find that its correlation with *Act Exposure* is only 0.043, suggesting that firms with employees well connected with the media and the public sector do not seem to coincide with those that actively pay bribes. In addition, when we decompose Panel B of Table 6 into executives and non-executive employees in Panel D, we find that the connections by non-executive employees are also significantly valuable in bringing benefits to their firm. This result once again highlights our novel addition to existing evidence on the value of executives' media connections and political connections. More importantly, to the extent that bribing for resources for their firm is mostly carried out by executives, bribery does not appear as the only driver of our findings.

5. Conclusion

This paper provides novel evidence that a firm's social capital derived from its employees' professional connections is a valuable production factor contributing to firm performance. We use a comprehensive dataset from a professional networking app with broad coverage of individual-level connections to measure firm-level employee social capital. Our analysis reveals that employee social capital is robustly and positively associated with firm performance. Our unique network data record the direction of connections, allowing us to separately account for those business contacts that remember the employee and those that the employee remembers. Our results

[31] This "dark-side" view of social connections is consistent with the evidence of crony lending documented in Haselmann et al. (2018) and the distortive allocation of government resources to politically connected firms (Schoenherr, 2019). While these rent-seeking activities are not allocatively efficient, they do benefit the connected borrowers and firms.

show that the positive effect on firm performance manifests primarily when external stakeholders remember a firm's employees.

To establish a causal interpretation of our results, we exploit the enactment of the Kim Young-ran Act in 2016 which imparted a negative shock to networking with specific sectors. Our evidence suggests that firms with employee connections more exposed to the Act derive less value from their employee social capital after the Act than firms that are less exposed. The results support a causal interpretation of employee social capital in boosting firm performance and creating firm value.

This paper makes several contributions to the literature. First, our study uses a comprehensive measure of employee social capital and establishes its contribution to firm performance. We quantify employee social capital at the firm level by identifying interpersonal networks that cover employees at all job levels. Second, our employee social capital measures are directional. Our finding that being remembered by others is more productive than remembering others echoes a popular saying about professional networking: "It is not who you know—it is who knows you." Third, our analysis of the connections with economically related industries provides novel insight into the economic mechanisms underlying the concomitant benefits of employee connections. One implication of our research is that social ties can be leveraged in business settings. Personal relationships and business contacts endow employees (and their firms) with resources, constituting an essential form of social capital that is convertible into firm value and performance.

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Table 1. Summary statistics: employee-level connections and firm-year sample

This table provides summary statistics for our data. Panel A presents summary statistics of the employee-level connections as of December 2018, based on the 1,016,023 public firm employees of our sample. *In-degree,* which measures "who knows you," is the number of employees of other firms who have uploaded the corresponding employee as a business contact as of December 2018. *Out-degree,* which measures "who you know," is the number of business contacts of other firms uploaded by the focal app-user employee as of December 2018; given the nature of our data, *Out-degree* is only available for the 119,423 public firm employees who are app-users. *Total degree* is the sum of *In-degree* and *Out-degree*. Panel B presents summary statistics of the main variables for our firm-year sample. *ESC in-degree* is the average *In-degree* across employees of firm *i* who are in the network in year *t*. *ESC out-degree* is the average *Out-degree* across app-user employees of firm *i* in year *t*. For reference, we also tabulate *ESC out-degree* computed as the average *Out-degree* across employees of firm *i* who are in the network in year *t*. *ESC total degree* is the average *Total degree* across employees of firm *i* who are in the network in year *t*. The sample period is 2014–2018. The definitions of all variables are provided in Internet Appendix II.

Panel A. Employee-level connections as of December 2018

	N	Mean	Median	SD	P25	P75
[App-users]						
In-degree	119,423	26.329	11	50.160	4	27
Out-degree	119,423	56.916	17	116.831	5	56
Total degree	119,423	83.244	30	161.819	11	84
[Non-app-users]						
In-degree = Total degree	896,600	4.820	2	9.826	1	5
[All public firm employees in the network (app-users + non-app-users)]						
In-degree	1,016,023	7.348	2	20.710	1	6
Total degree	1,016,023	14.038	2	61.652	1	7

Panel B. Firm-level employee social capital (ESC) measures and other main variables

	N	Mean	Median	SD	P25	P75
ESC in-degree	5,340	3.676	3.139	2.392	1.976	4.693
ESC out-degree	4,994	30.953	24.167	26.787	12.909	40.304
*ESC out-degree (*app-users + non-app-users*)*	5,340	3.210	2.031	4.190	0.740	4.057
ESC total degree	5,340	6.836	5.319	5.844	3.000	8.548
Tobin's q	5,340	1.456	1.106	1.099	0.890	1.575
ROA	5,340	0.043	0.042	0.087	0.009	0.082
Sales Growth	5,340	0.041	0.037	0.324	-0.066	0.141
R&D	5,340	0.024	0.003	0.067	0.000	0.022
Book Leverage	5,340	0.101	0.062	0.115	0.001	0.165
ln(1+*Assets*) (in million Korean won)	5,340	12.248	12.013	1.343	11.341	12.950
Volatility	5,340	0.130	0.115	0.068	0.085	0.156
Firm Age	5,340	28.666	25	16.163	16	40
ln(1+*Emp*)	5,340	5.478	5.429	1.154	4.771	6.071

Table 2. Employee social capital and firm performance

This table reports OLS regression estimates on the relation between employee social capital and future firm performance. We estimate the following specification:

$$Y_{i,t} = \beta_0 + \beta_1 \times \ln\left(1+ESC_{i,t\text{-}1}\right) + \gamma' X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t},$$

where $Y_{i,t}$ is one of the performance measures (*Tobin's q*, *ROA*, or *Sales Growth*), $ESC_{i,t\text{-}1}$ is the one-year lagged firm-level employee social capital of firm i in year t-1; $X_{i,t\text{-}1}$ is a set of lagged firm-specific control variables commonly included in the literature (Anderson and Reeb, 2003); $\alpha_{j,t}$ is a full set of industry-by-year fixed effects. Panel A reports the baseline estimates. Columns (1)–(3) report results when measuring employee social capital by *ESC total degree*, without accounting for the direction of connections; columns (4)–(9) report results when we measure employee social capital by *ESC in-degree* and *ESC out-degree* to differentiate the direction of connections. We perform one-tailed tests comparing the coefficient estimates of *ESC in-degree* and *ESC out-degree* and find the p-values less than 0.01 for all three performance measures. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2015–2018 for output variables. The definitions of all variables are provided in Internet Appendix II.

Panel A. Baseline estimates: ESC total degree, ESC in-degree, and ESC out-degree

	ESC total degree			*ESC in-degree* ("who knows you")			*ESC out-degree* ("who you know")		
Dep. var.	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln(1+*ESC*)	0.084	0.008**	0.038***	0.330***	0.021***	0.098***	0.042	0.004*	0.004
	(0.053)	(0.004)	(0.012)	(0.090)	(0.007)	(0.024)	(0.030)	(0.002)	(0.007)
R&D	4.634***	-0.182***	0.420***	4.536***	-0.187***	0.397***	4.565***	-0.176***	0.398***
	(0.576)	(0.034)	(0.125)	(0.577)	(0.034)	(0.124)	(0.573)	(0.034)	(0.125)
Book Leverage	0.172	-0.138***	0.076	0.160	-0.139***	0.073	0.059	-0.134***	0.091
	(0.179)	(0.016)	(0.054)	(0.178)	(0.016)	(0.053)	(0.163)	(0.016)	(0.057)
ln(1+*Assets*)	-0.134***	0.010***	-0.009	-0.142***	0.009***	-0.011	-0.126***	0.010***	-0.010
	(0.022)	(0.002)	(0.008)	(0.022)	(0.002)	(0.009)	(0.022)	(0.002)	(0.009)
Volatility	3.498***	-0.104***	0.050	3.504***	-0.103***	0.054	3.618***	-0.106***	0.023
	(0.388)	(0.026)	(0.080)	(0.388)	(0.026)	(0.079)	(0.409)	(0.027)	(0.083)
Firm Age	-0.005***	-0.000***	0.000	-0.005***	-0.000***	0.000	-0.005***	-0.000***	0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
ln(1+*Emp*)	0.064***	0.009***	-0.007	0.079***	0.010***	-0.003	0.075***	0.008***	-0.008
	(0.023)	(0.002)	(0.006)	(0.024)	(0.002)	(0.006)	(0.024)	(0.002)	(0.006)
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Observations	5,340	5,340	5,340	5,340	5,340	5,340	4,994	4,994	4,994
Adjusted R^2	0.248	0.148	0.035	0.252	0.150	0.038	0.252	0.142	0.035

Table 2. Employee social capital and firm performance (continued)

Panel B and Panel C present robustness checks for columns (4)–(9) in Panel A. Panel B addresses omitted variables bias related to firm sales activities and employee technical skills/job satisfaction. We measure employee social capital by excluding connections of a firm's customer-facing employees who perform sales functions (*ESC: Excl. Sales*) and by excluding connections with individuals working in a firm's customer industries (*ESC: Excl. Customers*). We also repeat the analysis in columns (4)–(9) in Panel A using subsamples, which exclude, respectively, firms rated at least once in the "top 20 most wanted employers by university students" in 2015–2018, or financial firms (SIC codes 61, 62, 65, 67) and firms in the top three percentile of asset size distribution. Panel C addresses measurement error issues in *ESC in-degree* and *ESC out-degree*. In the upper panel, *ESC* is measured as *ESC in-degree of non-app-user employees* in columns (1)–(3) and *ESC out-degree to app-users* in columns (4)–(6). In the lower panel, we include both *ESC in-degree* and *ESC out-degree* in the same regression in columns (1)–(3). In columns (4)–(6), we focus on connections of app-users in measuring both *ESC in-degree* and *ESC out-degree* and require the firm-year observations to have at least ten app-user employees to reduce measurement errors. In Panel D, firm-level employee social capital takes the lagged value of *ESC in-degree* averaged across executives (chairman, vice chairman, president, deputy president, executive vice president, and senior vice president) in columns (1)–(3) and averaged across non-executive employees (all other employees) in columns (4)–(6). In all panels, we include the same set of lagged control variables and fixed effects as in Panel A. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2015–2018 for output variables. The definitions of all variables are provided in Internet Appendix II.

