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## Information Exposure and Corporate Citizenship

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**Stigler Center**

for the Study of the Economy and the State

## **Information Exposure and Corporate Citizenship**

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New Working Paper Series No. #312

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# Information Exposure and Corporate Citizenship

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## Abstract

We explore how information exposure, specifically information transmission within organizations, facilitates companies' roles as corporate citizens. We study whether US firms' business networks with China and Italy become their information advantage, and examine whether firms use relevant information to mitigate the negative shocks of COVID-19. We start by validating our measurement of information exposure. Next, we find that a higher number of work-from-home ("WFH") policies, as evidenced by a higher stay-at-home ratio, are implemented in areas with more information-exposure companies, even before local governments impose a lockdown. To further demonstrate corporate citizenship, we document firms' positive social impact—lower COVID-19 growth and an influence on other firms' WFH policies—and show suggestive evidence on firms' social motives.

**Keywords:** Information exposure, information transmission, business networks, COVID, corporate citizenship.

**JEL Classification:** D83, D85, G3, M14

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# 1 Introduction

When governments fail to respond quickly and effectively to a crisis, can companies step in as corporate citizens to help address the issue? In this paper, we explore an important mechanism through which firms can act as corporate citizens: information transmission within organizations. While the literature has recognized the importance of regulators’ constraints and the ways in which agents in the market collect and use information to affect their economic outcomes,<sup>1</sup> the impact of information exposure (particularly information transmission within organizations) on corporate citizenship—a social outcome—is relatively unexplored. The objective of this paper is to address this question. Specifically, we study whether US firms’ business networks with China and Italy, including trade, executive, and branch-office networks, become firms’ information networks, and we examine whether firms with this information exposure are more likely to adopt earlier WFH policies. We consider the positive social impact of these firms, e.g., mitigating the pandemic in the US, as an example of corporate citizenship.

Empirically identifying the effect of information exposure on corporate citizenship is challenging for two reasons. First, corporate citizenship is a broadly-defined concept with no consistent agreement on exactly what constitutes corporate citizenship (Carroll, 1999; Matten and Crane, 2005). As such, it is also challenging to measure acts of corporate citizenship. Second, without a clearly-defined and exogenously-changed information advantage, it is difficult to attribute changes in corporate citizenship to changes in information exposure. More generally, it is hard to distinguish between an information advantage and a firm’s other fundamental characteristics.

To address the first challenge, we use the definitions of corporate citizenship in Christensen et al. (2021) and Matten and Crane (2005), and focus on firms’ concrete corporate citizenship behavior in the setting of COVID-19.<sup>2</sup> More specifically, we define corporate citizenship as firm actions with a positive social impact. We consider firms’ early adoption of WFH policies *and* the

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<sup>1</sup>See e.g., Coffee Jr (1984); Easterbrook and Fischel (1984); Zingales (2009); Leuz and Wysocki (2016); Baron and Besanko (1984); Demski and Sappington (1987); Innes (1996).

<sup>2</sup>Christensen et al. (2021) defines corporate citizenship as “activities and policies that assess, manage, and govern a firm’s responsibilities for and its impacts on society and the environment.” Relatedly, Matten and Crane (2005)’s definition is “the role of corporations in administering citizenship rights for individuals... *a descriptive conceptualization of what does happen, rather than a normative conceptualization of what should happen.*”

positive social impact of these policies as examples of corporate citizenship. Furthermore, because our outcome of interest is quickly realized, this setting allows us to measure and quantify social consequences sooner than the other long-term environmental outcomes.

To address the second challenge, we employ firms' existing trade, executive, or branch-office networks with China and Italy to operationalize firms' comparative information advantage. Because these firms were likely aware of the severity of COVID-19 early on and/or had already implemented safety measures in China and Italy, they can be more informed in planning their containment strategy in the US. For example, in Starbucks' Q2 earnings conference call, CEO Kevin Johnson said, "based on the experience we gained navigating COVID-19 in China, we have been as well prepared as anyone for this mitigate-and-contain phase in the US." One strategy for slowing the spread of COVID-19 is to keep individuals at home. Companies were able to verify the effectiveness of this measure by having employees work-from-home ("WFH") in China and Italy. Importantly, the information transmitted within an organization is likely to be credible, given that incentives are internally aligned (Crawford and Sobel, 1982).

The COVID-19 setting provides a plausibly exogenous information advantage for firms with business networks to China and Italy. Given the unprecedented nature of COVID-19, firms could not have anticipated the pandemic in 2019 (or earlier) in order to build business networks with China and Italy. This provides us with plausibly exogenous variation in information exposure and allows us to directly observe how firms adjust their behavior with differing information exposure to COVID-19 even before local governments impose measures.

We start by validating our measurement of *information exposure* and showing that firms have relevant information through their trade, executive, or branch-office networks with China and Italy. Similar to Larcker et al. (2020), we validate our measure of information exposure by studying firm disclosures, assuming that firms with relevant information will disclose more and earlier than do other firms. In line with this assumption, we descriptively show that the 309 SP500 firms exposed to the relevant information are more likely to issue COVID-19 press releases and to do so earlier than other firms.<sup>3</sup> Additionally, using data from Hassan et al. (2020), we examine and find that

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<sup>3</sup>In these press releases, firms typically claim that protecting "communities" is their reason for adopting early WFH

information-exposure firms provide more COVID-19 coverage, and that these firms are associated with more positive sentiment in the latest earnings conference calls before 1 May 2020, suggesting that these firms are more resilient and better prepared for COVID-19. Importantly, all of our results hold for firms with only executive networks (without trade or branch-office networks), mitigating the concern that our measure captures only these firms being more affected by COVID-19.<sup>4</sup> Collectively, the disclosure results validate our measure of information exposure.

Having demonstrated that firms have relevant information, we next examine whether firms act on this information as corporate citizens during COVID-19. Specifically, we test whether firms impose WFH policies earlier than do local governments in order to preserve employee health, and we investigate firms' positive social impact to further demonstrate corporate citizenship.

To explore whether information-exposure firms are more likely to adopt work-from-home policies, we use SafeGraph data on foot traffic to validate that employees stay at home. In this analysis, the outcome is the ratio of devices at home in a given day and zip code, a ratio which should increase when firms impose WFH policies. To illustrate, we randomly select a company, Albemarle Corp, and obtain its exact WFH date from its investor relations department. We find that the stay-at-home ratio increases when Albemarle Corp announces its WFH policy, which happens before local government issues a "stay at home" order, as shown in Figure 1.

In this analysis, we focus on the period *before* local governments impose mobility restrictions to demonstrate firms' proactive behavior. We identify firm location using InfoGroup data, and create an information exposure measure at the zip-code level that is based on the number of firms' trade, executive, or branch-office networks with China or Italy, scaled by the total number of companies (subsidiaries) in a given zip code. Using a Difference-in-Differences (DiD) design with zip code fixed effects and that controls for fundamental economic characteristics, we compare changes in stay-at-home ratios across zip codes with different information networks in the same county and day.

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policies. For example, Home Depot states that they want "to better serve **customers and communities** in response to COVID-19." McDonald's intention is "ensuring the health and safety of our people and our **communities**," while Ebay seeks "the safety and well-being of our employees, customers and **communities**."

<sup>4</sup>As another validation test, using insider trading data from Thomson Reuters, we find that on average, insiders in firms with (without) information exposure make 0.10 (0.06) of a sale transaction per day in February and March of 2020 (t-stats = 3.73).

We find that zip codes with a higher proportion of information-exposure firms have more individuals staying at home before local governments impose lockdown or shelter-in-place policies. Employees are around 1.5% more likely to stay at home for each unit increase in our information exposure measure.<sup>5</sup> To give a more direct interpretation, zip codes with information exposure have a 0.8% higher stay-at-home ratio than do other (non-exposed) zip codes. Our result is robust to including city-day and state-day fixed effects, using samples with a minimum population and with firm cutoffs, and only using China or Italy exposure factors. The DiD specification also mitigates the concern that our results are driven by other news sources, as zip codes in the same county should receive similar media coverage.

One potential concern is that the COVID-19 exposure in China and Italy can cause changes to firm fundamentals, e.g., if factories in the US are closed due to the pandemic in China or Italy. However, we find that our results hold for sub-samples of firms whose information exposure comes from higher executive networks (which are less likely related to firms' operation decisions). Furthermore, we would expect the change in firm fundamentals to take place earlier, but not before local lockdown policies. In contrast, we fail to observe differential trends in stay-at-home ratios during the long pre-period. We also plot the coefficients for five-day-intervals before lockdown and find no pre-existing differential trends, providing evidence that zip codes with different levels of information exposure have similar stay-at-home ratio trends in earlier periods.

Our main empirical challenges are that better information networks may be correlated with firms' sophistication, wealth, and risk management experiences, and that other information sources (e.g., social media) may have a confounding effect (Simonov et al., 2020; Bailey et al., 2020). While the DiD specification mitigates these concerns, to further strengthen our inferences, we conduct two additional tests. First, instead of using China and Italy as information exposure countries, we use several economically important but *non-early*-COVID placebo countries (Canada, UK, Belgium, Netherlands, Japan, and India) that experience the spread of COVID-19 much later than the countries of China, Italy, and the US, as shown in Appendix A3. We do not find any effects in areas

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<sup>5</sup>To put this magnitude into context, we follow the specification in Barrios et al. (2021) and control for social capital. We find that the economic importance of information exposure is comparable to the effect of social capital on citizens' stay-at-home ratios.

with a high proportion of placebo information networks, but find economically and statistically significant results for areas with a large number of Chinese and Italian connections, supporting our inference that the COVID-19 information from China and Italy works as an information exposure.<sup>6</sup>

For the second additional test, we examine whether our results are driven by employees with social-network information about conditions in China and Italy (Charoenwong et al., 2020). In addition to controlling for the percentage of Asian population in each zip code, which we do throughout the paper, we examine how the results change when firms have information networks with China but do not have many Chinese employees. We split our sample into low (high) Asian employee populations with firms' high (low) information networks with China. Our results hold in these subsamples, suggesting that our results are driven by firm policy. Additionally, because we focus on the period before government lockdown, many employees may find it hard to stay at home for an extended period of time absent firm WFH policies.

Having established that information-exposure firms impose WFH policies earlier than do local governments, to further demonstrate these firms' corporate citizenship, we study their positive social impact and provide suggestive evidence on their social motives. We document two positive social impacts from information-exposure firms—lower COVID-19 growth and an influence on other firms' WFH policies.

We start by exploring whether counties with more information-exposure firms are associated with a lower spread of COVID-19. We look at COVID-19 cases 7 days and 14 days into the future, as this disease has a long incubation period and any containment measures will likely take effect with a time lag (Lauer et al., 2020). We find that both 7-day and 14-day cases are lower in counties with more information exposure; the magnitude translates to a 2.2% standard deviation reduction in COVID-19 cases over a 7-day window. This outcome is particularly important, not only because WFH policies reduce employees' risk of infection, but also because they decrease the probability that employees will infect others, which helps contain the virus and benefits society at large.

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<sup>6</sup>We also examine whether early responders (who are positively associated with information exposure to China and Italy) are generally better at anticipating, managing, and preparing for risks before government intervention. We empirically assess firms' past experiences with other disasters, and find that early COVID-19 responders do not experience a higher number of previous disasters or health crises. We use the data on exposure to prior health crises in earnings conference calls from Hassan et al. (2020), and the natural disaster dataset from FEMA. For FEMA data, see <https://www.fema.gov/openfema-data-page/disaster-declarations-summaries-v2>.



Next, we show that firms with Chinese and Italian exposure also help non-exposure firms in related communities adopt early WFH policies. We define a related community as an area with firms that do not have direct Chinese or Italian exposure but that are in the same industry or are in a zip code near information-exposure firms. We find that related communities have higher WFH ratios when the influence of information-exposure firms is higher, consistent with information-exposure firms' positive social impact.

In our final analysis, we provide two sets of suggestive evidence showing that firms' choice to adopt a WFH policy is not purely profit-driven. We start by examining how socially responsible investors are associated with firms' WFH policies. We use *Principles for Responsible Investment* ("PRI") institutional investors and *Socially Responsible Investing* ("SRI") mutual funds to proxy for investors' socially responsible preferences. Consistent with the early adoption of WFH policy as a socially responsible action, we find that the results are stronger when investors have prosocial preferences.

Next, we exploit variation in industry teleworkability and examine whether firms with low teleworkability also implement WFH policies. While WFH policies are not necessarily a financial sacrifice for firms in highly teleworkable industries, firms with low teleworkability may face financial consequences from letting their employees WFH. We use the industry teleworkability constructed by Dingel and Neiman (2020). We test and find evidence that our results still hold in less teleworkable industries. Given that WFH may significantly hurt profit in these industries, the presence of WFH policies suggests firms' social motives.

Our paper makes three important contributions to the literature. First, our paper contributes to the literature on firms' internal networks and information transmission. Prior research has studied how local demand shocks spillover through firms' internal networks to affect aggregate employment in other areas (Giroud and Mueller, 2019), and that firms learn by exporting (e.g., De Loecker, 2013). Other research focuses on the economic consequences of intra-industry information transmission (e.g., Foster, 1981; Baginski, 1987; Thomas and Zhang, 2008). By testing how firms' information advantage from international business networks facilitates corporate citizenship, our paper contributes to the literature by examining how information transmission within an orga-

nization impacts a social outcome, which makes it very distinct from those in the prior literature.

Second, our paper adds to the literature on corporate citizenship in two ways. First, our setting allows us to observe a concrete act of corporate citizenship, which is usually hard to define and to test empirically. We study firms' early adoption of WFH policies during COVID-19 (prior to government lockdowns), and show that firms had a positive social impact by lowering the spread of the pandemic. Second, as our social outcome of interest is quickly realized, we can measure and quantify social consequences sooner than the other long-term environmental outcomes. Prior literature focuses on the impact of corporate social responsibility ("CSR") or of CSR reporting on firm outcomes (see Christensen et al. (2021) for a review); our research also complements this literature (e.g., Chen et al., 2018; Tomar, 2019), particularly in regards to employee safety (e.g., Christensen et al., 2017).

