Social Networks and Corporate Social Responsibility*

Romulo Alves†

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Abstract

This paper shows that the social networks of firms' executives and directors drive corporate social responsibility (CSR). Using a novel identification strategy exploiting the imperfect overlap between industry, geographic and social peers, I show that CSR spreads through corporate social networks. This result is robust to employing a regression discontinuity quasi-experimental design based on the passage of close-call CSR proposals. Social network effects are concentrated in firms in which the profit maximization incentives of managers and shareholders are aligned. This suggests that, when agency frictions are low, firms use social networks as market for information exchange on CSR policy with the goal of improving firm value. Accordingly, I find no evidence for alternative explanations related to herding and models of identity economics.

JEL classification: D85, G30, G34, M14

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†Department of Finance, Rotterdam School of Management. E-mail: romulo.t.alves@rsm.nl

Are corporate social responsibility (CSR) policies transmitted across firms through the social networks of their executives and board members? If so, which factors explain this phenomenon? Using social network data covering 83,604 top executives and directors of Russell 3000 firms from 2001 to 2016, I show that firms' CSR decisions are influenced by the CSR decisions of their social peers. My findings are consistent with the notion that firms use social networks as a strategic information sharing tool with the aim of increasing firm value. Overall, this paper suggests that social learning through corporate social networks leads to a social multiplier in CSR investment decisions that not only amplifies the externalities of CSR on society but is also consistent with profit maximization incentives.

The identification of peer effects is challenging due to the difficulty of (i) separating peer effects from common unobserved shocks, and (ii) disentangling whether firms respond to the CSR decisions of their peers or to some other peer characteristic (Mansky (1993)). I deal with these empirical challenges by extending the identification strategy of Bramoullé, Djebbari, and Fortin (2009) and Giacomo, Pellizzari, and Redaelli (2010). This strategy consists of exploring the fact that not all the peers of a firm's peers are socially connected with that firm. The CSR decisions of these indirect peers can thus be used as a valid instrumental variable for the CSR decisions of the firm's direct peers. I build on this strategy by exploiting the fact that the the CSR decisions of firms in the same industry are strategic complements (Cao, Liang, and Zhan (2019)). I thus define the indirect peers of a firm i as the industry peers of firm i's social peers that do not have social, industry or geographic ties with firm i. The identifying assumption is that, conditional on a high-dimensional set of control variables and geography-by-year, industry-by-year and state-of-incorporation-by-year fixed effects, the CSR decisions of indirect peers only systematically affect the CSR decision of firm i through its direct social peers. To further alleviate the concern that my results are driven by latent common factors, I show that the findings survive falsification tests based on placebo networks.

I find that firms with average levels of CSR increase their CSR by 16% in response to a one standard deviation shock to the average CSR of their social peers. This finding is robust to a wide range of exercises, including applying network community detection algorithms to form communities and control for endogenous network formation, employing different combinations of fixed effects, first-differencing the regression equations, alternating between contemporaneous and lagged specifications, using alternative definitions of peers based on different industry and geographic boundaries, excluding firms with a very low or a very high number of social peers, and controlling for a multitude of firm-level and peer-level variables.

I complement this instrumental variable approach with a dynamic regression discontinuity design based on the passage of close-call CSR shareholder-sponsored proposals (e.g. Cuñat, Gine, and Guadalupe (2012)). The causal estimates indicate that firms increase their CSR by as much as 17% of the standard deviation of CSR scores within two years of the marginal approval of CSR proposals by their social peers.

As for the channels, I explore whether or not peer effects operate through a social learning channel whereby firms exchange information and learn from one another with the goal of creating firm value. In that case, peer effects should be stronger for firms in which the firm value maximization incentives of managers and shareholders are more aligned. Consistent with this hypothesis, I find that peer effects are concentrated in firms with higher CEO pay-sensitivity to performance (CEO delta) and stronger board independence. Moreover, I find that peer effects are strongest for firms with higher risk-taking incentives as measured by CEO vega. Insofar as vega is used to overcome underinvestment by incentivizing risk-averse managers to invest in risky positive NPV projects (Guay (1999)), this suggests that social learning alleviates a CSR underinvestment problem caused by investment frictions such as uncertainty and irreversibility (e.g. Guiso and Parigi (1999)). Intuitively, the returns to social learning are highest for high vega firms that have incentives to pursue risky CSR projects.

A further implication of these findings is that it is unlikely that my results are explained by herding. For instance, managers may rationally choose to herd with the intent of sharing the blame and preserving their labor market reputation in the event that CSR investments underperform (Scharfstein and Stein (1990)). If peer effects were caused by either irrational or rational herding, we would expect peer effects to either be unrelated or negatively related to measures of incentive alignment. My results are thus inconsistent with herding channels.

I consider an additional channel related to the evidence that CSR decisions are influenced by the social preferences and values of executives and directors (e.g. DiGiuli and Kostovetsky (2014), Cronqvist and Yu (2017)). These studies suggest a social norm explanation of CSR whereby decision-makers experience disutility if they fail to invest in CSR in a way that is consistent with either their own social preferences or the dominant preferences in their social environment. Intuitively, this may happen either because (i) managers internalize their peers' notions of right and wrong and gain utility from acting according to those social norms, or (ii) because doing otherwise would challenge those social norms and could lead to punishment in the form of, for example, weaker social ties and fewer job market opportunities (e.g. Akerlof and Kranton (2000), Bénabou and Tirole (2011a)). At odds with this identity economics channel, however, I find no evidence that peer effects depend on social norms.

My paper contributes to the burgeoning literature documenting drivers of CSR (e.g. Flammer (2015b), Dimson, Karakaş, and Li (2015), Albuquerque, Koskinen, and Zhang (2019), Dai, Liang, and Ng (2019), Flammer and Kacperczyk (2019a), Dyck et al. (2019)) and to the broader literature on peer effects in corporate finance (e.g. Shue (2013), Leary and Roberts (2014), Kaustia and Rantala (2015), Fracassi (2017) and Grennan (2019)). The closest paper to mine is Cao, Liang, and Zhan (2019), who document that industry peer effects in CSR arise because firms mimic each other to stay competitive. The key difference is that, in contrast to industry peer effects that arise out of within-industry competitive dynamics that are unrelated to social networks, the peer effects documented in this paper occur through cross-industry collaborative social interactions.

A long-standing question in CSR research is whether CSR is a tool to create long-term value by catering to the needs of stakeholders (e.g., Edmans (2011) and Deng, Kang, and Low (2013)) or a value-destroying manifestation of agency problems.[1] I contribute to this debate by showing that, contrary to the predictions of the agency view of CSR, firms with fewer agency frictions put effort to learn from their social peers and actively engage in CSR. In doing so, I also provide a mechanism to explain the negative association between CSR investment and agency frictions documented in Ferrell, Liang, and Renneboog (2016).

I further contribute to the literature studying how social processes shape economic decision-making. Unlike extensive research that finds that social networks lead to suboptimal herding behavior or firm value destruction, my results reveal a bright side of social networks.[2] This is the case because peer effects are concentrated in firms with stronger incentives for value creation, suggesting that social networks may create value. In addition, by creating a social multiplier in CSR, social networks amplify the positive externalities of CSR on society.

Finally, I shed light on the extent to which different aspects of firm-level social capital complement or substitute for one another. As pointed out by Servaes and Tamayo (2017), a firm's social capital is multidimensional and we know little about how these different dimensions interact with each other. For instance, larger networks of top executives could substitute for the need of firms to invest in CSR as a way of building trust and reputation. My results suggest this is not the case. Network social capital, as measured by the social networks of top executives and directors, induces firms to accumulate more social capital in the form of CSR. However, I also fail to find evidence that social norms mediate peer effects. This suggests that not all dimensions of firm social capital complement each other.

[1]Examples of ways in which CSR can destroy value include managers using firm resources to further their own philanthropic causes (e.g. Masulis and Reza (2014)) or managers letting their moral and political beliefs guide CSR policy at the expense of value creation (e.g. DiGiuli and Kostovetsky (2014)).

[2]Refer to Grinblatt, Titman, and Wermers (1995), Nofsinger and Sias (1999), Hong, Kubik, and Solomon (2000), Sias (2004), Clement and Tse (2005) and Hong, Kubik, and Stein (2005) for examples of herding behavior over networks of analysts, mutual fund managers and institutional investors. For research uncovering value-destroying or outright illegal effects stemming from firms' social networks see, for instance, Barnea and Guedj (2009), Hwang and Kim (2009), Fracassi and Tate (2012), Nguyen (2012), Butler and Gurun (2012), Liu (2014), Ishii and Xuan (2014) and Ahern (2017).

1 Hypotheses Development

In this study, I analyze three possible channels through which social peer effects may arise: (i) social learning, (ii) identity and social norms, and (iii) rational herding.

In theory, peer effects can arise if socially connected firms share information and learn from each others' CSR practices (e.g. Ellison and Fudenberg (1993, 1995), Banerjee and Fudenberg (2004), Acemoglu et al. (2011)). Firms have incentives to share information insofar as estimating whether or not a given CSR investment is worth pursuing is often challenging. First, some of the benefits of CSR are intangible (e.g. reputation), are only realized over the long term or are state-dependent (e.g. only pay off during periods of low trust).[3] Second, only recently did CSR become a widely used strategic tool to which substantial research efforts are dedicated, creating limits to learning how to optimally design CSR from academic literature. On-the-job learning also falls short of being a complete solution as co-workers are subject to common informational frictions in terms of education and on-the-job experience. Third, CSR encompasses a wide range of topics requiring different types of expertise that may not be readily available to all firms. Fourth, CSR investments are often irreversible and costly.[4] In the presence of uncertainty about the net benefits of irreversible costly investments, firms often choose underinvestment over learning by trial-and-error (e.g. Guiso and Parigi (1999)).

These factors create an environment of uncertainty for managers trying to estimate the NPV of alternative CSR projects. The exchange of information among social peers may allow firms to tackle this challenge in two ways. First, it expands the information set firms rely on to identify which are the optimal CSR projects amongst a large pool of possibilities. Second, because firms have the option of mimicking successful projects and avoiding value-

[3]For instance, Amiraslani et al. (2017) show that CSR only pays off in the form of improved ability to raise debt capital during periods of low-trust, such as the 2008-09 financial crisis. Albuquerque, Koskinen, and Zhang (2019) show that CSR decreases systematic risk by increasing product differentiation. Koh, Qian, and Wang (2014) provide evidence that CSR creates insurance value against litigation risk.

[4]For the average firm in their sample in 2006, Lins, Servaes, and Tamayo (2017) estimate the cost of increasing CSR investment from the 1st to the 4th quartile to be $203.5 million.

destroying projects, information sharing spreads the downside risk of trial-and-error learning across social peers. Firms can achieve this by informally exchanging claims on streams of information in a way that insures themselves against the state of nature of choosing value-destroying sequences of trial-and-error CSR investments over time. This, in turn, incentivizes trial-and-error experimentation and can lead to faster learning across peers. This social learning channel thus posits that firms use the social networks of their top executives and directors as a market for information exchange with the aim of creating firm value.

Given the evidence that social preferences and values affect CSR decisions (e.g. DiGiuli and Kostovetsky (2014), Cronqvist and Yu (2017)), social transmission of CSR policies may instead be due to an identity economics channel (e.g. Akerlof and Kranton (2000), Bénabou and Tirole (2011a)). In this framework, an agent jointly maximizes a standard utility function that does not depend on social context and an identity utility function that does. Identity can be understood as an agent's internalized beliefs about who she is. Identity utility is highest if actions match the ideal norms of behavior that are associated with those beliefs. When actions do not match the prescription of behavior associated with the identity the agent believes she has, she experiences cognitive dissonance. Moreover, whenever the behavior of the agent's social peers contradicts behavior prescriptions, the agent loses utility as she is forced to confront a reality that is at odds with her internalized views about right and wrong. To avoid this utility loss, the agent may respond by punishing her peers. Peer effects can then arise as an equilibrium sustained by punishment threats off-the-equilibrum path.

In our setting, the standard utility function is related to how much CSR increases profit. The identity utility function is related to the predominant ideals about the role of firms in society. One can imagine business leaders either (i) internalizing a Friedmanite vision of the world whereby the objective function of the firm is to maximize profit (Friedman, 1970), or (ii) believing that firms have a social role beyond firm value maximization (e.g. Dodd (1931), Hart and Zingales (2017)). By shunning employee diversity and well-being or taking a soft stand on the importance of protecting the environment, a Friedmanite agent may negatively affect how pro-CSR peers perceive him and risks punishment in the form of future foregone social benefits, such as favors and information exchange. If the punishment is strong enough, the desire for conformity will lead to peer effects in CSR.[5] Furthermore, peer effects can also arise if executives internalize their social peers' pro-CSR identity through mechanisms of persuasion (DeMarzo, Vayanos, and Zwiebel, 2003) and esteem (Akerlof (2016, 2017)).

Finally, managers may rationally opt to mimic their social peers out of labor market reputation concerns. For example, in the rational herding model of Scharfstein and Stein (1990)) good managers receive an informative signal about the viability of an investment whereas bad managers receive a noisy signal. Insofar as the informative signals of good managers will be correlated around the true value, good managers will tend to behave alike. By contrast, noisy signals will be uncorrelated and bad managers decisions will be idiosyncratic. As a consequence, managers have the incentive to mimic their peers to signal that they are good managers. This incentive is particularly strong whenever it is difficult to determine the impact of the investment on financial performance. After all, if financial performance were completely informative about the quality of the investment choice, there would be no need to compare managers and no incentive to herd. Since the benefits of CSR tend to be intangible, state-dependent and oriented towards the long-term, it is often hard to assess the impact of specific CSR policies on financial performance. This implies that the incentives for rational herding in CSR policy are in place. Moreover, Hubbard, Christensen, and Graffin (2017) show that prior CSR investments increase the likelihood of CEOs being fired when financial performance is low. This further incentivizes managers to correlate their CSR decisions to decrease the probability of being fired in adverse states of nature.

[5]Note that this argument has strong bite because social networks are known to be important sources of job opportunities and financial returns to executives and directors (e.g. Hwang and Kim (2009), Fracassi and Tate (2012), Engelberg, Gao, and Parsons (2013), Ishii and Xuan (2014)). Conformity and cooperation are also to be expected in dense networks such as corporate social networks because social peers can enforce cooperation by threatening to harm the reputation of deviators within a common social network through word-of-mouth sanctions (e.g. Karlan, Mobius, Rosenblat, and Szeidl (2009), Lippert and Spagnolo (2011)).

2 Data and Summary Statistics

2.1 Social Network Construction

I construct social networks based on individual connections of top executives and directors for the largest 3000 publicly traded US companies (Russell 3000) with at least $10 million in assets. The data is sourced from the BoardEx database and covers the period 2001-2016.