Panel B. Omitted variables: sales activities and employee technical skills/job satisfaction

	ESC in-degree ("who knows you")			*ESC out-degree* ("who you know")		
Dep. var.	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*
	(1)	(2)	(3)	(4)	(5)	(6)
[Excluding connections of employees who perform sales functions]						
ln(1+ *ESC: Excl. Sales*)	0.389***	0.020***	0.093***	0.050*	0.003	0.002
	(0.084)	(0.007)	(0.024)	(0.028)	(0.002)	(0.006)
Observations	5,340	5,340	5,340	4,860	4,860	4,860
Adjusted R^2	0.254	0.150	0.037	0.252	0.139	0.038
[Excluding connections with the customer industries]						
ln(1+ *ESC: Excl. Customers*)	0.309***	0.014*	0.082***	0.044	0.003	0.005
	(0.083)	(0.007)	(0.025)	(0.029)	(0.002)	(0.007)
Observations	5,340	5,340	5,340	4,994	4,994	4,994
Adjusted R^2	0.251	0.148	0.036	0.252	0.141	0.035
[Excluding top 20 most wanted employers by university students]						
ln(1+*ESC*)	0.329***	0.021***	0.083***	0.043	0.004*	0.003
	(0.090)	(0.008)	(0.021)	(0.030)	(0.002)	(0.007)
Observations	5,258	5,258	5,258	4,913	4,913	4,913
Adjusted R^2	0.258	0.142	0.043	0.258	0.133	0.042
[Excluding financial sector and top 3% firms based on total assets]						
ln(1+*ESC*)	0.342***	0.019**	0.081***	0.044	0.004*	0.002
	(0.093)	(0.008)	(0.022)	(0.031)	(0.002)	(0.007)
Observations	5,056	5,056	5,056	4,715	4,715	4,715
Adjusted R^2	0.258	0.146	0.041	0.258	0.137	0.040
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year

Panel C. Measurement error in ESC in-degree and ESC out-degree

Dep. var.	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*
	(1)	(2)	(3)	(4)	(5)	(6)
[Differences in characteristics between app- and non-app-users]						
	ESC in-degree of non-app-user employees			*ESC out-degree to app-users*		
ln(1+*ESC*)	0.427***	0.029***	0.135***	0.089*	0.005*	0.006
	(0.110)	(0.009)	(0.029)	(0.047)	(0.003)	(0.010)
Observations	5,340	5,340	5,340	4,994	4,994	4,994
Adjusted R^2	0.252	0.151	0.039	0.253	0.142	0.035
[*ESC in-degree* and *ESC out-degree* in the same regression]						
	Based on app-users and non-app-users			Based on app-users		
ln(1+*ESC in-degree*)	0.371***	0.020**	0.118***	0.416***	0.023**	0.062**
	(0.103)	(0.008)	(0.028)	(0.119)	(0.010)	(0.031)
ln(1+*ESC out-degree*)	-0.015	0.001	-0.014*	-0.158	-0.004	-0.015
	(0.032)	(0.002)	(0.007)	(0.097)	(0.008)	(0.026)
Observations	4,994	4,994	4,994	2,322	2,322	2,322
Adjusted R^2	0.257	0.144	0.041	0.249	0.136	0.067
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year

Panel D. Executives versus non-executive employees

Dep. var.	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*
	(1)	(2)	(3)	(4)	(5)	(6)
	ESC in-degree of executives			*ESC in-degree* of non-executive employees		
ln(1+ *ESC in-degree*)	0.190***	0.013***	0.050***	0.207**	0.032***	0.090***
	(0.056)	(0.004)	(0.013)	(0.100)	(0.008)	(0.025)
Observations	5,321	5,321	5,321	5,340	5,340	5,340
Adjusted R^2	0.251	0.151	0.036	0.249	0.154	0.037

Table 3. Causal evidence: the 2016 Kim Young-ran Act as an exogenous shock to employee social capital

This table provides evidence on the causal effect of employee social capital on firm performance. We estimate the following difference-in-differences model surrounding the enactment of the Kim Young-ran Act:

$$Y_{i,t} = \beta_0 + \beta_1 \times Act\ Exposure_i + \beta_2 \times Act\ Exposure_i \times Post_t + \gamma' X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t},$$

where $Y_{i,t}$ is *Tobin's q*, $Act\ Exposure_i = ESC\ in\text{-}degree^{Act}_{i,2015} / ESC\ in\text{-}degree_{i,2015}$, and $ESC\ in\text{-}degree^{Act}_{i,2015}$ is *ESC in-degree* in 2015 that is due to connections to employees in industries subject to the Act. $Post_t$ is an indicator variable that equals one during and after the enactment year (2016–2018) and zero otherwise. d_t is an indicator variable for year *t*. $X_{i,t\text{-}1}$ is the same set of lagged controls as in Table 2; $\alpha_{j,t}$ is a full set of industry-by-year fixed effects. Column (1) reports results excluding the enactment year (2016); columns (2)–(4) report results including the year 2016. The sample period is 2015–2018 for output variables in columns (1)–(3) and is 2014–2018 for output variables in column (4). Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The definitions of all variables are provided in Internet Appendix II.

Dep. var.	*Tobin's q*			
	(1)	(2)	(3)	(4)
Act Exposure	6.578***	6.640***	6.642***	5.420***
	(1.273)	(1.272)	(1.272)	(1.050)
Act Exposure × *Post*	-4.930***	-4.726***		
	(1.132)	(1.052)		
Act Exposure × d_{2015}				1.169
				(0.793)
Act Exposure × d_{2016}			-4.155***	-2.973***
			(0.932)	(0.849)
Act Exposure × d_{2017}			-4.730***	-3.540***
			(1.162)	(1.006)
Act Exposure × d_{2018}			-5.162***	-3.980***
			(1.169)	(0.983)
R&D	5.431***	5.066***	5.065***	4.969***
	(0.689)	(0.677)	(0.678)	(0.653)
Book Leverage	0.183	0.233	0.232	0.227
	(0.185)	(0.182)	(0.182)	(0.177)
ln(1+*Assets*)	-0.139***	-0.146***	-0.146***	-0.139***
	(0.025)	(0.023)	(0.023)	(0.022)
Volatility	3.403***	3.400***	3.396***	3.238***
	(0.449)	(0.395)	(0.395)	(0.363)
Firm Age	-0.005***	-0.005***	-0.005***	-0.005***
	(0.002)	(0.001)	(0.001)	(0.001)
ln(1+*Emp*)	0.076***	0.067***	0.067***	0.068***
	(0.024)	(0.023)	(0.023)	(0.023)
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	Yes	Yes	Yes
Observations	3,778	5,101	5,101	6,048
Adjusted R^2	0.242	0.245	0.245	0.243

Table 4. Causal evidence: robustness analyses

Panel A uses a propensity score matched sample to estimate the specifications in Table 3. We use a probit regression to estimate the probability of being a treated firm (those with above-median *Act Exposure* in 2015) using the sample of 2015 with a set of industry fixed effects and the same set of control variables in 2015 as in Table 3. Each treated firm is matched to a control firm using nearest neighbor with replacement within each two-digit SIC industry, where the maximum absolute difference in propensity scores between the matched observations is 0.01. We first tabulate the means of the matched variables for the treated group (those with above-median *Act Exposure*) and the control group (those with below-median exposure) in the year 2015. We also report the mean differences between the two groups and their corresponding *p*-values based on heteroskedasticity-consistent standard errors. Panel B present the results estimating the specifications in Table 3 using the matched sample, and the same set of lagged control variables and fixed effects. In Panel C, we re-estimate the specification of column (1) in Table 3 using subsamples. Column (1) drops firms that belong to the industries directly affected by the Act (26 unique firms identified according to the industry codes in Internet Appendix II); column (2) additionally drops firms that belong more broadly to the media and the publishing activities sectors (KSIC 58, 59); column (3) further drops firms that belong to the supplier and customer industries of the media and the public sector using detailed Make-and-Use tables; column (4) focuses on a subsample with positive exposure of employee social capital to the Act. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The definitions of all variables are provided in Internet Appendix II.