Third, there is an emerging literature studying the COVID-19 pandemic. Our study is related to the branch that uses cellular phone data to track personal mobility (e.g., Barrios et al., 2021; Charoenwong et al., 2020; Goolsbee and Syverson, 2020). Our paper is most closely related to Charoenwong et al. (2020) and Bailey et al. (2020), which both show that the flow of social information is an economically significant driver of social distancing. Our paper, however, has three unique features. First, we focus on how firms use their trade, branch-office, or executive networks as an information advantage in their role as corporate citizens. Importantly, we are interested in how *firms* behave as corporate citizens *before* government policy is implemented, while Charoenwong et al. (2020) evaluates *households'* compliance behavior *after* state-wide government mobility restrictions. Second, there are important differences between firms and individuals. Firms are able to collect and act on information more efficiently and with a broader impact; an individual's power may be limited (Hart and Zingales, 2017). More specifically, even though individuals may want to work from home, it is hard for them to consistently do so without the institution of firm policy. Third, we use city-level government policy, while prior literature uses state and county policy (e.g., Charoenwong et al., 2020; Goolsbee and Syverson, 2020). Our paper shows that one potential omitted variable in prior literature is local firms' WFH policies.

## 2 Institutional Setting and Theoretical Underpinning

### 2.1 COVID-19 and Corporate Citizenship

The COVID-19 outbreak and the subsequent containment measures resulted in significant societal change, while the associated health and economic impacts required governments to take unprecedented measures. Meanwhile, company behavior came under a spotlight, as workplace infection can create negative social externalities due to the contagious nature of COVID-19.

Following Matten and Crane (2005) and Christensen et al. (2021), we define corporate citizenship as firm actions with a positive social impact.<sup>7</sup> We consider firms' protective measures in the absence of government policies *and* firms' positive social impact during COVID-19 as examples of corporate citizenship. Matten and Crane (2005) conceptualize corporate citizenship as the growing corporate management of government functions. Corporate citizenship aims to improve social welfare and/or to increase the sustainability of corporate activities (Christensen et al., 2021). Our definition of corporate citizenship is also consistent with Hart and Zingales (2017)'s claim that many business activities and social impacts are inseparable.

Empirically testing for corporate citizenship, however, is difficult. In particular, it is difficult for firms to decide on the correct path of corporate citizenship when there is uncertainty or when shareholder preferences differ. The COVID-19 provides a setting where there is more consensus as to what constitutes as an act of corporate citizenship.

We study firms' response to the COVID-19 crisis, and show that firms can act as corporate citizens by mitigating COVID-19 through information transmission within organizations. Specifically, firms can collect and verify information about the severity of COVID-19 and about effective containment policies through their international business networks. Since COVID-19 outbreaks in China and Italy occurred earlier than in the US, US firms with ties to China and Italy may have better information and may make timelier and more informed decisions to slow the spread of the pandemic. For example, one strategy to slow the spread of COVID-19 is to keep residents at home. The Chinese and Italian governments issued stay-at-home orders, and some local branches required

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<sup>7</sup>Specifically, corporate citizenship is “corporate activities and policies that assess, manage, and govern a firm’s responsibilities for and its impacts on society and the environment,” (Christensen et al., 2021) and “the role of corporation in administering citizenship rights for individuals.” (Matten and Crane, 2005)

employees to work from home, allowing companies to verify the effectiveness of these measures. Thus, US firms with business networks in China or Italy may foresee the spread of COVID-19 and ask employees to work from home before the local government imposes stay-at-home orders.

The COVID-19 setting is well suited to demonstrate that firms can act as powerful corporate citizens by using their comparative information advantage. During COVID-19 when governments and regulations are imperfect (e.g., Packer, 2020; Wallach and Myers, 2020; Silver and Hyman, 2020), firms with comparative advantages can be more efficient at addressing certain social issues (Edmans, 2020; Hart and Zingales, 2017). Another important advantage of the COVID-19 setting is that shareholder value may not be easily maximized instrumentally due to the uncertain market environment stemming from the pandemic. While the pandemic requires firms to consider and mitigate the risks to human capital, human capital may not be valuable in bankruptcies occurring because of market downturns from COVID-19 (Edmans, 2012). It is thus *ex ante* unclear whether mitigating human-capital risk is a profit-maximizing strategy. Finally, we can measure and quantify social impact in our setting because our outcome of interest is quickly realized, especially as compared to long-term environmental outcomes.

## **2.2 Government Constraints and Information Exposure**

There is a long-standing debate in the literature on whether mandatory regulation or market forces is more effective. Numerous papers highlight the conditions under which market failure is likely, when regulations (and mandatory actions in general) are most needed; these conditions include positive externalities, the limitations of private enforcement, the lack of binding commitment mechanisms, and the need for general cost savings (e.g., Coffee Jr, 1984; Easterbrook and Fischel, 1984; Zingales, 2009; Leuz and Wysocki, 2016). Regulation is not perfect, however, and prior research points out the difficulty of ensuring effective enforcement as well as the potential for regulatory capture (e.g., Stigler, 1971; Posner, 1974; Peltzman, 1976; Becker, 1983).

Various constraints can interfere with regulatory policy design and response. For example, there are political economy frictions (e.g., Ales et al., 2014; Yared, 2010) where policymakers are non-benevolent, cannot commit to policies, and/or have private information about the tightness of government budgets and rents (Aguiar and Amador, 2011). Governments can be subject to nation-

alism and interstate frictions (Fisman et al., 2014), and policymakers may have political concerns such as upcoming elections (Brown and Dinc, 2005), which can delay government interventions to support failing economies. Consistent with this concern, Leverty and Grace (2018) document that elections delay regulatory action, and that such delays are larger for elected than for appointed regulators.<sup>8</sup>

Furthermore, even if public regulators are benevolent, it is less clear whether they have the relevant information to identify and design practices that go beyond market forces. As does any economic agent, regulators face information barriers and frictions. These information frictions could lead to misleading and even false regulatory design.<sup>9</sup> The information economics literature has long recognized the importance of information asymmetry and its effect on economic behavior (e.g., Akerlof, 1970; Arrow, 1996). In policy design, an important economic problem is how to enable rapid adaptation to changes in time and place, so that the ultimate decisions are left to agents with complete and relevant information sets (e.g., those who are familiar with the circumstances and those who know the available resources). One solution is that prices can communicate information and coordinate the separate actions of different individuals (Hayek, 1945). However, when other problems arise, prices do not perfectly solve the information problem of scarcity (Stiglitz, 2000).

In the market system, firms have information about their own resources and objectives, and then choose actions for producing, redistributing, and consuming resources (Myerson, 2008). Furthermore, firms have experience in acquiring and verifying relevant information. This type of information acquisition can overcome barriers and can create comparative advantages, especially for companies acting as corporate citizens in a globalizing world. In contrast, regulators may lack relevant information and the economies of specialization. Firms gain a comparative advantage, not because they have the most complete market conditions, but because of experiences that constitute

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<sup>8</sup>COVID-19 provides anecdotal evidence of this phenomenon. For example, New Orleans Mayor LaToya Cantrell said that “Leadership matters. And if the federal government is not responding to or saying that we’re potentially on the verge of having a crisis for the pandemic coming to the U.S. – that would change everything. But that wasn’t happening” (Montgomery et al., 2020). Philadelphia Mayor Jim Kenney said at a news conference that “We may be healthier but the economy will be in the tank, and we can’t have that” (Walsh, 2020). Savannah Mayor Van Johnson claimed that he was hesitant to put families’ livelihoods at risk by shutting down restaurants. Instead, he would rather that business owners provide safe alternatives (Dikes, 2020).

<sup>9</sup>See e.g., Baron and Besanko (1984); Demski and Sappington (1987); Innes (1996); Kwerel (1977); Sappington (1982); Sibley (1989); Besanko and Spulber (1989). See also the informational lobbying literature: e.g., Calvert (1985); Potters and Van Winden (1992); Austen-Smith (1995); Bertrand et al. (2014); Krishna and Morgan (2001).

their firm-specific information sets.

During COVID-19, the information constraints faced by different economic agents, as well as agents' inability to validate and use the information to which they have access, can delay the response to the spread of the virus. Some local individuals and governments may have information they are not able to validate or may face constraints to taking timely action; this is especially true when trust in institutions is low (e.g., La Porta et al., 1996; Guiso et al., 2004). Direct experiences in China and Italy give firms an advantage by helping them learn about the severity of the virus and about the efficacy of containment measures. Thus, firms have the knowledge and the resources to act on the relevant COVID-19 information (e.g., Stein, 2002; Christie et al., 2003). For instance, Starbucks CEO Kevin Johnson said "Starbucks built a model to follow the company's business recovery in China, where the coronavirus pandemic began in late December....Starbucks' experience in China is very much informing its response to the global pandemic in other markets" (Stankiewicz, 2020). In Apple's press release, the company states that "One of those lessons [from China] is that the most effective way to minimize risk of the virus transmission is to reduce density and maximize social distance." Importantly, this type of information can be credibly transmitted within organizations, where incentives are internally aligned (Crawford and Sobel, 1982). Firms with branches and strategic partnerships in China and Italy can rely more on their own knowledge.

Ultimately, whether and how the information exposure facilitates corporate citizenship is an empirical question. To test the information exposure in corporate citizenship, we use the setting of COVID-19 in the US, where regulators' constraints are pronounced and where firms have relevant and direct information (creating a comparative advantage for firms).<sup>10</sup>

### 3 Data and Methodology

#### 3.1 Social Outcome

Our main outcome variable is the ratio of people who work from home before local governments introduce stay-at-home orders. To capture the movement of the populace, we use SafeGraph data on device mobility, which have been extensively used in prior literature (e.g., Charoenwong

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<sup>10</sup>Note that in our study, we do not examine the forces of competition, how these forces lead to information acquisition, or how this information is priced in the market.

et al., 2020; Goolsbee and Syverson, 2020; Simonov et al., 2020). These data track the location of mobile devices, contain data on the number of devices in a census tract based on home location, and show how many of these devices went to full-time work, part-time work, or stayed completely at home. SafeGraph collects information on almost 45 million cellular phones (about 10% of the total number of devices in the US), and calculates the number of visits to millions of “points of interest” in the US. It classifies the work locations of the devices based on the number of hours spent at each location during certain times of the day. Specifically, SafeGraph defines home as the common nighttime (6 pm - 7 am) location for the device over a six-week period. To use the continuous treatment variation and to capture firms’ positive social impact on their local communities, we use zip-code-level data. We aggregate the number of devices to the zip-code-day level, and create our main outcome measure from the percentage of devices that stayed at home (“stay-at-home ratio”). We drop weekends and public holidays to better capture the variation driven by firms’ WFH policies.

To illustrate that the zip code is a reasonable unit of analysis, we randomly select a company (Albemarle Corp), obtain its exact WFH date from its Investor Relations department, and plot the stay-at-home ratio in Figure 1. We use data for zip code 28209, which is the location where Albemarle Corp is headquartered in Charlotte, North Carolina. The x-axis is dates in March of 2020, the solid line is when Albemarle Corp announced a WFH policy on day 12, and the dashed line is when the local government’s “stay at home” order went into effect on day 26 . Figure 1 shows that the stay-at-home ratio starts increasing from 22% when Albemarle Corp announces their WFH policy. By the time of the local government’s “stay at home” order, the stay-at-home ratio is already at 35%. As an out-of-sample example, in Appendix A2, we plot the stay-at-home ratio in March 2020 for the University of Chicago. We can see that the stay-at-home ratio increases on day 17 when the University announces its WFH policy, further showing that the zip code is a reasonable unit of analysis.

We use two data sources to identify the period before local governments introduce COVID-19 policies. The first is county and state level data on COVID-19 policies from Keystone Strategy. This dataset contains the dates of Non-Pharmaceutical Interventions (“NPI”); these interventions include policies on social distancing, limitations on gatherings, lockdowns, and the closures of schools,

public venues, and non-essential services. The second source is city level data on COVID-19 policies from the COVID-19 Local Action Tracker created by the National League of Cities and Bloomberg Philanthropies.<sup>11</sup> For each zip code, we layer in the city-, county-, and state-level COVID-19 policy dates, keeping the earliest date as the first day of government intervention.

Additionally, we include daily data on COVID-19 cases and deaths as reported by The New York Times. These data are available at the county-level. In the analysis using state-day fixed effects, we include cases and deaths as control variables. Furthermore, we study the growth in COVID-19 cases as a social outcome. Lauer et al. (2020) find that the incubation period of COVID-19 ranges from 2 to 14 days at the 95% confidence interval, which suggests that any impact of WFH policies on the spread of COVID-19 likely happens with a time lag. To better capture the impact of WFH policies, we study changes in COVID-19 cases 7 and 14 days into the future.

### **3.2 Information Exposure**

We proxy for firm exposure to information about COVID-19 using firm ties to China and Italy. Both China and Italy experienced the COVID-19 outbreak before the US. As such, firms with connections to China and Italy are likely to have earlier access to information about the severity of and effective measures against COVID-19.

We use three sources of data for firm exposure to China and Italy. First, we use Factset Revere data to identify firms that have business relationships (including with partners, customers, and suppliers) in China or Italy. These data have been used in prior literature to capture the location of business relationships (Ding et al., 2020). Second, we use BoardEx data to identify firms with executives or board members who have connections to China and Italy. Specifically, we code a firm as exposed if any of its executives or board members received an undergraduate education from an institute in China or Italy. BoardEx and FactSet data include both public and private firms. Third, we use firms' segment reporting to identify firm branches in China and Italy. We assume that if firms have a reporting segment in China and Italy that is related to sales or to physical investment, these firms have branches (or at least physical presences) in China and Italy. The segment data are available for public firms. Finally, we use InfoGroup data in 2019

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<sup>11</sup><https://covid19.nlc.org/resources/covid-19-local-action-tracker/>



to augment information about firms' US branches. InfoGroup collects business location data from public sources (e.g., the yellow pages and credit card billing statements), and also has data on the number of employees and the location of business headquarters. The aggregate zip-code level employee count from InfoGroup is 92% correlated with the data from the 2018 Census Bureau's County Business Patterns, providing support for the representative nature of the InfoGroup data. We keep branches with over 100 employees, as branches with fewer employees are less likely to have an impact on the local workforce. We then aggregate the number of firms in a zip code that have Chinese or Italian exposure based on firm headquarters and branch location.