Following Fracassi (2017), I define top executives as the top five executives based on compensation data from ExecuComp. Since the ExecuComp universe is the S&P 1500, I cannot apply this definition to all the Russell 3000 sample firms. In those cases, I define the top executives to be the CEO, CFO and COO. The final sample comprises 83,604 individuals.

Building on Fracassi and Tate (2012), I consider four types of Boolean individual-level networks: current employment, past employment, other activities and education networks. Current employment networks capture professional relationships that occur when two individuals sit on the same board or C-suite. Past employment networks are defined in the same way except for the fact that they capture past relationships that are no longer active in the current year. As for the other activities network, individuals are defined to be connected if they have active roles in the same clubs, charities or organizations.[6] Finally, individuals are said to be connected via the education network if they graduated from the same university with the same degree type within one year of one another.

Finally, I aggregate the individual-level networks into firm-level networks. For each year and network type, I define two firms to be linked if at least two individuals working in those firms are connected in the underlying individual-level network. To capture board interlocks, two firms are also considered to be connected if they share a director. In addition, to

[6]As in Fracassi and Tate (2012), I assume that active memberships are those that are not simply described as *member* in the Boardex database (e.g. President or trustee). Furthermore, since BoardEx often does not provide information on membership periods, the other activities network is assumed not to vary over time. All the other networks are time-varying.

reduce the possibility of capturing spurious social connections, I require the headquarters of firms that are socially connected through education, other activities and past employment networks to lie in the same Combined Statistical Area (CSA).[7] For a detailed explanation of the network construction approach, refer to the Internet Appendix 1.

2.2 Data on Corporate Social Responsibility

I use data from the MSCI ESG Stats Database to construct CSR scores.[8] Following previous literature (e.g. Cao, Liang, and Zhan (2019)), I focus on the following CSR categories: employee relations, community relations, environment and workforcce diversity. [9] Scores in each category are available in the form of strengths and concerns in various subcategories. I follow Flammer and Kacperczyk (2019a, 2019b) and sum over all the strengths within each category. Since the maximum number of strengths within each category can vary over time, I scale by the total possible number of strengths for each firm-year. Finally, I sum the scaled scores across all four categories to obtain an overall measure of CSR.[10]

2.3 Other Variables

I use different proxies for whether managers' incentives are aligned with those of shareholders. The first two proxies are CEO delta and CEO vega. Following Coles, Daniel, and Naveen (2006), delta is defined as the dollar-value sensitivity of the executive's stock and option portfolio (executive's wealth) to a one percentage point change in the stock price. Vega is the dollar-value sensitivity of the executive's wealth to a 1/100 change in the annual-

[7]CSAs, as defined by the United States Office of Management and Budget (OMB), are geographic polygons that combine areas with strong economic, social and commuting links. There are 172 combined statistical areas in the US, the largest of which is New York - Newark, with over 22 million inhabitants.

[8]The MSCI ESG Stats Database was formerly known as Kinder, Lydenberg and Domini & Co. (KLD)).

[9]Scores are available for several categories. I focus on these four categories to avoid contaminating the CSR measure with governance, product market competition and other industry-specific information.

[10]Until recently, a typical approach in the literature was to subtract the concerns from the strengths. However, this procedure can be unreliable due to conceptual differences between strengths and concerns (e.g. Johnson-Cramer (2004), Mattingly and Berman (2006)).

ized volatility of stock returns. Both variables are sourced from Lathita Naveen's website.[11] The third proxy is board independence, measured as the fraction of board members who are independent directors (sourced from ExecuComp).

To quantify the extent to which firms are embedded in a social network rich in social capital, I use known proxies for county-level social capital (e.g. Rupasingha, Goetz, and Freshwater (2006)): non-profit and/or recreational association density (per capita), registered tax-exempt non-profit organization density (per capita), voter turnout and organ donation density. I obtain firm-specific measures of social capital by assigning county-level measures of social capital to each firm based on the county where the firm is headquartered.[12] A detailed description of these variables is provided in the Internet Appendix 2.

I collect voting data on CSR-related shareholder-sponsored proposals from Institutional Shareholders Service (ISS) voting analytics database and from SharkRepellent. The ISS database covers S&P 1500 firms from 2003 to 2016 and SharkRepellent covers Russell 3000 firms from 2005 to 2016.

Finally, I employ the standard control variables in the literature related to firm size, leverage, profitability, liquidity, dividend payout, indebtedness and institutional ownership. Accounting variables are sourced from Compustat and institutional ownership data from Thomson Reuters. Detailed variable definitions are provided in the Appendix Table A.1.

2.4 Summary Statistics

The leftmost plot in Figure 1 below shows the distribution of the number of social connections across firms in 2010. Slightly over 60% of the firms in the sample have between 1 and 50 connections. This number decays rapidly as we move along the network degree axis, with

[11]For in-depth details on the construction of these variables refer to Core and Guay (2002) and Coles, Daniel, and Naveen (2006) .

[12]Compustat only provides data on the most recent headquarters and, therefore, I do not account for headquarter relocations. As pointed out by Parsons, Sulaeman, and Titman (2018), this is unlikely to affect the results because relocations are very rare.

fewer than 1% of firms having more than 250 social peers.[13] The maximum number of connections is 336. Hence, there is a small number of firms with a large number of connections and a large number of firms with relatively few connections.

This suggests that there is substantial cross-firm variation in access to information and that the benefits of information exchange may be limited for some firms. This variation can be visualized in the plot on the right, which shows the heatmap of degree centrality in 2010. Lins, Servaes, and Tamayo (2017) conjecture that the high cost of CSR investments may be a reason for why not all firms engage in CSR. To the extent that smaller and more financially constrained firms also tend to have fewer network connections, lack of access to information via social networks might magnify the costs of engaging in CSR even further for these firms.

[Figure 1 About Here]

The histogram in Figure 2 depicts the distribution of the clustering coefficient of Watts and Strogatz (1998). This coefficient measures how closely knit the local network around each firm is. A score of one occurs when all of a firms' connections are also connected to each other. We observe that 25% of the sample firms exhibit high clustering coefficients above 0.5. To put this in perspective note that, if our network was generated by a Erdős and Rényi (1959) random graph model, the expected clustering coefficient would be 15 times smaller than the observed mean coefficient of 0.4.[14] This suggests that the structure of corporate social networks may be able to sustain information exchange within close-knit groups through schemes of reward and punishment. This is the case because locally dense networks make it possible for social peers to jointly punish firms who fail to cooperate, thus making deviations costlier (e.g., Karlan, Mobius, Rosenblat, and Szeidl (2009), Lippert and Spagnolo (2011)). Another implication is that, under the framework of identity economics,

[13] Network degree, or degree centrality, is simply the number of social connections a given firm has.

[14] This follows from the fact that the expected value of the local clustering coefficient for a Erdős and Rényi (1959) network is equal to the probability that any two given nodes are connected.

not acting according to the identity of the close-knit group can trigger a heavy punishment. Therefore, this structure may also favor conformity out of fear of punishment, potentially at the cost of ignoring one's own beliefs and private information.

[Figure 2 About Here]

Figure 3, left panel, shows the spatial cross-correlation of CSR in the form of Moran's I statistic for different degrees of separation in the corporate social network.[15] To ensure that the results are not contaminated by the increasing trend in CSR observed in the last two decades, I first compute the statistic separately for each year and then average across years. We observe a strong positive correlation in CSR scores among peer firms that are directly connected. Most striking is that the commonality in CSR completely vanishes once we consider indirect social peers, even when these indirect peers are just two steps away.

This result is reassuring in that it is inconsistent with the existence of strong unobservable common shocks that could bias our results. If omitted common shocks would completely drive the spatial correlation, we would expect to find stronger positive correlations between the CSR policies of firms that are not directly connected. Moreover, the fact that correlations are high and low exactly when they should be also suggests that our social networks are able to capture meaningful social links.

A relevant question is whether or not the absence of strong positive correlations between CSR policies of indirectly linked firms is consistent with firms bridging information selectively between their social peers (consistent with information selection effort and information being valuable). Indeed, the observed pattern could simply be a mechanic artifact of a sparse network. In sparse networks, the average shortest path length across all pairs of nodes is large and, therefore, it is difficult for information originating in a given node to percolate across the network. In denser networks with a few highly connected hubs, however, information origi-

[15] This statistic provides consistent estimates of spatial correlations for nodes located at different distances in the network and takes into account the strength of the connections (Kelejian and Prucha (2001)).

nating in a given node in the network is able to quickly reach a highly connected node which can then spread the information to many other nodes (e.g. Pastor-Satorras and Vespignani (2001), Barthélemy et al. (2004), Acemoglu, Bimpikis, and Ozdaglar (2014), Rantala (2019)).

To provide some insight into this question, I plot the 10th, 50th and 90th percentiles of the cross-firm distribution of the fraction of firms that are within a given distance of each sample firm, averaged over the period 2001-2016. The results are shown in the right panel of Figure 3. Consistent with the idea that social networks are small worlds in which everyone is just a few hops away from everyone else (e.g. Milgram (1967), Barabási (2003)), most firms can reach the entire network within 3 steps. More precisely, half of the firms can reach at least half of the network within 2 steps and 90% of the firms can reach at least 90% of the entire network within 3 steps. Overall, and with the caveat of being purely descriptive, this evidence suggests that the architecture of US corporate social networks is able to sustain the exchange of valuable information on CSR.

[Figure 3 About Here]

Finally, Table 1 provides summary statistics on the main variables for the full sample as well as for the lowest and highest terciles of the distribution of degree centrality. Firms with higher degree centrality tend to be larger, more profitable, distribute more dividends, spend more on advertising expenditures relative to their size (customer awareness), have more long-term debt and invest more in both R&D and CSR. The descriptive statistics for the full sample are similar to those in previous literature.

[Table 1 About Here]

3 Empirical Model and Identification Strategy

3.1 The Empirical Model

In line with a growing literature in corporate finance and banking (e.g. Leary and Roberts (2014), Grennan (2019), Silva (2019)), I employ the standard linear-in-means model of peer effects:

$$y_{ijklt} = \alpha + \beta \bar{y}_{-ijklt} + \lambda' \bar{X}_{-ijklt} + \gamma X_{ijklt} + \mu_{jt} + \delta_{kt} + \zeta_{lt} + \epsilon_{ijklt}, \tag{1}$$

where the unit of observation is a firm i in year t, operating in industry j, headquartered in CSA region k and incorporated in state l.

The dependent variable y_{ijklt} is the CSR score of firm i and is assumed to be a linear function of the mean outcome of its peer group ($\bar{y}_{-ijklt}$). The mean outcome for firm i is defined as the weighted average of the CSR scores across all its peers, excluding firm i itself. The weights are proportional to the strength of the social connection between each firm pair. As in Leary and Roberts (2014), I focus on a contemporaneous measure of peer effects. Nevertheless, I show that the results are robust to employing lagged specifications. The model further controls for the average characteristics of firm i's peer group ($\bar{X}_{-ijklt}$), its own characteristics (X_{ijklt}) and a set of three-digit SIC industry-by-year (μ_{jt}), region-by-year (δ_{kt}) and state-of-incorporation-by-year (ζ_{lt}) fixed effects.

3.2 The Identification Problem

As shown by Mansky (1993), the identification of peer effects is challenging for two reasons. First, it is necessary to separate peer effects that occur through actual social interactions (e.g. mimicking due to social learning) from correlated effects that arise due to latent common factors that induce changes in CSR in all firms within a peer group.[16] Second, the behavior of a firm within a peer group simultaneously affects and is affected by other firms in the group, generating collinearity between the mean CSR decisions and the mean characteristics of the group. This so-called reflection problem makes it difficult to identify whether firms' CSR responds to the CSR decisions of its peers or to some other peer characteristic.

3.3 The Identification Strategy

My identification strategy builds on Bramoullé, Djebbari, and Fortin (2009) and Giacomo, Pellizzari, and Redaelli (2010), who formally show that the reflection and correlated effects problems can be solved in networks with partially overlapping peer groups. The firm-level social networks that are the focus of this paper satisfy this requirement. The key intuition of this result is that such networks are rich in intransitive triads, meaning that firms are not connected with all the peers of their peers - thus generating indirect peers. Therefore, the actions of a firm's indirect peers affect that firm's actions through its peer group while being, by construction, orthogonal to that firm's peer group fixed effect. This, in turn, generates within peer-group variation and breaks the reflection problem.

In practice, this strategy is operationalized by using the behavior of a firm's indirect peers as an instrumental variable for the CSR decision of a firm's peer group. I extend this idea by exploiting the fact that firms are part of geographic, social and industry networks, all of which are partially overlapping with respect to each other. This allows me to define the indirect peers of each firm i as the industry peers of the direct social peers of firm i and to use their CSR policies as an instrument for the CSR policies of firm i's direct social peers. In addition, I impose that (i) indirect peers are neither social peers nor industry peers of firm i, and (ii) indirect peers and firm i are headquartered in different geographic areas (CSAs).

[16] Examples of correlated effects that have been documented in the literature are state-wide regulations that affect CSR, such as: state unemployment insurance benefits (Flammer and Luo (2015)), inevitable disclosure doctrines (Flammer and Kacperczyk (2019a)) and constituency statutes (Flammer and Kacperczyk (2019b)).

The validity of this strategy hinges on the instrument being strong and satisfying the exclusion restriction. The expectation that the instrument is strong is supported by previous literature documenting economically significant industry peer effects in CSR decisions (Cao, Liang, and Zhan (2019)). The exclusion restriction is that, conditional on CSA-by-year, state-of-incorporation-by-year and industry-by-year fixed effects (and remaining control variables), the average CSR decision of the industry peers of the social peers of a firm, who do not share any geographic, industry or social links with that firm, should only affect the CSR decision of that firm through its direct social peers. This seems reasonable for several reasons.

First, since there are no social, industry or geographic links between indirect peers and the firm being instrumented, there is no obvious channel based on industry competition, transfer of information over social networks or local events through which indirect peers would directly affect the firm. It is possible, however, that an indirect peer of a firm is involved in an attention grabbing event with news coverage and that the event leads both the indirect peer and the firm to increase or decrease CSR in tandem. For example, a human rights scandal relating to an indirect peer outsourcing activities to a socially irresponsible firm in another country could produce this effect. Note, however, that such events are rare both across time and across firms, making it unlikely that such events systematically confound the results. Moreover, since the instrument is constructed as the average of CSR scores over a large number of indirect peers, the instrument is bound to have very little correlation with such rare firm-specific events. To alleviate this concern further, I show in robustness tests that the results do not change if I restrict the sample to firms that have many indirect peers.