Panel A. Propensity score matched sample

	Above median (Obs. = 635)	Below median (Obs. = 635)	Above − Below	*p*-value
R&D	0.021	0.023	-0.002	0.587
Book Leverage	0.107	0.109	-0.002	0.679
ln(1+*Assets*)	12.347	12.304	0.043	0.574
Volatility	0.142	0.148	-0.006	0.189
Firm Age	29.191	30.710	-1.519	0.117
ln(1+*Emp*)	5.572	5.565	0.007	0.917

Panel B. Robustness tests based on the matched sample

Dep. var.	*Tobin's q*			
	(1)	(2)	(3)	(4)
Act Exposure	6.507***	6.531***	6.531***	5.521***
	(1.356)	(1.353)	(1.353)	(1.177)
Act Exposure × *Post*	-4.651***	-4.409***		
	(1.232)	(1.140)		
Act Exposure × d_{2015}				0.964
				(0.878)
Act Exposure × d_{2016}			-3.957***	-2.997***
			(1.050)	(1.002)
Act Exposure × d_{2017}			-4.064***	-3.102***
			(1.218)	(1.099)
Act Exposure × d_{2018}			-5.237***	-4.272***
			(1.306)	(1.150)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	Yes	Yes	Yes
Observations	3,541	4,811	4,811	5,721
Adjusted R^2	0.266	0.265	0.265	0.264

Panel C. Subsamples

Dep. var.	*Tobin's q*			
	(1)	(2)	(3)	(4)
Act Exposure	8.010***	8.350***	8.190***	6.362***
	(1.419)	(1.535)	(2.232)	(1.363)
Act Exposure × Post	-5.884***	-6.211***	-6.376***	-4.760***
	(1.304)	(1.407)	(2.046)	(1.196)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	No	No	No
Observations	3,708	3,464	2,686	3,344
Adjusted R^2	0.247	0.251	0.222	0.234

Table 5. Stock market reaction to the court ruling on the Act

This table reports the stock market reaction around July 28, 2016, when the Constitutional Court rejected the petition and ruled that the Kim Young-ran Act is constitutional. In the upper panel, we report the cumulative CAPM-adjusted abnormal returns in event windows [-1, 1], [-3, 3], and [-5, 5], where day 0 is the date of the announcement. Daily abnormal stock returns are computed based on the market model using the Korean equal-weighted market return as the market proxy. The estimation window is days [-200, -60] prior to the event date. In the lower panel, we report the cumulative size-adjusted abnormal returns in the same event windows. Following La Porta et al. (1997) and Ahern (2009), for each event window, we form a size-decile benchmark portfolio equally weighted using all stocks in that size decile, where size is measured as market capitalization as of one day prior to the start date of the event window. The daily size-adjusted abnormal returns are the difference between raw returns and the corresponding size-decile benchmark portfolios. In both panels, we report the average cumulative abnormal returns for firms with below-median exposure in column (1) and above-median exposure in column (2), where *Act Exposure* = $\textit{ESC in-degree}^{Act}_{2015}$/ $\textit{ESC in-degree}_{2015}$. Column (3) reports the mean difference between the above-median and the below-median subgroup; column (4) reports the cross-sectional pairwise correlation coefficient between *Act Exposure* and the cumulative abnormal returns. The *p*-values in square brackets are based on one-tailed tests for positive returns in column (1), for negative returns in columns (2)–(3), and for negative correlations in column (4), with the standard errors clustered at the industry (two-digit SIC) level. We exclude penny stocks with stock prices less than 1,000 Korean won (about 0.9 USD) as of June 28, 2016, one month prior to the court ruling.

	Act Exposure = $\textit{ESC in-degree}^{Act}_{2015}$/$\textit{ESC in-degree}_{2015}$			
	Below median	Above median	Diff Above − Below	Correlation coefficient
	(1)	(2)	(3)	(4)
[Cumulative CAPM-adjusted abnormal returns]				
[-1, 1]	0.07%	-0.27%	-0.34%	-0.009
	[0.325]	[0.080]	[0.083]	[0.363]
[-3, 3]	0.41%	-0.61%	-1.02%	-0.076
	[0.173]	[0.017]	[0.019]	[0.020]
[-5, 5]	0.62%	-1.04%	-1.66%	-0.086
	[0.131]	[0.007]	[0.008]	[0.014]
Observations	751	751		
[Cumulative size-adjusted abnormal returns]				
[-1, 1]	0.16%	-0.11%	-0.27%	-0.004
	[0.182]	[0.207]	[0.098]	[0.440]
[-3, 3]	0.52%	-0.43%	-0.95%	-0.065
	[0.119]	[0.041]	[0.014]	[0.035]
[-5, 5]	0.65%	-0.69%	-1.33%	-0.071
	[0.128]	[0.009]	[0.013]	[0.034]
Observations	788	782		

Table 6. Mechanisms: benefits of employee connections with the media and the public sector

In Panel A, we estimate changes in the value of connections with the media and the public sector around the Act using:

$$Y_{i,t} = \beta_0 + \beta_1 \times Act\ Exposure_i^{Media\ (Public)} + \beta_2 \times Act\ Exposure_i^{Media\ (Public)} \times Post_t + \gamma'\ X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t},$$

where $Y_{i,t}$ is *Tobin's q*, $Act\ Exposure_i^{Media}$ is $ESC\ in\text{-}degree_{i,2015}^{Media}/ESC\ in\text{-}degree_{i,2015}$ for columns (1)–(2) and $Act\ Exposure_i^{Public}$ is $ESC\ in\text{-}degree_{i,2015}^{Public}/ESC\ in\text{-}degree_{i,2015}$ for columns (3)–(4); $ESC\ in\text{-}degree_{i,2015}^{Media\ (Public)}$ is *ESC in-degree* in 2015 due to connections to the media (public) sector. $Post_t$ is an indicator variable for during and after the enactment year (2016–2018). $X_{i,t\text{-}1}$ is the same set of lagged controls as in Table 2; $\alpha_{j,t}$ is a full set of industry-by-year fixed effects. Columns (1) and (3) report results excluding the enactment year (2016), whereas columns (2) and (4) report results including 2016. Panel B reports results on the benefits of connections with the media and the public sector. *Act Exposure* is $Act\ Exposure^{Media}$ for columns (1)–(2) and $Act\ Exposure^{Public}$ for columns (3)–(5). Dependent variables in columns (1)–(2) are *Media Coverage*, the weighted count of news articles from RavenPack News Analytics covering a firm in a given year (the weight is the relevance score of each article provided by RavenPack; we only include articles with relevance scores greater than or equal to 75%), and *Positive Media Coverage Ratio*, the fraction of news articles with a positive sentiment (according to RavenPack's BMQ sentiment series) covering a firm in a given year. Dependent variables in columns (3)–(5) are the natural logarithm of one plus the number of newly signed procurement contracts, the amount of newly signed procurement contracts in Korean won, and the amount of newly signed procurement contracts in Korean won scaled by the firm's total assets. Panel C reports a falsification test where we repeat the analyses in Panel B but regress the media coverage outcomes on $Act\ Exposure^{Public}$ for columns (1)–(2) and regress the procurement contracting outcomes on $Act\ Exposure^{Media}$ for columns (3)–(5). Panel D repeats the analyses in Panel B when we differentiate the connections of executives (chairman, vice chairman, president, deputy president, executive vice president, and senior vice president) and non-executive employees (all other employees). Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The definitions of all variables are provided in Internet Appendix II.

Panel A. The value of connections with the media and the public sector: before and after the Act

Dep. var.	$Act\ Exposure^{Media}$		$Act\ Exposure^{Public}$	
	Tobin's q			
	(1)	(2)	(3)	(4)
$Act\ Exposure^{Media\ (Public)}$	8.016***	8.070***	6.181**	6.303***
	(1.591)	(1.588)	(2.414)	(2.407)
$Act\ Exposure^{Media\ (Public)} \times Post$	-5.655***	-5.431***	-4.782**	-4.735**
	(1.398)	(1.290)	(1.981)	(1.899)
R&D	5.455***	5.092***	5.449***	5.085***
	(0.697)	(0.685)	(0.686)	(0.674)
Book Leverage	0.183	0.233	0.185	0.235
	(0.187)	(0.185)	(0.187)	(0.183)
ln(1+*Assets*)	-0.141***	-0.148***	-0.124***	-0.132***
	(0.025)	(0.023)	(0.025)	(0.023)
Volatility	3.377***	3.376***	3.445***	3.443***
	(0.451)	(0.397)	(0.447)	(0.393)
Firm Age	-0.005***	-0.005***	-0.005***	-0.005***
	(0.002)	(0.001)	(0.002)	(0.001)
ln(1+*Emp*)	0.080***	0.070***	0.068***	0.059**
	(0.025)	(0.024)	(0.025)	(0.024)
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	Yes	No	Yes
Observations	3,778	5,101	3,778	5,101
Adjusted R^2	0.242	0.244	0.234	0.237

Panel B. The value of connections with the media and the public sector: economic benefits