To create a measure for information exposure, we aggregate the six variables for exposure to China and Italy from BoardEx, Factset, and segment data. Each firm has at most six sources of information exposure. In the zip-code-level analysis, our main information exposure variable is *Information Exposure Scaled*, which is the sum of information exposure sources for firms in a zip code scaled by the total number of firms in that zip code and then multiplied by 100. We truncate this variable at the 99th percentile to reduce the impact from outliers.

## 4 Results

### 4.1 Validating Information Exposure

We first validate that firms with exposure to China and Italy have timely and relevant information about COVID-19 by showing that these firms are more likely to provide earlier disclosures about the pandemic. We use SP500 firms as of 1 January 2020, and provide two sets of supporting evidence.<sup>12</sup> First, using RavenPack media data, we show that SP500 firms are more likely to make press releases about and donations for COVID-19, and do so earlier than do other firms. Second, using the textual analysis from Hassan et al. (2020), we show that these firms are more likely to discuss COVID-19 during their earning conference calls.

#### 4.1.1 First COVID-19 Response

In this section, we study whether firms with information exposure are more likely to provide press releases about COVID-19, and whether they do so earlier than other firms. If early information

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<sup>12</sup>We use SP500 firms as a sub-sample because they are more likely to be multinational companies with a broad social impact, thus increasing our sample power.

exposure allows these firms to better prepare for COVID-19, these firms are also more likely to have resources available for donation.

We use RavenPack’s COVID-19 News Monitoring Dataset to identify relevant press releases in February and March 2020, and use RavenPack’s topic classifications to identify articles about COVID-19 donations. Table 1 Panel A provides descriptive statistics for this information exposure validation analysis. Four of the SP 500 firms do not have measures for earnings conference call data, and one is missing financial information. Of the remaining 495 firms, 309 have information exposure from China or Italy; the average firm has 1.236 sources of exposure. With the RavenPack data, 63% of firms issued press releases related to COVID-19, and 16% issued press releases about COVID-19 donations.

Figure 2 plots the cumulative percent of SP500 firms issuing COVID-19 press releases and donation announcements over time. The figure splits observations by information exposure, where 309 firms are exposed from either China or Italy. Plot (a) shows that firms with information exposure start providing press releases in early February, while most firms without information exposure do not start releasing information until late March. Plot (b) shows that donation announcements start around mid-March, with a majority coming from the firms with information exposure. This figure shows that firms with higher information exposure are more likely to issue press releases about COVID-19 and to donate to COVID-19 relief, and these firms do so earlier than other firms.

Table 2 Panel A shows the results for an OLS regression using the measure by the total number of information exposure sources (*Information Exposure* in Columns (1) and (2)) and information sources coming from executive networks (*BoardEx Exposure* in Columns (2) and (4)). The outcomes are binary variables for whether or not a firm makes COVID-19 press releases and donation announcements. We control for size, leverage, and revenue. Larger firms are more likely to issue press releases and to do so earlier in the crisis. Higher leverage increases the incentives for disclosure. Revenue is related to resources available for donation.

In Column (1), the coefficient on *Information Exposure* is positive and statistically significant, and shows that one additional source of *Information Exposure* is associated with a 4% increase in the likelihood of issuing COVID-19 press releases. In Column (2), the positive and significant coefficient

on *Information Exposure* shows that firms with a higher *Information Exposure* are 3% more likely to provide donation announcements about COVID-19. These results provide suggestive evidence that firms with access to information can better prepare for COVID-19. Columns (3) and (4) show that the results for the COVID-19 press releases and donation announcements are similar to the firms with information sources coming from executive networks (*BoardEx Exposure*) with which these firms do not have branches or supply chains in China. Importantly, the results in Columns (3) and (4) mitigate the concern that our results may be driven by firms being more affected by COVID-19. Collectively, these results support our assumption that firms with business or personnel relationships with China and Italy have timely and relevant information about COVID-19.

#### 4.1.2 COVID-19 Discussions in Earnings Conference Calls

In this section, we study whether firms with information exposure are more likely to discuss COVID-19 in earnings conference calls. We use the text-based measures of COVID-19 exposure and sentiment created by Hassan et al. (2020). Using these measures, Hassan et al. (2020) find that firms previously exposed to SARS or H1N1 have higher COVID-19 exposure scores and more positive COVID-19 sentiment scores. Relatedly, we study whether firms with exposure to China and Italy have more relevant information about COVID-19 by examining their latest earnings conference call transcripts before 1 May 2020.

We use the measures of exposure and sentiment in Hassan et al. (2020) as outcome variables.<sup>13</sup> First, *COVID Exposure* captures the number of COVID-19 related words, scaled by the total number of bigrams in the transcripts. In Table 1 Panel A, *COVID Exposure* has an average value of 1.08 for the SP500 firms with data availability. Second, *COVID Negative Sentiment* and *COVID Positive Sentiment* capture the number of COVID-19 related words in close proximity (within 10 words) of negative or positive sentiments, respectively, scaled by the total number of bigrams in the transcripts. On average, *COVID Negative Sentiment* is 0.40 and *COVID Positive Sentiment* is 1.19. We also control for risk-related discussions in the transcripts, as some firms may have higher general risk. More specifically, we use *Overall Risk*, which is the total number of risk synonyms, and also the topic-related risk measures that capture risk synonyms around the topics of trade, health, and

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<sup>13</sup>We independently collect earnings conference call transcripts and conduct the textual analysis on firms' COVID-19 responses.

economics. All our risk measures are scaled by the number of total bigrams in the transcripts. Because of the frequency of risk-related words, we divide risk-related measures by 1,000 for better interpretation of the COVID-19 outcome variables.

Table 2 Panel B presents the results on COVID-19 discussions in earnings conference calls. The coefficient on *Information Exposure* is positive and significant in Column (1) after we add our risk controls, suggesting that firms' general risk exposure does not explain our results. The coefficient in Column (1) translates to a 15% (0.158/1.077) increase in *COVID Exposure* for each additional source of *Information Exposure*. Using the information exposure only coming from executive networks, the coefficient on *BoardEx Exposure* in Column (4) is also positive and significant after controlling for general risks. This result helps validate our measure of information exposure, and suggests that firms with higher information exposure have more information about COVID-19.

Columns (2) to (3) show the results for changes in COVID-19 sentiment. When firms have more business networks in China or Italy, the ex ante prediction for the direction of sentiment is unclear, as these firms may be negatively affected by COVID-19 abroad, but may be more optimistic if early information prepares them for COVID-19 in the US. However, when firms have only executive networks with China or Italy but do not have supply chains or branches in China or Italy, we should expect significant results for positive sentiment but less significant results for negative sentiment as these firms' operations are less affected by COVID-19. We find evidence supporting these predictions. Column (2) shows that firms with more information exposure have a higher *COVID Negative Sentiment*, consistent with these firms facing higher COVID-19 risks. The coefficient in Column (2) translates to a 15% (0.061/0.404) increase in *COVID Negative Sentiment* for each additional source of *Information Exposure*. Nevertheless, we do not find statistically significant result for the negative sentiment when firms' information network comes solely from their executive networks (*BoardEx Exposure*). Furthermore, Columns (3) and (6) show that firms with more information exposure also have higher *COVID Positive Sentiment*, suggesting that these firms are more resilient and better prepared for COVID-19. The coefficient in Column (3) translates to a 29% (0.054/0.188) increase in *COVID Positive Sentiment* for each additional source of *Information Exposure*. Our results remain similar after including risk control variables throughout the table, which continues to suggest that

firms' general risk exposure cannot explain our results.

## 4.2 The Effect of Information Exposure on Work-From-Home

In this section, we start by testing whether the regions with more information exposed firms contribute to earlier work-from-home practices. Next, we conduct a robustness analysis. Finally, we study cross sectional variation by examining regions that vary by industry characteristics, regulators' information constraints, and trust in information.

### 4.2.1 Main Results

In the main analysis, we study whether firms with better information exposure contribute to earlier work-from-home practices. Since we want to capture corporate impact, we focus on the period before local governments implement lockdown or shelter-in-place policies. We then test if zip codes with more information exposed firms have a higher stay-at-home ratio. Figure 3 Panel A plots the average stay-at-home ratio of zip codes with and without information exposure from China or Italy for the period before the local lockdown policy date. For days -40 to -20, the stay-at-home ratio is around 23%, and is similar for both zip code samples. Starting around -20 days, stay-at-home ratios gradually start increasing, with a larger jump for the zip codes with information exposure. By the time that lockdown policies are implemented, zip codes with information exposure have a stay-at-home ratio of 36%, while zip codes without information exposure have a stay-at-home ratio of 33%.

Given the descriptive findings in Figure 3 Panel A, our empirical specification focuses on changes in stay-at-home ratios starting 20 days before a lockdown policy. Specifically, we estimate the following specification:

$$\begin{aligned}
 \textit{Stay-at-Home Ratio}_{i,t} = & \beta_0 + \beta_1 \textit{Pre Lockdown 20 Days}_{i,t} X \textit{Information Exposure Scaled}_i \\
 & + \beta_2 \textit{Pre Lockdown 20 Days}_{i,t} + \beta_3 \textit{Information Exposure Scaled}_i \\
 & + \textit{Pre Lockdown 20 Days}_{i,t} X \textit{Controls} + \textit{Controls} + \sum \beta_j \textit{Fixed effects} + \epsilon \quad (1)
 \end{aligned}$$

The dependent variable is the stay-at-home ratio, calculated as the number of devices at home in a day divided by the number of devices in the zip code. We keep observations before

local lockdown policies are implemented. *Pre Lockdown 20 Days* takes the value of 1 for the 20 days before a local COVID-19 policy is implemented. *Information Exposure Scaled* is the total number of trade, executive, or branch networks from China or Italy, scaled by the number of firms in a zip code.  $\beta_1$  is our variable of interest, capturing how the stay-at-home ratio changes 20 days before a local policy intervention in regions with more information exposure, relative to the baseline stay-at-home ratio before the 20-day window. We account for firms' normal WFH arrangements by including long periods before the local policy implementation, with daily data starting on 1 January 2020.

Our controls are population, Asian percentage, and industry teleworkability. *Population* controls for the size of a zip code. *Asian Percentage* controls for the variation in work-from-home practice that is driven by the higher awareness of COVID-19 among Asian residents. We obtain data on *Population* and *Asian Percentage* from the 2010 Census. *Industry Teleworkability* controls for variation in the ease of implementing work-from-home policies. We use the teleworkability measurement created in Dingel and Neiman (2020) at the 3-digit NAICS industry level to identify regions that have more firms with work-from-home capability. Dingel and Neiman (2020) create the teleworkability measurement based on responses about the nature of work from a survey administered by O\*NET, an organization sponsored by the US Department of Labor. The teleworkability measure captures the percentage of people who are able to work from home. For each zip code, we create a teleworkability measure that is based on the industry-level teleworkability measure and that is weighted by the number of an industry's establishments in a given zip code.

Our main specification includes zip code and county-by-day fixed effects to account for time-invariant zip code characteristics and time-varying county characteristics, respectively. While zip-code fixed effects subsume time-invariant control variables, we include the interaction of control variables with *Pre Lockdown 20 Days* to account for differential changes in the stay-at-home ratio 20 days before policy intervention. By including county-day fixed effects, we are able to compare zip codes within the same county; each county should be similarly impacted by COVID-19. In specifications with state-day fixed effects, we include county-level COVID cases and deaths as well as the social capital score from the Social Capital Project from the U.S. Joint Economic Committee

(Barrios et al., 2021) to control for county-level variations that affect the stay-at-home ratio. Our *Social Capital* index is the overall index from the Social Capital Project.<sup>14</sup> The average *Social Capital* index is -0.250. Standard errors are clustered at the state level to address potential correlations across different counties within a state.

Table 1 Panel B provides descriptive statistics for the analysis on stay-at-home ratios. 42% of zip codes (with a COVID-19 government policy intervention) have firms with information exposure; the average *Information Exposure Scaled* is 0.226. The stay-at-home ratio is around 23%, meaning that on average, 23% of devices in a zip code stay at home on a given day. On average, a zip code has 12,409 people and an Asian percentage of 3%. There is an average of 774 firms in a zip code, with an industry-weighted teleworkability measure of 0.336.

Table 3 Panel A presents the main results with different fixed effects structures. In all specifications, the coefficient of interest on *Pre Lockdown 20 Days*  $\times$  *Information Exposure Scaled* is positive and statistically significant, supporting our hypothesis that knowledgeable firms play a role in increasing work-from-home practices before local mandates. In our main specification in Column (2), the coefficient on the interaction term suggests that on average, a one unit increase in *Information Exposure Scaled* leads to a 1.5% (0.348/23.247) increase in the stay-at-home ratio. Multiplying the coefficient by the average population of 12,409 people in a zip code, we get 43 new people working from home. In order to give a more intuitive interpretation, in Appendix Table A4, we replace *Information Exposure Scaled* with a binary variable that equals 1 for zip codes with information exposure; the coefficient translates to a 0.8% (0.195/23.247) higher stay-at-home ratio in areas with information exposure (relative to zip codes that are not exposed). This result suggests a concrete example of corporate citizenship, where firms implement WFH policies before local interventions.

Figure 3 Panel B plots the coefficients of interest over time. We replace the *Pre Lockdown 20 Days* in Table 3 Panel A Column (2) with the five-day-intervals before lockdown policies, and interact these intervals with *Information Exposure Scaled* and the control variables. We assign a

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<sup>14</sup><https://www.jec.senate.gov/public/index.cfm/republicans/2018/4/the-geography-of-social-capital-in-america>. The subcomponents of social capital include family unity, family interaction, social support, community health, institutional health, collective efficacy, and philanthropic health. This measurement is also used in Barrios et al. (2021).

coefficient of zero to time periods before 35 days, which is our benchmark period. Figure 3 Panel B shows that zip codes with higher information exposure have similar stay-at-home trends to other zip codes until around 21-25 days before lockdown policies. This figure provides visual support for the parallel trends assumption.

In Table 3 Panel B, we repeat the main analysis using the full-time work ratio as the outcome variable. Across all specifications, the coefficient of interest on *Pre Lockdown 20 Days × Information Exposure Scaled* is consistently negative and significant, supporting our main results. Using our main specification in Column (2), the coefficient on the interaction term suggests that on average, a one unit increase in *Information Exposure Scaled* leads to a 1.1% (0.090/8.338) decrease in the full-time work ratio. This magnitude is similar to our results using the stay-at-home ratio as the outcome variable. In the remainder of this paper, we use the stay-at-home ratio due to its more intuitive interpretation. All results remain similar when we use the full-time work ratio.

