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ABSTRACT

This dissertation consists of three chapters. First two chapters examines how nonprofit organizations (NPOs) react to the state level minimum wage increases, and the third chapter studies the effect of board interlock on the diffusion of innovation.

In the first chapter, I investigate the impact of minimum wage increases on employment. I extend the literature by hypothesizing and showing a differential impact of state-level minimum wage increases on nonprofit organizations relative to for-profit organizations. While I find that increases in minimum wages reduce employment growth in both types of organizations, this decrease is substantially larger for nonprofit organizations. I also find that investment in automation, i.e., information technology, rises in nonprofits post minimum wage increase, consistent with the substitution of capital for labor. Minimum wage increases also increase the likelihood of nonprofit exit.

In the second chapter, I investigate how CEO pay in nonprofit organizations responds to an exogeneous increase in labor cost resulting from state-level minimum wage hikes. I find that these increases in labor cost, which constrain budgets, are followed by declines in the total pay of NPO CEOs. In contrast, I do not find an impact on CEO pay in for-profit companies. I attribute the differential response between NPO and for-profit organizations to NPO CEOs acting as stewards of the NPO, whereby they are willing to take less to ensure the continued existence of the enterprise, as well as fulfillment of its mission. This phenomenon has previously been observed in the nonprofit sector and termed labor donation, whereby individuals who work for NPOs are intrinsically motivated and consequently, are willing to work for less money. Cross-sectionally I find the declines in compensation are larger in NPOs headquartered in smaller counties, in counties with higher levels of religiosity, and in counties with greater levels of social capital, and in NPOs that are run by their founders.

In the last chapter, I propose that board interlocks can act as a channel of information transmission and social learning, hence enhancing the diffusion of innovation among firms. I find that a firm's patents are more likely to be cited by patents from firms that have common directors (i.e., interlocked firm). The result is robust under a difference-in-differences setting, where the death or retirement of interlocking directors is used as an exogeneous shock to board interlock. The effect is more pronounced for interlocking directors who have longer experience in R&D-intensive industries, have a larger network, and have a higher compensation delta. While I find that board interlock enhances the diffusion of innovation across industries, it has no effect on within-industry knowledge diffusion. Finally, I document that board interlock enhances firms' overall innovation output, measured by patent counts and citation counts per patent. The paper sheds light on an important role played by board of directors in promoting knowledge spillover and innovation.

DEDICATION

I would like to dedicate this dissertation to my loving mom and dad

for three decades of support.

Oh, and,

to my joyful girlfriend for always keeping my sanity in check.

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CHAPTER 1

THE EFFECT OF STATE MINIMUM WAGE INCREASES ON NONPROFIT ORGANIZATIONS

"the Raise the Wage Act of 2021 … would raise the federal minimum wage, in annual increments, to $15 per hour by June 2025 and then adjust it to increase at the same rate as median hourly wages." **The congressional budget office estimates that "*Employment would be reduced by 1.4 million workers, or 0.9 percent*".** [1]

The Budgetary Effects of the Raise the Wage Act of 2021. February 2021

Introduction

While economic theory suggests that an increase in the cost of an input (e.g., the minimum wage) should result in a decrease in its usage (e.g., employment and employment growth), prior research has found mixed results. For example, two recent papers find inconsistent effects: Cengiz Dube, Lindner, and Zipperer (2019) conclude that minimum wage increases have no effect on employment, whereas Clemens and Wither (2019) find that minimum wage increases have significant and negative effects on the employment and income of affected workers. These mixed results are consistent with prior research (e.g., Aaronson and French 2007; Dube, Lester, and Reich 2010; Meer and West 2013; Neumark, Salas and Wascher 2014; Dube, Lester, and Reich 2016; Aaronson and Phelan 2017) that was almost entirely conducted economy-wide, or specifically on for-profit firms.

I extend this research by examining the impact of minimum wage increases in the nonprofit sector, an important sector that contributes roughly $2 trillion to the U.S. economy and employs more than one in 10 workers. I expect that the impact of minimum wage increases on the nonprofit

[1] Congressional Budget Office, The Budgetary Effects of the Raise the Wage Act of 2021. February 2021. Downloaded on March 22, 2021, from https://www.cbo.gov/system/files/2021-02/56975-Minimum-Wage.pdf

sector will differ from, and potentially be greater than, such an impact on for-profit businesses.[2] For example, while economic theory suggests that for-profit firms will substitute capital for labor when the price of the latter increases, the nonprofit environment is richer, e.g., minimum wage increases might be met with substitution of unpaid volunteers for paid employees.[3] Harasztosi and Lindner (2019), among others, suggest that consumers and firm owners share the costs associated with an increase in the minimum wage. However, nonprofit organizations have no owners, and in some cases, do not charge for their services. While a profitable organization can absorb some increased labor costs in the form of a lower profit margin, many nonprofits cannot, since they operate at or close to breakeven (Leone and Van Horn 2005), possibly leading to a greater impact on employment and other aspects of operations.

The effect of a minimum wage increase is particularly relevant to nonprofit organizations, since fiscal constraints, as well as the sectors they operate in, e.g., childcare centers, home healthcare organizations, senior care provider; often cause them to pay wages at or below the proposed minimum wage.[4] Some nonprofits, e.g., Opportunity Foundation in Oregon—a nonprofit offering work opportunities to people with disabilities, have gone as far to threaten to lay off workers in response to a state-mandated increase in the minimum wage.[5] These factors, i.e., the potential to substitute volunteers for paid employees, the reduced ability to absorb increased costs, and the propensity to operate in low wage sectors of the economy, may result in a greater impact

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[2] National Council of Nonprofits, Nonprofit Impact Matters: How America's Charitable Nonprofits Strengthen Communities and Improve Lives, Fall 2019. Downloaded on August 23, 2020, from https://www.nonprofitimpactmatters.org/?utm_source=web&utm_medium=site&utm_campaign=reports-page

[3] An additional difference between nonprofit and for-profit organizations is that the impact of a minimum wage increase in after-tax dollars is greater for tax-exempt nonprofits that cannot deduct the additional cost on their tax returns.

[4] This is consistent with some for-profit research which focuses on the sectors most likely to be affected by the minimum wage increase. For example, there is a line of research focusing on the restaurant industry (e.g., Aaronson and French 2007; Dube, Lester, and Reich 2010, 2016) motivated by their reliance on low wage workers who are most likely to be affected by the minimum wage increase.

[5] https://nonprofitquarterly.org/another-threatening-missive-on-wages-from-a-disability-adjacent-nonprofit/

of minimum wage increases on nonprofit organizations. To continue to operate after a minimum wage increase, nonprofits must increase revenue and/or decrease other costs. They may decrease delivery of services; automate tasks, i.e., substitute capital for labor; and/or (unique to the nonprofit sector) substitute unpaid volunteers for paid employees.

In this paper, I extend the prior literature on the impact of minimum wage increases by examining their economic effect on 924,862 (12,744,841) unique for-profits (firm-years), and 24,378 (287,447) unique nonprofit organizations (firm-years), based on 21 state minimum wage increases between 2013 and 2015, using a difference-in-differences design for identification.[6] I find that state-level minimum wage increases reduce employment growth in both for-profits and nonprofits, with the reduction being much larger in nonprofit firms. I then dig deeper, finding that nonprofits increase investment in automation, i.e., information technology, following a minimum wage increase, which is consistent with the substitution of capital for labor. In contrast, there is no systematic impact of the minimum wage on the use of volunteers. Furthermore, I find that minimum wage increases also lead to an increase in nonprofits ceasing to operate.

I add to the literature on the impact of minimum wage increases on employment, by incorporating the nonprofit sector, providing causal evidence that minimum wage increases adversely affect employment growth in both for-profit and nonprofit organizations, and importantly, to a significantly greater extent in NPOs. I also contribute to the literature on information technology (IT) investment, by showing that IT investment increases in response to minimum wage hikes, implying that as the cost of labor increases, investment in technology becomes more cost effective. While measuring delivery of services in NPOs is difficult due to heterogeneity in the sector as well as a lack of consistent reporting, I proxy for the impact on

[6] The data runs from 2010 to 2018, but I limit the sample to wage increases between 2013 and 2015 to allow for a minimum of three years before and after the wage increases.

service delivery by examining and contributing to the literature on firm exit. I find increased NPO exit after minimum wage increases.

The remainder of the paper is organized as follows. I derive the hypotheses in section 2. Section 3 describes the sample selection, data and variables. Section 4 presents the main empirical results. Finally, section 5 concludes.

Hypothesis Development

As discussed above, there is an extensive but inconclusive literature on the impact of minimum wage increases on employment. I extend this literature to a sector of the economy, nonprofit organizations, where this impact has not been thoroughly examined and where the impact may be more pronounced. For example, unlike for-profit firms (Aaronson, French, Sorkin and To 2018; Harasztosi and Lindner 2019), nonprofits are less likely to be able to pass along the increased costs to their customers. They are also less likely to be able to absorb those costs, as nonprofits "have fewer slack or discretionary resources to address sudden needs than comparable for-profit firms" (Allard, Romich, Buszkiewicz, Althauser, and Obara 2020). Thus, their need to cut costs is likely greater than those of for-profit organizations.

Exhibit 1 illustrates the impact of the recently passed minimum wage increase on nonprofit organizations in the state of Florida. I show that the 17% increase in the minimum wage, scheduled to go into effect on September 30, 2021, turned the average operating surplus of two percent of revenue into a deficit of four percent of revenue. While in the analysis below, following prior literature, I examine significant minimum wage increases, which are defined as increases greater than 25 cents per hour in real terms, the increase in Florida was much greater – with the minimum wage increasing by $1.44 per hour on September 30, 2021. Consequently, the large impact in the exhibit will be smaller in states with smaller minimum wage increases. Regardless of the magnitude involved, labor costs will increase.

The most obvious way to offset the increase in labor costs to cut the number of employees and/or the hours those employees' work. I note that this "cut" may not be instantaneous or involve layoffs, rather it may involve a reduction in headcount via attrition, or simply lower growth in employment than might otherwise have occurred.[7]

H1: Minimum wage increases lead to decreases in employment.

Cutting the number of employees and/or the hours those employees work would adversely affect the nonprofits' ability to provide services and fulfill their mission. To maintain "output" the nonprofit could replace employees with volunteers (Bittschi, Pennerstorfer, and Schneider 2015; Allard, Romich, Buszkiewicz, Althauser, and Obara 2020), or substitute what is now more cost effective, capital.

Given that volunteers work for free, one might question why nonprofits have employees at all. A plausible explanation is that employees differ from volunteers in terms of reliability, productivity, and turnover. Nonprofits therefore optimize the ratio of volunteers to employees based upon costs. As a minimum wage increase raises the cost of employees relative to volunteers, I should see more volunteers. Handy, Mook, and Quarter (2008) find evidence of substitution between volunteers and employees, but that substitution is limited to about 12 percent of tasks. Chum, Handy, Mook, and Quarter (2013) find that 80 percent of their sample reports some interchangeability between volunteers and employees, although that interchangeability decreases with the number of employees in the organization.[8]

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[7] I examine only the impact on number of employees due to data limitations, as number of hours is not disclosed.

[8] This latter point is important because although I endeavor for the sample to be comprehensive, I miss some smaller organizations for whom e-filing was not mandatory, i.e., organizations with assets below $10 million. Thus, the sample

It is also possible that employees and volunteers are complements rather than complements. For example, paid employees recruit, train and supervise volunteers, thus fewer employees could lead to fewer volunteers. An alternative scenario leading to the same outcome, i.e., fewer volunteers, is that the increase in the minimum wage by drawing people into the workforce (Ahn, Arcidiancono and Wessels 2011; Giuliano 2013) and increasing the opportunity cost of volunteering, reduces the potential volunteer pool.