	Act Exposure^{Media}		*Act Exposure*^{Public}		
Dep. var.	ln(1+*Media Coverage*)	*Positive Media Coverage Ratio*	ln(1+*# of Proc. Contracts*)	ln(1+*Tot Amt of Proc. Contracts*)	ln(1+*Tot Amt of Proc. Contracts / Assets*)
	(1)	(2)	(3)	(4)	(5)
Act Exposure$^{Media\ (Public)}$	4.495***	0.437**	3.756***	19.837***	0.091***
	(1.564)	(0.180)	(1.111)	(5.295)	(0.027)
Act Exposure$^{Media\ (Public)}$ × *Post*	-2.991**	-0.305*	-1.878**	-9.700**	-0.040*
	(1.445)	(0.172)	(0.839)	(4.443)	(0.022)
Tobin's q	0.116***	0.013***	-0.003	-0.015	-0.000*
	(0.017)	(0.004)	(0.008)	(0.041)	(0.000)
Book Leverage	0.131	-0.003	0.094	0.442	-0.003
	(0.158)	(0.027)	(0.125)	(0.538)	(0.002)
ROA	-0.931***	-0.107***	-0.191*	-1.668***	-0.005**
	(0.195)	(0.027)	(0.105)	(0.521)	(0.002)
R&D	0.611**	0.020	-0.367**	-1.883**	-0.013***
	(0.245)	(0.040)	(0.159)	(0.772)	(0.005)
ln(1+*Sales*)	0.267***	0.019***	0.030***	0.229***	-0.000
	(0.025)	(0.003)	(0.011)	(0.055)	(0.000)
Volatility	-0.204	-0.017	0.143	1.049*	0.005
	(0.181)	(0.032)	(0.104)	(0.596)	(0.003)
Firm Age	0.009***	0.001**	0.001	0.001	0.000
	(0.001)	(0.000)	(0.001)	(0.004)	(0.000)
ln(1+*Emp*)	0.069***	0.009***	0.107***	0.576***	0.002***
	(0.024)	(0.003)	(0.014)	(0.066)	(0.000)
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	No	No	No	No
Observations	3,775	3,775	3,775	3,775	3,775
Adjusted R^2	0.343	0.164	0.241	0.264	0.194

Panel C. The value of connections with the media and the public sector: falsification test

	Act Exposure^{Public}		*Act Exposure*^{Media}		
Dep. var.	ln(1+*Media Coverage*)	*Positive Media Coverage Ratio*	ln(1+*# of Proc. Contracts*)	ln(1+*Tot Amt of Proc. Contracts*)	ln(1+*Tot Amt of Proc. Contracts / Assets*)
	(1)	(2)	(3)	(4)	(5)
Act Exposure$^{Public\ (Media)}$	3.357*	0.320	-0.428	-2.443	-0.022**
	(1.889)	(0.239)	(0.495)	(2.617)	(0.011)
Act Exposure$^{Public\ (Media)}$ × *Post*	-2.868	-0.263	0.390	3.245	0.011
	(1.748)	(0.236)	(0.403)	(2.483)	(0.011)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	No	No	No	No
Observations	3,775	3,775	3,775	3,775	3,775
Adjusted R^2	0.339	0.162	0.231	0.255	0.186

Panel D. The value of connections with the media and the public sector: executives versus non-executive employees

Dep. var.	*Act Exposure*^{Media}		*Act Exposure*^{Public}		
	ln(1+*Media Coverage*)	*Positive Media Coverage Ratio*	ln(1+*# of Proc. Contracts*)	ln(1+*Tot Amt of Proc. Contracts*)	ln(1+*Tot Amt of Proc. Contracts / Assets*)
	(1)	(2)	(3)	(4)	(5)
[Executives]					
Act Exposure$^{Media(Public)}$	2.667***	0.269***	1.491***	7.535***	0.023**
	(0.744)	(0.094)	(0.438)	(2.284)	(0.010)
Act Exposure$^{Media(Public)}$ × *Post*	-2.013***	-0.182**	-0.703**	-2.660	-0.004
	(0.707)	(0.080)	(0.297)	(1.745)	(0.009)
Observations	3,748	3,748	3,748	3,748	3,748
Adjusted R^2	0.351	0.168	0.241	0.264	0.190
[Non-executive employees]					
Act Exposure$^{Media(Public)}$	5.317***	0.502***	3.015***	16.979***	0.081***
	(1.707)	(0.180)	(1.117)	(5.243)	(0.026)
Act Exposure$^{Media(Public)}$ × *Post*	-3.654**	-0.393**	-1.498*	-8.627**	-0.039*
	(1.548)	(0.193)	(0.822)	(4.366)	(0.021)
Observations	3,770	3,770	3,770	3,770	3,770
Adjusted R^2	0.343	0.164	0.236	0.260	0.191
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	No	No	No	No

For Online Publication

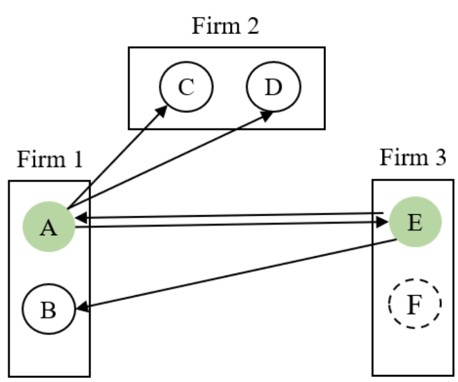
Internet Appendix I: Data on business card exchange network and an example

This Appendix provides descriptive statistics for the business card exchange network data based on all business cards uploaded as of December 31, 2018.

Number of connections	12,391,177
Number of employees	2,363,295
Number of employees who are app-users	411,039
Number of employees in public firms	1,016,023
Number of employees in public firms who are app-users	119,423
Number of firms with KIS identifiers	126,987
Number of public firms in OSIRIS Industrials	1,866

We use an example to illustrate the data structure of our business card exchange network and the method for constructing the measures of firm-level employee social capital. The example network is given by the following connection-level data, together with the network graph.

Employee_ID_From	Firm_ID_From	Job_From	Employee_ID_To	Firm_ID_To	Job_To
A	1	Staff	C	2	Staff
A	1	Staff	D	2	Vice president
A	1	Staff	E	3	Manager
E	3	Manager	A	1	Staff
E	3	Manager	B	1	Manager



Employees A and E are app-users, and all other employees are non-app-users. Employee F does not appear in the network data. Each connection is a directed link from the app-user employee (Employee_ID_From) who uploads the card to the employee (Employee_ID_To) whose card is uploaded. For example, the first entry shows that employee A, a staff of firm 1, has uploaded a card of employee C, a staff of firm 2. This link counts toward the out-degree for A and the in-degree for C. Based on the connection-level data, we construct the measures of firm-level employee social capital (ESC). *ESC in-degree* is the average *In-degree* across the firm's employees who are in the network. For example, the *In-degree* is one for both A and B, so firm 1's *ESC in-degree* = 1. *ESC out-degree* is the average *Out-degree* across the firm's app-user employees. Firm 1 has only one app-user employee, A, so its *ESC out-degree* equals the out-degree of employee A, which is three. Finally, *ESC total degree* is the average *Total degree* across the firm's employees who are in the network. The total degree is four for employee A and one for employee B, so its *ESC total degree* = 2.5(=5/2). Firm 2 does not have *ESC out-degree* because we can only observe the out-degree of app-users.

Firm_ID	Number of employees in the network	Number of app-user employees in the network	*ESC in-degree*	*ESC out-degree*	*ESC total degree*
1	2	1	1	3	2.5
2	2	0	1	-	1
3	1	1	1	2	3

Internet Appendix II: variable definitions

Variable name	Description
Measures of employee social capital (ESC)	
ESC in-degree	The average *In-degree*—the number of employees of other firms who have uploaded the employee as a business contact ("who knows you") by the end of year *t*—across employees of firm *i* who are in the network in year *t*
ESC out-degree	The average *Out-degree*—the number of business contacts of other firms uploaded by the corresponding employee ("who you know") by the end of year *t*—across app-user employees of firm *i* in year *t*
ESC total degree	The average *Total degree*—the sum of *In-degree* and *Out-degree*—across employees of firm *i* who are in the network in year *t*
ESC: Excl. Sales	*ESC* in which we exclude connections of a firm's customer-facing employees who perform sales functions
ESC: Excl. Customers	*ESC* in which we exclude connections with individuals working in a firm's customer industries
ESC in-degree of non-app-user employees	The average *In-degree*—the number of employees of other firms who have uploaded the employee as a business contact ("who knows you") by the end of year *t*—across non-app-user employees of firm *i* who are in the network in year *t*
ESC out-degree to app-users	The average *Out-degree* to app-users—the number of app-user business contacts of other firms uploaded by the corresponding employee ("who you know") by the end of year *t* —across app-user employees of firm *i* in year *t*
ESC: Executives	*ESC* based on the connections of executives (chairman, vice chairman, president, deputy president, executive vice president, and senior vice president)
ESC: Non-exec emp	*ESC* based on the connections of non-executive employees (all other employees)
ESC in-degree^{Act}	*ESC in-degree* using only the connections to employees in the industries subject to the Kim Young-ran Act according to the industry codes listed in the table below
ESC in-degree^{Media} (*ESC in-degree*^{Public})	*ESC in-degree* using only the connections to employees in the media (public) sector according to the industry codes listed in the table below
ESC: Sum	The sum of *In-degree* (or *Out-degree)* aggregated across employees of firm *i* who are in the network in year *t*
Other variables	
Tobin's q	Market value of assets divided by book value of assets, in which market value of assets is the sum of market value of equity (common shares outstanding times fiscal-year closing price) and book value of assets minus book value of equity
ROA	Return on assets, calculated as EBITDA divided by the lagged total assets
Sales Growth	Log growth rate of sales
R&D	The ratio of R&D expenses to sales; the ratio is set equal to zero when R&D expenses are missing
Book Leverage	Total debt (sum of total long-term interest-bearing debt and current long-term debt) divided by total assets
ln(1+*Assets*)	Log of one plus total assets (in million Korean won)
Volatility	Stock return volatility of a firm during the past 24 months
Firm Age	Current year minus year of incorporation
ln(1+*Emp*)	Log of one plus total number of employees