#### 4.2.2 Robustness Analyses

We conduct three robustness analyses to address potential endogeneity concerns. One concern is that information exposure could be correlated with omitted variables that affect stay-at-home ratios. For example, if zip codes with more information exposure have more globally-connected firms, international connections, or global media exposure, our results may be spurious. To address this concern, we do a placebo analysis using information networks from countries that are not affected by COVID-19 early in the pandemic, but that are economically important. Our placebo countries include Canada, UK, Belgium, Netherlands, Japan, and India. Appendix A3 shows that these countries experience COVID-19 much later than do China, Italy, and the US. We create a measure for *Placebo Information Exposure Scaled*, that is calculated as the total number of trade, executive, or branch networks from placebo countries and then scaled by the number of firms in a zip code. Conceptually, if our results are driven by the omitted variables described above, then the placebo information exposure should be correlated with a higher stay-at-home ratio. The lack of this relation is further evidence that our results are driven by information networks with China and Italy, which experience early COVID-19 outbreaks.

Table 4 Panel A shows the placebo analysis results. Since there are many zip codes with



information exposure from China or Italy as well as from placebo countries, we separate zip codes by the source of the information network. We then create a China-Italy Exposure Ratio, calculated as the number of information networks from China or Italy divided by the total number of information networks from China, Italy, and placebo countries. Column (1) shows the results using zip codes with a below-median China-Italy Exposure Ratio; zip codes with no information exposure are used as controls. The coefficient on *Pre Lockdown 20 Days*  $\times$  *Placebo Information Exposure Scaled* is not statistically different from zero, suggesting that zip codes with information exposure from placebo countries did not adopt higher stay-at-home ratios before local policy intervention. Column (2) shows the results from zip codes with an above-median China-Italy Exposure Ratio and from our control zip codes with no exposure; the coefficient on *Pre Lockdown 20 Days*  $\times$  *Information Exposure Scaled* is positive and statistically significant. The coefficient on the interaction term suggests that on average, a one unit increase in *Information Exposure Scaled* leads to a 1.5% (0.334/22.327) increase in the stay-at-home ratio. The early higher stay-at-home ratio only holds when the main source of information exposure is China and Italy (as opposed to placebo countries), mitigating the concern that other zip-code characteristics drive our results.

Our second robustness test addresses the concern that our main result could be driven by a higher Asian population in zip codes with more information exposure to China. To test this, we repeat the main analysis using a sub-sample with high information exposure to China but with a low Asian population. Specifically, in Table 4 Panel B, we keep zip codes in the top 25th percentile of *China Information Exposure Scaled*. In Column (1), we drop zip codes in the top 25th percentile of Asian population, and find that the coefficient on *Pre Lockdown 20 Days*  $\times$  *China Information Exposure Scaled* is positive and statistically significant. The coefficient on the interaction term suggests that on average, a one unit increase in *China Information Exposure Scaled* leads to a 3.2% (0.740/23.247) increase in stay-at-home ratio. This magnitude is comparable to the results in Column (2), where we keep the zip codes in the top 25th percentile of Asian population. This robustness test provides comfort that our results are not driven by a higher Asian population.

Our third robustness test addresses the concern that our main result picks up effects from firms that shut down operations because of the negative impact of COVID-19 in China and Italy.

We repeat the main analysis and separate our firms into treatment groups based on the source of their information exposure. Relative to firms with branch and business relationship connections, the operation of firms that have connections with executive networks in China and Italy are less likely to be directly affected by the COVID-19 outbreaks in China and Italy. Thus, we test whether our main results hold when we only keep information exposure from executive networks.

We present the results in Table 4 Panel C Columns (1) and (2), where Low (High) includes observations where less (more) than one third of the information exposure comes from executive networks. The coefficient on *Pre Lockdown 20 Days × Information Exposure Scaled* remains positive and statistically significant in both columns. More specifically, in Column (2), where there are firms with more information exposure from executive networks, we observe that a one unit increase in *Information Exposure Scaled* leads to a 1% (0.231/23.199) increase in the stay-at-home ratio. This robustness test provides comfort that our results are not driven by the operation impact from COVID-19 in China and Italy. Additionally, because our analysis includes the time when COVID-19 becomes more severe in the US, it should be less affected by the operation impact from China and Italy, which should have happened earlier in the pandemic.

We include two additional robustness tests in the Appendix. In Appendix A6, we find that our main results are stronger when information-exposure firms can better internalize the social benefit of local employees in their objective function (proxied for by a higher employee ratio in the zip code). In Appendix A5, we conduct sensitivity analyses on our sample composition. We repeat our main specification using zip codes with more than 5,000 or 10,000 people, and with more than 5 or 10 firms. The effects remain similar when we limit our sample to zip codes with a higher population or with more firms.

### 4.3 More Evidence of a Positive Social Impact

Having established that firms with information exposure implement WFH policies before local regulators impose stay-at-home orders, we now present evidence on how this corporate action can create a positive social impact that reflects a firm's corporate citizenship. We provide two sets of evidence. First, we show that in zip codes with more information exposure, the local COVID-19 case growth is lower. Second, we observe a higher stay-at-home ratio in regions without information

exposure when there are information-exposure firms nearby or in the same industry.

#### 4.3.1 Work-from-Home and COVID-19 Cases

Firms are powerful corporate citizens whose actions affect local residents; the way that firms treat their employees has an impact that goes beyond firm boundaries. During COVID-19, firms that implement WFH policies help lower the spread of COVID-19, but the benefit of the lower spread is likely larger than what is captured on a firm's income statement. More specifically, the societal benefit of WFH policy is likely to be significant due to the fact that COVID-19 is a highly contagious disease where lowering the spread in one area lowers the spread in nearby regions.

Thus, our first test of a positive social impact is whether firms with information exposure can help lower the spread of COVID-19. We repeat our main specification from Table 3 Panel A, and replace the outcome variable with COVID-19 cases. As COVID-19 case data is at the county-level, we aggregate all other data to the county-level. We use the number of cases 7 and 14 days into the future because the incubation period of COVID-19 ranges from 2 to 14 days at the 95% confidence interval, which means that any effects from containment actions are likely to be realized several days in the future (Lauer et al., 2020). As the distribution of COVID-19 cases is skewed with zero cases in the initial period (COVID-19 grows at an exponential rate), we use the log of 1 plus the number of COVID-19 cases.

Table 5 Panel A presents the results. Column (1) uses COVID-19 cases after 7 days and Column (2) uses COVID-19 cases after 14 days. In both regressions, the coefficient on *Pre Lockdown 20 Days × Information Exposure Scaled* is negative and statistically significant, which suggests that information exposure is correlated with a lower COVID-19 case growth. The magnitude in Column (1) means that a one unit increase in *Information Exposure Scaled* leads to a 0.297 lower number of COVID-19 cases in  $t+7$ , which is 41.0% ( $0.297/0.725$ ) of the mean and 2.2% ( $0.297/13.315$ ) of the standard deviation.<sup>15</sup> The finding that counties with a higher *Information Exposure* are associated with a lower spread of COVID-19 supports our hypothesis that firms with information exposure help lower the spread of COVID-19, which is a positive social impact of corporate citizenship.

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<sup>15</sup>The magnitude is high relative to the mean partially because COVID-19 cases are zero in earlier periods, which lowers the overall mean.

### 4.3.2 Nearby Firms and Peer Industries

Next, we provide evidence of a positive social impact by showing that firms with information exposure can help increase the WFH policies of other firms. Specifically, we study the influence of information-exposure firms on peers in the same industry and in nearby zip codes.

To study this positive social impact, we create two measures. First, to study the influence of peer firms, we create an industry-level information exposure measure that is weighted by the number of employees or by the sales of each firm in an industry. For each zip code, we then create a weighted average of the industry-level information exposure measure that is also weighted by the number of employees or of sales, depending on the specification. Second, to study the influence of nearby firms, we identify all zip codes within 25 miles of a zip code with information exposure, and create a nearby-information-exposure measure that is the weighted average of the zip-code-level information exposure of zip codes within 25 miles. As above, we weight by the number of employees or by the sales in each zip code. In constructing the nearby-information-exposure measure, we exclude zip codes within the same county, as these zip codes likely hold workers of the focal firm.

In Table 5 Panel B, we repeat the main regression and replace *Information Exposure Scaled* with the newly-created information exposure measure that is related to nearby or peer firms. We only keep zip codes with no information exposure, which means that the only information exposure comes from nearby or from peer firms. In all specifications, the coefficient on *Pre Lockdown 20 Days*  $\times$  *Information Exposure Scaled* is positive and statistically significant. The coefficients across the four specifications are comparable; taking the first column as an example, the magnitude means that a one unit increase in *Information Exposure Scaled* leads to a 0.1% (0.032/23.192) increase in the stay-at-home ratio. This magnitude is much smaller than the magnitude of our main result in Table 3 Panel A Column (2), where there is a 1.5% increase in the stay-at-home ratio. The smaller magnitude is reasonable given that we are only capturing the influence from related firms in zip codes without information exposure.

## 4.4 Motives

While we are agnostic about firms' motives for acting as corporate citizens, we show some suggestive evidence that these acts are not purely profit-driven. First, we show that our main effects

are more pronounced when the shareholders of firms in a region are responsible investors, which suggests that corporate citizenship is consistent with firms' preferences that go beyond shareholder-value maximization. Second, we show that our main results hold even when teleworkability is low, where firms are more likely to suffer financially from instituting WFH policies.

#### 4.4.1 Responsible Investing

To explore whether firms' WFH policies stem from social considerations, we study if our effects depend on shareholder preference. Prior literature finds that firms with more institutional investors, especially Principles for Responsible Investment (PRI) signatories and those from Europe, have better ESG scores (Dyck et al., 2019). Thus, if shareholder preference matters, we expect a larger effect when the firms in our sample are held by responsible investors. We create a measure for *Responsible Investors* using firms with PRI signatory investors and firms that use Socially Responsible Investing (SRI) funds. When we aggregate this measure at the zip-code level, we find that zip codes with responsible investors also have a higher number of firms. To enhance comparability, we only keep zip codes with over 100 firms.

Table 6 Panel A presents the results. Columns (1) and (2) contain zip codes with and without firms held by responsible investors, respectively, and show the results using the main specification in Table 3 Panel A Column (2). While the coefficient on *Pre Lockdown 20 Days × Information Exposure Scaled* is positive in both columns, it is only statistically significant in Column (1), with responsible investors. The magnitude in Column (1) means that a one unit increase in *Information Exposure Scaled* leads to a 3.2% (0.741/23.349) increase in the stay-at-home ratio. In contrast, the magnitude in Column (2), with no responsible investors, translates to a 0.7% (0.174/23.370) increase in the stay-at-home ratio.

#### 4.4.2 Less Teleworkable Industries

Next, we study if our main results hold in industries that are less teleworkable. While it is true that acting socially responsible during a crisis can be a profit-maximizing choice (Lins et al., 2017), we argue that this is less likely to be true for firms where teleworkability is low. In other words, it is more likely that profit is not the motive for WFH policies in industries where WFH policies are costly for firms.

In this specification, we use industry teleworkability as a partitioning variable and only consider industries with fewer essential businesses, as the operation of essential businesses is still necessary during the COVID-19 crisis. Since our teleworkability measure is based on 3-digit NAICS codes, which all include some essential businesses, we only consider the codes with a below-median ratio of essential businesses. We then use this sample to create a teleworkability measure for each zip code weighted by firm counts.

Table 6 Panel B presents the results. Column (1) includes observations with below-median teleworkability, where a WFH policy is likely to be more costly to firms. In this column, our main results still hold, and the coefficient on *Pre Lockdown 20 Days*  $\times$  *Information Exposure Scaled* remains positive and statistically significant. The magnitude in Column 1 means that a one unit increase in *Information Exposure Scaled* leads to a 1.8% (0.416/23.428) increase in the stay-at-home ratio. This magnitude is similar to that in zip codes with an above-median teleworkability, where a one unit increase in *Information Exposure Scaled* leads to a 1.5% (0.354/23.057) increase in the stay-at-home ratio. This finding provides suggestive evidence that firms' do have a social impact objective.

## 5 Conclusion

We explore how information exposure, particularly the information transmission within organizations, facilitates the role of companies as powerful corporate citizens who can mitigate societal problems. Specifically, we find that US firms' trade, executive, or branch-office networks with China and Italy act as information networks and that firms use relevant information to mitigate the negative impacts of COVID-19.

By providing empirical evidence, our paper also adds to the literature on corporate citizenship (Matten and Crane, 2005). More specifically, the setting of COVID-19 allows us to demonstrate that firms can act as powerful corporate citizens by using their comparative information advantage. Given the rapidly escalating severity of COVID-19, governments face constraints, and we find that firms contribute to higher stay-at-home practices before local governments impose mobility restrictions.

There are several potential caveats for our research. First, all of our results are robust to using

a full-time work ratio as the outcome variable, which mitigates the concern that the stay-at-home ratio is affected by changes in unemployment or by consumers staying at home (at least over the short period of interest). Second, Second, to study information-exposure firm's WFH policies, our observations in our main analysis are aggregated at the zip code level. However, higher WFH ratios at the zip-code level could also capture WFH policies of firms geographically close to firms with information-exposure, which incorporates the idea that information-exposure firms have positive social impacts. Third, even though we validate our information exposure measurement, our main analysis on stay-at-home ratios is a joint hypothesis, where we test firms that have information advantages and then test firms that use this advantage to enforce WFH policies.

Finally, the effect we capture is a short-run result; while corporate citizenship can temporarily compensate for the failure of local government, the two are not complete substitutes. External laws and regulations are necessary and important tools to protect stakeholders and society, and we do not intend to argue that corporate actions are perfect substitutes for governmental action. However, government protections can be complemented by corporate citizenship.