Second, it is reasonable to assume that the instrument is orthogonal to the omitted variables causing endogenous sorting into social networks. Suppose there is such a variable (e.g. political views) that causes both social connections and CSR investment in firms. It would have to be the case that the average CSR decisions of indirect peers are systematically correlated with this variable. This seems particularly unlikely because I impose that the indirect social peers are not social, geographic or industry peers of the firm being instrumented. Moreover, recall that, as shown in Figure 3, the spatial cross-correlation of CSR in the social network completely vanishes once we look beyond immediate social peers. If endogenous sorting is not strong enough to generate unconditional strong and positive spatial correlation with indirect social peers, it is unlikely that it generates such a correlation with firms that operate in completely different social, geographic and industry networks. Despite that, I reduce this concern in robustness tests by using community detection algorithms to identify network communities and controlling for network community-by-year fixed effects.

Third, I include high-dimensional fixed effects that capture a wide-range of common unobserved shocks. Industry-by-year peer effects control for time-varying industry-specific shocks as well as industry peer effects in CSR. In addition, CSA-by-year fixed effects separate social peer effects from geographic peer effects in CSR. These could arise due to, for example, exposure to common laws or geographic variation in social norms (e.g. Rupasingha, Goetz, and Freshwater (2006)). In order to better control for the time varying-effect of laws, I also include state-of-incorporation-by-year fixed effects. Furthermore, I show the results are robust to defining indirect peers based on different geographic and industry boundaries and applying first-differences to equation (1), which eliminates time-invariant firm unobservables.

Finally, I show that the results break down when using placebo networks. If correlated effects were driving the results, we should find peer effects in these placebo networks. Therefore, the lack of evidence for such spurious effects is evidence for the reliability of this identification strategy. Nevertheless, I acknowledge that no strategy based on a non-shock instrumental variable can completely rule out endogeneity concerns (Atanasov and Black (2016)). For this reason, I complement this IV strategy with a quasi-experimental regression discontinuity design based on close-call CSR proposals.

4 Social Network Peer Effects of CSR

4.1 Baseline Results

Table 2 presents the results from estimating model (1) via two-stage least squares (2SLS). The instrument is the average CSR score of indirect peers. The table reports estimates of the CSR (endogenous) peer effect of interest in the first row and exogenous peer effects in the other rows. All the coefficients are measured in standard deviation units for ease of interpretation. T-statistics are reported in parentheses and standard errors are heteroskedasticity-robust and clustered at the firm-level. All the regressions include firm-level controls for the same variables that are listed as peer-level controls. I present results of using both the benchmark contemporaneous specification (columns (1) through (3)) and a fully lagged specification (columns (4) through (6)). These lagged specifications alleviate concerns that reverse causality drives the results or lead to a non-negligible overestimation of the economic significance of peer effects.

[Table 2 About Here]

The results in column (1) show that the estimated CSR peer effect is 0.439 (t-statistic of 2.643). Therefore, firms increase CSR by 0.44 standard deviations in response to a one standard deviation shock to social peers' CSR.[17] This corresponds to a 16% increase in CSR for a firm with an average level of CSR. The results in column (4) show that lagging all right-hand side variables, including the instrument, has little impact on the statistical and economic significance of the results. In both regressions, the Kleiberg-Paap F-statistic is large and well above the standard cutoff value of 10, suggesting that the instrument is sufficiently

[17]Note that a one standard deviation shock to the average CSR of social peers is equivalent to a one standard deviation shock to the CSR of all peers. Since the average firm in the sample has 64 peers, this is a very large shock for the typical firm. Hence, the large economic magnitude of peer effects should be interpreted in light of the large magnitude of the shock.

strong. The coefficient sign of the first-stage instrument is positive, in line with the previous literature documenting positive industry peer effects of CSR (Cao, Liang, and Zhan (2019)).

One concern is that I allow direct social peers to be industry peers. While industry-by-year fixed effects should absorb time-varying industry peer effects, it may be that there is within-industry heterogeneity in industry peer effects that are not captured by the fixed effects. To alleviate this concern, in columns (2) and (5), I re-estimate the regressions excluding all social peers who are also industry peers. The results suggest that the fixed effects do a good job in capturing time-varying industry peer effects as the coefficients barely change.

In columns (3) and (6) I include two additional controls to rule out two potentially important sources of bias: customer awareness and R&D investment. Servaes and Tamayo (2013) find evidence that the ability of CSR to create value is concentrated in firms with high customer awareness, as measured by the advertisement expenditure ratio. If firms with high customer awareness tend to have peers with high customer awareness, peer effects may spuriously reflect time-varying commonalities in that variable. As for R&D investment, Shen, Tang, and Zhang (2019) show that innovative firms use CSR as a signal of long-run orientation to overcome information frictions related to risky transaction-specific investments between the firm and its stakeholders (e.g. suppliers). Since there are economically significant social peer effects in R&D (e.g. Fracassi (2017), Zacchia (2019)), peer effects in CSR may just be an artifact of R&D peer effects. The results show that our estimates are not confounded by the exclusion of these variables.

To gauge the extent to which our specification is prone to biases stemming from time-varying unobservables, I follow Silva (2019) and apply the method of Altonji, Elder, and Taber (2005). This method estimates how large the bias from selection on unobservables must be to completely explain our results, under the assumption that the degree of selection on unobservables is of the same magnitude as the degree of selection on observables. Based on the benchmark regressions in Table 2, I find that selection on unobservables would have to be 3.06 to 3.42 times as large as selection on observables. Following Altonji, Elder, and Taber (2005), I interpret a value of 3.55 as evidence that selection on unobservables does not play much of a role. This suggests our results are unlikely to be driven by unobservables.

Finally, in the Internet Appendix Table IA.1, I show that these results are robust to: (i) excluding firms with fewer than five or 10 peers; (ii) excluding firms with more than 250 peers; (iii) imposing that firms and their indirect peers cannot be in the same one-digit SIC industry (instead of three-digit SIC); (iv) imposing that firms and their indirect peers cannot be in the same headquarter state (instead of CSA); (v) using headquarter state-by-year fixed effects instead of CSA-by-year fixed effects; (vi) using firm and year fixed effects; (vii) lagging the instrument and not lagging the variable being instrumented; (viii) lagging the instrument and the control variables and not lagging the variable being instrumented. This battery of robustness tests alleviates concerns related to overmeasurement and undermeasurement (error) of social connections, dependence of results on the definition of geographic and industry boundaries, firm-specific time-invariant unobservables and reverse causality. In addition, since there is a reduction in the number of subcategories in the KLD database after 2013, I replicate the results in Table 2 restricting the sample period to end in 2013 instead of 2016. In all instances, the magnitude of CSR peer effects increases by about 30% . This shows that the results are not qualitatively affected by missing values for subcategories after 2013. The results can be found in the Internet Appendix Table IA.2.

4.2 Network Placebo Tests

The main challenge in our empirical setting is to separate endogenous peer effects from correlated effects. While the results above suggest that correlated effects play a limited role, I can conduct a stricter test of this statement by forming placebo peer groups. If our results are driven by latent common factors, we would expect to find that the average CSR of a firm's social peers is systematically related to the CSR decisions of other firms.

I construct placebo peer groups by randomly matching each firm's direct and indirect peer groups to another firm in that year. A welcome feature of this approach is that it preserves the specific latent common factors that potentially drive our results in Table 2. The matching process is repeated 1000 times, both with and without replacement. The regressions control for state-of-incorporation-by-year fixed effects, industry-by-year fixed effects, CSA-by-year fixed effects and all firm-level and peer-level control variables used throughout the paper, including the additional controls customer awareness and R&D (as in model (3) of Table 2). The results reported in Table 3 show that the t-statistics and associated coefficient estimates obtained in the non-placebo regression in Table 2 occur in fewer than 1% of the placebo simulations. Moreover, the mean and median placebo peer effect is zero. This suggest that our results are unlikely driven by latent common factors.[18]

[Table 3 About Here]

4.3 Tests of Endogenous Sorting into Networks

An additional concern is that our results are driven by endogenous sorting into networks. For instance, DiGiuli and Kostovetsky (2014) document that firms with Democrat directors and CEOs invest less in CSR compared to Republican-leaning firms. If Democrats are more likely to be socially connected to Democrats than to Republicans, peer effects may be an artifact of common preferences across socially linked firms. To alleviate this concern, I use a community detection algorithm, the Louvain algorithm (Blondel et al. (2008)), to partition the social networks into densely connected communities of firms. This makes it possible to employ different types of network community-by-year fixed effects that vary with average community size and to cluster standard errors to account for within-community dependence. The results are shown in Table 4. Depending on the size of the communities, the magnitude

[18]Plots with the full distribution of placebo coefficients and t-statistics are shown in the Internet Appendix Figure IA.1.

of the peer effects and associated t-statistics are either very similar or slightly larger than our baseline estimates in Table 2. The stability of the estimates is, therefore, consistent with the notion that the instrumental variable is orthogonal to the omitted variables that simultaneously influence firm network formation and CSR investment decisions.

[Table 4 About Here]

4.4 Subnetworks Results

The current employment network is the one which poses more challenges in terms of causal identification. For example, positive peer effects estimates could be an artifact of directors with pro-CSR preferences sitting on boards of multiple companies and pushing for CSR in those companies. In that case, there is no actual mimicking of peers (e.g. information sharing or herding) since the same individuals are making decisions without considering the actions of their peers. While there is little reason to believe the instrument fails to eliminate this source of bias, I investigate this possibility more carefully in this section.

If peer effects were completely driven by traditional board interlocks, we would expect that (i) the peer effects would be concentrated in the current employment subnetwork, and (ii) adjusting the definition of direct peers to exclude peers connected through subnetworks other than current employment should not substantively affect the results. Table 5 shows the results of estimating model (1) for each of these cases.

The results in column (1) shows that there is no evidence of peer effects when I use the current employment network by itself. In other words, the existence of current employment social links is not a sufficient condition to produce peer effects. Column (2) shows that current employment social connections are, however, a necessary condition for peer effects. Indeed, there is no evidence for peer effects when I construct a network that sets aggregate social connections to zero whenever a firm-pair is linked through the current employment network. Interestingly, with the exception of past employment, a similar pattern emerges when excluding the other subnetworks from the aggregate network (columns (3) through (5)). This indicates that non-employment social connections are just as important as current employment connections.

[Table 5 About Here]

5 Quasi-Experimental Evidence from Close-call CSR Shareholder Proposals

Overall, the evidence so far suggests that the IV identification strategy is identifying a causal effect. Nevertheless, to further enhance the credibility of the findings, I complement this strategy with quasi-experimental evidence. In particular, I examine the response of firms' CSR decisions to the passage of close-call CSR shareholder proposals by their social peers in a dynamic regression discontinuity design (RDD) framework. The identifying assumption is that whether the social peers of a firm i pass or fail CSR proposals around the pass threshold (e.g. 51% vs 49% of the votes) is as good as random with respect to the other determinants of firm i's CSR.[19] Under this assumption, I estimate the causal peer effects of CSR by comparing the CSR outcomes of firms whose peers failed to pass CSR proposals by a small number of votes with the CSR outcomes of firms whose peers barely passed CSR proposals.

I test for violations of the identifying assumption in three ways. First, I check that there is no evidence of a discontinuity around the approval threshold using the non-parametric density estimator test of Cattaneo, Jansson, and Ma (2019) (p-value of 0.28).[20] Second, I show that there is little evidence of pre-treatment differences in peer firm characteristics

[19]This seems reasonable in light of the fact that previous literature using CSR proposals in a similar setting has consistently failed to find evidence for manipulation (e.g. Flammer (2015a), Cao, Liang, and Zhan (2019), Dai, Liang, and Ng (2019)).

[20]This recent method improves on the discontinuity test of McCrary (2008) by providing robust bias-corrected confidence intervals and overcoming the need to pre-bin the data, thus yielding more statistical power.

within narrow voting windows around the pass threshold. The results are in the Internet Appendix Table IA.3.

A non-voting firm-year is assigned to the treatment (control) group if at least one peer proposal passes (fails) in that year. Therefore, a firm is either in the control or the treatment group. Furthermore, as in Cuñat, Gine, and Guadalupe (2012) and Flammer (2015a), I aggregate the votes of all the peer proposals associated with a given non-voting firm in a given year as follows. If the firm is in the treatment group, I sum the distances to the threshold across all peer proposals that pass. Similarly, if a firm is in the control group, I sum across failed proposals. This ensures that observations will only lie within a short-window of the threshold if all the peer proposals that pass or fail are individually close to threshold. Finally, if the peers of firm i vote on proposals in year t, I only include firm i in the analysis if firm i did not pass any proposals itself in the period ranging from year t-2 until t+3. This ensures that I do not spuriously attribute peer effects to firms passing proposals around the same time their peers happen to be voting on their own proposals.[21]

The dynamic RDD is implemented as follows. First, for each non-voting firm i and each proposal year t in which social peers vote on proposals, the CSR scores at time $t + h$ are stacked together for $h = -2, -1, 0, 1, 2, 3$. This affords efficiency gains compared to just running a separate regression for each horizon h and allows me to include firm-by-proposal

[21] An alternative approach in the literature (Cao, Liang, and Zhan (2019) and Dai, Liang, and Ng (2019)) consists of stacking all pairs of voting and non-voting firms together in a regression discontinuity design framework. This approach, however, has some disadvantages. First, suppose a non-voting firm i is associated with two proposals j_1 and j_2, that pass and fail within a 5% margin, respectively. In this case, firm i appears on both the control and treatment groups. This is at odds with a key feature of regression discontinuity designs: having a running variable that deterministically assigns observations to either the treatment or control groups (Angrist and Pischke, 2008). Intuitively, the problem here is that while the marginal approval of proposal j_1 represents an exogenous shock to the CSR of the voting firm and, therefore, to the CSR of the non-voting firm, the proposal that marginally fails, j_2, is an exogenous shock with null economic magnitude since it induces no changes in the CSR of the voting firm. Therefore, firm i would be assigned to the control group despite being effectively treated. Second, suppose firm i is associated with two proposals j_1 and j_2 that pass with 5% and 30%, respectively. Further assume that firm i responds to j_2 but not j_1. In this scenario, the discontinuity estimate will mistakenly attribute the (non-causal) effects of proposal j_2 far away from the threshold to proposal j_1 close to the threshold. By stacking the outcome of firm i twice, it is impossible to separate the causal effect of proposals close to the threshold from the non-causal effect of proposals far away.

year fixed effects, thus absorbing all firm-specific unobservables in the window around the proposal year. In addition, I include calendar year fixed effects to absorb firm-invariant time-varying unobservables as well as time-elapsed-since-proposal fixed effects to ensure that the estimates only capture within-horizon variation. Second, I allow the peer effects estimates to vary across horizons. Third, I aggregate proposals using the treatment assignment rule explained above. This leads to a total of 22,229 non-voting firm-years for horizon $h = 0$, 482 of which are associated with passing proposals within the 10% threshold. However, it is important to stress that only 16 proposals, out of a total of 3,010 proposals, pass within the 10% threshold. The small number of passing proposals is consistent with previous literature (Flammer (2015a)). Hence, while this strategy scores high on internal validity, its external validity is not guaranteed.