Act Exposure	$ESC\ in\text{-}degree_{2015}^{Act} / ESC\ in\text{-}degree_{2015}$, that is, the fraction of *ESC in-degree* in 2015 that is due to connections to employees in industries subject to the Act (we use the industry codes listed in the table below to identify these connections)
Post	An indicator variable that takes the value of one during and after the enactment year (2016–2018) and zero otherwise
d_t	An indicator variable for year *t*
$Act\ Exposure^{Media\ (Public)}$	$ESC\ in\text{-}degree_{2015}^{Media\ (Public)} / ESC\ in\text{-}degree_{2015}$, that is, the fraction of *ESC in-degree* in 2015 that is due to connections to employees in the media (public) sector subject to the Act (we use the industry codes listed in the table below to identify these connections)
ln(1+*Media Coverage*)	Log of one plus the weighted count of news articles from RavenPack News Analytics covering a firm over a year in which the weight is the relevance score of each article which ranges from 0 to 100%. We only include news articles with relevance scores greater than or equal to 75%.
Positive Media Coverage Ratio	The ratio of positive media coverage to media coverage. Positive media coverage is the weighted count of news articles with BMQ sentiment scores of 100 from RavenPack News Analytics covering a firm over a year. The BMQ sentiment score represents the news sentiment of a given story according to the BMQ classifier, which specializes in short commentary and editorials. We only include news articles with relevance scores greater than or equal to 75%.
ln(1+# *of Proc. Contracts*)	Log of one plus the total number of newly signed procurement contracts of firm *i* in year *t*, from the Korea online e-Procurement Service, which is managed by the Public Procurement Service, Ministry of Economy and Finance
ln(1+*Tot Amt. of Proc. Contracts*)	Log of one plus the total amount of newly signed procurement contracts of firm *i* in year *t*, from the Korea online e-Procurement Service
ln(1+*Tot Amt. of Proc. Contracts / Assets*)	Log of one plus the total amount of newly signed procurement contracts normalized by total assets of firm *i* in year *t*, from the Korea online e-Procurement Service
ln(1+*Sales*)	Log of one plus sales

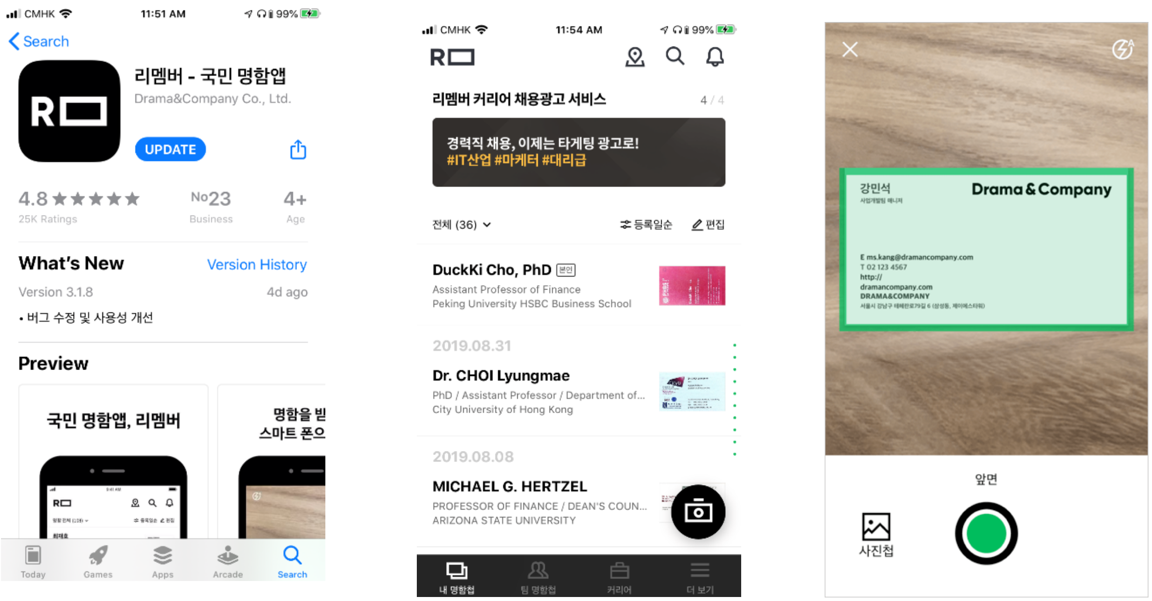
List of industries subject to the Kim Young-ran Act

KSIC code	**Sector**	**Industry**
5812	Media	Publishing of newspapers, magazines, and periodicals
59114	Media	Broadcasting program production
5912	Media	Motion picture, video, and broadcasting program post-production activities
5913	Media	Motion picture, video, and broadcasting program distribution activities
60	Media	Broadcasting activities
63910	Media	News agency activities
6411	Public	Central bank
64991	Public	Public fund management business
6513	Public	Social security insurance
65303	Public	Pension funding
6611	Public	Administration of financial markets
66191	Public	Securities issuance, management, deposit and settlement services
84	Public	Public administration and defense; compulsory social security
85	Public	Education

Internet Appendix III: additional figures and tables

Figure IA.1. Remember, the professional business card management app

This figure displays screenshots of the Remember app's user interface. Panel A shows the app available on App Store, Panel B presents the basic user interface, and Panel C illustrates how to scan and upload business cards using the app.



Panel A. Remember on App Store *Panel B. User interface* *Panel C. Uploading a card*

Figure IA.2. Employee social capital and firm performance: quantile regressions

This figure plots quantile regression estimates on the relation between employee social capital and firm performance based on the specification in columns (4)–(6) of Panel A of Table 2. Firm-level employee social capital takes the lagged value of *ESC in-degree* ("Who Knows You"). In each panel, the solid red line represents the estimated coefficients on ln(1+ *ESC in-degree*) from quantile regressions, and the solid black line represents those from OLS estimates. The shaded area indicates the 95% confidence interval of quantile regression estimates, and the dotted line indicates that of OLS estimates.