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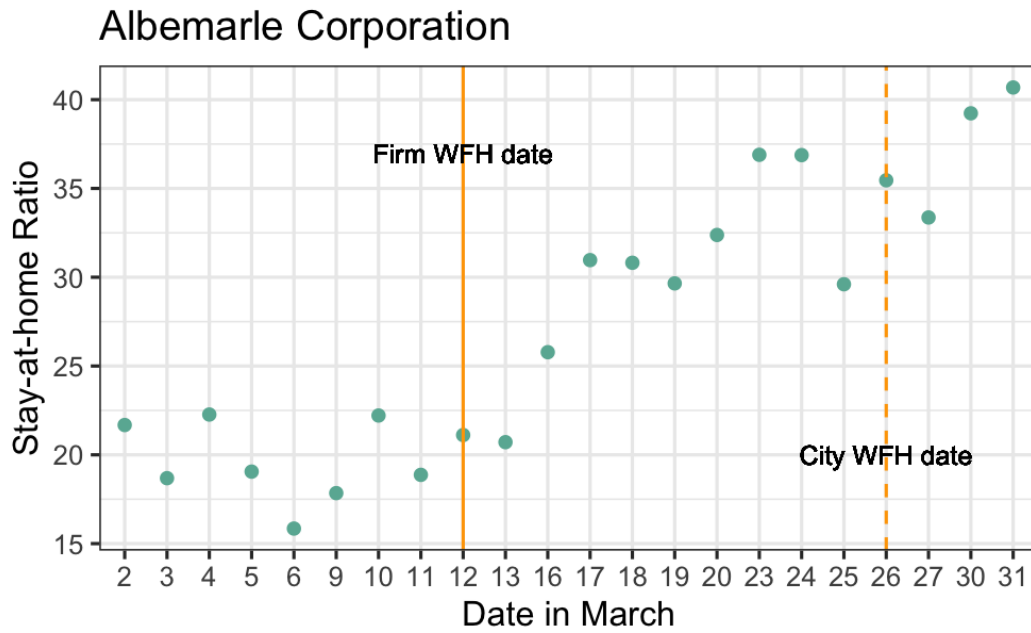
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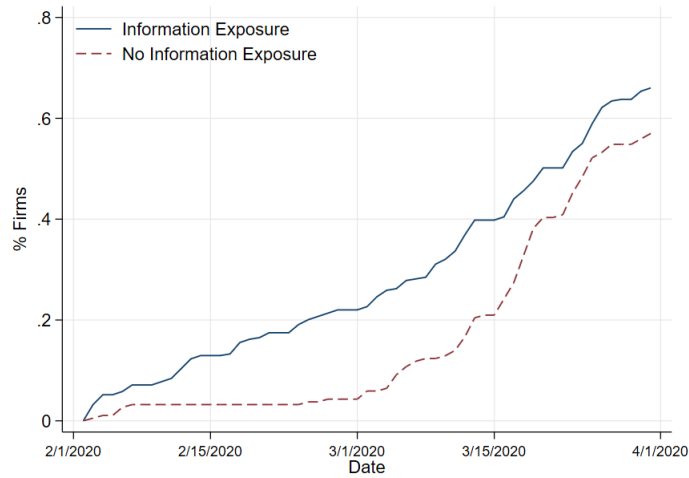
Figure 1: Case Study: Albemarle Corporation



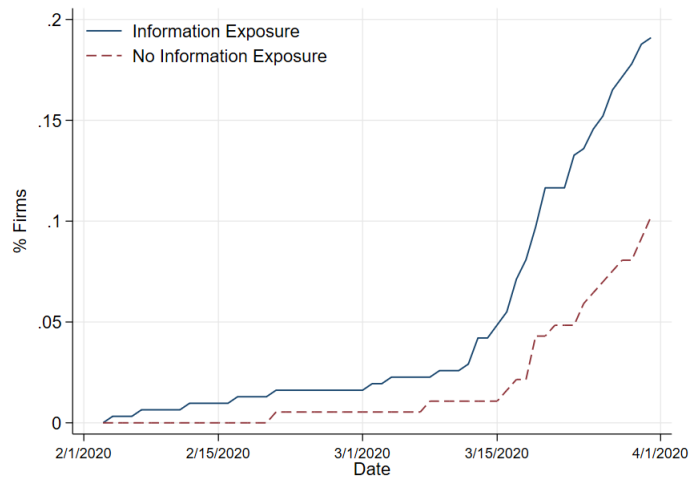
This figure uses the Albemarle Corporation as an example to explain the SafeGraph data. It illustrates the change in the stay-at-home ratio in March 2020 for the zip code where Albemarle Corporation is headquartered. The solid line on day 12 is when Albemarle Corporation announced its WFH policy, and the dashed line on day 26 is when the local government’s “Stay at Home” order went into effect.

**Figure 2: SP500 First COVID-19 Responses**

Panel A Cumulative Plot of COVID-19 Press Releases

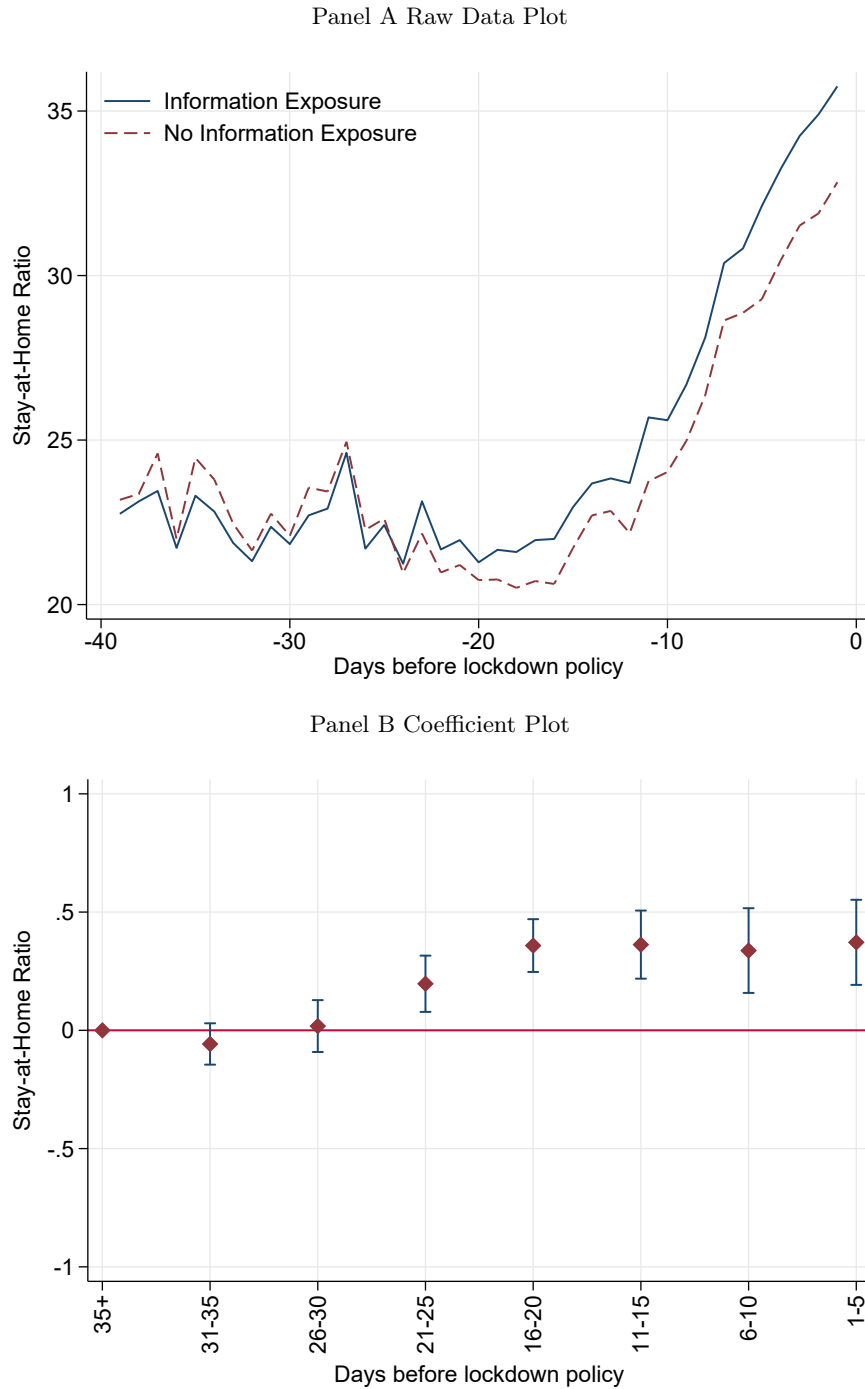


Panel B Cumulative Plot of COVID-19 Donation Announcements



This figure shows the cumulative distribution plot of COVID-19 press releases (Panel A) and donation announcements (Panel B). The data include the first press release or donation announcement by SP500 firms between 1 February 2020 and 15 April 2020. The solid line represents firms with information exposure. The dashed line represents firms without information exposure.

Figure 3: Stay-at-Home Ratio Relative to Lockdown Dates



This figure shows the stay-at-home ratio before local lockdown policies go into effect. Panel A plots the raw stay-at-home ratio for zip codes with and without information exposure. Panel B plots the coefficients and 95% confidence interval of the main regression in Table 3 Panel A Column (2) during event time. We replace *Pre Lockdown 20 Days* with the five-day-intervals before lockdown dates. We omit the indicator for periods before 35 days, which serves as our benchmark period, and which has a coefficient and standard error of zero.



**Table 1: Summary Statistics**

<b>Panel A: SP500 Analysis Summary Statistics (Firms level)</b>						
	N	Mean	Std. dev.	P25	P50	P75
Information Exposure	495	1.236	1.235	0.000	1.000	2.000
BoardEx Exposure	314	0.468	0.609	0.000	0.000	1.000
Press Release	495	0.628	0.484	0.000	1.000	1.000
Donation	495	0.162	0.368	0.000	0.000	0.000
COVID Exposure	495	1.077	1.255	0.090	0.649	1.603
COVID Negative Sentiment	495	0.404	0.596	0.000	0.161	0.601
COVID Positive Sentiment	495	0.188	0.313	0.000	0.000	0.274
Overall Risk	495	0.099	0.068	0.050	0.084	0.132
Trade Risk	495	5.899	18.393	0.974	2.405	5.469
Health Risk	495	6.538	17.235	1.251	3.133	6.800
Economic Risk	495	6.384	9.419	1.691	3.760	7.559
Revenue (log)	495	9.339	1.180	8.515	9.240	10.013
Total Assets (log)	495	10.130	1.298	9.200	9.956	10.913
Leverage	495	0.664	0.227	0.513	0.665	0.799

<b>Panel B: SafeGraph Analysis Summary Statistics (Zipcode-day Level)</b>						
	N	Mean	Std. dev.	P25	P50	P75
Stay-at-Home Ratio	1,053,424	23.247	6.570	18.655	22.316	26.799
Full-Time-Work Ratio	1,053,424	8.338	3.667	5.521	7.965	10.695
Information Exposure Scaled	1,042,901	0.226	0.368	0.000	0.000	0.372
Information Exposure Binary	1,053,424	0.416	0.493	0.000	0.000	1.000
Information Exposure Firms Scaled	1,043,108	0.104	0.169	0.000	0.000	0.170
Placebo Information Exposure Scaled	1,043,154	0.827	1.196	0.000	0.129	1.349
China Information Exposure Scaled	1,042,652	0.139	0.228	0.000	0.000	0.229
Italy Information Exposure Scaled	1,042,694	0.084	0.152	0.000	0.000	0.125
Asian Percentage	1,053,424	2.847	5.981	0.300	0.800	2.600
Population	1,053,424	12,409	13,433	2,864	7,261	17,711
Firms	1,053,424	774	928	135	404	1,102
Industry Teleworkability	1,053,424	0.336	0.092	0.292	0.331	0.377
COVID Deaths	1,035,108	0.020	0.273	0.000	0.000	0.000
COVID Cases	1,035,108	1.220	13.006	0.000	0.000	0.000
Social Capital	1,053,424	-0.265	0.931	-0.949	-0.232	0.398
BoardEx Exposure Ratio	438,658	0.279	0.224	0.000	0.300	0.444
Placebo Exposure Ratio	546,425	0.810	0.125	0.738	0.794	0.875
Responsible Investors Binary	1,053,424	0.455	0.498	0.000	0.000	1.000
Democratic Mayor	186,619	0.618	0.486	0.000	1.000	1.000
Republican Mayor	186,619	0.167	0.373	0.000	0.000	0.000
Democratic Counties	1,049,927	0.440	0.496	0.000	0.000	1.000
Republican Counties	1,049,927	0.560	0.496	0.000	1.000	1.000

Continued on following page

**Table 1 (continued)**

<b>Panel C: COVID-19 Analysis Summary Statistics (County-day Level)</b>						
	N	Mean	Std. dev.	P25	P50	P75
COVID-19 Cases t+7	164,070	0.725	13.315	0.000	0.000	0.000
COVID-19 Cases t+14	164,070	1.621	24.409	0.000	0.000	0.000
Information Exposure Scaled TR	162,433	0.273	0.262	0.000	0.264	0.446
Population	164,070	80124	286174	6877	17296	47093
Asian Percentage	164,070	1.243	2.718	0.270	0.500	1.139
Industry Teleworkability	162,659	0.332	0.053	0.310	0.329	0.351
Social Capital	164,070	-0.065	0.951	-0.667	-0.007	0.534

This table presents descriptive statistics for variables used in the analysis. Panel A provides descriptive statistics for SP500 firms with variables used in the information exposure validation analysis. Panel B provides descriptive statistics for zip-code-day-level data used in the SafeGraph analysis. Panel C provides descriptive statistics for county-day-level data used in the COVID-19 cases analysis. See Appendix A1 for the variable definitions.