Aaronson and Phelan (2017), in their examination of the impact of minimum wage hikes, decompose their sample of low-wage occupations into those that are cognitively routine versus those that are manually routine. Only the former is susceptible to technological substitution, and in fact, only in the former do they find employment declines. Dai and Qiu (2021) show in the for-profit sector that increases in the minimum wage led to increased use of/spending on information technology. As labor costs rise, nonprofits can increase their use of information technology, for supporter engagement, fundraising, and even offering counseling through health education bots.[9] However, capital assets are utilized by employees, e.g., laptops, tablets, etc., and fewer employees require fewer capital assets. Further, to the extent that nonprofits are cash constrained, they may cut their capital budget to counteract the increase in the labor budget. This leads to the second hypothesis, which I frame in two parts, although this time I state it in the null because of the countervailing forces in play.

H2a: Minimum wage increases do not affect the use of volunteers.

H2b: Minimum wage increases do not affect the IT budget.

is biased towards larger nonprofits that, following Chum, Mook, Handy, Schugurensky, and Quarter (2013), are less likely to substitute volunteers for paid employees.

[9] As an example, Facebook Messenger is often used by nonprofits for supporter engagement, fundraising, and cultivation. See http://www.bethkanter.org/bots-nonprofits/. For a discussion of the use of information technology by nonprofits, see Hackler and Saxton (2007).

Data and Methodology

Sample Selection and Data Sources

State-level minimum wages are obtained from Vaghul and Zipperer (2016).[10] As many states permit subminimum wages under certain circumstances, I use the maximum of the minimum wages in a year for each state following Dai and Qiu (2021). As shown in Table 1-1, the final sample consists of 21 significant minimum wage increases between 2013 and 2015.[11] Though the nonprofit data described in the next paragraph is available from 2010 to 2018, I require a minimum of three years before and three years after the minimum wage increases to conduct the difference-in-differences (DiD) analysis, hence I only look at minimum wage increases from 2013 and 2015. As shown in Table 1-1, the 21 minimum wage increases occurred in 15 unique states, with six states experiencing two significant events. I treat these events as being independent; however, I cluster standard errors at the state level to control for serial correlation over time within a state.[12] Treated states are those that experience significant minimum wage increases during the 2013–2015 window. Control states are those that are: 1) located in the same census division[13] as the treatment state; and 2) do not have a minimum wage increase or have only small minimum wage increases within the seven-year window surrounding the event year.

The primary data source on nonprofits is Form 990, the informational return nonprofit organizations file with the IRS to maintain their tax-exempt status, while Infogroup is the data source on for-profit firms. For the nonprofit sample, I download Form 990 data directly from the

[10] Available at https://github.com/benzipperer/historicalminwage.
[11] As described below, following prior literature (Cengiz, Dube, Lindner, and Zipperer 2019 and Dai and Qiu 2021) I define a significant increase as one exceeding 25 cents per hour in 2018 dollars. I convert all minimum wages to 2018 dollars using the Consumer Price Index research series (CPI-U-RS) from the Bureau of Labor Statistics (BLS), then calculate the increase in real terms. The CPI-U-RS data is available at https://www.bls.gov/cpi/research-series/home.htm
[12] Results remain robust if I include states with only one minimum wage increase during the sample period.
[13] The United States is broken down into four census regions, which are further divided into 9 census divisions (https://www.bls.gov/lau/laurdqa.htm). These areas are grouped by the BLS for purposes of analysis and presentation.

IRS for all available nonprofit organizations for the years 2010–2018,[14] which results in an initial sample of 1,416,191 organization-years.[15] I limit this sample to 501(c)(3) organizations[16] that have at least three consecutive years of data and have at least ten employees in all years.[17] I also require the organization to be in either a treatment or control state. After deleting organizations with missing data or outliers following Aggarwal, Evans, and Nanda (2012),[18] I end up with a final sample of 287,447 organization-year observations.

Infogroup provides calendar year-end snapshot of local business data on millions of U.S. business establishments beginning in 1997. The dataset contains business identification, location, industry, corporation hierarchy, employment, sales, and other fields. In addition to a unique business ID, it also includes a parent company's ID for each business, which allows me to aggregate the data at the parent company level. To construct the for-profit sample, I start with 163,927,484 establishment-year observations from Infogroup from 2010 to 2018. Then, I aggregate establishment-level number of employees and sales to the parent firm-level. I require the parent? firms to be headquartered in either a treatment or a control state, have at least three consecutive years of data, and have at least ten employees in all years. Applying the

[14] I retrieve electronically filed Form 990s through Amazon Web Services (https://registry.opendata.aws/irs990/). I download all 990 filings in XML format with tax years from 2010 to 2018 and identify each variable according to the schema files provided on the IRS website (https://www.irs.gov/e-file-providers/current-valid-xml-schemas-and-business-rules-for-exempt-organizations-modernized-e-file).

[15] Not all nonprofits are required to file electronically, so the sample is biased toward larger nonprofits. A nonprofit is required to file electronically if it "files at least 250 returns of any type during the calendar year ending with or within the organization's tax year and has total assets of $10 million or more at the end of the tax year". I note that the 250-return threshold includes all filings, including wage and tax statements for employees, a.k,a., W-2's, so any nonprofit with at least 250 employees would meet this threshold.

[16] 501(c)(3) is the most common charitable designation and has been the focus of most nonprofit research.

[17] The minimum number of employees in the for-profit sample is also ten.

[18] I follow Aggarwal, Evans, and Nanda (2012) and identify observations for which one of the four variables, total assets, total compensation, total revenue, and total program expenses, falls below (above) the 1% (99%) of the sample distribution for that year. If an outlier is detected in any year, all observations for that organization are excluded from the sample.

aforementioned filters yields a sample of 12,744,841 firm-year observations in the for-profit sample.

The source for IT investment is the Ci Technology (CiTDB) from Aberdeen, an internet-based marketing firm. The Aberdeen Group surveys and interviews high-level IT staff to obtain information on IT investment. The data is at the establishment level, with a unique ID tracking each establishment over time. CiTDB covers more than 3 million establishments per year, including both for-profit and nonprofit organizations. For each establishment, the database provides firm-level information, e.g., name, location (latitude, longitude, ZIP code, county, and state), industry, estimated revenue, estimated number of employees, and information technology budget (IT budget). Since there is no common identifier between the Form 990 and the CiTDB database, I employ fuzzy matching to link the two data sources based on establishment name and zip code. Among a total of 419,727 NPOs with unique name and zip code from the Form 990 dataset, I are able to match 80,255 (19%), yielding 269,827 observations. I manually check the names from Form 990 to the CiTDB database to confirm the accuracy of the matching procedure. After cleaning the sample, I end up with 151,642 observations.

I retrieve state-level characteristics from various sources.[19] Real GDP per capita and total population are from the Bureau of Economic Analysis (BEA) Regional Economic Accounts.[20] The unemployment rate is obtained from the Local Area Unemployment Statistics Program through the Bureau of Labor Statistics,[21] while the maximum corporate income tax rate by state is from the

[19] I include these variables to control for the impact of locality specific factors that may influence the variables of interest. The fact that some nonprofits and for-profits operate and employ workers across state boundaries adds noise to the analysis, making it harder for me to find significant results.
[20] https://www.bea.gov/data/economic-accounts/regional.
[21] https://www.bls.gov/lau/.

Tax Policy Center.[22] The status of the Right-to-Work (RTW) laws in each state is collected from the National Right-to-Work Committee.[23]

Empirical Strategy

The empirical strategy is twofold. I begin by examining the impact of minimum wage increases on employment using pooled regressions including both for-profit and nonprofit organizations. I then dig deeper into NPOs by running NPO-only models. Following Cengiz, Dube, Lindner, and Zipperer (2019), I employ a difference in differences (DiD) approach. In particular, I examine the changes in employment as well as use of volunteers and IT investment, over the seven years (t-3, t+3) surrounding the significant state-level minimum wage increases, using as controls NPOs headquartered in states that have small or no minimum wage changes within the event window. I follow Cengiz, Dube, Lindner, and Zipperer (2019) and Dai and Qiu (2021) and define the minimum wage increase to be significant if the rise in the real minimum wage (in 2018 dollars) is at least 25 cents per hour.[24] Following Heider and Ljungqvist (2015) and Meer and West (2016), I estimate the following multiple regression using DiD to control for observed variation in firm-level and state-level conditions. I also include *Event×Firm-* and *Event×Industry×Year-* fixed effects, and as noted above, cluster standard errors at the state level:

$$\Delta \boldsymbol{Ln}(\boldsymbol{Employees})_{i,j,t} = \boldsymbol{\alpha} + \boldsymbol{\beta_1 NPO_i} \times \boldsymbol{Treat_{i,j}} \times \boldsymbol{Post_t} + \boldsymbol{\beta_2 Treat_{i,j}} \times \boldsymbol{Post_t} + \Delta \boldsymbol{Firm\ Controls_{i,t}} + \Delta \boldsymbol{State} \& \Delta \boldsymbol{County\ Controls_{j,t}} + \boldsymbol{\varepsilon_{i,j,t}}, \quad (1)$$

where i represents firm, j represents state or county, and t represents year. The dependent variable in model (1) is the annual change in the natural logarithm of the number of employees. The number of employees is from Form 990 for NPOs and the Infogroup database for for-profits. I focus on

[22] https://www.taxpolicycenter.org/statistics/state-corporate-income-tax-rates.
[23] https://nrtwc.org/facts/state-right-to-work-timeline-2016/
[24] There are 22 trivial minimum wage increases that are below 25 cents (in 2018 dollars).

growth since Meer and West (2016) argue that the "minimum wage will impact employment over time through changes in growth rather than an immediate drop in relative employment levels" and find that "minimum wage reduces job growth over a period of several years." In contrast, level variables like number of employees may be sticky. That is, an organization may not layoff or fire an employee immediately, but simply reduce headcount via attrition.

NPO is an indicator that equals one for nonprofit organizations and zero for for-profits. *Treated* is an indicator that equals one for an organization located in a state with a significant minimum wage increase and zero for control organizations. *Post* is an indicator that equals one for the year of and years after the minimum wage increase, and zero for years before the minimum wage increase. As all the models include *Event*-, *Firm*-, and *Year*-fixed effects, Equation (1) does not include main effects for either *NPO*, *Treated* or *Post*. The differential impact of the minimum wage increases on employment growth between NPOs and for-profits is captured by the coefficient on the triple interaction term, $NPO \times Treated \times Post$.

Whereas Form 990 contains many NPO characteristics, the InfoGroup database reports fewer bits of data, for example it reports sales but not assets. Therefore, I include $\Delta ln(Sales)$ as a firm-level control variable. As many nonprofits do not have sales in the traditional sense, I use program service revenue from Form 990 to proxy for NPO size. I also include controls for county- and state-level economic variables. $\Delta ln(GDP\ per\ Capita)$ is the annual change in the logarithm of county-level GDP per capita. I control for county population ($\Delta ln(Population)$) and county unemployment ($\Delta Unemployment\ Rate$) growth. *RTW Laws* is an indicator variable that equals one for years in which a state has a Right-to-Work Law, and zero otherwise. Since political ideology could affect minimum wages, as well as nonprofit funding, I control for the state-level political ideology. Following Hoi, Wu, and Zhang (2013), I use data from The Green Papers to measure the relative strength of Democratic Party compared to Republican Party for each state (Δ*Democrat Points*). The Green Papers updates its measure every other year, and consequently in the years when it is not available, I use its lagged value.

After testing in Equation (1) for a differential impact of minimum wage increases on employment growth between for-profits and NPOs, I then focus on the NPO-only sample using the following model.

$$\boldsymbol{\Delta Ln(NPO\ Variable)_{i,j,t} = \alpha + \beta_1 Treat_{i,j} \times Post_t + \Delta NPO\ Controls_{i,t} +}$$
$$\boldsymbol{\Delta State \& \Delta County\ Controls_{j,t} + \varepsilon_{i,j,t},} \qquad (2)$$

where *i* represents firm, *j* represents state or county, and *t* represents year. The county- and state-level economic control variables remain the same as equation (1). The dependent variable is alternatively the annual change in the natural logarithm of the number of employees, number of volunteers, both obtained from Form 990 and investment in information technology, obtained from the CiTBD database.