As in Cuñat, Gine, and Guadalupe (2012) and Flammer (2015a), the model is formalized as a global polynomial regression:

$$y_{i,t+h} = \alpha + \beta_h T_{i,t} + P_l(R_{i,t}, \beta_{l,h}) + P_r(R_{i,t}, \beta_{r,h}) + \theta_h + \eta_{i,t} + \lambda_c + \mu_{i,t+h} \tag{2}$$

where $y_{i,t+h}$ is the CSR score of firm i in year $t + h$, $P_l(R_{i,t}, \beta_{l,h})$ and $P_r(R_{i,t}, \beta_{r,h})$ are polynomials in the running variable to the left and to the right of the threshold, and $T_{i,t}$ is the number of proposals that pass. Time-elapsed-since-proposal, firm-by-proposal year and calendar year fixed effects are denoted by θ_h, $\eta_{i,t}$ and λ_c, respectively. The coefficient of interest is β_h, which is allowed to vary for $h \geq 0$ and constrained to zero for $h < 0$. As before, I exclude same-industry social peers to abstract from industry peer effects.

Table 6 presents the results of estimating equation (2) under different assumptions on the degree of the polynomial (columns (1) through (6)). In addition, columns (2), (4) and (6) include the full range of firm-level controls used in the previous sections.

[Table 6 About Here]

The results show evidence of jumps in CSR scores of 11% to 17% of a standard deviation in the year that peer proposals pass. There is also some evidence of peer effects in the two years after the passing of the proposals. This result, however, is not robust across all choices of polynomial order. By the third year, there is no evidence of peer effects. In addition, adding firm-level controls to the regression has virtually no effect on the point estimates, providing further evidence that the identifying assumption is satisfied. If the results were sensitive to the inclusion of covariates, it could be an indication that peers firms on both sides of the threshold are not comparable.

One major concern about this approach is that these results depend on the choice of how to aggregate proposals. To alleviate this concern, I define an alternative aggregation rule. Instead of summing across proposal votes to define the running variable, I define the running variable based on the most extreme outcomes of peer proposals votes. If the firm is in the treatment group, I set the running variable to be equal to the largest distance to the threshold across all peer proposals that pass. If a firm is in the control group, I set the running variable to be equal to the lowest distance to the threshold across all peer proposals that fail.[22] The results in Internet Appendix Table IA.4 shows that the conclusions are qualitatively robust to changing the running variable, with the bulk of the peer effects occurring contemporaneously.

Overall, the results in this section are in line with previous results, thus providing some confidence that our IV method is reliable and is capturing a true causal effect while having much stronger external validity than the regression discontinuity design.

6 Social Learning Channel

The social learning channel posits that social networks mitigate uncertainty and limits to learning by allowing firms to exchange information on how to optimally design value-creating

[22]For instance, if a firm is associated with five proposals that pass by 5%, the running variable takes value 5% instead of 25% using the original rule. This ensures that the proposal is much closer to the threshold.

CSR policies. To bring this hypothesis to the test, I start by noting that, under this social learning channel, the incentives for information sharing and learning stem solely from the desire to maximize firm value. If this is the case, we should observe that peer effects are stronger for firms with stronger ex-ante incentives to maximize firm value, that is, firms with better incentive alignment between shareholders and managers. If, instead, social peer effects are a manifestation of non-profit maximization motives (e.g. irrational herding or reputation concerns), we would expect either (i) peer effects to be strongest when incentive alignment is weakest, or (ii) no difference in peer effects across firms with different incentives.

I capture alignment of incentives with CEO pay-related managerial incentives and the strength of board monitoring. I employ two measures of CEO pay-related managerial incentives: CEO delta and CEO vega. CEO delta is a measure of CEO pay-sensitivity to performance. Hence, if firms try to learn from their social peers with the goal of maximizing value, their incentives to do so are stronger the higher CEO delta is. CEO vega is a measure of risk-taking incentives and is used, in practice, to avoid costly underinvestment by incentivizing risk-averse managers to invest in risky positive NPV projects (Guay (1999)). Since, as argued before, uncertainty and limits to learning can theoretically lead to costly underinvestment in CSR, social learning should be a particularly valuable tool for high vega firms interested in allocating resources to risky CSR projects. Finally, I measure the strength of board monitoring by the fraction of independent directors in the board. More monitoring should be a good governance force aligning the incentives of managers and shareholders and, therefore, leading to peer effects.

The results are reported in Table 7. The regressions in columns (1), (2), (3) use variables in levels while the regressions in columns (4), (5) and (6) use first-differences regression specifications. The latter regressions eliminate all time-invariant omitted variables. This rules out concerns that, for example, firms with higher CEO delta are fundamentally different from firms with low CEO delta along unobservable dimensions. I further control for all four measures of network specific geographic social capital described in Section 2.3.[23] This alleviates the concern that the identification of the social learning channel is confounded by a social capital channel.

[Table 7 About Here]

In each regression, I split one of the incentives variables (delta, vega, fraction independent directors) in three mutually exclusive groups based on the within-year distribution of each variable. The first group includes observations below the median of the distribution. The second group includes observations above the median and below the 80th percentile. The third group includes the remaining firms. The asymmetric division captures the fact that delta and vega are relatively stable for the bottom half of the firms, and then increase at an increasing rate with percentile.[24] This ensures that the observations within each group are more similar than if I just divide the firms based on the median.

It is worth noting that the sample size drops by roughly 50% for two reasons. First, the data for CEO delta and vega is sourced from Latitha Naveen's website and is only available until 2014. Second, the three incentive alignment variables are constructed from ExecuComp data that is only available for S&P 1500 companies.

The results show that: (i) the economic significance of peer effects increases monotonically with firm value maximization incentives in all specifications, (ii) there is no evidence of peer effects for the bottom half of firms with weak incentives, and (iii) the bulk of the peer effects is concentrated in firms with very strong incentives.

While this conclusion holds based on all three variables, the results are substantially stronger when using vega. For example, based on the first-differences specification, we see

[23]These measures are organ donation density, registered organization density, association density and voter turnout rates at the county-level. The county-level measures are matched to each firm based on the location of firm headquarters. For each firm-year, I take the average of county-level social capital across all of a firm's social peers, including the firm itself, to obtain a time-varying measure of the social capital embedded in each firm's social network. Refer to Internet Appendix 2 for details on how these variables are constructed.

[24]For example, the mean delta within the 3rd tercile (2nd tercile) of delta's distribution is roughly 15 times (5 times) larger than the mean delta within the 2nd tercile (1st tercile).

that the peer effects are 37% stronger for high-vega firms relative to high-delta or high-board independence firms. Since vega is known to deter risk-averse managers from underinvesting in risky positive NPV projects (Guay (1999)), this is consistent with social learning alleviating underinvestment problems in CSR by reducing investment frictions such as uncertainty and limits to learning (e.g. Guiso and Parigi (1999)).[25]

The economic magnitude of the peer effects for firms with strong incentives is large, ranging from 0.68 to 0.79 standard deviations in response to a one standard deviation change in social peers' CSR investment. To put the numbers in perspective: this corresponds to a 25% increase in CSR for a firm with an average level of CSR. This suggests that, at least for those firms that have the right incentives in place, social peer effects may influence a firm's CSR as much as the industry peer effects and customer peer effects documented by Cao, Liang, and Zhan (2019) and Dai, Liang, and Ng (2019), respectively.

Overall, these results provide evidence that firms mimic their social peers only when managers have strong incentives to maximize firm value. This is consistent with the idea that social networks are a value-creating resource that firms can strategically use to overcome uncertainty and costly underinvestment. As a consequence, these findings also cast doubt on explanations based on non-profit maximization motives such as irrational or rational herding.

Furthermore, these results provide a mechanism that can at least partially explain the negative association between CSR investment and agency frictions documented by Ferrell, Liang, and Renneboog (2016). In detail, my results suggest that when agency frictions are low, firms are able to use their social networks to obtain information that decreases

[25]Another possibility is that CEO vega is capturing excessive risk-taking incentives as opposed to firm value maximization incentives. From an ex-ante perspective, this is very unlikely. First, if this was the case, we would not expect peer effects to be stronger when board monitoring and CEO delta are higher. More monitoring should curb excessive risk-taking and higher delta, by itself, incentivizes underinvestment in risky projects because CEO wealth is not diversified (e.g Coles, Daniel, and Naveen (2006)). Second, excessive risk-taking in CSR strongly increases the probability of CEO dismissal when performance is poor (Hubbard, Christensen, and Graffin (2017)). Hence, from an ex-ante perspective, there is a large downside risk to gambling. Third, contracts with high vega tend to be structured, albeit imperfectly, in such a way that curbs excessive risk-taking incentives (e.g. Kubick, Robinson, and Starks (2018)).

uncertainty about CSR investments. This, in turn, decreases the real option value of waiting for more information before investing and leads to more CSR investment.

7 Identity-Economics Channel

In the previous section, I provided evidence that social network serve as a valuable conduit of information that allows firms to improve the design of CSR projects. It is also possible, however, that social peer effects are mediated by social norms. For instance, pro-CSR values and norms can be transmitted through social networks and lead to commonalities in CSR policies across socially connected firms. Alternatively, firms may rationally choose to behave according to their peers' identities out of fear of punishment (e.g. Akerlof and Kranton (2000), Bénabou and Tirole (2011a)).

If social peer effects in CSR are the result of identity and norms, I would expect peer effects to be stronger for firms whose executives and directors have social networks that are rich in the following two dimensions of social capital: (i) social capital as a measure of civic engagement and pro-social preferences, capturing the likelihood that firms believe they have a role beyond value maximization; (ii) social capital as a measure of the extent to which social networks can enforce punishment schemes that sustain cooperation, trust and pro-social behavior.

To quantify social capital at the network level, I exploit the fact there is ample cross-sectional variation in geographic social capital at the county level in the US and that there is evidence that firms absorb the social capital rules of the county where the firm is headquartered.[26] Geographic social capital is likely to capture the desired dimensions of firm-level social capital because high social capital counties are, by definition, communities in which trust, reciprocity and pro-social behavior are sustained through internalized community values and networks of relationships. In such a community, individuals act with the well-being of the community in mind and expect others to do the same. This expectation is self-fulfilling because shared values and norms of behavior are rewarded by the community and deviant behavior is punished.[27]

Following an extensive literature (e.g. Putnam (2000), Guiso, Sapienza, and Zingales (2004), Rupasingha, Goetz, and Freshwater (2006), Lin and Pursiainen (2018)), I measure geographic social capital with county-level data on organ donation per capita, association density per capita, registered organization density per capita, voter turnout and the principal component of the last three variables. I then assign county-level data to each firm based on the location of firm headquarters. To create a measure of the social capital embedded in the local network of each firm, I average the social capital variables across each firm's direct social peers, including the firm itself.

Table 8 reports the findings of whether or not peer effects are stronger for firms whose local social networks are richer in social capital. I split firms in terciles based on the distribution of social capital in each year. Each column (1) through (5) refers to one social capital variable. For each variable, there is evidence of economically large peer effects across all terciles. For

[26] For instance, Hasan, Hoi, Wu, and Zhang (2017a) find that firms engage in less tax evasion in US counties with more social capital. Hasan, Hoi, Wu, and Zhang (2017b) find that firms headquartered in low social capital counties have access to cheaper debt. Jha and Chen (2014) find evidence that audit firms infer trustworthiness of clients based on whether firms are headquartered in a low or high social capital county, and adjust audit fees accordingly. Relatedly, Lin and Pursiainen (2018) provide quasi-experimental evidence that the social capital of an entrepreneurs' county improves campaign performance by signalling the level of trustworthiness of the entrepreneur and overcoming moral hazard in crowdfunding.

[27] Furthermore, insofar as individuals internalize community values and norms and derive satisfaction (e.g: self-esteem) from behaving according to those values and norms, individuals can punish themselves for deviating from the norm. Such punishments can include fear of feeling guilty, shame, lack of self-esteem and discomfort arising from cognitive dissonance. Ultimately, as pointed out by Bénabou and Tirole (2011b), the standards of communities regarding the enforcement of punishments and rewards for pro-social behavior will affect not only social norms of stigma and esteem but also moral sentiments of shame and pride.

all five variables, however, the peer effects in the lowest and highest tercile are very similar and, in three out of five cases, peer effects are the strongest for firms in the middle tercile. This suggests that peer effects do not increase monotonically with social capital. I confirm this by formally testing for equality of peer effects in the highest and lowest tercile of social capital. I always fail to reject the null of equality, with high p-values ranging from 0.3 to 0.9.

[Table 8 About Here]

One possible caveat of these results is that our local network measure of social capital is not the relevant measure. It could be the case that, for instance, own social capital fully determines the extent to which a firm mimics its peers. This could arise if own social capital leads some firms to always want to invest in a level of CSR that is deemed adequate by society, irrespective of threats of punishment (and, therefore, irrespective of peers' social capital). In that case, firms may gauge what the adequate level of CSR is through interactions with their peers, thus leading to peer effects. As shown in the Internet Appendix Table IA.5, this turns out not to be the case. Table IA.6 and Table IA.7 in the Internet Appendix show that these results are qualitatively similar when I use regression specifications in first differences.

8 Conclusion

This study provides evidence that CSR polices are transmitted across firms through the social networks of their executives and directors. Based on rich social network data for 83,604 top executives and directors of Russell 3000 firms, my estimates indicate that firms increase their CSR scores by at least 0.44 standard deviations in response to a one standard deviation increase in the average CSR scores of the peer group. This result seems to be driven by the conjunction of professional, educational and leisure networks, suggesting that the strength of the social links is more important than the type of network. Overall, the economic magnitude of social peer effects is comparable to the industry peer effects and customer-supplier peer effects of CSR documented by Cao, Liang, and Zhan (2019) and Dai, Liang, and Ng (2019).

Furthermore, I find that these peer effects are concentrated in firms with high board independence and CEO delta. In other words, social mimicking occurs in firms in which the firm value maximization incentives of managers and shareholders are aligned. This suggests that firms use social networks to learn about CSR policy design with the aim of creating firm value. Moreover, I find that peer effects are strongest for high CEO vega firms. Since vega is often employed to incentivize risk-averse managers to pursue risky positive NPV projects (e.g. Guay (1999)), this finding is in line with the notion that uncertainty and limits to learning in CSR policy design lead to underinvestment in CSR for those firms in which CEOs do not have adequate risk-taking incentives. Once those incentives are in place, the evidence indicates that firms are able to, at least partly, overcome the underinvestment problem through social learning. The intuition is that the benefits of social learning are highest in uncertain settings. This implies that the returns to investing in social learning are highest for high vega firms that have incentives to pursue uncertain projects in the first place.