**Table 2: Validation of Information Exposure Measure**

<i>Panel A: SP500 COVID-19 Press Release and Donation</i>				
	(1)	(2)	(3)	(4)
	All information Exposure		BoardEx Exposure	
	Press Release	Donation	Press Release	Donation
Information Exposure	0.041** (2.26)	0.029** (2.17)		
BoardEx Exposure			0.086* (1.89)	0.063* (1.87)
Revenue (log)	0.076*** (2.78)	0.065*** (3.18)	0.057* (1.81)	0.046** (1.98)
Total Assets (log)	0.027 (1.11)	0.039** (2.17)	0.047 (1.57)	0.059*** (2.67)
Leverage	0.080 (0.85)	-0.004 (-0.05)	0.036 (0.25)	-0.049 (-0.46)
N	495	495	314	314
Adj. R-squared	0.083	0.123	0.083	0.128

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**Table 2 (continued)**

<i>Panel B: SP500 COVID-19 Earnings Conference Call</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	All information Exposure			BoardEx Exposure		
	COVID Exposure	Negative Senti-ment	Positive Senti-ment	COVID Exposure	Negative Senti-ment	Positive Senti-ment
Information Exposure	0.158*** (3.46)	0.061*** (2.79)	0.054*** (4.66)			
BoardEx Exposure				0.214* (1.96)	0.066 (1.43)	0.078*** (2.71)
Revenue (log)	0.171** (2.44)	0.042 (1.26)	0.024 (1.33)	0.235*** (3.09)	0.047 (1.46)	0.037* (1.83)
Total Assets (log)	-0.213*** (-3.39)	-0.069** (-2.30)	-0.041** (-2.53)	-0.135* (-1.80)	-0.023 (-0.72)	-0.029 (-1.45)
Leverage	0.120 (0.50)	-0.057 (-0.49)	0.043 (0.70)	-0.414 (-1.17)	-0.207 (-1.38)	-0.080 (-0.86)
Overall Risk	5.596*** (6.13)	2.703*** (6.20)	0.941*** (4.04)	5.320*** (5.32)	2.655*** (6.31)	0.688*** (2.61)
Trade Risk	-0.004 (-1.15)	-0.002 (-1.22)	-0.001 (-1.36)	-0.018* (-1.88)	-0.006 (-1.55)	-0.003 (-1.38)
Health Risk	0.006 (1.63)	0.001 (0.64)	0.001 (1.48)	0.008* (1.66)	0.001 (0.70)	0.001 (1.09)
Economic Risk	0.000 (0.05)	0.004 (0.98)	-0.000 (-0.04)	0.004 (0.45)	0.004 (0.98)	0.001 (0.57)
N	495	495	495	314	314	314
Adj. R-squared	0.125	0.114	0.085	0.139	0.152	0.051

This table reports results from our validation of the information exposure measure. Panel A reports the results of regressing RavenPack press release measures on *Information Exposure* and *BoardEx Exposure*. *Press Release* and *Donation* are indicators for issuing a press release or donation announcement related to COVID-19 in February and March 2020. Panel B reports the results of regressing COVID-19 disclosures in the earnings conference calls on *Information Exposure* and *BoardEx Exposure*. The measures from earnings conference calls are taken from Hassan et al. (2020); we use data for the latest earnings conference call before 1 May 2020. We provide a detailed description of the variables in Appendix A1. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 3: Change in Stay-at-Home Ratio before Policy Intervention**

<i>Panel A: Stay-at-Home Ratio</i>				
	(1)	(2)	(3)	(4)
	Stay-at-Home Ratio			
Pre Lockdown 20 Days × Information Exposure Scaled	0.285** (2.36)	0.348*** (5.36)	0.262*** (3.09)	0.256*** (3.10)
Pre Lockdown 20 Days × Asian Percentage	0.126*** (3.75)	0.069*** (3.35)	0.105** (2.56)	0.093** (2.43)
Pre Lockdown 20 Days × Population (log)	1.599*** (13.83)	0.794*** (16.33)	1.414*** (15.16)	1.468*** (16.61)
Pre Lockdown 20 Days × Industry Teleworkability	5.291*** (6.35)	4.524*** (6.83)	7.327*** (7.95)	6.325*** (7.69)
Pre Lockdown 20 Days × Social Capital				0.810*** (7.07)
Pre Lockdown 20 Days × COVID Deaths				0.396** (2.24)
COVID Deaths				-0.079 (-0.47)
Pre Lockdown 20 Days × COVID Cases				-0.021 (-0.27)
COVID Cases				0.055 (0.74)
N	687874	1012190	1042901	1024769
Adj. R-squared	0.751	0.785	0.729	0.734
Zip-code FE	Yes	Yes	Yes	Yes
City-Day FE	Yes	No	No	No
County-Day FE	No	Yes	No	No
State-Day FE	No	No	Yes	Yes
Clusters	State	State	State	State

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**Table 3 (continued)**

<i>Panel B: Full-Time-Work Ratio</i>				
	(1)	(2)	(3)	(4)
	Full-Time-Work Ratio			
Pre Lockdown 20 Days × Information Exposure Scaled	-0.078* (-1.87)	-0.090*** (-3.45)	-0.100*** (-2.91)	-0.092*** (-2.70)
Pre Lockdown 20 Days × Asian Percentage	-0.033** (-2.67)	-0.014 (-1.40)	-0.023 (-1.63)	-0.021 (-1.50)
Pre Lockdown 20 Days × Population (log)	-0.235*** (-5.96)	0.019 (0.84)	-0.106*** (-3.86)	-0.138*** (-4.85)
Pre Lockdown 20 Days × Industry Teleworkability	-1.663*** (-6.89)	-1.019*** (-6.25)	-1.793*** (-7.91)	-1.534*** (-7.35)
Pre Lockdown 20 Days × Social Capital				-0.315*** (-7.89)
Pre Lockdown 20 Days × COVID Deaths				-0.263*** (-3.07)
COVID Deaths				0.228** (2.55)
Pre Lockdown 20 Days × COVID Cases				0.035 (0.89)
COVID Cases				-0.042 (-1.06)
N	687874	1012190	1042901	1024769
Adj. R-squared	0.739	0.796	0.743	0.745
Zip-code FE	Yes	Yes	Yes	Yes
City-Day FE	Yes	No	No	No
County-Day FE	No	Yes	No	No
State-Day FE	No	No	Yes	Yes
Clusters	State	State	State	State

This table reports the results from our analysis of the effect of information exposure on the stay-at-home ratio and full-time-work ratio 20 days before local policy intervention. Panel A shows the results using stay-at-home ratio as the outcome variable. Panel B shows the results using full-time-work ratio as the outcome variable. We provide a detailed description of the variables in Appendix A1. Columns (1) and (2) include city-day and county-day fixed effects, respectively. Columns (3) and (4) include state-day fixed effects. All columns include zip-code fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t-statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 4: Robustness Analysis**

<i>Panel A: Placebo</i>	Stay-at-Home Ratio as dependent variable	
	(1)	(2)
	Information Exposure Countries	
	Placebo	China or Italy
Pre Lockdown 20 Days × Placebo Information Exposure Scaled	0.027 (0.83)	
Pre Lockdown 20 Days × Information Exposure Scaled		0.334*** (4.68)
Pre Lockdown 20 Days × Asian Percentage	0.071*** (3.35)	0.076*** (3.37)
Pre Lockdown 20 Days × Population (log)	0.860*** (12.88)	0.775*** (13.83)
Pre Lockdown 20 Days × Industry Teleworkability	3.433*** (5.52)	3.419*** (6.06)
N	734864	740287
Adj. R-squared	0.760	0.764
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

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**Table 4 (continued)**

<i>Panel B: Asian Population</i>		
	Stay-at-Home Ratio as dependent variable	
	(1)	(2)
	Asian Population	
	Low	High
Pre Lockdown 20 Days × China Information Exposure Scaled	0.740*** (4.33)	0.838* (1.91)
Pre Lockdown 20 Days × Asian Percentage	0.673*** (5.82)	0.030*** (3.34)
Pre Lockdown 20 Days × Population (log)	0.698*** (6.40)	1.110*** (5.13)
Pre Lockdown 20 Days × Industry Teleworkability	8.772*** (5.22)	13.571*** (6.89)
N	181519	74179
Adj. R-squared	0.869	0.884
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

Continued on following page



**Table 4 (continued)**

<i>Panel C: BoardEx Ratio</i>		
	Stay-at-Home Ratio as dependent variable	
	(1)	(2)
	BoardEx Ratio	
	Low	High
Pre Lockdown 20 Days × Information Exposure Scaled	0.405*** (3.40)	0.231*** (3.30)
Pre Lockdown 20 Days × Asian Percentage	0.065*** (3.68)	0.075*** (3.08)
Pre Lockdown 20 Days × Population (log)	0.840*** (14.41)	0.817*** (13.24)
Pre Lockdown 20 Days × Industry Teleworkability	3.956*** (5.96)	3.710*** (6.20)
N	804471	785823
Adj. R-squared	0.768	0.763
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

This table shows the robustness tests on our analysis of the effect of information exposure on the stay-at-home ratio 20 days before local policy intervention. Panel A shows the results of the placebo analysis, where the placebo information exposure comes from Canada, UK, Belgium, Netherlands, Japan, and India. *China-Italy Exposure Ratio* is the percentage of total information exposure, including those of the placebo countries, in a zip code that comes from China and Italy. Columns (1) and (2) include observations with less than or equal to, and more than the median *China-Italy Exposure Ratio*, respectively. Panel B shows the results using the main specification in Table 3 Panel A Column (2), and partitions samples by the percentage of Asian population in a zip code. We focus on information exposure to China, and keep zip codes with above 25th percentile of *China Information Exposure Scaled*. Columns (1) and (2) include observations with less than or equal to, and more than the 75th percentile of Asian population, respectively. Panel C shows the results using the main specification in Table 3 Panel A Column (2), and partitions samples by the source of information exposure. Columns (1) and (2) include observations with less (more) than one third of information exposure source from BoardEx. We provide a detailed description of the variables in Appendix A1. All regressions include zip-code and county-day fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 5: Information Exposure and Positive Social Impact**

<i>Panel A: Information Exposure and COVID-19 Cases</i>		
	Log (1+ COVID-19 Cases) as dependent variable	
	(1)	(2)
	7-Day	14-Day
Pre Lockdown 20 Days × Information Exposure Scaled	-0.266*** (-7.35)	-0.351*** (-8.98)
Pre Lockdown 20 Days × Social Capital	0.032 (1.12)	0.003 (0.08)
Pre Lockdown 20 Days × Asian Percentage	0.034** (2.67)	0.041** (2.53)
Pre Lockdown 20 Days × Population (log)	0.283*** (9.02)	0.391*** (12.73)
Pre Lockdown 20 Days × Industry Teleworkability	1.361*** (6.95)	1.533*** (6.75)
N	160837	160728
Adj. R-squared	0.612	0.683
County FE	Yes	Yes
State-Day FE	Yes	Yes
Clusters	State	State

**Table 5 (continued)**

<i>Panel B: Peer Industry and Nearby Zipcodes</i>				
	Stay-at-Home Ratio as dependent variable			
	(1)	(2)	(3)	(4)
	Peer Industry Exposure		Nearby Zipcode Exposure	
	Employees	Sales	Employees	Sales
Pre Lockdown 20 Days × Information Exposure Scaled	0.032** (2.12)	0.028*** (3.40)	0.041** (2.29)	0.031*** (3.32)
Pre Lockdown 20 Days × Asian Percentage	0.074*** (3.21)	0.072*** (3.22)	0.069*** (3.05)	0.069*** (3.20)
Pre Lockdown 20 Days × Population (log)	0.880*** (11.04)	0.882*** (11.44)	0.896*** (11.52)	0.890*** (11.47)
Pre Lockdown 20 Days × Industry Teleworkability	3.167*** (4.83)	3.117*** (4.93)	2.892*** (4.61)	2.898*** (4.49)
N	568271	566829	570142	569693
Adj. R-squared	0.736	0.735	0.733	0.733
Zip-code FE	Yes	Yes	Yes	Yes
County-Day FE	Yes	Yes	Yes	Yes
Clusters	State	State	State	State

This table reports the results on the positive social impact created by firms with information exposure. Panel A shows our analysis of the effect of information exposure on COVID-19 cases at a county-day level. The outcome variable is log of 1 plus COVID-19 cases 7 days and 14 days into the future. All regressions include county and state-day fixed effects. Panel B shows the influence of firms with information exposure on nearby or peer firms. We repeat the main specification in Table 3 Panel A Column (2), but only include zip codes with no information exposure, and re-define information exposure as those from peer firms in Columns (1) and (2), and from nearby firms in Columns (3) and (4). The related firm's information exposure are weighted by total employees in Columns (1) and (3), and by sales in Columns (2) and (4). We provide a detailed description of the variables in Appendix A1. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t-statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 6: Firm Motives**

<i>Panel A: Responsible Investors</i>		
	Stay-at-Home Ratio as dependent variable	
	(1)	(2)
	Responsible Investors	
	Yes	No
Pre Lockdown 20 Days × Information Exposure Scaled	0.741*** (6.42)	0.174 (0.53)
Pre Lockdown 20 Days × Asian Percentage	0.051*** (3.01)	0.064*** (3.61)
Pre Lockdown 20 Days × Population (log)	0.882*** (11.83)	1.062*** (7.89)
Pre Lockdown 20 Days × Industry Teleworkability	12.709*** (9.86)	7.075*** (5.13)
N	414515	327566
Adj. R-squared	0.865	0.771
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

Continued on following page

**Table 6 (continued)**

<i>Panel B: Teleworkability</i>		
	Stay-at-Home Ratio as dependent variable	
	(1)	(2)
	Teleworkability	
	Low	High
Pre Lockdown 20 Days × Information Exposure Scaled	0.416*** (3.33)	0.354*** (3.17)
Pre Lockdown 20 Days × Asian Percentage	0.092*** (5.04)	0.056*** (2.77)
Pre Lockdown 20 Days × Population (log)	0.832*** (12.27)	0.822*** (11.98)
Pre Lockdown 20 Days × Industry Teleworkability	2.408*** (3.44)	6.679*** (5.16)
N	472838	480760
Adj. R-squared	0.766	0.806
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

This table shows our results relating to firm’s motives. Panel A shows the results using the main specification in Table 3 Panel A Column (2), and partition samples by the presence of responsible investors. Columns (1) and (2) contain zip codes with and without firms held by responsible investors, respectively. Panel B shows the results using the main specification in Table 3 Panel A Column (2), and partition samples by teleworkability. Columns (1) and (2) contain zip codes with below and above median teleworkability, respectively. We provide a detailed description of the variables in Appendix A1. All regressions include zip-code and county-day fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