In the NPO only tests, I include additional controls not included in Equation (1) following Balsam, Harris, and Saxton (2020). All of these variables are measured in the change form, with scalars being logged before differencing. To control for efficiency in delivering program services, I use the *Program Ratio*, which is measured as the ratio of *Program Service Expenses* to *Total Expenses*. To control for governance, I use *Board Independence*, which is the ratio of independent to total directors, and the *Governance Index*, which is the sum of five 1/0 indicator variables that take the value of one if the organization has a CEO salary setting policy, an audit committee, a majority independent board, does not outsource management functions, and provides applicable forms on its website. I include three variables to control for NPO income sources: *Government*

Grants, *Program Service Revenue* and *Direct Donations*. Fundraising expenses are also included. Size is now measured using total assets. More detailed variable definitions are in Appendix 1.

I report summary statistics for the main sample in Table 1-2 with Panel A providing information on firm-level characteristics for the nonprofit sample, Panel B providing data on the for-profit sample and Panel C providing data on state- and county-level variables. I have more variables in Panel A than in Panel B since Form 990 contains many variables for NPOs, whereas the InfoGroup database provides a more limited set of variables for for-profits. I emphasize that I do many analyses with the nonprofit sample, while I only do one analysis with the for-profit sample, for the purpose of comparing the impact of minimum wage increases on employment growth between nonprofits and for-profits. Many of the variables are highly skewed. The mean and median number of employees in the nonprofit sample are 202 and 89 respectively, while the mean and median number of volunteers are 180 and 35 respectively. On average, the nonprofits have more employees than the for-profits, as the mean and median number of employees in the for-profit sample are 34 and 17 respectively. Similarly, employment growth is greater at the nonprofit firms as mean (median) annual employment growth rate in the nonprofits is approximately 2% (1%), whereas the comparable growth rate for the for-profit sample is zero. Comparing revenues and revenue growth between the two samples I again find that nonprofits are larger, with mean total revenue of $13.17 million versus mean sales of $9.91 million, and growing faster, with mean total revenue growth of 3% versus -1%. Turning solely to the nonprofit sample in Panel A, I observe that the program services (*PSR*) is the largest revenue source, with a mean of $9.18 million, and that the average nonprofit is profitable, i.e., has mean (median) *Total Revenue* of $13.17 ($4.69) million, that exceeds mean (median) *Total Expense* of $10.52 ($3.74) million. Both the mean and median *ROA* are positive, i.e., 1.25% and 1.68% respectively. The fact that most nonprofits are "profitable" is long established in the nonprofit literature (e.g., Chang and Tuckman 1990). Mean *Leverage* is 48.5% which is almost identical to the 47% reported by Balsam and Harris (2018). Similarly, the mean of the *Program Ratio*, 84%, is very close to the 85% reported by Balsam and Harris (2018).

Main Results

Effect of State Minimum Wage Increases on Employment Growth in NPOs

In Table 1-3, I report the results of the baseline estimation where the dependent variable is the annual change in the natural logarithm of the total number of employees. I test whether minimum wage increases result in decreased employment growth as predicted by hypothesis H1. In column (1), the coefficient estimate on *Treated×Post*, which reflects the average impact on employment growth for for-profit firms, is -0.438 and statistically significant at the 10% level. In addition, and more important for the purposes of the investigation, the coefficient estimate on *NPO×Treated×Post*, which represents the incremental effect on employment growth for nonprofit firms above and beyond that of for-profit firms, is -3.896, which is statistically significant at the 1% level. In terms of economic significance, the results are mixed. The coefficient on *Treated×Post*, -0.438 suggests that a significant minimum wage increase leads to a 0.04% lower growth rate of employment, compared to firms that do not experience a minimum wage hike. Thus, the impact on for-profit firms, while statistically significant, is marginal, consistent with the mixed results of the prior literature. The impact of minimum wage hikes on NPOs is captured by the sum of the coefficients on *Treated×Post* and *NPO×Treated×Post*, -4.334, suggesting that a significant minimum wage increase leads to a 0.433% lower growth rate of employment in treated NPOs, compared to those that do not experience a minimum wage hike.[25] Given that mean employment

[25] Calculated as $\exp^{.004334}$ -1, given that the coefficient estimates in column (1), -3.896 and -0.438 are after the dependent variable is scaled by 1000.

growth in Table 1-2 is 2.3% per year, a reduction of 0.433% in annual employment growth post-event is economically meaningful.

A major assumption of the difference-in-differences approach is that employment growth should follow a parallel trend between treated and control nonprofits in the absence of significant minimum wage events, i.e., before the event. To verify this assumption empirically, I estimate equation (3) below to investigate the dynamic effects of state minimum wage hikes.

$$\boldsymbol{\Delta Ln(EMP)_{i,j,t} = \alpha + \beta_{-3}NPO_i \times Treat_{i,j} \times Year_{i,t}^{-3} + \beta_{-2}NPO_i \times Treat_{i,j} \times Year_{i,t}^{-2} + \beta_0 NPO_i \times Treat_{i,j} \times Current_{i,t} + \beta_1 NPO_i \times Treat_{i,j} \times Year_{i,t}^{+1} + \beta_2 NPO_i \times Treat_{i,j} \times Year_{i,t}^{+2} + \beta_3 NPO_i \times Treat_{i,j} \times Year_{i,t}^{+3} + d_{-3} \times Treat_{i,j} \times Year_{i,t}^{-3} + d_{-2} \times Treat_{i,j} \times Year_{i,t}^{-2} + d_0 \times Treat_{i,j} \times Current_{i,t} + d_1 \times Treat_{i,j} \times Year_{i,t}^{+1} + d_2 \times Treat_{i,j} \times Year_{i,t}^{+2} + d_3 \times Treat_{i,j} \times Year_{i,t}^{+3} + \Delta Firm\ Controls_{i,t} + \Delta State \& \Delta County\ Controls_{j,t} + \varepsilon_{i,j,t}.} \tag{3}$$

where $Year_{i,t}^{-3}$, and $Year_{i,t}^{-2}$ are dummy variables that equal one if the year is three or two years prior to the minimum wage increase, respectively, and zero otherwise. $Current_{i,t}$ is a dummy variable that equals one in the year when a significant minimum wage increase occurs and zero otherwise. $Year_{i,t}^{+3}$, $Year_{i,t}^{+2}$, and $Year_{i,t}^{+1}$ are dummy variables that equal one if the year is three, two, or one years after a significant minimum wage increase, respectively, and zero otherwise. The omitted time dummy is $Year_{i,t}^{-1}$, the year immediately before the significant minimum wage event. d_n captures the change in *Emp Growth* between for-profits in treated and control states for year n, relative to year −1. If employment growth has similar pre-trend between the treated and control groups, then d_{-3} and d_{-2} would be statistically insignificant. β_n captures the differences in the pre-trends of NPOs and for-profits (FPOs).

In column (2) of Table 1-3, the coefficients on $Treat \times Year^{-3}$ and $Treat \times Year^{-2}$, as well as $NPO \times Treat \times Year^{-3}$ and $NPO \times Treat \times Year^{-2}$ are all statistically insignificant, consistent with the parallel trend condition of the DiD design for both NPOs and FPOs. I observe significantly lower employment growth in *Year*$^{+1}$ for FPOs, consistent with H1. As in column (1) I find an incremental negative treatment effect for nonprofits, relative to for-profits, which is statistically significant at the 1% level in the first and second year following the minimum wage increase, and the 5% level in the third year following the minimum wage increase. These results suggest a long-term negative impact on employment growth overall, with a significantly greater negative impact in nonprofit organizations. The findings support hypothesis H1—minimum wage increases lead to a decline in employment growth for both FPOs and NPOs, but greater for NPOs.

Given the focus on nonprofit organizations, in Table 1-4 I present the results of Equation (2) where I once again examine the impact of increases in state level minimum wages on employment growth. The difference between Equations (1) where I pool both NPOs and FPOs together but only control for firm size, and (2) is that in the latter I control for many nonprofit specific variables that could influence employment growth. In column (1) I find that the coefficient on *Treated*×*Post* is once again negative, -4.576, and statistically significant at the 1% level. Similarly, in column (2) I find the coefficients on *Treated*×*Current*, *Treat*×*Year*$^{+1}$, *Treat*×*Year*$^{+2}$, and *Treat*×*Year*$^{+3}$, are all negative, with the coefficients on *Treat*×*Year*$^{+1}$ and *Treat*×*Year*$^{+2}$ statistically significant at the 1% level, and *Treat*×*Year*$^{+3}$ statistically significant at the 5% level.

The control variables, to the extent that they are statistically significant, behave as expected. Employment growth is positively associated with improvements in the program ratio, a proxy for the efficiency of the nonprofit, as well the growth in government grants and program service revenues, sources of funding for the nonprofit. The positive association with increases in fundraising expense is consistent with at least some of those expenditures going towards the hiring of in-house fundraisers.

Effect of State Minimum Wage Increases on Substitutes for Paid Employees

In Table 1-5, I test hypothesis 2, by examining the impact of state minimum wage increases on the use of volunteers and information technology. In column (1) of Table 1-5, where the dependent variable is Δ*Ln(Volunteer)*, the coefficient estimate on *Treat*×*Post* is negative yet statistically insignificant, indicating that compared to control NPOs, volunteer growth in treatment NPOs is not significantly affected by minimum wage increases. Consequently, I do not find evidence of volunteers being substitutes or complements for employees.

In column (2), to investigate whether there is any substitution between labor and capital, I examine the effect of minimum wage increases on the investment in information technology. The sample used in column (2) consists of establishment-year observations for which financial information is available in the Form 990 and that are covered in the CiTDB database from Aberdeen. The dependent variable is the change of natural logarithm of the IT budget. I find a significant increase (at the 5% level) in Δ*Ln(IT Budget)* after significant minimum wage hikes. Based on the coefficient in column (2), the growth rate in total IT budget rises by 4.18% in treated NPOs relative to control NPOs after significant minimum wage increases, which is also economically significant given the sample mean growth rate of 17.42%.

With respect to the signs and significance of the control variables, I find that most of the control variables in columns (1) and (2) are insignificantly different from zero. In column (1) I observe positive and significant coefficients on Δ*ln(PSR)* (p<0.05), Δ*Governance Index* (p<0.01), Δ*Unemployment Rate* (p<0.01), and Δ*Democrat Points* (p<0.01). These coefficients are consistent with volunteers being essential in generating program services, as well as individuals being more likely to volunteer when they feel a nonprofit is well governed, there is less competition for their services, i.e., the unemployment rate is high, and their state is becoming more Democratic. In column (2) the coefficients on Δ*ln(Direct Donation)* and Δ*Unemployment Rate* are negative and significant ($p<0.05$), suggesting that the substitution of capital for labor is greater when both donations and the unemployment rate are lower.

In sum the findings reject H2b, but not H2a. That is the results are consistent with NPOs substituting information technology, but not volunteers, for employees after a significant increase in the minimum wage.

NPO Exit

Since measuring delivery of services in NPOs is difficult due to a lack of consistent reporting as well as heterogeneity in the sector, I proxy for the impact on service delivery by examining NPO exit. Prior literature has shown that minimum wage increases can lead to firm exit in the for-profit sector (Chava, Oettl, and Singh 2019; Luca and Luca 2019; Aaronson, French, Sorkin, and To 2018). Consequently, in this section, I investigate how state minimum wage increases affect the probability of nonprofit organizations exiting or ceasing to do business. I define a nonprofit as exiting if it appears in the database of electronic Form 990 filers for at least three consecutive years (a requirement for entering the sample) and then no longer appears. This requirement eliminates the years 2010 through 2012 from this analysis. Since the files for 2017 and 2018 do not appear to be complete, i.e., the IRS appeared to be in the process of updating at the time of the download, I do not include these two years to identify NPO exit. As a result, this analysis is based on the years 2013 through 2016.