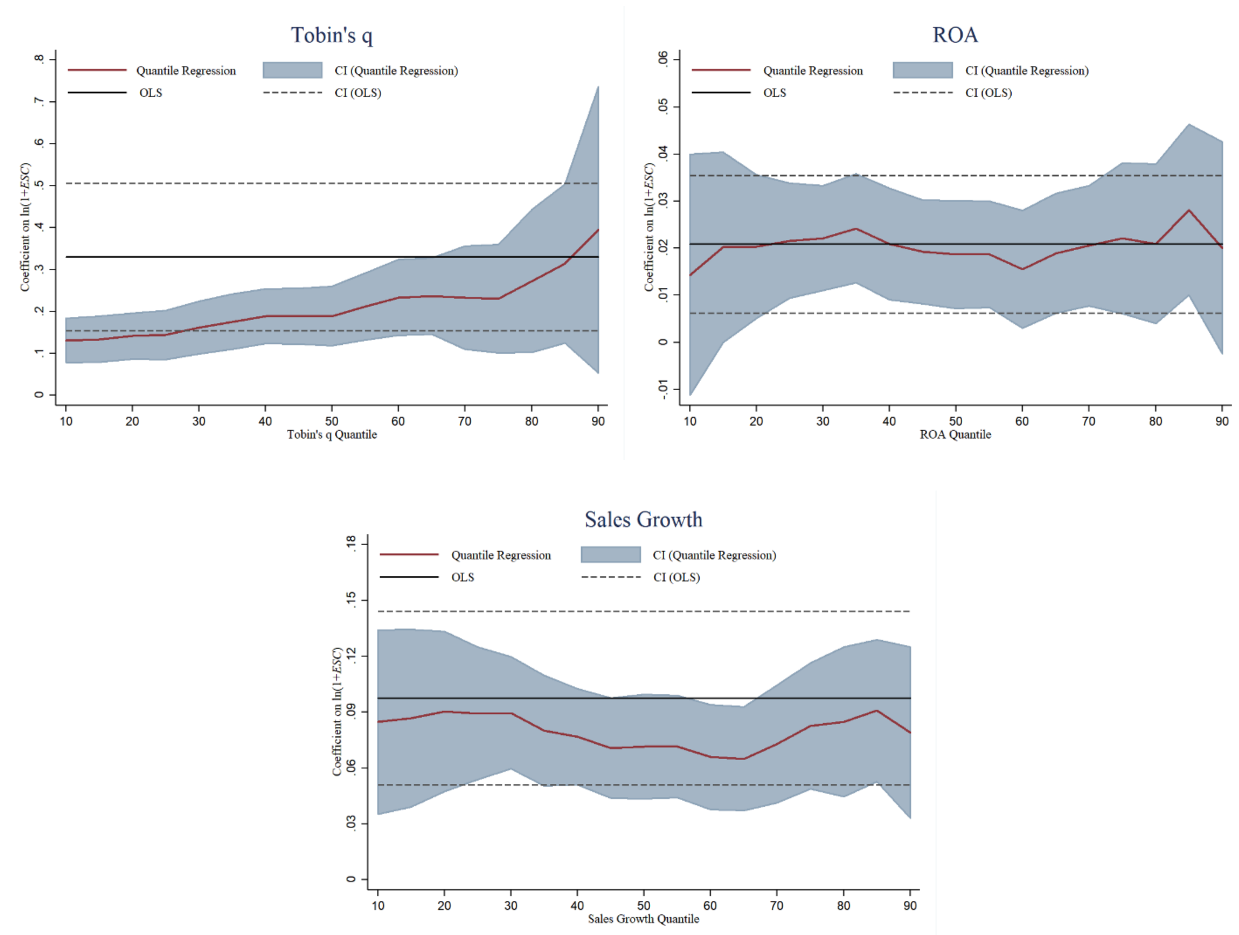


Figure IA.3. Employee social capital and firm performance: measurement error in *ESC out-degree*

To address the potential measurement error in constructing *ESC out-degree* because the *Out-degree* of non-app-users is unobservable, we randomly draw *Out-degree* for non-app-users from the distribution of app-users' *Out-degree* in the same firm with replacement. We then reconstruct *ESC out-degree* using users' actual *Out-degree* and non-app-users' bootstrapped *Out-degree* and rerun the analyses in columns (7)–(9) of Panel A of Table 2. We repeat this procedure 500 times to generate a distribution of the estimated coefficients. This figure plots the kernel density of the coefficient distribution, with a vertical line indicating the actual coefficient estimates in columns (7)–(9) in Panel A of Table 2.

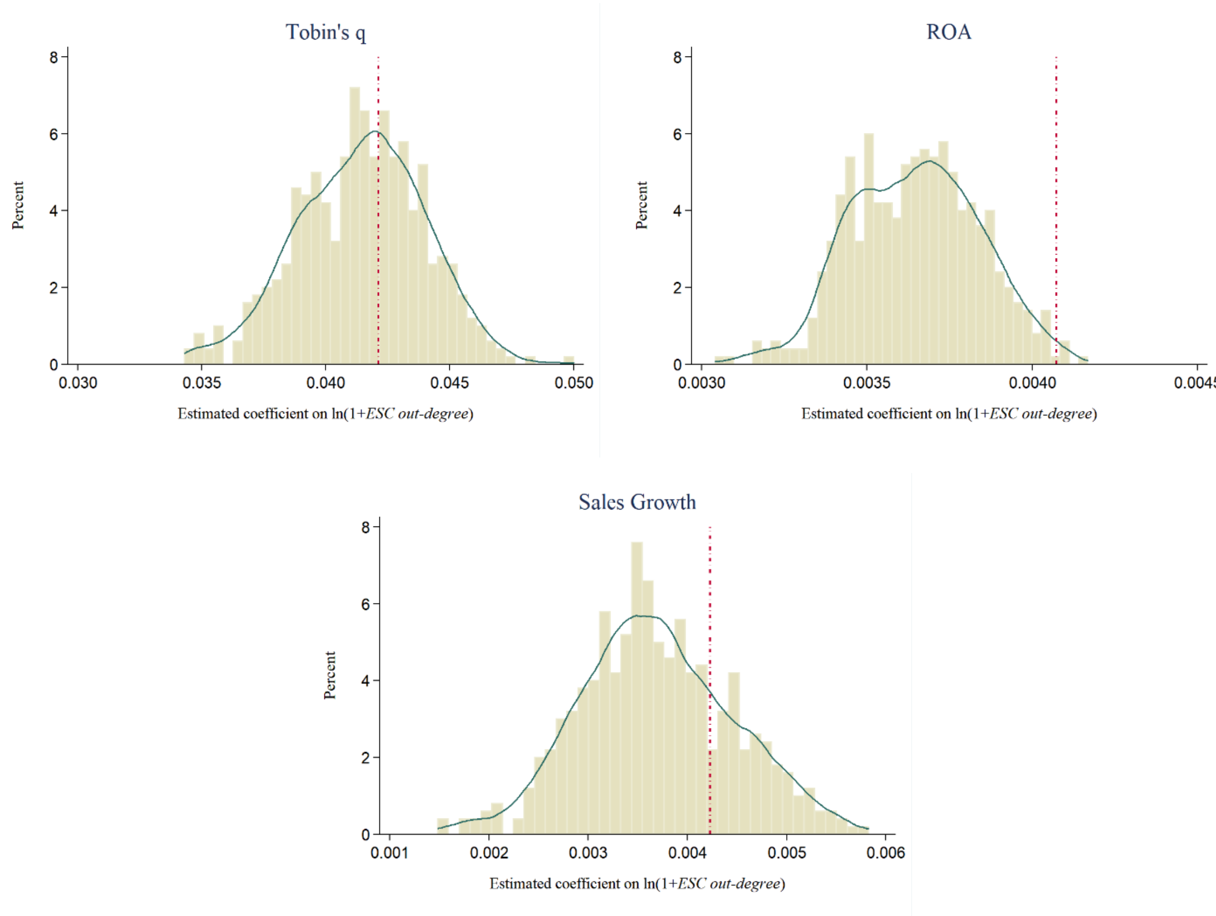


Figure IA.4. Causal evidence: effect of employee social capital on firm performance year by year

This figure plots the point estimates of β_t in the following regression:

$$Y_{i,t} = \beta_0 + \beta_1 \times Act\ Exposure_i + \sum_{t=2015}^{2018} \beta_t \times Act\ Exposure_i \times d_t + \gamma' X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t},$$

where $Y_{i,t}$ is *Tobin's q*, $Act\ Exposure_i = ESC\ in\text{-}degree_{i,2015}^{Act} / ESC\ in\text{-}degree_{i,2015}$, and $ESC\ in\text{-}degree_{i,2015}^{Act}$ is *ESC in-degree* in 2015 that is due to connections to employees in industries subject to the Act. d_t is an indicator variable for year *t*. We extend our pre-treatment sample to include the year 2014 and set 2014 as the baseline year, omitting the 2014 interaction term. The vertical bars correspond to the 95% confidence intervals based on standard errors clustered by firm.

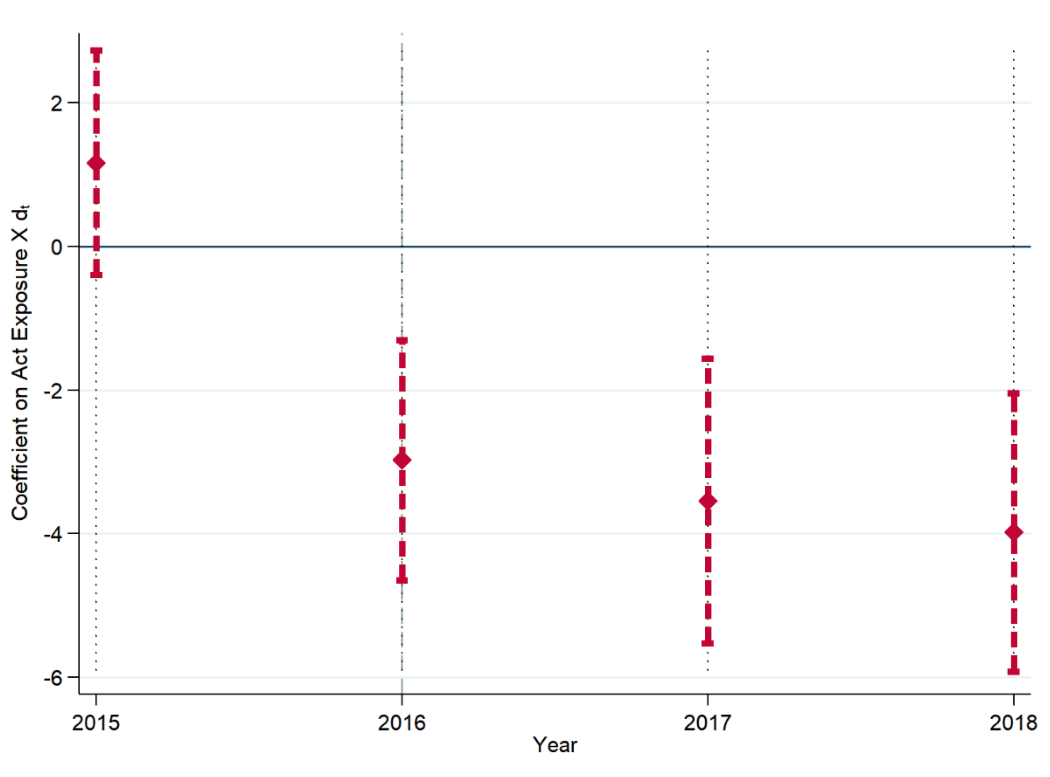


Table IA.1. Descriptive statistics of the business card exchange network by sector