## Appendix

- A1. Variable Definitions
- A2: SafeGraph Zip-Code Level Data (Out of Sample)
- A3: Cumulative Confirmed Cases for All Sample Countries
- A4. Robustness Check: Binary Information Exposure
- A5. Robustness Check: Information Exposure Firms
- A6. Information Exposure Weighted by Employees or Sales
- A7. Robustness Check: Sample Sensitivity
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- A10. Robustness Check: Heat Map by States
- A11. Change in Device-at-Home Days before Policy Intervention: New York, California, and Texas
- A12 - A13. Robustness Check: Firm Level Evidence

**A1: Variable Definition**

Variable	Description	Data Source
Information Exposure	The total number of trade, executive, or branch networks from China or Italy	
BoardEx Exposure	The total number of executive networks from China or Italy	
Press Release	An indicator variable equal to 1 if the firm issued a press release related to COVID-19 in February or March 2020	RavenPack
Press Release Date	The earliest date a firm issued a press release related to COVID-19 in February or March 2020. Coded as 15 May 2020 if there is no relevant press release	RavenPack
Donation	An indicator variable equal to 1 if the firm issued a press release about COVID-19 donation in February or March 2020	RavenPack
Donation Date	The earliest date a firm issued a press release about COVID-19 donation in February or March 2020. Coded as 15 May 2020 if there is no relevant press release on donation	RavenPack
COVID Exposure	COVID mentions scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2020)
COVID Negative Sentiment	COVID mentions around words with negative sentiment scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2020)
COVID Positive Sentiment	COVID mentions around words with positive sentiment scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2020)
Overall Risk	Risk synonyms scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2019)
Trade Risk	Trade topic keywords around risk synonyms scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2019)
Health Risk	Health topic keywords around risk synonyms scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2019)
Economic Risk	Economic topic keywords around risk synonyms scaled by the total number of bigrams in the latest earnings conference call transcript released before 1 May 2020	Hassan et al. (2019)
Stay-at-Home Ratio	The ratio of devices staying at home divided by the total devices in a region	SafeGraph
Full-Time-Work Ratio	The ratio of devices that went to full-time work divided by the total devices in a region	SafeGraph

Continued on following page

Table A.1, continued

Variable	Description	Data Source
Information Exposure Scaled	The total number of trade, executive, or branch networks from China or Italy, scaled by the number of firms in the zip code	
Information Exposure Binary	An indicator variable equal to 1 if any firms in the region has trade, executive, or branch networks from China or Italy	
Information Exposure Firms Scaled	The total number of firms with any information exposure from China or Italy, scaled by the number of firms in the zip code	
Placebo Information Exposure Scaled	The total number of trade, executive, or branch networks from Canada, UK, Belgium, Netherlands, Japan or India, scaled by the number of firms in the zip code	
Placebo Information Exposure Binary	An indicator variable equal to 1 if any firms in the region has trade, executive, or branch networks from Canada, UK, Belgium, Netherlands, Japan or India	
China Information Exposure Scaled	The total number of trade, executive, or branch networks from China, scaled by the number of firms in the zip code	
China Information Exposure Binary	An indicator variable equal to 1 if any firms in the region has supplier, customer, or partnership networks from China	
Italy Information Exposure Scaled	The total number of trade, executive, or branch networks from Italy, scaled by the number of firms in the zip code	
Italy Information Exposure Binary	An indicator variable equal to 1 if any firms in the region has trade, executive, or branch networks from Italy	
Asian Percentage	The percentage of Asian population in a region based on Census 2010	Census 2010
Population	Population in the region based on Census 2010	Census 2010
Firms	The number of firms in a zip code	Infogroup
Industry Teleworkability	The teleworkability score weighted by industries in the region	Dingel and Neiman (2020)
COVID cases	The number of COVID cases in a region	New York Times
COVID deaths	The number of COVID deaths in a region	New York Times
Social Capital	Index from the Social Capital Project	U.S. Joint Economic Committee
Placebo Exposure Ratio	The number of information exposure networks from placebo countries divided by the total number of information exposure networks from China, Italy, or placebo countries	
Democratic Counties	An indicator variable equal to 1 if the county has minority vote for Donald Trump in the 2020 election	Townhall

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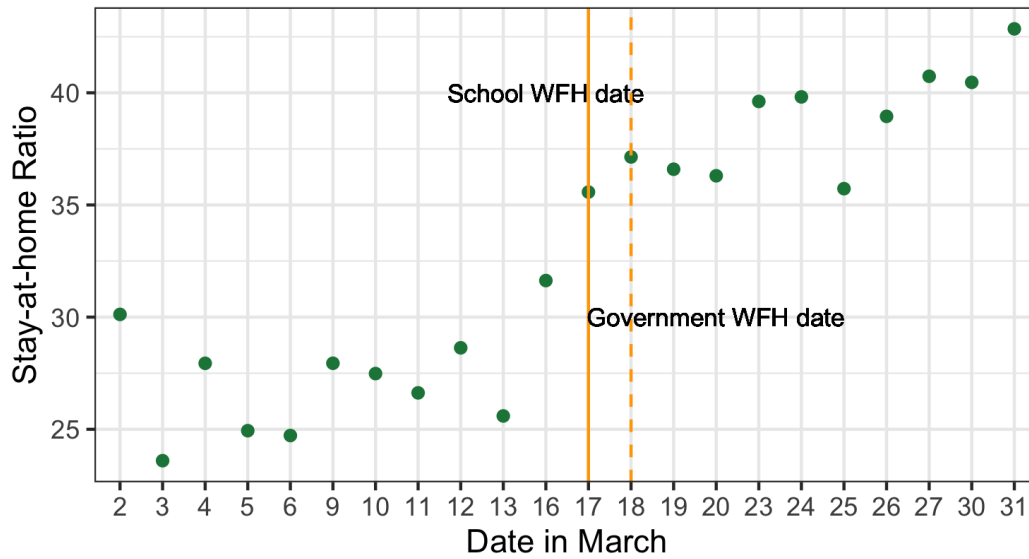
Table A.1, continued

Variable	Description	Data Source
Republican Counties	An indicator variable equal to 1 if the county has majority vote for Donald Trump in the 2020 election	Townhall
Democratic Mayors	An indicator variable equal to 1 if the mayor is affiliated with the Democratic party	Our Campaigns
Republican Mayors	An indicator variable equal to 1 if the mayor is affiliated with the Republican party	Our Campaigns
BoardEx Exposure Ratio	The number of information exposure source from executive networks divided by the total number of information exposure networks	
Essential Industry Ratio	The number of firms that belongs to an essential industry based on 6 digit NAICS divided by the total number of firms in a zip code	CDC
Responsible Investors Binary	An indicator variable equal to 1 if there are firms in the zip code with SRI or PRI investors	SRI, PRI

This table provides the descriptions and sources of variables used in this paper.

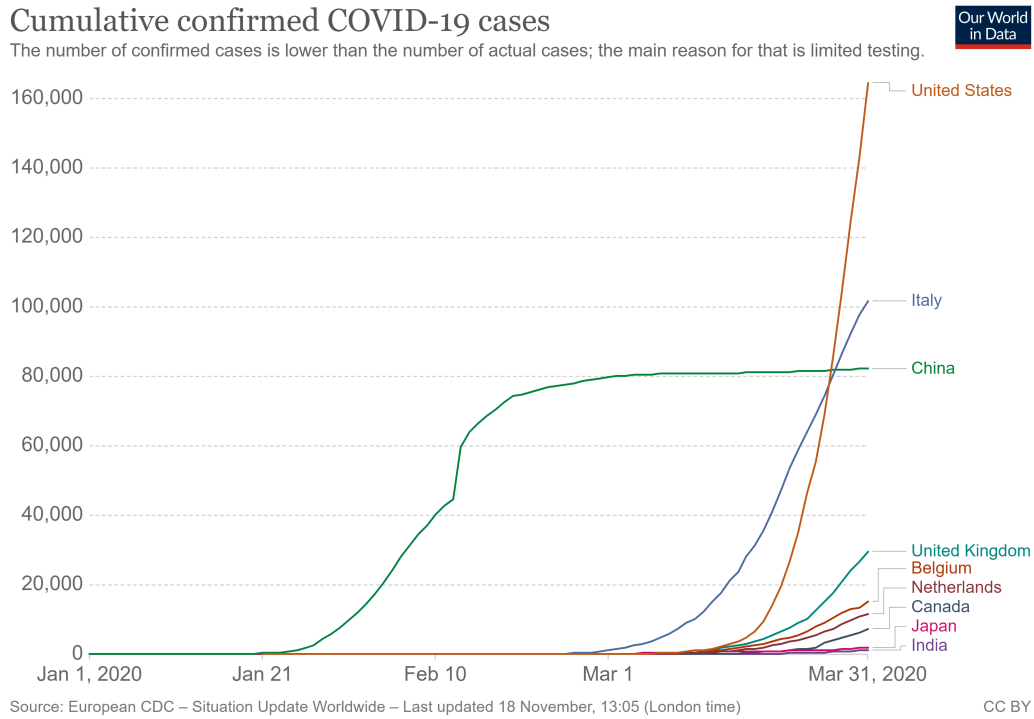
## A2: SafeGraph Zip-Code Level Data (Out of Sample)

### The University of Chicago



This figure uses the University of Chicago as an out-of-sample example to explain the SafeGraph data. The figure illustrates the change in the stay-at-home ratio in March of 2020 for the University of Chicago’s zip code. The solid line on day 17 is when the University of Chicago announced its WFH policy, and the dashed line on day 18 is when the local government’s “Stay at Home” order went into effect.

### A3: Cumulative Confirmed Cases for All Sample Countries



This figure uses information from Our World in Data, and plots the cumulative confirmed COVID-19 cases for our sample countries between January and March 2020. Source: <https://ourworldindata.org/coronavirus>

**A4: Change in Device at Home Days before Policy Intervention: Binary Information Exposure**

	(1)	(2)	(3)	(4)
		Stay-at-Home Ratio		
Pre Lockdown 20 Days × Information Exposure Binary	0.215** (2.31)	0.195*** (3.67)	0.140 (1.55)	0.158* (1.81)
Pre Lockdown 20 Days × Asian Percentage	0.129*** (3.53)	0.072*** (3.20)	0.106** (2.51)	0.095** (2.38)
Pre Lockdown 20 Days × Population (log)	1.871*** (11.46)	1.077*** (9.92)	1.672*** (10.06)	1.807*** (10.91)
Pre Lockdown 20 Days × Firms (log)	-0.316*** (-2.78)	-0.293*** (-3.12)	-0.280* (-1.95)	-0.369*** (-2.89)
Pre Lockdown 20 Days × Industry Teleworkability	5.632*** (7.25)	4.937*** (7.30)	7.667*** (8.80)	6.766*** (8.51)
Pre Lockdown 20 Days × Social Capital				0.833*** (7.29)
Pre Lockdown 20 Days × COVID Cases				-0.021 (-0.27)
COVID Cases				0.055 (0.74)
Pre Lockdown 20 Days × COVID Deaths				0.394** (2.19)
COVID Deaths				-0.078 (-0.46)
N	696036	1023084	1053424	1035108
Adj. R-squared	0.751	0.784	0.728	0.734
Zip-code FE	Yes	Yes	Yes	Yes
City-Day FE	Yes	No	No	No
County-Day FE	No	Yes	No	No
State-Day FE	No	No	Yes	Yes
Clusters	State	State	State	State

This table repeats the main results in Table 3 Panel A using *Information Exposure Binary* instead of the continuous variable. We provide a detailed description of the variables in Appendix A1. Columns (1) and (2) include city-day and county-day fixed effects, respectively. Columns (3) and (4) include state-day fixed effects. All columns include zip-code fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**A5: Change in Device at Home Days before Policy Intervention: Information Exposure Firms**

	(1)	(2)	(3)	(4)
	Stay-at-Home Ratio			
Pre Lockdown 20 Days × Information Exposure Firms Scaled	0.842*** (3.48)	0.778*** (5.06)	0.868*** (4.30)	0.857*** (4.68)
Pre Lockdown 20 Days × Asian Percentage	0.126*** (3.67)	0.070*** (3.25)	0.106** (2.51)	0.093** (2.38)
Pre Lockdown 20 Days × Population (log)	1.591*** (14.14)	0.797*** (15.98)	1.398*** (15.00)	1.452*** (16.47)
Pre Lockdown 20 Days × Industry Teleworkability	5.252*** (6.33)	4.481*** (6.69)	7.295*** (7.94)	6.291*** (7.66)
Pre Lockdown 20 Days × Social Capital				0.811*** (7.13)
Pre Lockdown 20 Days × COVID Deaths				0.396** (2.24)
COVID Deaths				-0.080 (-0.48)
Pre Lockdown 20 Days × COVID Cases				-0.021 (-0.28)
COVID Cases				0.056 (0.75)
N	688409	1012278	1043108	1024971
Adj. R-squared	0.751	0.785	0.729	0.734
Zip-code FE	Yes	Yes	Yes	Yes
City-Day FE	Yes	No	No	No
County-Day FE	No	Yes	No	No
State-Day FE	No	No	Yes	Yes
Clusters	State	State	State	State

This table repeats the main results in Table 3 Panel A using *Information Exposure Firms Scaled*. We provide a detailed description of the variables in Appendix A1. Columns (1) and (2) include city-day and county-day fixed effects, respectively. Columns (3) and (4) include state-day fixed effects. All columns include zip-code fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t-statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**A6: Change in Device at Home Days before Policy Intervention: Information Exposure Weighted by Employees or Sales**

	Stay-at-Home Ratio as dependent variable	
	(1) Employees	(2) Sales
Pre Lockdown 20 Days × Information Exposure Weighted by Employees	2.968*** (5.59)	
Pre Lockdown 20 Days × Information Exposure Weighted by Sales		1.759*** (5.41)
Pre Lockdown 20 Days × Asian Percentage	0.201*** (8.24)	0.203*** (8.38)
Pre Lockdown 20 Days × Population (log)	0.855*** (13.64)	0.875*** (14.52)
Pre Lockdown 20 Days × Industry Teleworkability	8.181*** (6.67)	8.203*** (6.60)
N	2344521	2344131
Adj. R-squared	0.863	0.863
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

This table repeats the main specification in Table 3 Panel A Column (2), and replace information exposure with versions weighted by employees and sales. We provide a detailed description of the variables in Appendix A1. All regressions include zip-code and county-day fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**A7: Change in Device at Home Days before Policy Intervention: Sample Sensitivity**