To examine the effect of state minimum wage on the likelihood of NPO exiting business, I run a logit model where the dependent variable is a binary variable equal to one if an NPO filed its last Form 990 in a given year, and zero otherwise. Independent variable includes *Treated* that is a dummy variable that equals one for treated NPOs and zero for control NPOs. *Post* is a dummy variable that equals one for the year when minimum wage increase occurs and the years afterward, and zero otherwise. Regression results are reported in Table 1-6. I control for other likely correlates of NPO exit; i.e., size (*Ln(Assets)*), capital structure (*Leverage*), profitability (*ROA*), and *Liquidity* (*Liquid Assets/Total Assets*). I also include the same set of county- and state-level controls as in Equation (2). I control for *Event*- and *Year*-fixed effects in column (1) and *Event*-, *Industry*-, and *Year*-fixed effects in column (2). The coefficient estimate for *Treated*×*Post* is positive and statistically significant (p<0.01) in both columns, suggesting that significant minimum wage hikes raise the likelihood of nonprofit exit. Thus, I document an additional cost of minimum wage increases, i.e., that the minimum wage increase results in a reduction of services provided. Regarding control variables, I observe positive and statistically significant coefficients on *Ln(Assets), Leverage,* and *Ln(Population)*, and negative and statistically significant coefficients on *Liquid Asset/Total Assets, Ln(GDP per Capita), Unemployment Rate,* and *RTW Law*.

Conclusion

Increases in the minimum wage are often presented as a means to improve social welfare, e.g., by reducing the number of working individuals earning less than the poverty level. However, there are adverse consequences, for example some of the employees who were supposed to benefit from the increased minimum wage might lose or be unable to find a job. This issue has been discussed and debated at length in both theoretical and empirical literature. It is hard to consider any job loss that does occur as unanticipated when the Congressional Budget Office provides estimates of job loss for these proposals in advance of votes, for example see "The Budgetary Effects of the Raise the Wage Act of 2021." In contrast, no formal regulatory analysis that I are aware of is provided on the impact on provision of services. Thus, if increases in the minimum wage result in a reduction in services provided, for example through organizations ceasing operations, that could be considered an unintended consequence.

Prior minimum wage studies have focused on for-profit firms. In particular, a line of focus has been the restaurant industry, because of its reliance on low wage, minimum, and sometimes, sub-minimum wage labor. In contrast, I focus on nonprofit enterprises. As discussed above, there are reasons to suspect the impact of minimum wage increases can be broader and more significant in nonprofit enterprises.

In this study, I find decreases in employment growth after state minimum wage hikes, relative to control organizations. This decrease in employment, while occurring in both for-profit and nonprofit organizations, is statistically and economically greater in nonprofit organizations. Furthermore, the societal impact of a minimum wage increase goes beyond the decline in employment. I also provide evidence consistent with a reduction in nonprofit services provided, by showing an increase in nonprofit exit in the years following a minimum wage increase. The decrease in services provided can have a greater impact on the clientele of nonprofits, for whom the nonprofit often provides a safety net, which is not the case for for-profit organizations. That is, while the literature suggests that an increase in the minimum wage reduces restaurant employment, and perhaps even the number of restaurants in operation, restaurants do not provide as vital a service as food banks. To be precise, a person relying on a food bank or soup kitchen, likely goes hungry in the absence of these services, whereas a restaurant customer may visit another restaurant if one closes the door.

Exhibit 1. Illustration of impact of minimum wage increase

On Tuesday, November 3, 2020, the citizens of Florida voted to increase the state minimum wage from its current rate of $8.56 an hour to $10 per hour on September 30, 2021, and by an additional dollar per year until it hits $15 per hour on September 30, 2026. To illustrate the potential impact of this increase I calculated the profitability of Florida-headquartered NPOs with at least ten employees for their 2018 tax year, the last year for which I had data available. As expected, most NPOs were profitable, but their margin was thin, 2% of revenue. I then simulated the effect of a proportional increase in their salaries expense equal to the increase from $8.56 to $10 per hour, an increase of almost 17%, finding that the 2% profit margin was now a 4% loss.[26]

To further drive home this point, consider as a representative illustration, Central Credit Union of Florida. In 2018 it had 86 employees (just below the sample median), revenue of $10,071,254, expenses of $9,874,374, yielding a surplus of $196,880 which is 2% of revenue. Included in expenses are salaries of $3,287,463. Increasing this amount by 17% turns the surplus of $196,880 into a deficit of $356,151, almost 4% of revenues.

[26] Note this calculation does not assume that any or all of the employees of the NPO are currently making the minimum wage. Rather for illustration purposes, it assumes that increases in the MW will ripple through the pay scale, so that someone earning 10% above the minimum before the increase will receive a commensurate pay raise so that they still earn 10% above the minimum wage–to avoid wage compression and maintain the pay scale (Jardim and Inwgen 2019; Allard, Romich, Buszkiewicz, Althauser, and Obara 2020)

Table 1-1. State Minimum Wage Increase Events

This table reports the state and the year and month for each of the 21 significant minimum wage increase events used in the paper. The minimum wage (after the increase) and increase are in 2018 constant dollars.

Event ID	State	Abbreviation	Year	Month	Min. Wage	Increase	Increase (%)
1	Connecticut	CT	2014	1	$9.24	$0.33	3.70
2	Connecticut	CT	2015	1	$9.70	$0.46	4.99
3	Massachusetts	MA	2015	1	$9.54	$1.05	12.31
4	Rhode Island	RI	2013	1	$8.37	$0.26	3.17
5	Rhode Island	RI	2015	1	$9.54	$1.05	12.31
6	Vermont	VT	2015	1	$9.70	$0.43	4.63
7	New Jersey	NJ	2014	1	$8.76	$0.93	11.90
8	New York	NY	2013	12	$8.64	$0.69	8.70
9	New York	NY	2014	12	$9.29	$0.65	7.55
10	Michigan	MI	2014	9	$8.65	$0.66	8.30
11	Minnesota	MN	2014	8	$8.49	$0.67	8.51
12	Minnesota	MN	2015	8	$9.54	$1.05	12.31
13	Nebraska	NE	2015	1	$8.48	$0.78	10.16
14	South Dakota	SD	2015	1	$9.01	$1.31	17.04
15	District of Columbia	DC	2014	7	$10.09	$1.18	13.23
16	District of Columbia	DC	2015	7	$11.13	$1.04	10.34
17	Delaware	DE	2014	6	$8.23	$0.40	5.12
18	Delaware	DE	2015	6	$8.74	$0.52	6.27
19	Maryland	MD	2015	7	$8.74	$1.05	13.60
20	West Virginia	WV	2015	1	$8.48	$0.78	10.16
21	Arkansas	AR	2015	1	$7.95	$0.25	3.27

Table 1-2. Summary Statistics

The sample in Panels A and B consists of 287,447 and 12,744,841 firm-year observations of non-profit and for-profit organizations, respectively, that fall within a (-3, 3) years window around the years in which minimum wage events occurred during 2013 to 2015. Panel A reports summary statistics on firm-level characteristics for NPOs, Panel B for for-profit organizations, and Panel C presents summary statistics on state- and county-level variables.

Panel A. Firm-Level Characteristics for NPOs

	N	Mean	STD	P25	Median	P75
EMP	287,447	202.43	274.92	34.00	89.00	249.00
VTR	287,447	179.95	383.72	4.00	35.00	167.00
Total Assets (million $)	287,447	19.60	38.26	1.42	4.90	17.16
Program Ratio (%)	287,447	83.89	8.95	78.84	85.02	90.07
Fundraising Exp (million $)	287,447	0.16	0.37	0.00	0.01	0.14
Gov Grant (million $)	287,447	1.23	3.50	0.00	0.00	0.61
Direct Donation (million $)	287,447	1.14	2.85	0.01	0.17	0.84
PSR (million $)	287,447	9.18	17.98	0.41	1.97	9.25
PSE (million $)	287,447	10.52	17.34	1.27	3.74	11.46
Total Revenue (million $)	287,447	13.17	22.84	1.61	4.69	14.12
Total Expense (million $)	287,447	12.36	20.00	1.55	4.52	13.60
Governance Index	287,447	3.64	0.81	3.00	4.00	4.00
Board Independence (%)	287,447	92.48	18.86	94.74	100.00	100.00
ROA (%)	287,447	1.25	18.50	-2.64	1.68	7.13
Leverage (%)	287,447	48.50	264.47	9.64	27.39	54.92
Liquid Asset (million $)	287,447	6.47	17.82	0.26	1.00	4.08
IT Budget (Thousand $)	151,642	619.64	4227.62	26.80	75.53	279.22
ΔLn(EMP) (%)	287,447	2.30	15.81	-4.88	1.00	8.70
ΔLn(VTR) (%)	287,447	4.32	88.34	0.00	0.00	4.57
ΔLn(Total Assets) (%)	287,447	4.75	20.18	-3.63	2.22	10.27
ΔLn(Fundraising Exp) (%)	287,447	0.40	3.66	0.00	0.00	0.39
ΔLn(Gov Grant) (%)	287,447	0.42	11.43	0.00	0.00	0.47
ΔLn(Direct Donation) (%)	287,447	1.08	22.01	-2.93	0.00	4.56
ΔLn(PSR) (%)	287,447	2.35	9.52	-1.27	1.39	5.60
ΔLn(PSE) (%)	287,447	2.76	9.29	-1.26	2.21	6.46
ΔLn(Total Revenue) (%)	287,447	2.84	17.36	-2.44	2.27	7.94
ΔLn(IT Budget) (%)	151,642	17.42	83.96	-6.01	-0.34	56.25
ΔProgram Ratio (%)	287,447	-0.01	3.43	-1.13	0.00	1.10
ΔGovernance Index	287,447	0.01	0.31	0.00	0.00	0.00
ΔBoard Independence (%)	287,447	0.04	4.17	0.00	0.00	0.00

Panel B. Firm-Level Characteristics for For-profit Organizations						
	N	Mean	STD	P25	Median	P75
EMP	12,744,841	34.48	153.35	12.00	17.00	30.00
Total Sales (million $)	12,744,841	9.91	74.28	1.47	2.86	6.80
ΔLn(EMP) (%)	12,744,841	0.21	5.31	0.00	0.00	0.00
ΔLn(Sales) (%)	12,744,841	-0.95	36.94	0.00	0.00	0.00
Panel C. State- and County-Level Characteristics						
	N	Mean	STD	P25	Median	P75
GDP per Capita (thousand $)	11,504	45.14	28.42	31.25	40.16	52.28
Population (thousand people)	11,504	139.18	313.25	20.27	44.03	123.63
Unemployment Rate (%)	11,504	5.81	2.32	4.10	5.40	7.20
Democrat Points	11,504	38.86	27.92	11.00	41.00	61.00
RTW Law	11,504	0.59	0.49	0.00	1.00	1.00
ΔLn(GDP per Capita) (%)	11,504	2.59	9.67	0.46	2.84	5.11
ΔLn(Population) (%)	11,504	-0.87	19.77	-0.51	0.00	0.63
ΔUnemployment Rate (%)	11,504	-0.66	0.66	-1.00	-0.60	-0.30
ΔDemocrat Points	11,504	-1.46	7.29	0.00	0.00	0.00

Table 1-3. Effect of State Minimum Wage Increases on Employment Growth: Nonprofit (NPO) vs. For Profit (FPO) Organizations