This table presents descriptive statistics by sector (based on the KSIC codes) of the business card exchange network and the firm-level employee social capital measures as of December 2018. We report the number of public firm employees, the number of public firm employees who are app-users, the number of public firms in OSIRIS Industrials, and the average firm-level ESC measures: *ESC in-degree, ESC out-degree,* and *ESC total degree.*

	Business card exchange network			Average firm-level employee social capital measures		
	Employee	App-user employee	Public firms	*ESC in-degree*	*ESC out-degree*	*ESC total degree*
Agriculture, forestry and fishing	1,172	161	6	2.752	22.890	4.568
Mining and quarrying	32	5	3	18.929	73.000	34.571
Manufacturing	545,205	54,502	1,203	3.273	27.669	5.938
Electricity, gas, steam and air conditioning supply	17,698	1,892	11	3.145	25.507	5.670
Water supply; sewage, waste management, materials recovery	417	65	7	4.073	24.706	7.299
Construction	58,462	8,526	51	3.622	30.050	7.430
Wholesale and retail trade	74,745	8,441	148	3.663	29.820	6.694
Transportation and storage	23,843	2,924	26	3.619	37.821	7.231
Accommodation and food service activities	1,272	211	3	3.327	30.388	6.771
Information and communication	105,078	13,648	211	5.119	42.925	9.905
Financial and insurance activities	141,713	23,286	103	5.758	53.176	12.381
Real estate activities	347	100	2	9.217	92.867	21.470
Professional, scientific and technical activities	27,155	3,057	52	4.707	36.251	8.459
Business facilities management and business support services; rental and leasing activities	12,229	1,764	17	4.049	32.126	7.761
Education	2,289	279	10	4.323	32.527	7.758
Arts, sports, and recreation related services	2,467	317	12	3.315	19.571	5.168
Membership organizations, repair and other personal services	1,899	245	1	2.907	16.040	4.741

Table IA.2. Employee social capital and firm performance: additional robustness analyses

This table reports a battery of robustness tests for Panel A of Table 2. Panel A reports the results of a propensity score matching analysis. We match the above-median ESC firms with their below-median counterparts on year, industry (two-digit SIC), and the controls as in Table 2, using the nearest-neighbor-matching algorithm with a caliper of 0.01, and with replacement. Standard errors in parentheses are bootstrapped based on five hundred replications with replacement. Panel B repeats the analysis in columns (4)–(9) of Panel A of Table 2 with alternative sample selection criteria where we restrict our sample to firm-year observations where at least 20 employees are observed in the network or at least 20% of the firm's employees are observed in the network. We also present an alternative aggregation method of employee social capital: *ESC: Sum* is the sum of *In-degree* (or *Out-degree*) aggregated across employees of firm *i* in the network that year. We include an additional control, the number of employees of firm *i* in the network that year. In both panels, we include the same set of lagged control variables (unless specified) and fixed effects as in Table 2. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The sample period is 2015–2018 for output variables. The definitions of all variables are provided in Internet Appendix II.

Panel A. Propensity score matching

	Tobin's q	*ROA*	*Sales Growth*	Number of matches
	(1)	(2)	(3)	(4)
Above median – Below median	0.203***	0.014***	0.065***	2,456
(*ESC in-degree*)	(0.047)	(0.004)	(0.016)	
Above median – Below median	0.025	0.005	-0.002	2,237
(*ESC out-degree*)	(0.047)	(0.004)	(0.015)	

Panel B. Alternative sample selection criteria and measures of employee social capital

	ESC in-degree ("who knows you")			*ESC out-degree* ("who you know")		
Dep. var.	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*
	(1)	(2)	(3)	(4)	(5)	(6)
[At least 20 individuals]						
ln(1+*ESC*)	0.353***	0.026***	0.128***	0.047	0.003	0.007
	(0.097)	(0.008)	(0.025)	(0.032)	(0.002)	(0.007)
Observations	4,842	4,842	4,842	4,680	4,680	4,680
Adjusted R^2	0.259	0.147	0.048	0.257	0.140	0.040
[At least 20% of employees]						
ln(1+*ESC*)	0.289***	0.024***	0.105***	0.035	0.005*	0.007
	(0.098)	(0.008)	(0.027)	(0.040)	(0.003)	(0.008)
Observations	4,209	4,209	4,209	4,014	4,014	4,014
Adjusted R^2	0.263	0.170	0.043	0.267	0.154	0.039
[Sum of *In-degree* (*Out-degree*) across employees]						
ln(1+*ESC: Sum*)	0.251***	0.016***	0.067***	-0.004	0.002	0.007
	(0.070)	(0.006)	(0.017)	(0.022)	(0.002)	(0.005)
Observations	5,340	5,340	5,340	4,994	4,994	4,994
Adjusted R^2	0.254	0.150	0.037	0.253	0.142	0.036
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year

Table IA.3. Adverse impact of the 2016 Kim Young-ran Act on employee social capital

We examine the adverse impact of the 2016 Kim Young-ran Act on social relations with the media and the public sector by estimating changes in the fraction of ESC subject to the Act around the enactment as follows:

$$\frac{ESC\ in\text{-}degree_{i,t}^{Act}}{ESC\ in\text{-}degree_{i,t}} = \beta_0 + \beta_1 \times Post_t + \gamma' X_{i,t\text{-}1} + \alpha_j + \varepsilon_{i,t},$$

where $\frac{ESC\ in\text{-}degree_{i,t}^{Act}}{ESC\ in\text{-}degree_{i,t}}$ measures the fraction of a firm's employee social capital that is derived from connections with employees in the industries affected by the Act. $Post_t$ is an indicator variable that takes the value of one during and after the enactment year (2016–2018) and zero otherwise. $X_{i,t\text{-}1}$ is the same set of lagged control variables as in Table 2; α_j is a full set of two-digit SIC industry fixed effects. We no longer include year fixed effects in the regressions due to the collinearity with the dummy variable *Post*. Since the Act became effective in the latter half of 2016, we report results excluding the enactment year of 2016 in column (1) and results including the year 2016 in column (2) for robustness; the sample period is 2015–2018 for output variables. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The definitions of all variables are provided in Internet Appendix II.

Dep. var.	*ESC in-degree*^{Act}/ *ESC in-degree (%)*	
	(1)	(2)
Post	-0.266***	-0.260***
	(0.068)	(0.062)
R&D	0.496	0.549
	(0.789)	(0.831)
Book Leverage	-0.284	-0.114
	(0.536)	(0.538)
ln(1+*Assets*)	0.498***	0.492***
	(0.111)	(0.110)
Volatility	1.609*	1.528*
	(0.891)	(0.856)
Firm Age	0.000	0.001
	(0.005)	(0.005)
ln(1+*Emp*)	-0.201*	-0.178
	(0.113)	(0.112)
Fixed effects	Ind	Ind
Including year 2016	No	Yes
Observations	4,017	5,340
Adjusted R^2	0.274	0.277

Table IA.4. Causal evidence: full measures of firm performance

This table presents evidence that a firm's employee social capital due to connections with industries affected by the Kim Young-ran Act has a positive impact on firm performance, with the effect concentrated in *Tobin's q*, but not in *ROA* or *Sales Growth*. As in Table 3, we estimate the following difference-in-differences model surrounding the enactment of the Act:

$$Y_{i,t} = \beta_0 + \beta_1 \times Act\ Exposure_i + \beta_2 \times Act\ Exposure_i \times Post_t + \gamma' X_{i,t\text{-}1} + \alpha_{j,t} + \varepsilon_{i,t},$$

where $Y_{i,t}$ is *Tobin's q*, *ROA*, and *Sales Growth*. $Act\ Exposure_i = ESC\ in\text{-}degree^{Act}_{i,2015} / ESC\ in\text{-}degree_{i,2015}$, where $ESC\ in\text{-}degree^{Act}_{i,2015}$ is *ESC in-degree* in 2015 that is due to connections to employees in industries subject to the Act. $Post_t$ is an indicator variable that takes the value of one during and after the enactment year (2016–2018) and zero otherwise. $X_{i,t\text{-}1}$ is the same set of lagged controls as in Table 2; $\alpha_{j,t}$ is a full set of industry-by-year fixed effects. In Panel A, columns (1)–(3) report results excluding the enactment year (2016), whereas columns (4)–(6) report results when we include the year 2016. The sample period is 2015–2018 for output variables. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. In Panel B, we present summary statistics of the Act Exposure variables. The definitions of all variables are provided in Internet Appendix II.