Overall, these results reveal a bright side of corporate social networks for both firms and society at large. For firms, our results suggest that social networks may allow firms to design better CSR policies with higher likelihood of creating firm value. For society, the existence of endogenous peer effects implies the existence of a social multiplier in CSR investing, thus amplifying the positive externalities of CSR on society.[28]

One interesting implication of this study is that there may be frictions in CSR investment, such as uncertainty and limits to learning, that create a need for tools like social learning. If so, it should be the case that these frictions lead to underinvestment in viable CSR projects as otherwise there would be no room for further investment to create value via social learning.

[28] The existence of a social multiplier is implied by the existence of endogenous peer effects. See Glaeser, Sacerdote, and Scheinkman (2003) for a proof. Intuitively, any exogenous shock that increases the CSR of a firm i will lead to an increase in CSR of social peers via endogenous peer effects. The increase in CSR by social peers will, in turn, lead to increases in CSR investment by their own social peers, including firm i.

Therefore, the large cross-firm variation in CSR investment that exists nowadays may be partly due to differences in firm exposure to investment frictions and differences in ability to mitigate those frictions.

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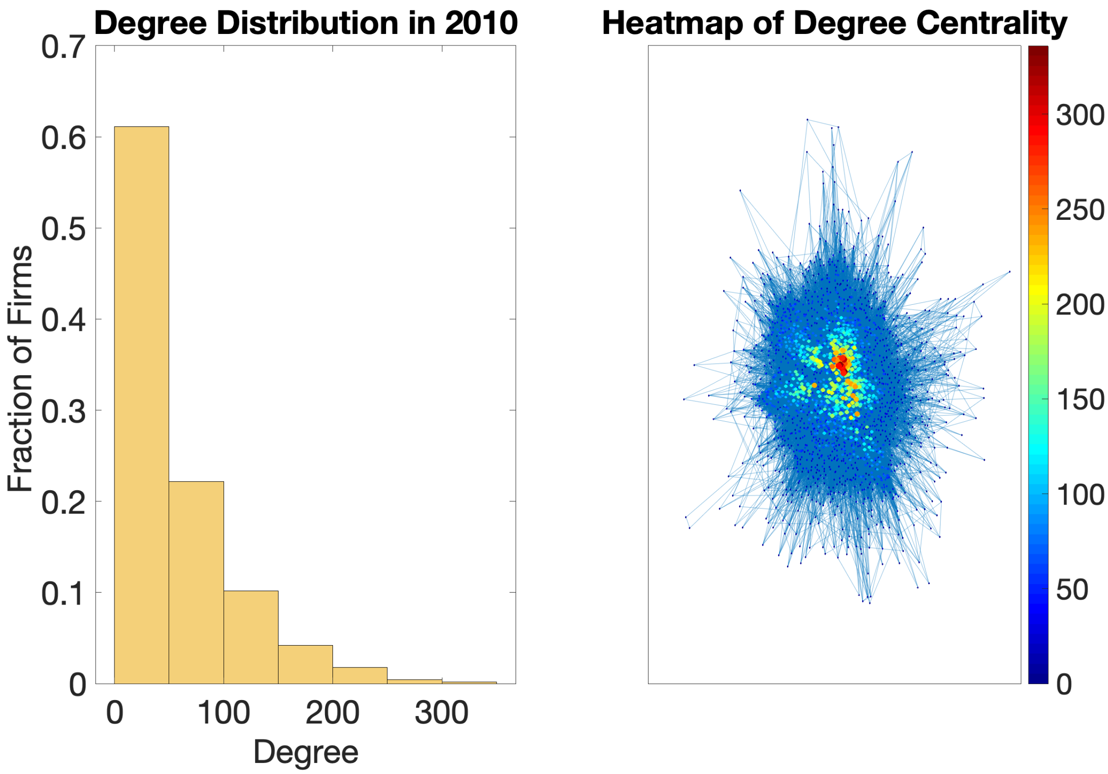

Figure 1. Network Degree Distribution.
The histogram on the left depicts the degree distribution of firm-level social connections in 2010. The degree (or degree centrality) of a given firm is defined as the number of social connections of that firm. The heatmap on the right displays the firm-level social network in 2010 and the degree centrality of each firm. Warmer colors indicate higher degree centrality. To ease color visualization in the heatmap, I exclude firms that have strictly less than three peers.

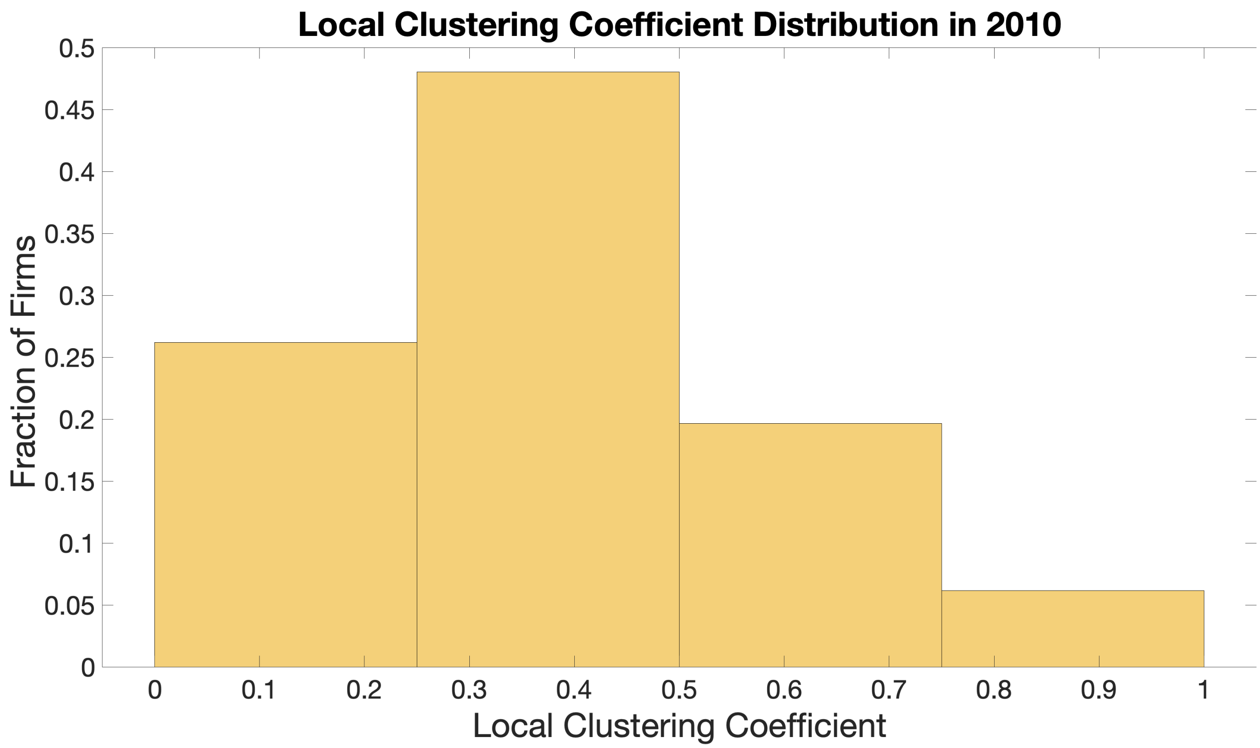

Figure 2. Local Clustering Coefficient Distribution.
This figure shows the distribution of the local clustering coefficient of Watts and Strogatz (1998) in 2010. The local clustering coefficient for a given firm is defined as the number of connections among the social peers of that firm divided by the number of possible connections. It takes value one (zero) if all (none) of a firm's connections are connected to each other.

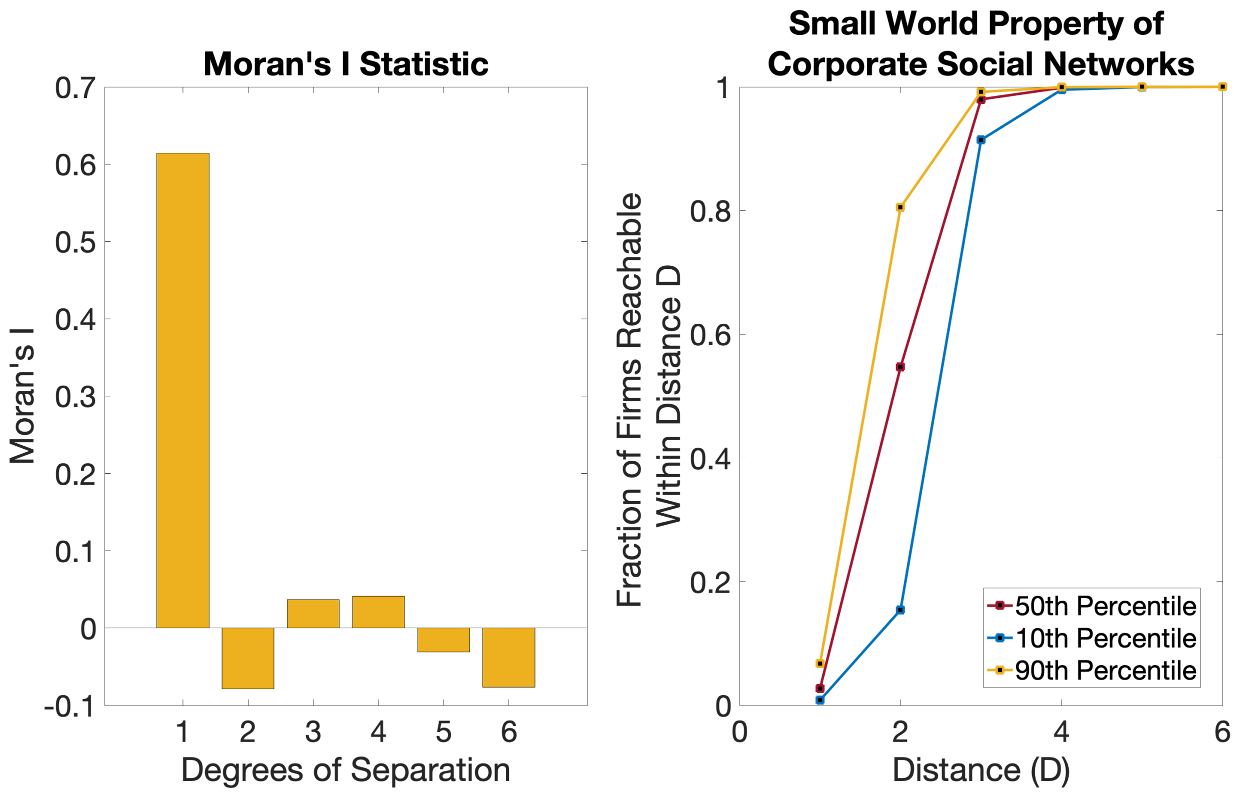

Figure 3. Spatial cross-correlation of CSR scores and small world property of corporate social networks.

The histogram on the left shows the spatial cross-correlation (Moran's I statistic) of CSR scores for various degrees of separation between 2001 and 2016. To control for time trends in CSR scores, Moran's I statistic is first computed separately for each year and then averaged over the time dimension. The plot on the right depicts the 10th, 50th and 90th percentiles of the cross-firm distribution of the fraction of firms that are within a given distance of each sample firm. The fractions are computed separately for each firm-year-distance combination and then aggregated across firms over the period 2001-2016 for each distance.

Table 1

Summary Statistics

This table reports the summary statistics for the main variables during the sample period 2001-2016. See Appendix Table A.1 for definitions. The columns under the *Low Degree (L)* (*High Degree (H)*) label contain means and standard deviations for the firm-years that belong to the lowest (highest) tercile of the distribution of degree centrality. The column *H minus L* presents the difference in means between the high and low degree centrality subsamples. The last two columns present statistics for the full sample. All the control variables are winsorized at the 1% and 99% levels. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Low Degree (L)		High Degree (H)		H Minus L	Full Sample	
	Mean	SD	Mean	SD	Diff. Means	Mean	SD
Size	6.996	1.439	8.550	1.783	1.555***	7.631	1.706
MB Ratio	2.662	3.442	3.223	4.261	0.561***	2.978	3.889
Debt Ratio	0.219	0.218	0.254	0.195	0.035***	0.237	0.209
ROA	0.026	0.111	0.032	0.105	0.006***	0.025	0.114
Net Income	69.831	227.036	780.103	1619.125	710.273***	331.219	1035.211
Cash Ratio	0.159	0.191	0.157	0.178	-0.002	0.165	0.193
Dividend Ratio	0.012	0.023	0.015	0.022	0.003***	0.013	0.022
Cust. Awaren.	0.008	0.023	0.012	0.028	0.004***	0.010	0.025
R&D	12.235	36.770	183.932	477.208	171.697***	78.103	295.021
Inst. Own.	0.614	0.287	0.594	0.289	-0.02***	0.606	0.286
CSR score	1.113	0.245	1.473	0.620	0.360***	1.257	0.460

Table 2

Social Peer Effects of CSR

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Contemporaneous			Lagged		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' CSR	0.439***	0.450***	0.461***	0.382**	0.393**	0.407**
	(2.643)	(2.729)	(2.837)	(1.996)	(2.073)	(2.209)
Peers' Size	-0.145*	-0.138*	-0.137*	-0.094	-0.094	-0.100
	(-1.693)	(-1.699)	(-1.906)	(-0.963)	(-1.006)	(-1.251)
Peers' MB Ratio	-0.000	-0.000	-0.002	0.006	0.007	0.006
	(-0.040)	(-0.016)	(-0.209)	(0.758)	(0.877)	(0.688)
Peers' Debt Ratio	0.023*	0.019	0.018	0.022	0.018	0.015
	(1.794)	(1.504)	(1.432)	(1.522)	(1.291)	(1.113)
Peers' ROA	0.006	0.003	0.007	0.015	0.013	0.015
	(0.447)	(0.310)	(0.666)	(1.045)	(1.078)	(1.264)
Peers' Net Income	-0.004	-0.017	-0.006	-0.016	-0.028	-0.026
	(-0.103)	(-0.491)	(-0.241)	(-0.389)	(-0.727)	(-0.962)
Peers' Cash Ratio	-0.070	-0.065	-0.046	-0.050	-0.053	-0.042
	(-1.482)	(-1.597)	(-1.511)	(-0.969)	(-1.171)	(-1.280)
Peers' Dividend Ratio	-0.026**	-0.024*	-0.025**	-0.023	-0.018	-0.021
	(-2.060)	(-1.747)	(-2.029)	(-1.589)	(-1.169)	(-1.520)
Peers' Inst. Ownership	0.008	-0.001	-0.003	0.008	-0.002	-0.005
	(0.855)	(-0.154)	(-0.343)	(0.764)	(-0.209)	(-0.479)
Peers' Cust. Awareness			-0.029*			-0.025
			(-1.828)			(-1.471)
Peers' R&D			-0.034			-0.014
			(-1.074)			(-0.394)
Kleiberg-Paap F-stat	78.755	65.057	64.396	58.101	47.673	50.154
First Stage Instrument	0.200***	0.204***	0.205***	0.186***	0.189***	0.195***
	(8.87)	(8.07)	(8.02)	(7.62)	(6.9)	(7.08)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Exc. Industry Peers	No	Yes	Yes	No	Yes	Yes
No. Obs	25,815	25,815	25,815	22,964	22,964	22,964

Table 3

Falsification Tests

This table shows the mean and percentiles of the distribution of placebo peer effects based on 1000 runs of model (3) in Table 2. In each run, each sample firm-year is randomly matched with another firm's direct and indirect social peers in that year. Results are shown for the cases of random matching with and without replacement. In both cases, the process is repeated 1000 times. The instrument is the average CSR score of indirect placebo peers. A firm's indirect placebo peers are thus defined as the three-digit SIC industry peers of the social peers of a randomly selected firm subject to the restrictions that the indirect peers and that firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). All regressions include all firm-level and peer-level control variables described in Appendix Table A.1. Each peer-level control variable is computed as a weighted average of that variable across a firm's real (non-placebo) social peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The coefficients are measured in standard deviation units. All regressions include state-of-incorporation-by-year fixed effects, CSA-by-year fixed effects and industry-by-year fixed effects. Standard errors are heteroskedasticity-robust and clustered at the firm-level.