	Stay-at-Home Ratio as dependent variable		
	(1)	(2)	(3)
Pre Lockdown 20 Days × Information Exposure Scaled	0.452*** (5.10)	0.730*** (6.83)	0.730*** (6.83)
Pre Lockdown 20 Days × Asian Percentage	0.055*** (3.37)	0.047*** (3.20)	0.047*** (3.20)
Pre Lockdown 20 Days × Population (log)	0.410*** (5.00)	0.366*** (3.26)	0.366*** (3.26)
Pre Lockdown 20 Days × Industry Teleworkability	13.225*** (9.67)	17.180*** (10.23)	17.180*** (10.23)
N	575570	401530	401530
Adj. R-squared	0.872	0.891	0.891
Sample	Pop>5000 Firm>5	Pop>10000 Firm>5	Pop>10000 Firm>10
Zip-code FE	Yes	Yes	Yes
County-Day FE	Yes	Yes	Yes
Clusters	State	State	State

This table shows the sample sensitivity tests on our analysis of the effect of information exposure on the stay-at-home ratio before local policy intervention. This sensitivity analysis looks at samples at different population and firm cutoffs. We provide a detailed description of the variables in Appendix A1. All regressions include zip-code and county-day fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t-statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**A8: Change in Device at Home Days before Policy Intervention: Essential Industries**

	Stay-at-Home Ratio as dependent variable	
	(1)	(2)
	Essential Industry Ratio	
	Low	High
Pre Lockdown 20 Days × Information Exposure Scaled	0.492*** (5.74)	0.051 (0.38)
Pre Lockdown 20 Days × Asian Percentage	0.056*** (3.02)	0.093*** (3.76)
Pre Lockdown 20 Days × Population (log)	0.797*** (12.74)	0.817*** (10.21)
Pre Lockdown 20 Days × Industry Teleworkability	9.046*** (7.67)	1.467** (2.35)
N	496963	470181
Adj. R-squared	0.815	0.756
Zip-code FE	Yes	Yes
County-Day FE	Yes	Yes
Clusters	State	State

This table repeats the main specification in Table 3 Panel A Column (2), and partitions samples by the percentage of firms in a zip code that belongs to an essential industry. Essential industry is based on 6 digit NAICS identified on the CDC website. Columns (1) and (2) partition by this essential industry ratio, where *Low* (*High*) include observations with less than or equal to (more than) the median essential industry ratio. We provide a detailed description of the variables in Appendix A1. All regressions include zip-code and county-day fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t-statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.



In Appendix A9, we test whether firms are more powerful as corporate citizens in regions where the local government is less effective at dealing with the pandemic. During COVID-19, some local governments may have information constraints about the severity of the pandemic. If local governments wait to impose COVID-19 policies, firms are more likely to make an impact by adopting a work-from-home policy. To proxy for local governments' information constraints and/or willingness to act, we use political affiliation, as prior research shows that partisan bias influences information acquisition and information constraints (Gitmez et al., 2020). Belief in the severity of the pandemic and in appropriate containment policies is influenced by political affiliation. A COVID-19 household survey conducted in April 2020 finds that Democrats are more likely than Republicans to accept a longer initial lockdown and to believe in the need for subsequent lockdowns (Bertrand et al., 2020). Additionally, areas with Republican mayors were slower to adopt social distancing policies (Adolph et al., 2020). Thus, we use political affiliation with the Republican party to proxy for information constraint in local governments.

Table A9 shows the results of separating samples by the political affiliation of a city's mayor. Mayor data comes from Our Campaign, and is available for 732 cities. Columns (1) and (2) shows the results where the most recent mayor in the data at the end of 2019 is Republican and is Democratic, respectively. The coefficient on the interaction term is positive and significant for both columns, but is higher in magnitude in cities with a Republican mayor, where a one unit increase in *Information Exposure Scaled* is associated with a 4.8% (1.118/23.518) higher stay-at-home ratio. In cities with a Democratic mayor, the coefficient on *Information Exposure Scaled* is lower, translating into a 1.7% (0.430/25.082) increase in the stay-at-home ratio.

In Columns (3) and (4), we use the county-level vote ratio in the 2020 presidential election to proxy for political affiliation, where *Republican* is defined as counties with more than a 50% vote for Trump in 2020, and where *Democrat* is defined as counties with less than a 50% vote for Trump in 2020. In this analysis, we limit the sample to zip codes with a population greater than 10,000; we do this because we observe a high differential, where the average population is 8,000 in *Republican* and 18,000 in *Democrat* regions. The coefficient on the interaction term is positive and significant for both columns, but is slightly higher in magnitude in counties with more votes for

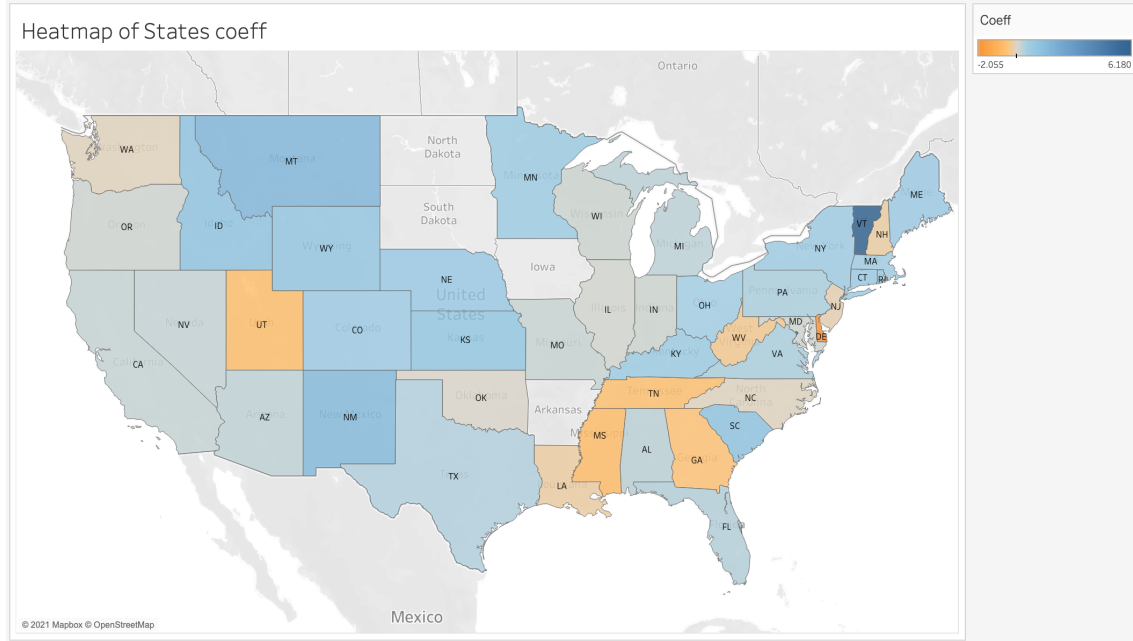
Trump. In Column (3), a one unit increase in *Information Exposure Scaled* is associated with a 3.3% (0.760/22.953) higher stay-at-home ratio in *Republican* counties. In contrast, in Column (4), the coefficient on *Information Exposure Scaled* translates to a 2.8% (0.675/23.975) higher stay-at-home ratio in *Democratic* counties. The results in this panel provide evidence that firms play a bigger role as corporate citizens when governments face constraints.

**A9: Change in Device at Home Days before Policy Intervention: Political Affiliation**

	Stay-at-Home Ratio as dependent variable			
	(1)	(2)	(3)	(4)
	Mayor		2020 Election	
	Republican	Democratic	Republican	Democratic
Pre Lockdown 20 Days × Information Exposure Scaled	1.118** (2.63)	0.430* (2.01)	0.760*** (4.40)	0.675*** (5.35)
Pre Lockdown 20 Days × Asian Percentage	0.126* (1.71)	0.035*** (2.73)	0.206*** (3.35)	0.042*** (3.47)
Pre Lockdown 20 Days × Population (log)	1.023*** (3.20)	1.091*** (4.74)	0.378* (1.69)	0.338*** (2.91)
Pre Lockdown 20 Days × Industry Teleworkability	6.121*** (2.78)	5.846*** (2.94)	13.720*** (6.32)	17.678*** (9.56)
N	29090	111984	119576	281180
Adj. R-squared	0.848	0.818	0.909	0.885
Zip-code FE	Yes	Yes	Yes	Yes
County-Day FE	Yes	Yes	Yes	Yes
Clusters	State	State	State	State

This table shows the results using the main specification in Table 3 Panel A Column (2), and partitions samples by political affiliation. Columns (1) and (2) partition by the political affiliation of the city mayor. Columns (3) and (4) partition by the percentage of vote for Trump in the 2020 election, where *Republican* is defined as counties with more than a 50% vote for Trump, and *Democratic* is defined as counties with less than a 50% vote for Trump. We provide a detailed description of the variables in Appendix A1. All regressions include county-day fixed effects, and regressions in Panels B and C include zip-code fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics based on robust standard errors clustered by state. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

### A10: Change in Device-at-Home Days before Policy Intervention: Heat Map by States



This figure plots a heat map of our main results for each state. The heat map is based on the coefficient of *Pre Lockdown 20 Days*  $\times$  *Information Exposure Scaled* in the main specification in Table 3 Panel A Column (2), but we run regressions separately for each state, and cluster standard errors at the county-level.

Appendix A10 provides evidence that our results are not driven by certain states. In Appendix A11, we look at our main analysis in New York, California, and Texas to demonstrate the relation between firms and government. New York is one of the first states to be severely affected by COVID-19, which means that firms may play a bigger role in responding to the crisis, even when local government takes strong action. Column (1) shows a positive coefficient on the interaction term, and the magnitude is larger than that of our main result in Table 3 Panel A Column (2). California is one of the earliest states to respond, declaring a state of emergency on March 8, which means that firms may play a smaller role.<sup>16</sup> Consistent with this conjecture, Column (2) shows that in California, having more firms with information exposure is not associated with a higher stay-at-home ratio before local policy intervention. Texas is a state that has large corporations and is more Republican, where firms may play a bigger role and where local government may react less quickly to COVID-19. The results for Texas in Column (3) show a positive and statistically significant positive coefficient. Finally, Column (4) shows that excluding these three states, our main results remain robust.

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<sup>16</sup>See <https://www.gov.ca.gov/wp-content/uploads/2020/03/3.4.20-Coronavirus-SOE-Proclamation.pdf>

**A11: Change in Device at Home Days before Policy Intervention: New York, California, and Texas**

	Stay-at-Home Ratio as dependent variable			
	(1)	(2)	(3)	(4)
	Information Exposure			
	NY	CA	TX	All Other
Pre Lockdown 20 Days × Information Exposure Scaled	0.566* (1.89)	0.202 (1.02)	0.401*** (3.36)	0.297*** (3.51)
Pre Lockdown 20 Days × Asian Percentage	0.065** (2.32)	0.030*** (3.92)	0.123*** (7.72)	0.106*** (5.09)
Pre Lockdown 20 Days × Population (log)	0.769*** (4.64)	0.662*** (5.43)	0.491*** (3.55)	0.828*** (15.73)
Pre Lockdown 20 Days × Industry Teleworkability	5.872*** (2.77)	5.592** (2.61)	2.266* (1.78)	4.295*** (6.61)
N	58927	67047	65354	820862
Adj. R-squared	0.751	0.778	0.834	0.784
Zip-code FE	Yes	Yes	Yes	Yes
City-Day FE	No	No	No	No
County-Day FE	Yes	Yes	Yes	Yes
State-Day FE	No	No	No	No
Clusters	County	County	County	County

This table repeats the main results in Table 3 Panel A Column (2) separately for the states of New York, California, Texas, and all other states except these three. We provide a detailed description of the variables in Appendix A1. All columns include county-day and zip-code fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t-statistics based on robust standard errors clustered by county. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**A12: SP500 Information Exposure and WFH dates**

	WFH Date	WFH Date
Information Exposure	-1.496* (-1.77)	
BoardEx Exposure		-5.061** (-2.32)
Revenue (log)	0.369 (0.27)	-0.207 (-0.14)
Total Assets (log)	-0.130 (-0.11)	0.587 (0.39)
Leverage	14.037* (1.97)	14.926 (1.61)
N	109	75
Adj. R-squared	0.045	0.078

This table reports the results of regressing WFH dates on *Information Exposure* and *BoardEx Exposure*. WFH dates are hand collected from SP500 firms, with a response from 109 firms. We provide a detailed description of the variables in Appendix A1. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**A13: Change in Store Visits before Policy Intervention**

	(1)	(2)	(3)
		Store Visits	
Pre Lockdown 20 Days × Information Exposure Binary	-2.485*** (-3.47)	-2.655*** (-4.00)	-3.425*** (-10.80)
N	366947	408522	444803
Adj. R-squared	0.827	0.827	0.833
Firm FE	Yes	Yes	Yes
City-Day FE	Yes	No	No
County-Day FE	No	Yes	No
State-Day FE	No	No	Yes
Clusters	Firm	Firm	Firm

This table repeats the main results in Table 3 Panel A using Safegraph’s store-level daily visit data. *Information Exposure Binary* equals 1 for stores where the firm has information exposure. We provide a detailed description of the variables in Appendix A1. Columns (1), (2), and (3) include city-day, county-day, and state-day fixed effects, respectively. All columns include store fixed effects. The table reports ordinary least squares (OLS) coefficient estimates and (in parentheses) t -statistics based on robust standard errors clustered by store. \*\*\*, \*\*, and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.



In Appendix A12-13, we provide firm-level evidence on information exposure and WFH policies. First, we validate that information exposure is correlated with earlier WFH policies by approaching SP500 firms and asking them about the first day they implemented WFH policies as a response to COVID-19. We received 109 responses. In Appendix A12, we regress WFH dates from these 109 responses on *Information Exposure* and *BoardEx Exposure*, and find evidence that firms with more information exposure implement earlier WFH policies. Second, we repeat our main analysis using firm-level Safegraph foot-traffic data on store visits. The results are presented in Appendix A13. Our coefficient of interest on *Pre Lockdown 20 Days*  $\times$  *Information Exposure Binary* remains positive and significant.