This table reports the effect of state minimum wage increases on the annual change in the natural logarithm of employment in both nonprofit organizations and for-profit firms during 2010 through 2018. I employ a difference-in-differences (DiD) design, where treated firms are those with headquarters located in the states that experience a significant minimum wage increase in a given year, and control are those that are headquartered in the same census division as the treatment state but have small or no minimum wage increases within the seven-year window. The dependent variable is the change in the natural logarithm of employment. *NPO* is an indicator that equals to one for nonprofit organizations and zero for for-profit firms. *Treated* is a dummy variable that equals one for treated firms and zero for control firms. *Post* is a dummy variable that equals one for the year when minimum wage increase occurs and the years afterward, and zero for the years before the minimum wage increases. $Year^{-3}$ and $Year^{-2}$ are dummy variables that equal one if the sample year is three and two years prior to minimum wage increases, respectively, and zero otherwise. $Year^{+3}$, $Year^{+2}$, and $Year^{+1}$ are dummy variables that equal one if the sample year is three, two, and one years after minimum wage increases, respectively, and zero otherwise. *Current* is a dummy variable that equals one if the sample year is in the same year as minimum wage increases, and zero otherwise. All variable definitions are reported in the Appendix 1. I include *Event*×*Firm*- and *Event*×*Industry*×*Year*-fixed effects in columns (1) and (2). The dependent variables are scaled by multiplying by 1,000. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*ΔLn(EMP)*	*ΔLn(EMP)*
	(1)	(2)
	NPO & FPO	NPO & FPO
NPO×*Treated*×*Post*	-3.896***	
	(0.008)	
Treated×*Post*	-0.438*	
	(0.086)	
NPO×*Treated*×$Year^{-3}$		1.381
		(0.712)
NPO×*Treated*×$Year^{-2}$		-1.452
		(0.448)
NPO×*Treated*×*Current*		-2.893
		(0.115)
NPO×*Treated*×$Year^{+1}$		-4.516***
		(0.004)
NPO×*Treated*×$Year^{+2}$		-6.126***
		(0.003)
NPO×*Treated*×$Year^{+3}$		-4.800**
		(0.032)
Treated×$Year^{-3}$		-0.006

		(0.980)
Treated×*Year*$^{-2}$		-0.206
		(0.464)
Treated×*Current*		0.170
		(0.436)
Treated×*Year*$^{+1}$		-0.596**
		(0.032)
Treated×*Year*$^{+2}$		-0.447
		(0.429)
Treated×*Year*$^{+3}$		-0.244
		(0.456)
Δ*ln(Sales)*	2.313***	2.314***
	(0.000)	(0.000)
Δ*ln(GDP per Capita)*	1.235	1.251
	(0.389)	(0.382)
Δ*ln(Population)*	1.646	1.642
	(0.141)	(0.141)
Δ*Unemployment Rate*	0.164	0.153
	(0.291)	(0.322)
Δ*Democrat Points*	-0.009	-0.008
	(0.340)	(0.354)
RTW Law	-0.042	-0.107
	(0.938)	(0.836)
Event×*Firm* Fixed Effects	Yes	Yes
Event×*Industry*×*Year* Fixed Effects	Yes	Yes
Observations	13,032,288	13,032,288
Adj. R-squared	0.023	0.023

Table 1-4. Effect of State Minimum Wage Increases on Employment Growth: NPOs Only

This table reports the effect of state minimum wage increases on the annual change in the natural logarithm of employment in nonprofit organizations during 2010 through 2018. I employ a difference-in-differences (DiD) design, where treated NPOs are those with headquarters located in the states that experience a significant minimum wage increase in a given year, and control NPOs are those that are located in the same census division as the treatment state but have no minimum wage increase or has only trivial minimum wage increases within the seven-year window. The dependent variable is the change in the natural logarithm of employment. All variable definitions are reported in Appendix 1. I include *Event*×*Firm*- and *Event*×*Industry*×*Year*-fixed effects in columns (1) and (2). The dependent variables are scaled by multiplying 1,000. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*ΔLn(EMP)*	*ΔLn(EMP)*
	(1)	(2)
Treated×Post	-4.576***	
	(0.003)	
Treated×Year^{-3}		1.113
		(0.802)
Treated×Year^{-2}		-2.085
		(0.354)
Treated×Current		-2.528
		(0.263)
Treated×Year^{+1}		-4.410***
		(0.008)
Treated×Year^{+2}		-6.797***
		(0.005)
Treated×Year^{+3}		-6.611**
		(0.018)
Δ*ln(Total Assets)*	6.540	6.545
	(0.189)	(0.188)
Δ*Program Ratio*	0.508***	0.508***
	(0.005)	(0.005)
Δ*ln(Fundraising Exp)*	104.930***	104.923***
	(0.000)	(0.000)
Δ*ln(Gov Grant)*	68.641***	68.640***
	(0.000)	(0.000)
Δ*ln(PSR)*	181.498***	181.480***
	(0.000)	(0.000)

Δ*ln(Direct Donation)*	1.774	1.770
	(0.629)	(0.630)
Δ*Governance Index*	2.232	2.235
	(0.309)	(0.308)
Δ*Board Independence*	6.935	6.936
	(0.593)	(0.593)
Δ*ln(GDP per Capita)*	-3.450	-3.408
	(0.727)	(0.730)
Δ*ln(Population)*	-4.793	-4.809
	(0.210)	(0.208)
Δ*Unemployment Rate*	0.468	0.543
	(0.841)	(0.815)
Δ*Democrat Points*	0.011	0.011
	(0.847)	(0.846)
RTW Law	2.946	2.982
	(0.293)	(0.307)
Event×*Firm* Fixed Effects	Yes	Yes
Event×*Industry*×*Year* Fixed Effects	Yes	Yes
Observations	287,447	287,447
Adj. R-squared	0.017	0.017

Table 1-5. Effect of State Minimum Wage Increases on Substitutes for Paid Employees

This table presents the results on how state minimum wage increases affect the use of volunteers and information technology in nonprofit organizations during 2010 through 2018. The dependent variable in column (1) is the change in natural logarithm of the number of volunteers. The sample for column (2) consists of establishment-year observations of NPOs of which financial information is available in the Form 990 and that are covered in the CiTDB database from Aberdeen. The dependent variable in column (2) is the change of natural logarithm of total IT budget. All variable definitions are reported in the Appendix 1. I include *Event×Firm*- and *Event×Industry×Year*-fixed effects in both regressions. The dependent variables are scaled by multiplying 1,000. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*ΔLn(VTR)*	*ΔLn(IT Budget)*
	(1)	(2)
Treated×Post	-11.329	40.972**
	(0.441)	(0.031)
Δ*ln(Total Assets)*	0.577	-55.063
	(0.983)	(0.200)
Δ*Program Ratio*	0.675	-0.773
	(0.551)	(0.439)
Δ*ln(Fundraising Exp)*	74.259	23.366
	(0.467)	(0.729)
Δ*ln(Gov Grant)*	41.634	14.314
	(0.180)	(0.501)
Δ*ln(PSR)*	103.784**	-8.486
	(0.026)	(0.825)
Δ*ln(Direct Donation)*	6.360	-36.991**
	(0.664)	(0.013)
Δ*Governance Index*	26.290***	-16.695
	(0.005)	(0.198)
Δ*Board Independence*	-47.223	-17.704
	(0.555)	(0.821)
Δ*ln(GDP per Capita)*	21.563	26.171
	(0.794)	(0.528)
Δ*ln(Population)*	17.406	24.074
	(0.438)	(0.136)
Δ*Unemployment Rate*	27.229***	-35.806**
	(0.000)	(0.027)
Δ*Democrat Points*	1.187***	-0.027

	(0.006)	(0.978)
RTW Law	28.615	-13.746
	(0.272)	(0.483)
Event×*Firm* Fixed Effects	Yes	Yes
Event×*Industry*×*Year* Fixed Effects	Yes	Yes
Observations	287,447	151,642
Adj. R-squared	-0.101	0.316

Table 1-6. State Minimum Wage Increases and NPO Exits

This table presents the results on how state minimum wage increases affect the probability of nonprofit organizations exit during 2012 through 2016. I employ logit model in the tests where the dependent variable is terminated operations (Exit in the table) from Form 990, which is a dummy variable that equals one for the year when the NPO terminates its operation. All variable definitions are reported in the Appendix 1. I include *Event*- and *Year*- fixed effects in column (1), *Event*-, *Industry*-, and *Year*-fixed effects in column (2). *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Exit*	*Exit*
	(1)	(2)
Treated×Post	14.116***	14.372***
	(0.000)	(0.000)
Post	0.261	0.222
	(0.847)	(0.872)
Treated	-14.478***	-14.098***
	(0.000)	(0.000)
Ln(Total Assets)	0.344***	0.487***
	(0.006)	(0.010)
Leverage	0.000***	0.000***
	(0.003)	(0.004)
ROA	0.033	0.027
	(0.235)	(0.439)
Liquid Asset/Total Assets	-5.207***	-5.166***
	(0.000)	(0.000)
Ln(GDP per Capita)	-2.453***	-2.228***
	(0.000)	(0.000)
Ln(Population)	1.181***	1.134***
	(0.000)	(0.000)
Unemployment Rate	-3.127***	-3.005***
	(0.000)	(0.000)
RTW Law	-1.802***	-1.273*
	(0.007)	(0.096)
Democrat Points	-0.006	-0.012**
	(0.288)	(0.042)
Event Fixed Effects	Yes	Yes
Industry Fixed Effects	No	Yes
Year Fixed Effects	Yes	Yes
Observations	80,753	41,893

Appendix A. Variable Definition

Variables	Description
State- or County-Level Characteristics Variable	
Annual State Maximum Min. Wage	The annual maximum of the daily state minimum wage obtained from Vaghul and Zipperer (2016), which is available at https://github.com/benzipperer/historicalminwage.
GDP per Capita	The county level Gross Domestic Product divided by the total population from the Bureau of Economic Analysis (BEA) Regional Economic Accounts at https://www.bea.gov/data/economic-accounts/regional.
Unemployment Rate	The county level unemployment rate from Local Area Unemployment Statistics Program through the Bureau of Labor Statistics at https://www.bls.gov/lau/.
Corporate Income Tax	The state-level maximum corporate income tax rate reported by the Tax Policy Center at https://www.taxpolicycenter.org/statistics/state-corporate-income-tax-rates.
RTW Law	An indicator variable that equals to 1 for years in which a state has Right-to-Work Laws enacted, 0 otherwise, collected from the National Right-to-Work Committee at https://nrtwc.org/facts/state-right-to-work-timeline-2016/.
Democrat Points	An index of the partisan dominance of the state where the firm is headquartered. The raw data is obtained from The Green Papers website (http://www.thegreenpapers.com/G14/Comparative_Political_Party_Predominance.phtml), where they calculate their scores by tabulating the results of the General Elections in each state, assigning a weight for each election. The weights are: Presidential or Gubernatorial (20 points each), Senatorial (15 points each), U.S. House of Representatives and Houses of the State's legislature (10 points each). The website reports values ranging from 51 to 100, prefixed by a D or R to indicate which party is dominant in a state. I convert the score prefixed by R by subtracting it from 100, yielding an index value between 0 and 100, where the higher the score, the greater the power of the Democratic party in the state is.
Non-Profit Organization Variable (as reported on Form 990)	
NPO	An indicator that equals one for nonprofit organizations and zero otherwise.
EMP	The total number of employees.
VTR	The total number of volunteers.
Program Ratio	The ratio of program service expenses to total expenses.
Fundraising Exp	The total fundraising expenses.
Gov Grant	The government grants received by the organization.
Direct Donation	The direct donations received by the organization.
PSR	The program service revenues.

PSE	The program service expenses.
Governance Index	Equal to the sum of indicator variables for the following 5 governance policies reported on Section VI of the Form 990: the existence of CEO salary setting policies, the existence of an audit committee, majority of directors are independent, there are no outsourced management functions, and the organization provides regulatory filings on its website.
Board Independence	The ratio of the number of independent board members to total voting board members.
ROA	Total revenue minus total expenses, divided by total assets.
Leverage	Total liabilities divided by total assets.
Liquid Asset	The sum of year-end non-interest-bearing cash, savings and temporary cash investments and publicly traded securities.
Exit	An indicator variable that equals 1 for the year the NPO terminates its operations, and zero otherwise.
Information Technology Variable	
IT Budget	The estimated total IT budget as reported on CiTDB.