Panel A. Full measures of firm performance

Dep. var.	*Tobin's q*	*ROA*	*Sales Growth*	*Tobin's q*	*ROA*	*Sales Growth*
	(1)	(2)	(3)	(4)	(5)	(6)
Act Exposure	6.578***	0.152	0.178	6.640***	0.156	0.185
	(1.273)	(0.099)	(0.306)	(1.272)	(0.098)	(0.308)
Act Exposure × Post	-4.930***	-0.173**	-0.172	-4.726***	-0.148*	-0.193
	(1.132)	(0.087)	(0.338)	(1.052)	(0.080)	(0.339)
R&D	5.431***	-0.158***	0.379***	5.066***	-0.155***	0.439***
	(0.689)	(0.040)	(0.138)	(0.677)	(0.040)	(0.134)
Book Leverage	0.183	-0.132***	0.075	0.233	-0.139***	0.059
	(0.185)	(0.017)	(0.057)	(0.182)	(0.016)	(0.055)
ln(1+*Assets*)	-0.139***	0.010***	-0.006	-0.146***	0.009***	-0.007
	(0.025)	(0.002)	(0.009)	(0.023)	(0.002)	(0.009)
Volatility	3.403***	-0.111***	0.049	3.400***	-0.103***	0.078
	(0.449)	(0.027)	(0.093)	(0.395)	(0.026)	(0.081)
Firm Age	-0.005***	-0.000***	-0.000	-0.005***	-0.000***	0.000
	(0.002)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
ln(1+*Emp*)	0.076***	0.010***	-0.007	0.067***	0.010***	-0.007
	(0.024)	(0.002)	(0.007)	(0.023)	(0.002)	(0.006)
Fixed effects	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	No	No	Yes	Yes	Yes
Observations	3,778	3,778	3,778	5,101	5,101	5,101
Adjusted R^2	0.242	0.151	0.035	0.245	0.146	0.031

Panel B. Summary statistics of Act Exposure variables

	N	Mean	Median	SD	P25	P75
Act Exposure	3,778	0.036	0.026	0.038	0.012	0.049
$Act\ Exposure^{Media}$	3,778	0.019	0.008	0.029	0.000	0.024
$Act\ Exposure^{Public}$	3,778	0.017	0.013	0.019	0.005	0.024

Table IA.5. Causal evidence: randomization of the exposure to the Act

This table reports the empirical distribution of the coefficient estimate on *Pseudo Exposure* × *Post* when re-estimating column (1) in Table 3 for 1,000 times using the bootstrapped sample. To obtain the bootstrapped sample, we randomly assign a false treatment intensity, *Pseudo Exposure*, to each firm by maintaining the true distribution of *Act Exposure*. We also plot the kernel density of the coefficient estimate distribution and draw a vertical line to indicate the actual coefficient of -4.930.

Actual estimate *Act Exposure* × *Post*	Regression coefficient on *Pseudo Exposure* × *Post*									
	Mean	p1	p5	p10	p25	p50	p75	p90	p95	p99
-4.930	0.045	-1.563	-1.081	-0.827	-0.389	0.062	0.476	0.858	1.069	1.687

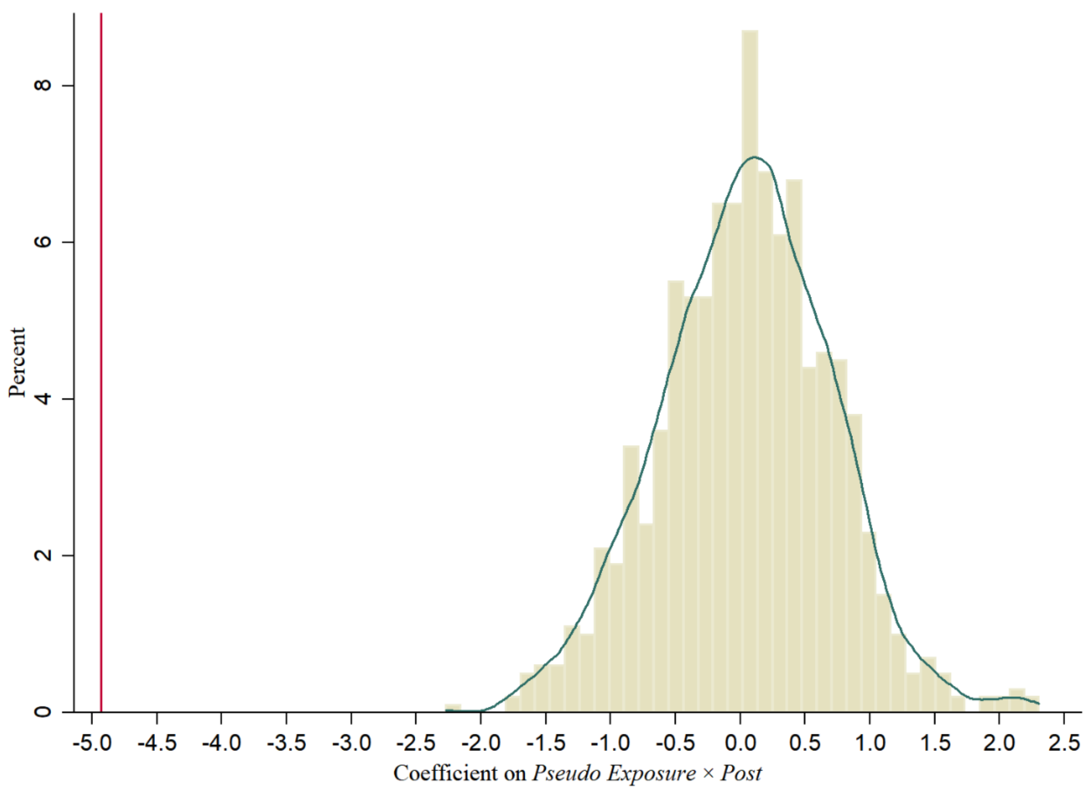


Table IA.6. Causal evidence: additional robustness analyses

This table presents robustness checks for the results in Table 3. Panel A considers alternative measures of *Act Exposure* and alternative sample selection criteria. In column (1), we additionally include *Act Exposure out-degree* and *Act Exposure out-degree* × *Post* to the estimation of equation (2). Here, $\textit{Act Exposure out-degree}_i = \textit{ESC out-degree}^{Act}_{i,2015} / \textit{ESC out-degree}_{i,2015}$, and $\textit{ESC out-degree}^{Act}_{i,2015}$ is *ESC out-degree* in 2015 that is due to connections to employees in industries subject to the Act. In columns (2) and (3), we repeat the analysis in column (1) of Table 3 with alternative sample selection criteria: we restrict our sample to firm-year observations where at least 20 employees are observed in the network in column (2), and to those where at least 20% of the firm's employees are observed in the network in column (3). In panel B, we include the interaction terms between the firm-level control variables and the dummy variable $Post_t$ to the estimation of equation (2). Column (1) reports results excluding the enactment year of 2016; column (2) reports results including the year 2016. The sample period is 2015–2018 for output variables. Standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The definitions of all variables are provided in Internet Appendix II.

Panel A. Alternative measures of Act Exposure and alternative sample selection criteria

Dep. var.	*Tobin's q*		
	(1)	(2)	(3)
Act Exposure	6.338***	6.068***	4.828***
	(1.465)	(1.266)	(1.700)
Act Exposure × *Post*	-4.165***	-3.530***	-2.600*
	(1.315)	(1.214)	(1.543)
Act Exposure out-degree	0.408		
	(0.772)		
Act Exposure out-degree × *Post*	-0.782		
	(0.764)		
R&D	5.179***	5.550***	5.286***
	(0.705)	(0.693)	(0.733)
Book Leverage	0.026	0.124	0.054
	(0.183)	(0.188)	(0.217)
ln(1+*Assets*)	-0.141***	-0.141***	-0.137***
	(0.026)	(0.025)	(0.028)
Volatility	3.585***	3.420***	3.690***
	(0.491)	(0.479)	(0.509)
Firm Age	-0.005***	-0.005***	-0.004**
	(0.002)	(0.002)	(0.002)
ln(1+*Emp*)	0.094***	0.094***	0.083***
	(0.027)	(0.027)	(0.025)
Fixed effects	Ind × Year	Ind × Year	Ind × Year
Including year 2016	No	No	No
Observations	3,577	3,390	2,895
Adjusted R^2	0.249	0.245	0.245

Panel B. Including the control variables interacted with the dummy variable Post

Dep. var.	*Tobin's q*	
	(1)	(2)
Act Exposure	7.380***	7.380***
	(1.319)	(1.318)
Act Exposure × *Post*	-5.847***	-5.544***
	(1.175)	(1.100)
R&D	1.997***	1.997***
	(0.712)	(0.711)
Book Leverage	0.564*	0.564*
	(0.314)	(0.314)
ln(1+*Assets*)	-0.249***	-0.249***
	(0.034)	(0.034)
Volatility	3.742***	3.742***
	(0.666)	(0.666)
Firm Age	-0.010***	-0.010***
	(0.002)	(0.002)
ln(1+*Emp*)	0.137***	0.137***
	(0.038)	(0.038)
R&D × *Post*	4.337***	3.711***
	(0.851)	(0.805)
Book Leverage × *Post*	-0.481	-0.393
	(0.359)	(0.331)
ln(1+*Assets*) × *Post*	0.141***	0.123***
	(0.033)	(0.030)
Volatility × *Post*	-0.334	-0.352
	(0.789)	(0.729)
Firm Age × *Post*	0.008***	0.007***
	(0.002)	(0.002)
ln(1+*Emp*) × *Post*	-0.070*	-0.081**
	(0.036)	(0.034)
Fixed effects	Ind × Year	Ind × Year
Including year 2016	No	Yes
Observations	3,778	5,101
Adjusted R^2	0.253	0.252