	Without Replacement		With Replacement	
	Coefficient	T-statistic	Coefficient	T-statistic
Percentile 1%	-0.181	-2.192	-0.146	-2.088
Percentile 5%	-0.128	-1.612	-0.106	-1.487
Percentile 10%	-0.099	-1.268	-0.084	-1.216
Percentile 50%	0.002	0.023	0.003	0.040
Percentile 90%	0.099	1.241	0.086	1.202
Percentile 95%	0.123	1.573	0.109	1.562
Percentile 99%	0.188	2.318	0.145	2.094
Mean	0.001	0.004	0.002	0.028

Table 4

Tests of Endogenous Sorting into Networks

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores when controlling for endogenous sorting into networks. The Louvain algorithm (Blondel et al. (2008)) is used to partition the social network each year into communities of different sizes: small, intermediate and large. Communities are defined as sets of firms that are highly connected among themselves and sparsely connected to firms outside the community. Each size category corresponds to one of the three algorithm recursions needed for convergence. These communities are used to cluster standard errors and define community-by-year fixed effects. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at either the firm-level or the community-by-year level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Small Network Communities (First Pass Algorithm) (1)	Intermediate Network Communities (Second Pass Algorithm) (2)	Large Network Communities (Third Pass Algorithm) (3)	Small Network Communities (First Pass Algorithm) (4)	Intermediate Network Communities (Second Pass Algorithm) (5)	Large Network Communities (Third Pass Algorithm) (6)
Peers' CSR	0.538***	0.454***	0.442***	0.538***	0.454***	0.442***
	(3.055)	(2.802)	(2.761)	(4.391)	(3.576)	(3.301)
Kleiberg-Paap F-stat	61.454	65.340	65.597	97.583	98.754	72.701
First-Stage Instrument	0.204***	0.205***	0.207***	0.204***	0.205***	0.207***
	(7.84)	(8.08)	(8.10)	(9.88)	(9.94)	(8.53)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Community-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
Firm Clustered SEs	Yes	Yes	Yes	No	No	No
Community Clustered SEs	No	No	No	Yes	Yes	Yes
No. Community-Years	823	58	18	823	58	18
No. Obs	25,887	25,945	25,945	25,887	25,945	25,945

Table 5

Social Peer Effects of CSR by Network Type

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores per network type. Column (1) corresponds to the case in which only the current employment network is used to define direct social peers. The remaining columns correspond to cases in which networks are constructed by setting aggregate social connections to zero whenever a firm-pair has an active social connection of one of the following types: current employment (*Excluding CE*), education (*Excluding Edu*), other activities (*Excluding OA*) and past employment (*Excluding PE*). The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	CE only (1)	Excluding CE (2)	Excluding Edu (3)	Excluding OA (4)	Excluding PE (5)
Peers' CSR	-0.122	-0.185	0.189*	0.121	0.419***
	(-1.140)	(-1.070)	(1.732)	(1.000)	(2.663)
Kleiberg-Paap F-stat	78.199	58.276	105.111	75.982	62.983
First Stage Instrument	0.191***	0.177***	0.216***	0.202***	0.200***
	(8.840)	(7.630)	(10.250)	(8.720)	(7.940)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs	21,067	23,401	24,545	24,575	25,768

Table 6

Impact of the Approval of Close-call CSR Proposals on the CSR scores of Social Peers

This table reports the dynamic regression discontinuity design estimates of the impact of the approval of close-call CSR policies on the CSR scores of social peers. Social peers are defined as firms that are not part of the same three digit SIC industry and that are socially connected through current employment, past employment, education or other activities. The estimates come from global polynomial regressions with polynomial degree of either one, two or three. All regressions include time-elapsed-since-proposal (*T.E.S.P.*), firm-by-proposal and calendar year fixed effects. The controls included are all the firm-specific controls detailed in Appendix Table A.1. The coefficients are measured in standard deviation units. *T*-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Polynomial Order 1		Polynomial Order 2		Polynomial Order 3	
	(1)	(2)	(3)	(4)	(5)	(6)
CSR Score$_t$	0.111***	0.112***	0.168***	0.168***	0.113***	0.112***
	(5.826)	(5.814)	(5.824)	(5.811)	(3.860)	(3.827)
CSR Score$_{t+1}$	0.048*	0.047*	0.080*	0.079*	0.040	0.037
	(1.688)	(1.685)	(1.781)	(1.777)	(0.761)	(0.716)
CSR Score$_{t+2}$	0.109***	0.109***	0.149***	0.150***	0.088	0.087
	(3.925)	(3.994)	(3.005)	(3.045)	(1.553)	(1.561)
CSR Score$_{t+3}$	0.043	0.045	0.070	0.071	-0.024	-0.023
	(1.344)	(1.395)	(1.329)	(1.363)	(-0.385)	(-0.378)
T.E.S.P. FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-by-Proposal FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial Order	1	1	2	2	3	3
No. Obs	119,870	119,870	119,870	119,870	119,870	119,870

Table 7

Social Learning Channel

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. The magnitude of peer effects is allowed to vary as a function of either firm-level CEO delta, CEO vega or fraction of independent directors on the board. D_{High} (D_{Med}) are binary indicators equal to unity if one of these variables is larger or equal than the 80th percentile (between the 80th and the 50th percentile) of the within-year distribution of that variable. D_{Low} is equal to one for the remaining observations. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness, R&D investment and the following local network measures of social capital: organ donation density, voter turnout, registered organization density and association density. The coefficients are measured in standard deviation units. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Levels			First Differences		
	Delta (1)	Vega (2)	Frac.Indep. (3)	Delta (4)	Vega (5)	Frac.Indep. (6)
Peers' CSR × D_{Low}	0.459	0.39	0.349	-0.005	-0.047	-0.047
	(1.433)	(1.185)	(1.165)	(-0.029)	(-0.262)	(-0.271)
Peers' CSR × D_{Med}	0.567*	0.563*	0.376	0.207	0.083	0.019
	(1.889)	(1.892)	(1.284)	(1.310)	(0.505)	(0.114)
Peers' CSR × D_{High}	0.686**	0.794***	0.676**	0.339**	0.464***	0.359**
	(2.243)	(2.790)	(2.473)	(2.020)	(2.937)	(2.253)
Sanderson-Windmeijer F-Stat						
Ind. Peers' CSR × D_{Low}	23.47***	22.12***	27.88***	32.2***	31.62***	34.64***
Ind. Peers' CSR × D_{Med}	24.07***	24.23***	31.39***	32.45***	34.93***	35.36***
Ind. Peers' CSR × D_{High}	25.41***	24.48***	28.89***	37.06***	33.94***	35.27***
First Stage Instrument						
Ind. Peers' CSR × D_{Low}	0.763***	0.421***	0.444***	0.508***	0.458***	0.44***
	(14.84)	(9.64)	(9.8)	(14.75)	(13.13)	(13.5)
Ind. Peers' CSR × D_{Med}	0.679***	0.693***	0.546***	0.661***	0.647***	0.569***
	(16.52)	(18.54)	(18.16)	(19.54)	(21)	(20.87)
Ind. Peers' CSR × D_{High}	0.496***	0.835***	0.663***	0.711***	0.76***	0.71***
	(11.37)	(25.03)	(22.67)	(25.89)	(28.45)	(31.61)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs	13,847	14,056	14,200	12,586	12,749	13,207

Table 8

Identity Economics Channel

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. The magnitude of peer effects is allowed to vary as a function of each firm's local network social capital. Local network social capital is measured as the peer group average, including the firm itself, of one of the following variables: organ donation density, voter turnout, registered organization density, association density or the principal component of the previous three variables. D_{High}, D_{Med} and D_{Low} are binary indicators equal to unity if the associated local network social capital variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the *p*-value obtained from testing the hypothesis that peer effects in the highest tercile are equal to the peer effects in the lowest tercile. The Sanderson-Windmeijer *F*-stat refers to the Sanderson and Windmeijer (2016) weak instrument *F*-test for models with multiple endogenous variables. *T*-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Local Network Social Capital				
	Organ Don. (1)	Voter Turnout (2)	Reg. Density (3)	Org. Density (4)	PC Index (5)
Peer's CSR × D_{Low}	0.446***	0.482***	0.467***	0.489***	0.495***
	(2.713)	(3.149)	(2.825)	(3.065)	(3.120)
Peer's CSR × D_{Med}	0.557***	0.521***	0.550***	0.508***	0.583***
	(3.864)	(3.708)	(3.845)	(3.738)	(4.198)
Peer's CSR × D_{High}	0.520***	0.545***	0.516***	0.534***	0.486***
	(3.341)	(3.113)	(3.678)	(3.405)	(3.123)
P(H = L)	0.3001	0.4273	0.4455	0.5838	0.9033
Sanderson-Windmeijer F-Stat					
Ind. Peer's CSR × D_{Low}	76.46***	82.87***	80.85***	84.78***	85.95***
Ind. Peer's CSR × D_{Med}	87.74***	86.48***	89.84***	103.79***	95.39***
Ind. Peer's CSR × D_{High}	98.97***	69.55***	93.62***	89.99***	88.27***
First Stage Instrument					
Ind. Peer's CSR × D_{Low}	0.457***	0.504***	0.418***	0.47***	0.472***
	(17.54)	(16.68)	(15.05)	(15.31)	(15.15)
Ind. Peer's CSR × D_{Med}	0.546***	0.611***	0.584***	0.516***	0.483***
	(23.05)	(29.08)	(26.98)	(20.66)	(19.51)
Ind. Peer's CSR × D_{High}	0.399***	0.35***	0.524***	0.415***	0.473***
	(15.74)	(11.5)	(16.91)	(13.24)	(16.18)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs	25,472	25,472	25,472	25,472	25,472

Appendix Table A.1

Variable Definitions

This table provides the definitions and data sources of the variables used throughout the paper.

Corporate Social Responsibility Variables	
CSR score	Sum of KLD strengths over the following categories: employee relations, community relations, environment and workforce diversity. The score is normalized by the possible number of strengths for each firm-year and ranges between one and five. Sourced from the MSCI ESG Stats Database (formerly known as Kinder, Lydenberg and Domini & Co. (KLD))
Firm-Level Control Variables	
Size	Natural logarithm of total assets in millions of dollars (Compustat item AT).
MB Ratio	Market value of equity (Compustat item PRCC_F) divided by the book value of equity (Compustat item BKVLPS).
Debt Ratio	Total long-term debt (Compustat ites DLTT plus DLC) divided by total assets (Compustat item AT).
ROA	Income before extraordinary items (Compustat item IB) divided by total assets (Compustat item AT).
Net Income	Net income before extraordinary items and discontinued operations in millions of dollars (Compustat item XSGA).
Cash Ratio	Cash balances (Compustat item CHE) divided by total assets (Compustat item AT).
Dividend Ratio	Cash dividends (Compustat items DVC plus DVP) divided by total assets (Compustat item AT).
Customer Awareness	Cost of advertising media (radio, television, newspapers, periodicals) and promotional expenses (Compustat item XAD) divided by total assets (Compustat item AT)
R&D	Stock of research and development expenses computed by capitalizing firms R&D expenses following the method of Shen, Tang, and Zhang (2019).
Institutional Ownership	Fraction of firm stock owned by institutional investors (Thomson Reuters).

Appendix Table A.1 (Continuation) This table provides the definitions and data sources of the variables used throughout the paper.

Peer-Level Control Variables	For each firm-level control variable, there is a corresponding peer-level control variable constructed as a weighted average of the values of the firm-level control variable across all of a firm's social peers, excluding the firm itself. The weights are the normalized strengths of social connections between firm-pairs in a given year.
Other Variables	
CEO Delta	Change in the dollar value of the CEO's stock and option portfolio for a one percentage point change in stock price. Sourced from Lathita Naveen's website.
CEO Vega	Change in the dollar value of the CEO's stock and option portfolio for a 0.01 change in the annualized standard deviation of stock returns. Sourced from Lathita Naveen's website.
Fraction of Independent Directors	Fraction of independent directors on the board. Sourced from ExecuComp.
Organ Donation Density	Number of organ donations per capita in each state, calculated using data from the Organ Procurement and Transplantation Network (OPTN).
Registered Organization Density	Number of tax-exempt non-profit organizations per capita in each county, sourced from the National Center for Charitable Statistics (NCCS).
Association Density	Number of non-profit and/or recreational associations per capita in each US county, obtained from the County Business Patterns (CBP) compiled by the Census Bureau. The following association types are included: civic and social organizations, bowling centers, gold course and country clubs, fitness and recreational centers, sports teams and clubs, religious organizations, political organizations, labor unions and similar labor organizations, business associations and professional organizations.
Voter Turnout	Ratio of the number of votes cast in the closest presidential election to the population eligible to vote. Data on votes cast and population eligible to vote are obtained from the American Community Survey and the MIT Election Lab, respectively.
Principal Component Index	Principal component of registered organization density, association density and voter turnout. Following Lin and Pursiainen (2018), the principal component is computed for each year separately after standardizing and winsorizing each variable each year at 1% and 99% levels.