CHAPTER 2

THE EFFECT OF STATE MINIMUM WAGE INCREASES ON CEO COMPENSATION: EVIDENCE FROM NONPROFIT ORGANIZATION

Introduction

Agency theory (e.g., Jensen and Meckling 1976) underlies most of the empirical archival compensation literature in Accounting and Finance, whereby owners and managers have partially divergent goals, and owners rely on a combination of compensation incentives and monitoring to mitigate value reducing behavior. In contrast, stewardship theory (e.g., Davis, Schoorman and Donaldson 1997) assumes a level of objective alignment between owners and managers, whereby managers are concerned with both the continued existence of the enterprise, as well as fulfillment of its mission. While managerial motive and motivation are hard for an archival researcher to infer, prior research suggests that under certain circumstances, employees and executives accept lower pay at nonprofit organizations (henceforth NPOs) than they could earn in corresponding for-profit organizations. This finding, which is consistent with stewardship theory, has been termed 'labor donation' (e.g., Handy and Katz 1988; Preston 1989). In this study, I attempt to provide causal evidence for labor donation by employing a series of exogenous events; specifically, I examine state level minimum wage increases that likely raise the costs of the organization, examining whether those increases lead to decreases in CEO pay, a.k.a., labor donation.

While the federal minimum wage of $7.25 has not increased since 2009, states and cities are allowed to set higher minimum wages, which most have done. According to Wolters Kluwer (2021), 26 states and several cities have announced minimum wage increases that will take effect in 2022, with the highest rate being $17.64 per hour for hotel workers in West Hollywood, California.[27] Minimum wage hikes constitute exogeneous shocks to labor costs, raising wage costs for many employers; this is true even for employers that do not pay minimum wages as they now have to compete with other organizations that have raised their wages. In response NPOs, which normally operate on a thin margin, must raise additional revenue, cut costs, or engage in some combination of the two. While Chapter 1 shows that employment growth in NPOs is adversely affected by these increases, their findings are more consistent with a phased-in decrease, as NPOs reduce headcount via attrition as opposed to firings or layoffs. A more immediate approach could be to reduce compensation, which could be better absorbed by highly paid top executives. Thus, I propose that as NPOs face increased costs imposed by state minimum wage hikes, they are likely to cut the pay of higher paid executives to preserve the services they provide, as well as the jobs of their employees. I argue that this will be the case since client welfare is the organizational goal and intrinsically motivated NPO executives internalize its objectives.

In this chapter, same as Chapter 1, I focus on 21 state-level minimum wage increases occurring between 2013 and 2015 and use a staggered difference-in-difference (DiD) analysis like that in Cengiz, Dube, Lindner and Zipperer (2019). I show that a significant increase in the state-level minimum wage causes a significant decrease in CEO compensation in NPOs after controlling for firm characteristics like profitability. In contrast, I find that minimum wage increases have no impact on CEO compensation in for-profit organizations. The findings are consistent with both stewardship theory and the prior

[27]https://www.wolterskluwer.com/en/news/more-than-half-of-us-states-to-institute-a-minimum-wage-increase-in-2022

literature, which posit that individuals working in NPOs are willing to work for less money because they are intrinsically motivated and enjoy nonmonetary benefits from fulfilling the NPOs mission, i.e., labor donation.

I recognize that not all NPOs are subject to financial constraints, and as such they can absorb the increased costs associated with the minimum wage increase. Those NPOs are less likely to need a labor donation from their CEO. Consequently, I partition the sample based on financial need. I find that CEOs in NPOs with a smaller financial cushion are more likely to take pay cuts.

I then dig deeper to ascertain whether the results are consistent with stewardship theory, i.e., labor donations. While in the NPO setting there are no owners, there are multiple stakeholders or principals who include, but are not limited to, donors and recipients of services. Likewise, agents include executives and non-executive employees, as well as volunteers. Prior literature examines differences in wages between the for-profit and nonprofit sectors generally finding evidence that nonprofit workers earn less (Handy and Katz 1998, Roomkin and Weisbrod 1999).[28] The most prominent explanation for this finding is the "labor donation hypothesis," which indicates that some individuals enjoy nonmonetary benefits from working in a nonprofit and are therefore willing to work for less (Preston 1989, Francois 2000, Leete 2006). Burgess and Ratto (2003) argue that people who choose to work for NPOs are endowed with intrinsic motivation. Perry and Wise (1990) argue that the intrinsic motivation in public service consists of both the "desire to

[28] Not all studies find evidence of labor donation. Ruhm and Borkoski (2003), DeVaro and Brookshire (2007), and Ben-Ner, Ren, and Paulson (2011) find no wage differential between NPOs and for-profits, while Borgas, Frech, and Ginsburg (1983), Preston (1988), and Mocan and Tekin (2003) find situations where NPOs pay more than their for-profit counterparts. Thus, while there is mixed evidence of labor donation for employees, when I focus on top management, the evidence is clear that labor donations are likely to occur at the highest level of the organization (Handy and Katz 1998, Roomkin and Weisbrod 1999, Ballou and Weisbrod 2003, Hallock 2004).

serve" and the "desire to participate" as the latter "can be exciting, dramatic, and reinforcing of an individual's image of self-importance" (p. 368).[29]

In the NPO setting, stewardship theory suggests "shared collective interests" among stakeholders (Van Slyke 2007, 159). Consequently, as the increased costs associated with exogenously imposed wage increases have the potential to cause a reduction in services provided and/or layoffs, I expect NPO CEOs who are intrinsically motivated to reduce their compensation to reduce costs incurred by other stakeholders such as layoffs of lower-level employees, or reduction in client services.

I employ several proxies to measure the degree to which CEOs are intrinsically motivated. Founders are more intrinsically motivated than non-founders and derive greater nonmonetary benefits from working in the organizations they created (Wasserman 2006). More directly, founder CEOs identify closely with their organizations, and hence, derive high satisfaction from actions that uphold the organizations' interests (Davis, Schoorman, and Donaldson 1997). Such founder CEOs aspire to preserve their vision and are thus more likely to reduce their compensation as their organizations face financial constraints. Unfortunately, as I are unable to identify the nonprofit founders in the sample, I use the age of the NPO to proxy for the likelihood of the CEO being a founder, i.e., the younger the NPO, the more likely the founder remains in charge. Consistent with the expectation, I find that CEOs of younger NPOs are more likely to take a pay cut in response to state-level minimum wage increases.

[29] Surveys find that nonprofit workers are more likely to report that their work is more important than their pay (Mirvis and Hackett, 1983), higher levels of job satisfaction (Benz, 2005), and a higher "ideal number of hours worked" (Lanfranchi, Narcy, and Larguem, 2010). Gregg, Grout, Ratcliffe, Smith, and Windmeijer (2011) show that workers who are more likely to "donate labor" sort into the nonprofit sector.

Since I have little data on NPO CEOs' personal characteristics, I use the level of religiosity and social capital in NPOs' headquarter counties to proxy for CEOs' intrinsic motivation. Social identity theory suggests that individuals are shaped by local culture and beliefs through social interactions, which reinforce their preferences for shared social norms (e.g., Tajfel 1978; Hogg and Abrams 1988). I expect a greater pay cut among nonprofit CEOs who are more likely to be intrinsically motivated, which I argue are nonprofit CEOs of NPOs that are headquartered in a more religious county or a county with a higher-level of social capital. Consistent with the expectation, I find that increases in minimum wages lead to CEO compensation reductions, but only in nonprofits headquartered in counties with higher levels of religiosity and social capital.

Analogously, I expect that CEOs of NPOs located in smaller towns are more likely to be known to the community and to the recipients of the NPOs' services. Due to concerns about his/her reputation, I expect such CEOs to be less likely to reduce services provided and more likely to cut their compensation instead. Consistent with the expectation, I find that CEOs of NPOs headquartered in smaller counties (based on county population) experience a pay cut in response to the increase in the state-level minimum wage, while those in larger counties do not.

This chapter makes several contributions to the literature. First, I add to the literature examining the determinants of CEO compensation, by showing that an exogenous shock to labor cost, results in a decrease in NPO CEO compensation, after controlling for other determinants of compensation including size and profitability. In contrast, I do not find an impact of such a shock on CEO compensation in for-profit organizations. This differential finding between for-profits and NPOs is consistent with stewardship theory, and more specifically labor donation as suggested by Preston (1989) among others.

Second, the study contributes to the literature on NPOs by showing how executive compensation responds to an increase in the minimum wage. Prior studies on CEO compensation in nonprofits have primarily focused on internal determinants, such as program spending (Baber, Daniel and Roberts 2002), endowments (Core, Guay, and Verdi 2006), and profitability (Balsam and Harris 2018). One exception is Dhole, Khumawala, Mishra and Ranasinghe (2015) who find that the California Nonprofit Integrity Act of 2004 had the unintended consequence of raising CEO compensation in affected NPOs. In this study, I add to this literature by investigating the effect of a public policy, i.e., increases in minimum wages, on NPO CEO compensation.

Lastly, I contribute to the literature on the consequences of minimum wage increases, which has mostly examined the impact on employment and income of lower wage employees in for-profits (Cengiz, Dube, Lindner, and Zipperer 2019; Clemens and Wither 2019). An exception is Chapter 1 of this dissertation that studies the effect of state-level minimum wage increases on employment growth, the substitution of labor and capital, and firm exit in nonprofit organizations. In contrast to prior research, I investigate the effect of minimum wage increases on NPO compensation and document a likely unintended consequence, decreases in the compensation of higher wage employees, i.e., the CEOs. This fulfills what Freeman (1996) suggests is the goal of a minimum wage “to redistribute earnings to low-paid workers.”

This chapter continues with section 2 where I describe the data and explains the methodology. Section 3 provides the primary empirical results, including the comparison of the response of for-profits and NPOs. Section 4 presents the cross-sectional analysis. I present the conclusions in section 5.

Data and Methodology

Data Sources and Sample Construction

I obtain state-level minimum wages from Vaghul and Zipperer (2016).[30] As many states permit subminimum wages under certain circumstances, I use the maximum of the minimum wages in a year for each state following Dai and Qiu (2021). The final sample consists of 21 significant minimum wage increase events between 2013 and 2015 as listed in Table 2-1.[31] The reason I use events between 2013 and 2015 despite nonprofit data availability from 2010 to 2018 is that I require a minimum of three years before and three years after the event to conduct the difference-in-differences (DiD) analysis. As shown in Table 2-1, the 21 events occur in 15 unique states, i.e., 6 states experience two significant events. In the study, these multiple events that occur in the same state are treated as independent. Standard errors are clustered at the *State*-level to control for serial correlation over time within a state. Treated states are those that experience a significant minimum wage increase during the 2013–2015 window. For each treated state, I identify control states that meet the following two criteria: 1) located in the same census division[32] as the treatment state; and 2) have only small or no minimum wage increases within the seven-year window surrounding the event year.

[30] Available at https://github.com/benzipperer/historicalminwage.
[31] Following prior literature (Cengiz, Dube, Lindner, and Zipperer 2019 and Dai and Qiu 2021) I define a significant increase as one exceeding 25 cents per hour in 2018 dollars. I convert all minimum wages to 2018 dollars using the Consumer Price research series (CPI-U-RS) from the Bureau of Labor Statistics (BLS), then calculate the increase in real terms. The CPI-U-RS data is available at https://www.bls.gov/cpi/research-series/home.htm.
[32] The United States is broken down into four regions, which are further divided into 9 divisions (https://www.bls.gov/lau/laurdqa.htm). These areas are grouped by the BLS for purposes of analysis and presentation.

The primary data source for non-profits is Form 990, which is the informational return that nonprofit organizations file with the IRS to maintain their tax status. To construct the nonprofit sample, I download Form 990 data directly from the IRS for all available nonprofit organizations with non-missing Schedule J filings for the years 2010–2018,[33] which results in an initial sample of 380,961 organization-year observations.[34] I then limit this sample to 501(c)(3) organizations[35] that have at least three consecutive years of data and have at least ten employees in all years.[36] I also limit the sample to organizations that are headquartered in either a treatment or a control state. After deleting organizations with missing data or outliers following Aggarwal, Evans, and Nanda (2012),[37] I end up with a final sample of 105,213 organization-year observations.