Internet Appendix to
"Social Networks and Corporate Social Responsibility"

1 Details on Social Network Construction

To ensure that the analysis is free of survivorship bias, I follow a network construction approach similar to that of Engelberg, Gao, and Parsons (2012). First, since Boardex coverage is very limited before 2000, I impose that the sample period runs from 2001 until 2016. Second, BoardEx does not provide CUSIP or ticker symbol information for inactive firms. For these cases, I use the Levenshtein (1966) textual algorithm to match the names of inactive firms to firm names in the Compustat Funda tables. The algorithm is applied several times until no further matches are identified. All matches are manually checked to avoid errors.

Following Fracassi (2017), I define top executives as the top five executives based on compensation data from ExecuComp. Since there is no common individual-level identifier between Boardex and ExecuComp, I employ the Levenshtein (1966) algorithm again to textually match executives names in each firm-year. All the matches are manually checked. One difficulty is that the names in the two databases often come in different formats. For instance, one database might use the nickname Chuck Smith to refer to Charles Smith or Doctor Smith in the other database, in which case the textual algorithm may fail to produce a correct match. It is also frequent that surnames of female executives change or that one of the databases does not clearly distinguish between members of the same family (e.g. James Smith can refer to James Smith Jr. or James Smith Sr.). I resolve these ambiguities by manually searching for name and professional history information in LinkedIn, company websites, SEC reports, Bloomberg executives profiles and news articles. Since the ExecuComp universe is restricted to the S&P 1500, I cannot identify the top five executives by compensation for all Russell 3000 sample firms. In those cases, I define the top executives to be the CEO, CFO and COO of the firm. My final sample consists of 83,604 individuals based on which I construct 83,604-by-83,604 time-varying adjacency matrices.

In constructing the education network, I follow the general strategy of Engelberg, Gao, and Parsons (2012), but with a few differences worth noting. As a first step, I assign each of the several thousands of degree descriptions into 7 categories: (i) undergraduate, (ii) masters and non-research post-graduate degrees, (iii) MBA, (iv) PhD and post-doc, (v) non-research law degrees, (vi) medical degrees, (vii) and other qualifications.[1] I exclude online programs and short-term certifications that only require a few days or weeks of contact hours. Since many short-term courses are repeated several times within a year for different cohorts, it is often impossible to assign names to cohorts and infer social connections.

In the second step, I map each institution into a unique identifier to correct for the fact that Boardex assigns different names and abbreviations to the same institution.[2] I further account for name changes by checking the history of each institution and I conduct international translations whenever necessary.[3] I also assign university research centers to the respective university campus. For example, the Carolina Center for Genome Sciences is assigned to the University of North Carolina at Chapel Hill, one of the 17 campuses of the University of North Carolina system.[4]

In the third step, I refine the matching by excluding ambiguous cases. For instance, I exclude the Indian Institute of Technology since there are 23 campuses in 23 states across India. However, when information is available elsewhere (e.g. Bloomberg Executives Profile or LinkedIn), I use that information to pin down the specific campus or college where an

[1]Unlike Engelberg, Gao, and Parsons (2012), I create a specific category for medical degrees. The motivation is that the facilities where medical students are trained are often geographically separate from those of non-medical students, thus diminishing the chance of meaningful social interaction.

[2]For instance, I assign KU Leuven to the same identifier as the Catholic University of Leuven.

[3]For example, Arthur D. Little School of Management was renamed Hult Business School in 2003 and Rensselaer's Education for Working Professionals was known as Hartford Graduate Center. The Academie du Droit Internationale de la Haye is assigned the same identifier as Hague Academy of International Law

[4]Due the extensive amount of manual matching involved in the construction of the education network, the matching is done twice, once by me and once by a research assistant. I then compare the output of both matches and correct mistakes.

individual studied. I exclude cases in which the link between an individual and an academic institution takes the form of a fellowship instead of a degree. I also ignore academic links to professional organizations such as the American Academy of Forensic Sciences.[5]

2 Detailed Description of Social Capital Proxies

Association density is the number of non-profit and/or recreational associations per capita in each US county, obtained from the County Business Patterns (CBP) compiled by the Census Bureau. The following association types are included: civic and social organizations, bowling centers, golf courses and country clubs, fitness and recreational centers, sports teams and clubs, religious organizations, political organizations, labor unions and similar labor organizations, business associations and professional organizations. Registered organization density is the number of tax-exempt non-profit organizations per capita in each county, sourced from the National Center for Charitable Statistics (NCCS). Voter turnout is the ratio of the number of votes cast in the closest presidential election to the population eligible to vote. Data on votes cast and population eligible to vote are obtained from the American Community Survey and the MIT Election Lab, respectively. Organ donation density is the number or organ donations per capita in each state, calculated using data from the Organ Procurement and Transplantation Network (OPTN).

The organ donation measure has been used in previous studies in the finance and economics literature as a proxy for social capital (e.g. Guiso, Sapienza, and Zingales (2004), Buonanno, Montolio, and Vanin (2009) and Hasan, Hoi, Wu, and Zhang (2017b)). The first three measures build on the work of Putnam (2000) in the sociology literature and were introduced in the finance and economics literature by Rupasingha, Goetz, and Freshwater (2006) in the form of a social capital index. Since then, this index has dominated the em-

[5]Such institutions do not grant degrees and often have members spread over many states and even countries. In addition, BoardEx does not provide information about whether individuals are active or not in these organizations, making it impossible to define meaningful social connections.

pirical literature on social capital (e.g. Hasan, Hoi, Wu, and Zhang (2017a), Hasan, Hoi, Wu, and Zhang (2017b), Lin and Pursiainen (2018)). The social capital index is constructed as the first principal component of the three measures and the county-level response rate to the Census Bureau's decennial census. I deviate from this methodology because, as pointed out by Lin and Pursiainen (2018), the index comes with several methodological shortcomings. These include the variables being contaminated by significant outliers (e.g. voting rates higher than 100%) and the data not being available on a yearly basis (thus requiring data extrapolation across years).

Following Lin and Pursiainen (2018), I only use variables for which yearly data is available (thus justifying only using three out of the four original variables of Rupasingha, Goetz, and Freshwater (2006)) and I transform the data in two ways. First, I winsorize each variable at the 1% and 99% levels within each year to remove the influence of extreme observations. Second, I standardize the variables within each year to capture cross-sectional differences in social capital as opposed to trends.

Lin and Pursiainen (2018) then proceed by computing the within-year principal component of the three measures. Unlike Lin and Pursiainen (2018), I use the individual measures for two reasons. First, I allow for the fact that these measures capture different dimensions of social capital. Regional and organizational density capture the frequency of social interactions and proxy for the existence of dense networks that enforce cooperation (Putnam (2000)). Voter turnout and organ donation, however, are measures of civic engagement (Scrivens and Smith (2013)).[6] There is, to the best of my understanding, little motivation to ignore the possibility that different dimensions of social capital matter in different settings. Second, the measure of organizational density has received some criticism as it ignores the emergence of new forms of organizations and technologies that sustain interpersonal networks (e.g. Sobel (2002)). Therefore, variation in these measures may reflect a substitution between types of

[6] Refer to Scrivens and Smith (2013) for an in-depth discussion on the different dimensions of social capital and corresponding empirical measures.

organization instead of actual changes in social capital. Hence, given these concerns, using a principal component methodology may mask important dynamics. Nevertheless, I show that the results are robust to using the principal component approach.

Finally, as in the extant literature, I obtain firm-specific measures of social capital by assigning county-level measures of social capital to each firm based on the county where the firm is headquartered. A drawback of this measure is that it does not account for the fact that many of a firms' social peers are headquartered in a different county. Therefore, it may be a poor proxy for amount of social capital in a firms' social network. To account for this possibility, I also create a measure of a firm-specific local network social capital (as opposed to own social capital) by averaging the social capital of all the peers of that firm, including the firm itself.

Table IA.1

Robustness Checks of Baseline Results

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores under a variety of different specifications. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Panel A: Alternative Definitions of Peer Groups			
	Excludes Firms with $<$ 10 peers (1)	Excludes Firms with $>$ 250 peers (2)	Excludes Indirect Peers in Firm's SIC1 Industry (3)	Excludes Indirect Peers in Firm's Headquarter State (4)
Peers' CSR	0.708***	0.468***	0.465***	0.388**
	(2.998)	(2.878)	(2.759)	(2.407)
Kleiberg-Paap F-stat	91.685	63.961	74.872	66.902
CSA-by-year FE	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes
Incorporation-State-by-year FE	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes
No. Obs	23,384	25,574	25,548	25,815

	Panel B: Additional Combinations of Fixed Effects And Lagged Specifications			
	Headquarter State-by-Year FE (1)	Firm and Year FE (2)	Lagging the Instrument (3)	Lagging the Instrument and Controls (4)
Peers' CSR	0.273**	0.166**	0.607**	0.612**
	(2.310)	(2.308)	(2.249)	(2.102)
Kleiberg-Paap F-stat	81.176	132.780	31.492	27.289
CSA-by-year FE	No	No	Yes	Yes
Industry-by-year FE	Yes	No	Yes	Yes
Incorporation-State-by-year FE	Yes	No	Yes	Yes
Headquarter-State-by-Year FE	Yes	No	No	No
Firm and Year FE	No	Yes	No	No
Additional Controls	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes
Lagged Controls	No	No	No	Yes
Lagged Instrument	No	No	Yes	Yes
No. Obs	26,912	27,817	22,964	22,964

Table IA.2

Robustness Checks of Baseline Results: Sample Period from 2001 to 2013

This table reports the results of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores when restricting the sample period to 2001-2013. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. The Kleiberg-Paap F-stat is the cluster-robust Kleibergen and Paap (2006) F-statistic for weak instruments. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Contemporaneous			Lagged		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' CSR	0.574***	0.607***	0.603***	0.578**	0.626**	0.637**
	(2.599)	(2.610)	(2.876)	(2.061)	(2.063)	(2.299)
Peers' Size	-0.231**	-0.233**	-0.215**	-0.206	-0.218	-0.207*
	(-2.031)	(-2.036)	(-2.394)	(-1.481)	(-1.510)	(-1.856)
Peers' MB Ratio	-0.004	-0.004	-0.005	-0.000	-0.000	-0.002
	(-0.525)	(-0.477)	(-0.591)	(-0.055)	(-0.049)	(-0.268)
Peers' Debt Ratio	0.038**	0.031**	0.027**	0.033**	0.027*	0.023
	(2.531)	(2.152)	(1.998)	(1.973)	(1.667)	(1.459)
Peers' ROA	0.009	0.003	0.003	0.025	0.020	0.019
	(0.612)	(0.232)	(0.291)	(1.591)	(1.512)	(1.552)
Peers' Net Income	-0.019	-0.034	-0.014	-0.021	-0.040	-0.025
	(-0.387)	(-0.702)	(-0.435)	(-0.386)	(-0.704)	(-0.728)
Peers' Cash Ratio	-0.096*	-0.091*	-0.060*	-0.093	-0.093	-0.066
	(-1.687)	(-1.720)	(-1.716)	(-1.330)	(-1.411)	(-1.566)
Peers' Dividend Ratio	-0.037**	-0.034**	-0.035**	-0.033*	-0.026	-0.029*
	(-2.384)	(-2.029)	(-2.449)	(-1.916)	(-1.357)	(-1.755)
Peers' Inst. Ownership	0.003	-0.007	-0.009	0.006	-0.005	-0.008
	(0.303)	(-0.709)	(-0.948)	(0.541)	(-0.436)	(-0.782)
Peers' Cust. Awareness			-0.032*			-0.035
			(-1.802)			(-1.635)
Peers' R&D			-0.062			-0.057
			(-1.334)			(-0.940)
Kleiberg-Paap F-stat	45.135	36.263	42.609	32.61	25.988	31.279
First Stage Instrument	0.173***	0.168***	0.184***	0.154***	0.146***	0.159***
	(6.72)	(6.02)	(6.53)	(5.71)	(5.10)	(5.59)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	No	Yes	No	No	Yes
Ex. Industry Peers	No	Yes	Yes	No	Yes	Yes
No. Obs	21,528	21,528	21,528	18,740	18,740	18,740

Table IA.3

Regression Discontinuity Design Test of Pre-treatment Differences in Treatment and Control Groups

This table tests the validity of the regression discontinuity design by comparing whether or not treated and control firms are fundamentally different in terms of several firm-level characteristics. The characteristics are measured in the year prior to the vote date. The composition of treatment and control group is defined in terms of different bandwidths around the voting threshold: 10%, 5%, 2.5% and 1%. The table reports the difference in means among the two groups for each variable as well as the t-statistics associated with the test of the null hypothesis that there is no difference between treatment and control group. The row *Benjamini-Hochberg* indicates the number of firm characteristics that are statistically significant after correcting for multiple hypotheses testing by applying the Benjamini and Hochberg (1995) procedure under the assumption of a 5% false discovery rate. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Bandwidth Around Threshold							
	1%		2.5%		5%		10%	
Characteristics	Diff.	T-stat	Diff.	T-stat	Diff.	T-stat	Diff.	T-stat
Size	0.132	(1.115)	-0.041	(-0.599)	-0.019	(-0.297)	-0.278***	(-5.606)
MB Ratio	-0.058	(-0.401)	0.075	(0.898)	0.109	(1.431)	0.09	(1.541)
Debt Ratio	0.016	(0.128)	0.062	(0.883)	0.103	(0.362)	0.016	(0.321)
ROA	-0.223**	(-2.093)	0.062	(-0.527)	0.081	(-0.628)	-0.08*	(-1.842)
Net Income	0.091	(0.614)	-0.017	(-0.201)	0.03	(0.383)	-0.321***	(-5.035)
Cash Ratio	0.126	(1.14)	0.062	(0.988)	0.072	(1.242)	0.096**	(2.191)
Dividend Ratio	-0.068	(-0.514)	-0.007	(-0.1)	-0.007	(-0.098)	-0.036	(-0.665)
Inst. Ownership	-0.153	(-1.285)	0.033	(0.487)	0.021	(0.327)	0.05	(1.016)
Cust. Awareness	-0.039	(-0.314)	-0.083	(-1.164)	-0.06	(-0.915)	-0.014	(-0.275)
R&D	0.108	(0.802)	-0.036	(-0.464)	0.001	(0.018)	-0.341***	(-5.503)
Benjamini-Hochberg	0/10		0/10		0/10		3/10	
No. Obs	2,597		2,752		2,804		3,050	

Table IA.4

Impact of the Approval of Close-call CSR Proposals on the CSR scores of Social Peers: Robustness Check using Alternative Running Variable