I construct a for-profit sample for comparison. I start with 18,203 firm-year observations with CEO compensation data from Execucomp from 2010 to 2018, and then merge it with Compustat. I require the firms to be headquartered in either a treatment or a control state, have at least three consecutive years of data, and have at least ten employees in all years. This yields a for-profit sample of 16,948 firm-year observations.

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[33] I retrieve electronically filed Form 990s through Amazon Web Services (https://registry.opendata.aws/irs990/). I download all 990 filings in XML format with tax years from 2010 to 2018 and identify each variable according to the schema files provided on the IRS website (https://www.irs.gov/e-file-providers/current-valid-xml-schemas-and-business-rules-for-exempt-organizations-modernized-e-file).

[34] Not all nonprofits are required to file electronically, so the sample is biased toward larger nonprofits. A nonprofit is required to file electronically if it "files at least 250 returns of any type during the calendar year ending with or within the organization's tax year and has total assets of $10 million or more at the end of the tax year". I note that the 250-return threshold includes all filings, including wage and tax statements for employees, aka, W-2s, so any nonprofit with at least 250 employees would meet this threshold.

[35] 501(c)(3) is the most common charitable designation and has been the focus of most nonprofit research.

[36] The minimum number of employees in the for-profit sample is also ten.

[37] I follow Aggarwal, Evans, and Nanda (2012) and identify observations for which one of the four variables, total assets, total compensation, total revenue, and total program expenses, falls below (above) the 1% (99%) of the sample distribution for that year. If an outlier is detected in one year, all observations for that organization are excluded from the sample.

The compensation data for nonprofits is from Schedule J of Form 990,[38] where annual compensation is reported for each key employee. Following Balsam and Harris (2018) among others, I identify the CEO as the key employee with the highest total compensation in a given year, since the CEO role is not identified in the digitalized Form 990. I retrieve state/county-level characteristics from various sources.[39] Real GDP per capita and total population are from the Bureau of Economic Analysis (BEA) Regional Economic Accounts.[40] Unemployment rate is obtained from the Local Area Unemployment Statistics Program through the Bureau of Labor Statistics.[41] The status of the Right-to-Work (RTW) laws in each state is collected from the National Right-to-Work Committee.[42] Religiosity information at the county level is obtained from the "Church and Church Membership" files of the Association of Religion Data Archive (ARDA).[43] County-level social capital index is obtained from the Northeast Regional Center for Rural Development (NRCRD) which conducted surveys in 1997, 2005, 2009, and 2014.[44]

Empirical Strategy

I follow Cengiz, Dube, Lindner, and Zipperer (2019) and Dai and Qiu (2021) and define a minimum wage increase as significant if the increase in the real minimum wage (in 2018 dollars) is at least 25 cents per hour.[45] Following Cengiz, Dube, Lindner, and Zipperer (2019), I employ a difference-in-differences (DiD) approach to examine the level of NPO

[38] Only nonprofits with at least one executive earning more than $150,000 would generally be required to file Schedule J.
[39] I include these variables to control for the impact of locality-specific factors that may influence the variables of interest. The fact that some nonprofits and for-profits operate and employ workers across state boundaries adds noise to the analysis, making it harder for me to find significant results.
[40] https://www.bea.gov/data/economic-accounts/regional.
[41] https://www.bls.gov/lau/.
[42] https://nrtwc.org/facts/state-right-to-work-timeline-2016/
[43] https://www.thearda.com/Archive/Files/Descriptions/RCMSMGCY.asp
[44] https://aese.psu.edu/nercrd/community/social-capital-resources
[45] There are 22 small minimum wage increases that are below 25 cents (in 2018 dollars).

CEO compensation over the seven years (t-3, t+3) surrounding the significant state-level minimum wage increases, using as controls, NPOs headquartered in states that do not have any significant minimum wage increases. I estimate the following multivariate DiD regression to control for firm-, state- and county-level characteristics. I also include *Event*-, *Firm*- and *Year*-fixed effects, and as noted above, cluster standard errors at the state level:

$$\boldsymbol{Ln(Total\ Comp)_{i,j,t} = \alpha + \beta_1 Treat_{i,j} \times Post_t + \beta_2 Post_t + Firm\ Controls_{i,t} +}$$
$$\boldsymbol{State\&County\ Controls_{j,t} + Event\ FE + Firm\ FE + Year\ FE + \varepsilon_{i,j,t},} \qquad (4)$$

where *i* represents organization (NPO or for-profit), *j* represents state or county, and *t* represents year. The dependent variable is the natural logarithm of the total compensation of a nonprofit or for-profit CEO. *Treated* is an indicator that equals one for an organization headquartered in a state with a significant minimum wage increase and zero for control firms. *Post* is an indicator that equals one for the year of and the years after the minimum wage increase, and zero for the years before the minimum wage increase. The main effect for *Treated* is not included in the model since it is absorbed by the fixed effects.

Following Balsam and Harris (2018), I include several controls in Equation (4). All variables for the NPO sample are obtained from Form 990, whereas variables for for-profit firms are retrieved from Compustat. *Ln(NI)*is the natural logarithm of net income, which is a proxy for profitability.[46] NPO and for-profit competition are included to proxy for the competition for executive services, which can affect CEO compensation. I define *Nonprofit Competition* as the number of nonprofits headquartered within a given metropolitan statistical area (MSA) that are in the same industry classification, based on National

[46] Alternatively, in untabulated analyses I use return on assets which is calculated as net income scaled by total assets; and operating margin which is net income scaled by total revenues as profitability measures and the results remain robust.

Taxonomy of Exempt Entities (NTEE), and same size (total assets) quartile. *For-profit Competition* is the number of for-profit organizations in the same NAICS industry and the same size (total assets) quartile.[47] *Ln(Assets)* is a proxy for size, which is the natural logarithm of total assets. To control for governance, I include *board size* and *Indep Ratio* (fraction of independent directors).

In the analysis of nonprofits only, I also include a few NPO-specific variables. *Administrative Efficiency* is one less the ratio of administrative expenses over total expenses, so that a higher ratio indicates more efficient operations. I include three variables to control for NPO income sources: natural logarithms of *Government Grants*, *Program Service Revenue* and *Direct Donations*. Following Balsam and Harris (2018) I include *Charitable*, which is a dummy variable that is equal to one for organizations whose ratio of donations to total revenues is above the sample median, and zero otherwise, expecting that charitable nonprofits will pay less compensation than commercially oriented nonprofits.

In addition to the firm-level variables, I also include controls for county- and state-level economic conditions. *Ln(GDP per Capita)* is the logarithm of county-level GDP per capita. I control for county-level population (*Ln(Population)*) and county-level unemployment (*Unemployment Rate*). I control for the state-level political ideology (*Democrat Points*) following Hoi, Wu, and Zhang (2013), since political ideology could potentially affect minimum wages, nonprofit funding, and CEO compensation. I use data from The Green Papers to measure the relative strength of the Democratic Party compared

[47] I do not require the same MSA because the for-profit sample is relatively small in number, so matching by MSA yields very few competitors.

to the Republican Party for each state-year following their methodology.[48] *RTW Laws* is an indicator variable equals to one for years in which a state has a Right-to-Work Law in effect, zero otherwise. Detailed variable definitions are provided in Appendix 2.

Summary Statistics

Summary statistics for the main samples are reported in Table 2-2 with Panel A providing information on firm-level characteristics for the nonprofit sample, Panel B on the for-profit sample and Panel C on state- and county-level variables. The mean and median *Total Compensation* of nonprofit CEOs are $403.32 thousand and $258.43 thousand, respectively. On average for-profit CEOs earn significantly higher compensation than nonprofit CEOs, as the mean and median of *Total Compensation* in the for-profit sample are $7.23 million and $5.30 million respectively. To some extent this is consistent with prior research showing that the primary determinant of CEO compensation is firm size (e.g., Tosi, Werner, Katz and Gomez-Mejia 2000, Gabaix and Landier 2008, Gabaix, Landier and Sauvagnat 2014). That is, the for-profits are much larger than the nonprofits in the sample, with mean (median) total assets of $15,716.86 ($3,164.00) million for for-profits and $35.90 ($14.86) million for nonprofits. Similarly, comparing net incomes between the two samples I find that for-profits are much more profitable than nonprofits, having a mean net income of $598.35 million versus $1.51 million, respectively.[49] Thus, the differences in compensation between NPOs and for-profits in the sample may not be driven by their status, but by their relative size and profitability.

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[48] The Green Papers updates its measure every other year, and consequently I use its lagged value in the years when it is not available. Please see http://www.thegreenpapers.com/G14/Comparative_Political_Party_Predominance.phtml

[49] Technically, NPOs do not have net income, thus the amount reported in the text and tables is the excess of revenues over expenses.

On average, the nonprofits have a bigger board with slightly higher independence ratio than for-profits in the sample. The mean (median) *Board Size* and *Indep Ratio* for nonprofits are 16.69 (14.00) and 90.19% (100.00%), while for for-profits are 9.54 (9.00) and 85.59% (87.5%) respectively. Focusing on the nonprofit sample in Panel A, I observe that program services (*PSR*) is the largest revenue source, with a mean of $18.72 million, and that the average nonprofit is profitable, i.e., both the mean and median *ROA* are positive at 3.53% and 2.54%, respectively. The fact that most nonprofits are "profitable" is long established in the nonprofit literature (e.g., Chang and Tuckman 1990). The mean (median) *Unrestricted Cash* is $4.09 ($1.67) million.

Main Results

Effect of State Minimum Wage Increases on CEO Compensation in NPOs

I start with investigating whether minimum wage increases result in a decline in nonprofit CEO compensation. The regression results of the baseline estimation are reported in Table 2-3, where the dependent variable is the natural logarithm of total compensation of a nonprofit CEO. In column (1), the test variable is the interaction of *Treated* and *Post*, which is the DiD estimate. The coefficient estimate on *Treated*×*Post* is -0.024 and statistically significant at the 5% level. level. The results are also economically significantly. The coefficient estimate in column (1) suggests that a significant minimum wage increase leads to a 2.37% drop in the CEO total compensation, compared to CEOs in NPOs that do not experience a minimum wage hike. [50] Given the mean annual CEO total compensation in the pre-event period is $386,108, a decrease of 2.37% is equivalent to a decline of $9,156 in total compensation.

[50] Calculated as $\exp^{-0.024}$ -1=0.0237.

A major assumption of the difference-in-differences approach is that the growth of CEO total compensation follows a parallel trend between treated and control nonprofits prior to the significant minimum wage increases. To explore this assumption empirically, I estimate Equation (5) below to investigate the dynamic effects of state minimum wage hikes on compensation.

$$\boldsymbol{Ln(Total\ Comp)_{i,j,t} = \alpha + \beta_{-3} \times Treat_{i,j} \times Year_{i,t}^{-3} + \beta_{-2} \times Treat_{i,j} \times Year_{i,t}^{-2} + \beta_0 \times Treat_{i,j} \times Current_{i,t} + \beta_1 \times Treat_{i,j} \times Year_{i,t}^{+1} + \beta_2 \times Treat_{i,j} \times Year_{i,t}^{+2} + \beta_3 \times Treat_{i,j} \times Year_{i,t}^{+3} + \gamma_{-3} Year_{i,t}^{-3} + \gamma_{-2} Year_{i,t}^{-2} + \gamma_0 Current_{i,t} + \gamma_1 Year_{i,t}^{+1} + \gamma_2 Year_{i,t}^{+2} + \gamma_3 Year_{i,t}^{+3} + Firm\ Controls_{i,t} + State\&County\ Controls_{j,t} + Event\ FE + Firm\ FE + Year\ FE + \varepsilon_{i,j,t},} \quad (5)$$

where $Year_{i,t}^{-3}$, and $Year_{i,t}^{-2}$ are dummy variables that equal one if the year is three or two years prior to minimum wage increases, respectively, and zero otherwise. $Current_{i,t}$ is a dummy variable that equals one in the year when a significant minimum wage increase occurs and zero otherwise. $Year_{i,t}^{+3}$, $Year_{i,t}^{+2}$, and $Year_{i,t}^{+1}$ are dummy variables that equal one if the year is three, two, or one year after a minimum wage increase, respectively, and zero otherwise. The omitted time dummy is $Year_{i,t}^{-1}$, the year immediately before the minimum wage hikes. β_n captures the average change in *Total Compensation* between NPOs in treated and control states across all the 21 events during year n, relative to year −1. If CEO compensation growth has similar pre-trend between the treated and control groups, then β_{-3} and β_{-2} would be statistically insignificant.