This table reports the dynamic regression discontinuity design estimates of the impact of the approval of close-call CSR policies on the CSR scores of social peers. Social peers are defined as firms that are not part of the same three digit SIC industry and that are socially connected through current employment, past employment, education or other activities. The estimates come from global polynomial regressions with polynomial degree of either one, two or three. The running variable is defined based on the most extreme outcomes of peer proposals votes. If the firm is in the treatment group, the running variable is equal to the largest distance to the threshold across all peer proposals that pass. If a firm is in the control group, the running variable is equal to the lowest distance to the threshold across all peer proposals that fail. All regressions include time-elapsed-since-proposal (*T.E.S.P.*), firm-by-proposal and calendar year fixed effects. The controls included are all the firm-specific controls detailed in Appendix Table A.1. The coefficients are measured in standard deviation units. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Polynomial Order 1		Polynomial Order 2		Polynomial Order 3	
	(1)	(2)	(3)	(4)	(5)	(6)
CSR Score$_t$	0.062***	0.062***	0.147***	0.148***	0.101***	0.102***
	(3.167)	(3.161)	(4.456)	(4.473)	(3.510)	(3.571)
CSR Score$_{t+1}$	0.007	0.006	0.051	0.051	0.083*	0.081*
	(0.261)	(0.231)	(1.120)	(1.135)	(1.682)	(1.664)
CSR Score$_{t+2}$	0.059**	0.060**	0.103**	0.106**	0.106*	0.105*
	(2.058)	(2.114)	(2.095)	(2.169)	(1.947)	(1.941)
CSR Score$_{t+3}$	-0.006	-0.005	0.008	0.013	0.045	0.045
	(-0.188)	(-0.148)	(0.159)	(0.244)	(0.731)	(0.735)
T.E.S.P. FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-by-Proposal FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial Order	1	1	2	2	3	3
No. Obs	119,870	119,870	119,870	119,870	119,870	119,870

Table IA.5

Peer Effects and Own Social Capital: Levels

This table reports the output of two-stage least squares (2SLS) regressions of firm CSR scores on social peers' CSR scores. The magnitude of peer effects is allowed to vary as a function of each firm's own social capital. Own social capital is proxied by of the following variables: organ donation density, voter turnout, registered organization density, association density or the principal component of the previous three variables. D_{High}, D_{Med} and D_{Low} are binary indicators equal to unity if the associated social capital variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. $P(H = L)$ is the p-value obtained from testing the hypothesis that peer effects in the highest tercile are equal to the peer effects in the lowest tercile. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Own Social Capital				
	Organ Don. (1)	Voter Turnout (2)	Reg. Density (3)	Org. Density (4)	PC Index (5)
Peer's CSR × D_{Low}	0.597***	0.534***	0.481***	0.492***	0.505***
	(3.336)	(3.454)	(3.129)	(3.196)	(3.267)
Peer's CSR × D_{Med}	0.514***	0.512***	0.499***	0.506***	0.512***
	(3.334)	(3.404)	(3.207)	(3.296)	(3.308)
Peer's CSR × D_{High}	0.408**	0.430***	0.578***	0.559***	0.516***
	(2.541)	(2.627)	(3.733)	(3.424)	(3.311)
P(H = L)	0.1239	0.1562	0.0506	0.3324	0.8627
Sanderson-Windmeijer F-Stat					
Ind. Peer's CSR × D_{Low}	74.59***	82.35***	85.09***	82.86***	83.58***
Ind. Peer's CSR × D_{Med}	87.08***	87.09***	83.7***	82.1***	88.65***
Ind. Peer's CSR × D_{High}	96.74***	76.77***	80.73***	79.62***	84.19***
First Stage Instrument					
Ind. Peer's CSR × D_{Low}	0.385***	0.569***	0.541***	0.56***	0.555***
	(9.92)	(17.68)	(20.15)	(18.92)	(17.24)
Ind. Peer's CSR × D_{Med}	0.403***	0.543***	0.518***	0.558***	0.485***
	(11.49)	(20.2)	(18.26)	(23.26)	(17.6)
Ind. Peer's CSR × D_{High}	0.382***	0.376***	0.499***	0.414***	0.472***
	(11.84)	(12.25)	(16.34)	(12.45)	(15.78)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs	25,472	25,472	25,472	25,472	25,472

Table IA.6

Peer Effects and Local Network Social Capital: First-Differences

This table reports the output of two-stage least squares (2SLS) regressions of year-on-year changes in firm CSR scores on the changes in social peers' CSR scores. The magnitude of peer effects is allowed to vary as a function of each firm's local network social capital. Local network social capital is measured as the peer group average, including the firm itself, of one of the following variables: organ donation density, voter turnout, registered organization density, association density or the principal component of the previous three variables. D_{High}, D_{Med} and D_{Low} are binary indicators equal to unity if the associated local network social capital variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is first-differenced and included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects in the highest tercile are equal to the peer effects in the lowest tercile. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Local Network Social Capital				
	Organ Don. (1)	Voter Turnout (2)	Reg. Density (3)	Org. Density (4)	PC Index (5)
Δ Peer's CSR × D_{Low}	0.089	0.189*	0.115	0.128	0.119
	(0.862)	(1.760)	(1.065)	(1.193)	(1.069)
Δ Peer's CSR × D_{Med}	0.316***	0.301***	0.300***	0.298***	0.317***
	(3.232)	(3.117)	(3.234)	(3.026)	(3.271)
Δ Peer's CSR × D_{High}	0.190*	0.112	0.252***	0.156	0.121
	(1.650)	(1.023)	(2.729)	(1.381)	(1.107)
P(H = L)	0.1889	0.2898	0.0358	0.7172	0.9838
Sanderson-Windmeijer F-Stat					
Δ Ind. Peer's CSR × D_{Low}	99.91***	109.66***	91.36***	93.68***	91.7***
Δ Ind. Peer's CSR × D_{Med}	110.82***	107.94***	125.71***	108.27***	109.58***
Δ Ind. Peer's CSR × D_{High}	94.05***	94.58***	126.24***	109.74***	106.39***
First Stage Instrument					
Δ Ind. Peer's CSR × D_{Low}	0.473***	0.477***	0.434***	0.43***	0.42***
	(18.94)	(18.81)	(17.38)	(15.48)	(14.99)
Δ Ind. Peer's CSR × D_{Med}	0.559***	0.582***	0.555***	0.56***	0.546***
	(29.29)	(27.11)	(30.87)	(23.75)	(26.15)
Δ Ind. Peer's CSR × D_{High}	0.426***	0.448***	0.558***	0.436***	0.466***
	(14.69)	(17.36)	(22.77)	(16.76)	(19.02)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs	22,659	22,659	22,659	22,659	22,659

Table IA.7

Peer Effects and Own Social Capital: First-Differences

This table reports the output of two-stage least squares (2SLS) regressions of year-on-year changes in firm CSR scores on the changes in social peers' CSR scores. The magnitude of peer effects is allowed to vary as a function of each firm's own social capital. Own social capital is proxied by of one of the following variables: organ donation density, voter turnout, registered organization density, association density or the principal component of the previous three variables. D_{High}, D_{Med} and D_{Low} are binary indicators equal to unity if the associated local network social capital variable in a given firm-year belongs to the third, second and first tercile of the within-year distribution of that variable, respectively. The instrument is the average CSR score of indirect peers. A firm's indirect peers are defined as the three-digit SIC industry peers of the social peers of that firm subject to the restrictions that the indirect peers and the firm: (i) are in a different industry; (ii) are not social peers; (iii) are headquartered in different combined statistical areas (CSA). Every control variable is first-differenced and included in all regressions at both the firm-level and the peer-level. Each peer-level variable is computed as a weighted average of that variable across a firm's peers, excluding the firm itself. The weights are the normalized strengths of social connections between the firm and each of its social peers. The additional controls are customer awareness and R&D investment. The coefficients are measured in standard deviation units. *P(H = L)* is the p-value obtained from testing the hypothesis that peer effects in the highest tercile are equal to the peer effects in the lowest tercile. The Sanderson-Windmeijer F-stat refers to the Sanderson and Windmeijer (2016) weak instrument F-test for models with multiple endogenous variables. T-statistics are reported in parentheses. Standard errors are heteroskedasticity-robust and clustered at the firm-level. ***, **, * denotes statistical significance at the 1%, 5% and 10% level.

	Own Social Capital				
	Organ Don. (1)	Voter Turnout (2)	Reg. Density (3)	Org. Density (4)	PC Index (5)
Δ Peer's CSR × D_{Low}	0.205*	0.226**	0.185*	0.165	0.185*
	(1.933)	(2.159)	(1.866)	(1.583)	(1.778)
Δ Peer's CSR × D_{Med}	0.238**	0.172*	0.096	0.199*	0.143
	(2.283)	(1.705)	(0.903)	(1.935)	(1.349)
Δ Peer's CSR × D_{High}	0.094	0.136	0.263**	0.205*	0.223**
	(0.798)	(1.218)	(2.520)	(1.880)	(2.058)
P(H = L)	0.1756	0.2384	0.1248	0.5573	0.5862
Sanderson-Windmeijer F-Stat					
Δ Ind. Peer's CSR × D_{Low}	110.38***	101.64***	94.81***	95.8***	101.04***
Δ Ind. Peer's CSR × D_{Med}	110.68***	108.32***	105.11***	101.41***	96.82***
Δ Ind. Peer's CSR × D_{High}	87.11***	98.21***	103.85***	111.03***	103.78***
First Stage Instrument					
Δ Ind. Peer's CSR × D_{Low}	0.516***	0.54***	0.55***	0.54***	0.518***
	(17.94)	(19.92)	(22.99)	(20.05)	(18.53)
Δ Ind. Peer's CSR × D_{Med}	0.477***	0.557***	0.512***	0.555***	0.49***
	(15.7)	(23.89)	(20.06)	(25)	(18.47)
Δ Ind. Peer's CSR × D_{High}	0.394***	0.419***	0.542***	0.482***	0.523***
	(12.3)	(16.76)	(25.23)	(20.1)	(24.58)
CSA-by-year FE	Yes	Yes	Yes	Yes	Yes
Industry-by-year FE	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	Yes	Yes
Firm-Level Controls	Yes	Yes	Yes	Yes	Yes
Peer-Level Controls	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
Ex. Industry Peers	Yes	Yes	Yes	Yes	Yes
No. Obs	22,659	22,659	22,659	22,659	22,659

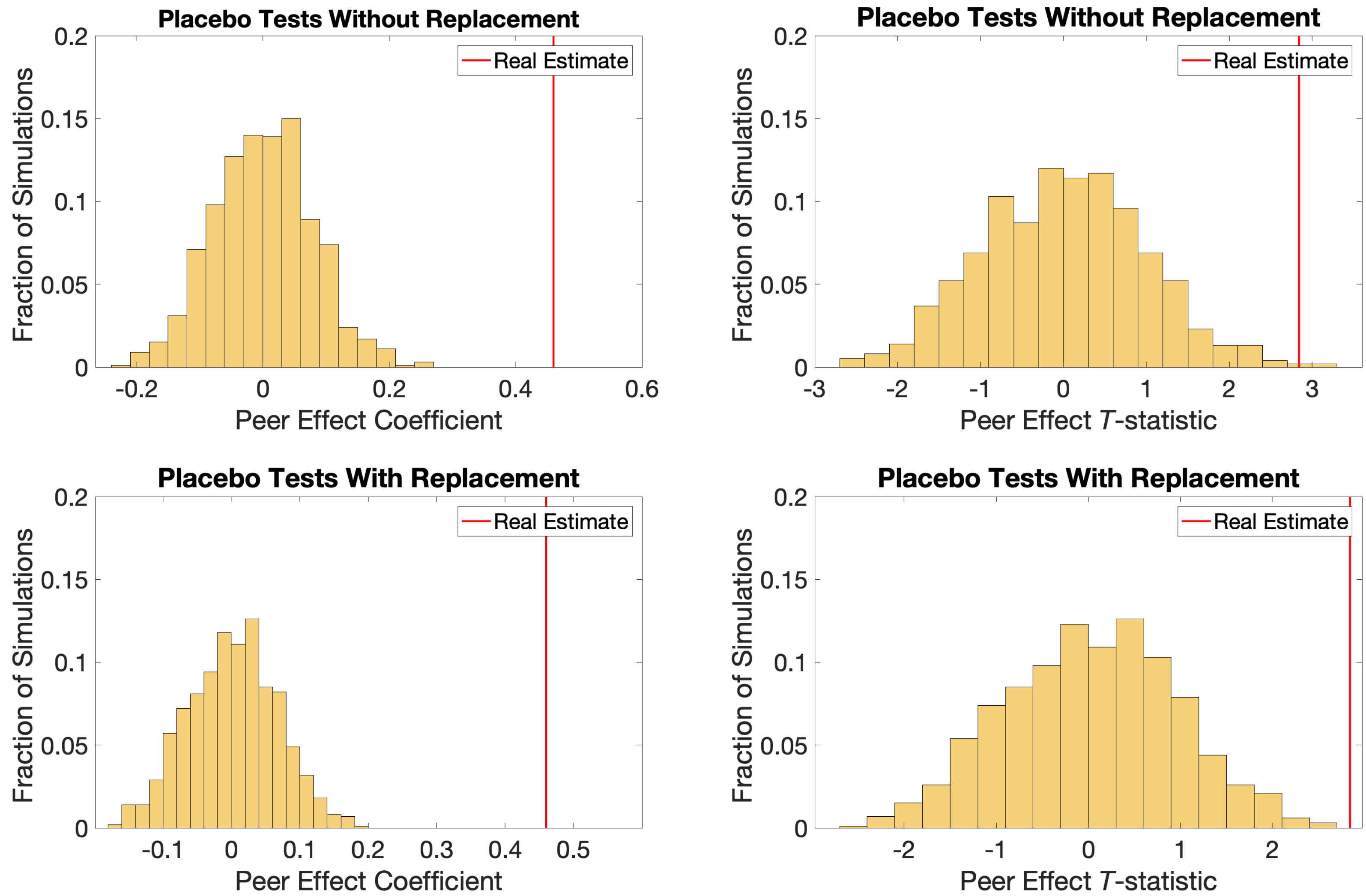

Figure IA.1. Distribution of Placebo CSR Peer Effect Estimates.
These histograms show the distribution of placebo CSR peer effect estimates and associated t-statistics obtained from 1000 runs of model (3) in Table 2. In each run, the direct and indirect peer groups of each firm are randomly matched to a firm that is active in that year. The top plots are obtained from simulations with random matching without replacement. The bottom plots are obtained from simulations with random matching with replacement. The red lines indicate the location of the real (non-placebo) estimates.