The dynamic model results are reported in column (2) of Table 2-3. I see that the coefficients on $Treat \times Year^{-3}$ and $Treat \times Year^{-2}$ are both statistically insignificant, consistent with the parallel trend condition of the DiD design. I observe a significant reduction in CEO total compensation in the year of the minimum wage increase, and the decline continues in the subsequent three years, suggesting a long-term impact on compensation in nonprofit organizations.[51] The findings provide evidence that the minimum wage increases lead to a decline in CEO total compensation.

Among the control variables, net income is not significantly related to CEO compensation. There is a significant negative relationship between administrative efficiency and CEO compensation, which is consistent with the findings of Frumkin and Keating (2010). Both coefficients on direct donations and government grants are insignificant, whereas the program service revenue is significantly positively related to CEO compensation, indicating nonprofit CEOs are rewarded for generating higher revenues from the program services. I find no significant evidence that the CEO compensation is associated with outside job opportunities, which are reflected by the insignificant coefficients on nonprofit competition and for-profit competition. CEOs are paid more in larger NPOs, however the nature of the organization, i.e., whether charitable or not, is not significantly associated with CEO compensation. Governance appears to play a role in compensation in NPOs—stronger governance, i.e., a smaller board and a greater fraction of independent directors, is associated with a lower level of CEO compensation. Turning to the state- and county-level control variables, NPOs located in regions with higher GDP per capital, lower unemployment rate, and more support for the Democratic Party pay a higher-level of CEO compensation. In contrast, population size and the existence of a right-to-work law are not related to CEO compensation.

For-profits vs. Nonprofits

Next, I investigate if significant minimum wage hikes affect CEO compensation in for-profit firms. If the decline in CEO pay in NPOs is the result of CEO labor donation, I

[51] In untabulated analysis, I examined the effect in year-4 and year+4 and found an insignificant effect for year-4 and significant negative effect for year+4.

would not expect a corresponding decline in for-profit firms, since labor donation is a phenomenon documented in the nonprofit arena. I obtain data on for-profit CEO compensation from the ExecuComp database for years 2010 – 2018. Summary statistics for the for-profit sample are reported in Panel B of Table 2-2. Overall, the for-profit sample is much smaller in number than the nonprofit sample since Execucomp only covers large firms in the S&P1500 index. As noted above, for-profit CEOs earn greater compensation than their nonprofit counterparts, a difference that could be explained by these for-profits firms being more profitable and much larger in size.

I estimate the DiD models in Equations (4) and (5) using the for-profit sample, however I include fewer control variables than above, since the NPO-specific variables including *Admin Efficiency*, *Ln(Direct Donation)*, *Ln(Gov Grants)*, *Ln(PSR)*, and *Charitable*, are not available for for-profit firms. The dependent variable for the for-profit sample is the natural logarithm of CEO total compensation, which is the sum of the salary, bonus, stock awards, option awards, non-equity incentives, change in pension value and non-qualified deferred compensation earnings,[52] and other compensation. I report the main model and the dynamic model in Table 2-4 columns (1) and (3) respectively. To ensure that excluding the NPO-specific variables does not affect the results, I re-estimate Equations (4) and (5) using the nonprofit sample without the aforesaid NPO-specific variables, and report results in columns (2) and (4), respectively.

The coefficient on *Treat×Post* is positive but statistically insignificant for the for-profit sample as shown in column (1). However, the DiD estimate on *Treat×Post* is significantly negative for NPOs in column (2), with the results very similar to those in

[52] Composed of a) above-market or preferential earnings from deferred compensation plans; and b) aggregate increase in actual value of defined benefit and actual pension plans during the year.

Table 2-3, suggesting that dropping the NPO-specific variables does not affect the DiD estimate.

Cross-sectional Analysis

The Existence of a Financial Cushion

The results in Table 2-3 are consistent with stewardship theory/labor donation. The interpretation is that these CEOs are taking pay cuts to preserve services and employee jobs. If so, this effect is likely to be stronger in NPOs that do not have a financial cushion to absorb the increased costs associated with the increased minimum wage. To examine this conjecture, I partition the sample into NPOs with low- vs. high-net assets, where net assets are computed as total assets minus total liabilities, then scaled by total assets. For each minimum wage event, I partition the sample into two groups: those with low-net assets are the NPOs with pre-event net assets below the sample median and the remaining NPOs are categorized as those with high-net assets. The higher the ratio, the more likely a nonprofit is able to absorb the increased cost associated with the increased minimum wage, without cutting CEO compensation. The expectation is that I will observe stronger effects for NPOs with lower-net assets.

In Table 2-5, I estimate the baseline DiD regression for the two subsamples of NPOs with low- and high-net assets, reporting results in columns (1) and (2), respectively. Consistent with this expectation, I observe a significant negative coefficient on *Treat*×*Post* only in NPOs with low-net assets. In contrast, the coefficient on *Treat*×*Post* is much smaller in magnitude and statistically insignificant in NPOs with high-net assets.

In sum, I find that minimum wage increases have a negative effect on CEO total compensation, however only in NPOs with lower level of net assets. This implies that the adverse effect of increased labor costs can be mitigated in NPOs with significant financial resources.

Impact of Intrinsic motivation

The theory underlying labor donation is that the individual, in this case the CEO, is intrinsically motivated and thus will accept lower compensation than he or she would otherwise. While I cannot directly observe CEOs' intrinsic motivation, I expect that it is influenced by local culture because of the following two premises. First, people choose to work and live in the areas where they are comfortable with the local culture and beliefs.[53] Second, social identity theory suggests that individuals are shaped by local culture and beliefs through social interactions, which reinforce their preferences for shared social norms (e.g., Tajfel 1978; Hogg and Abrams 1988). In this section, I partition the sample based on factors I believe are correlated with CEO intrinsic motivation, then examine whether CEO compensation, or more precisely, reductions in CEO compensation in response to the increase in minimum wage, is related to those factors.

Religiosity

The first proxy for intrinsic motivation is religiosity. Theoretical models have emphasized how beliefs in divine rewards and punishments (or a Calvinistic desire to self-signal one's predestined fate) can induce individuals to behave more prosaically and cooperatively (Saleam and Moustafa 2016). The Bible is full of verses imploring people to help the poor and the weak and protect the helpless. I therefore expect that religious people

[53] For example, Tom (1971) shows that people prefer environments that have the "same" personality profile as they do. Similarly, Hilary and Hui (2009) find that when they switch jobs, CEOs tend to join firms with a similar local religious belief as that of their previous firm. In addition, individuals are shaped by local culture and beliefs through social interactions, which reinforce their preferences for shared social norms.

are more likely to care about the poor and the needy, and therefore will be more likely to cut their pay to avoid reductions in employment or services.

I obtain county level religiosity data from the "Church and Church Membership" files of the ARDA. *Religiosity* is computed as total number of adherents reported by all denominations divided by total population, which is the fraction of religious people in the population of the county. For each minimum wage event, I sort NPOs based on the average *Religiosity* in the pre-event period, defining low-religiosity NPOs as those headquartered in counties with religiosity below or equal to the sample median, and high-religiosity otherwise. I estimate the baseline DiD regression in the two subsamples of NPOs with low- and high-religiosity, results of which are reported in columns (1) and (2) of Table 2-6, respectively. The DiD estimate is negative in both columns, however only statistically significant in column (2), i.e., among NPOs located in counties with a high level of *Religiosity*. The finding that the decline in CEO compensation post minimum wage increase is statistically significant only in areas with high level of religiosity provides additional evidence of labor donation.

Social Capital

The second proxy for intrinsic motivation is *Social Capital*, which reflects secular norms (i.e., altruism, trust, and cooperation). High levels of social capital facilitate collective action and economic transactions, encourage cooperation, and discourage opportunistic behaviors, resulting in positive economic outcomes (Jha and Chen 2015; Hoi, Wu, and Zhang 2019; Hasan, Hoi, Wu, and Zhang 2020). Either through self-selection or through social influence from family members, friends, neighbors, coworkers, etc., people who reside in the areas with a higher level of social capital are more likely to care about the welfare of the poor, the weak, the needy, and the community, and have a stronger desire to serve and participate, which reinforces their image of self-importance. Thus, I expect CEOs of NPOs located in areas with high levels of social capital will be more willing to take a pay cut to preserve the jobs and services their organizations provide.

I obtain county-level social capital data from the Northeast Regional Center for Rural Development (NRCRD), which compiled the database from surveys conducted in 1990, 1997, 2005, 2009, and 2014. The data contain information on voter turnout in presidential elections (*Pvote),* response rates to U.S. census surveys (*Respn*), total number of ten types of social organizations (*Assn*), and total number of nonprofit organizations (*Nccs*). Following Hoi, Wu, and Zhang (2019), I measure *Social Capital*, using the first principal component from a factor analysis of *Pvote, Respn, Nccs*, and *Assn*. Since estimated *Social Capital* is only available in 1990, 1997, 2005, 2009, and 2014 I follow Hilary and Hui (2009) and Hoi, Wu, and Zhang (2019) to backfill data for the missing years using values in the preceding year when data are available.

For each minimum wage event, I sort NPOs based on the average *Social Capital* score in the pre-event period, defining low-social capital NPOs as those headquartered in counties with social capital below or equal to the sample median, and high-social capital NPOs as all others. I estimate the baseline DiD regression in the two subsamples of NPOs with low- and high-social capital, reporting results in columns (3) and (4) of Table 2-6, respectively. I find that the coefficient on Treat*Post is significantly negative only in column (4), which is the subsample of NPOs located in counties with high level of social capital. The finding of a stronger effect in areas with a high level of social capital provides additional evidence of labor donation.

County Population

Next, I examine the impact of county size. I expect that CEOs of NPOs located in smaller counties are more likely to be known to the community, and to the recipients of their NPOs' services. Because of this personal connection, I expect such CEOs to be less likely to reduce services provided and instead, be more likely to take a voluntary pay cut.

As noted above I get county population data from the Bureau of Economic Analysis (BEA) Regional Economic Accounts. For each minimum wage event, I sort NPOs based on the average county population in the pre-event period, defining low-county population NPOs as those headquartered in counties with population below or equal to the sample median, and high-county population NPOs as all others. I estimate the baseline DiD regression in the two subsamples of NPOs with low- and high-county population, reporting results in columns (5) and (6) of Table 2-6, respectively. I find that the coefficient on Treat*Post is significantly negative only in column (5), which is the subsample of NPOs located in smaller counties. The finding of a stronger effect in NPOs headquartered in smaller counties lends support to labor donation.

NPO Age

Stewardship theory explains why founders CEOs are willing to accept lower compensation than non-founder CEOs. This is because founders are more intrinsically motivated and derive more nonmonetary benefits from working in the organizations they created. Founders feel a strong sense of attachment to and psychological ownership of the organization, and thus are more likely to behave as stewards (Wasserman 2006). Therefore, I argue the founder CEOs of NPOs are more likely to be organizationally centered executives (Davis, Schoorman, and Donaldson 1997) who identify closely with their organizations, and hence, derive high satisfaction from actions that uphold their organizations' interests. Such founder CEOs aspire to preserve their vision and will be more likely to take a pay cut than to reduce services.

Since I do not have information on whether the CEO is a founder, I proxy for it using the age of the NPO, where NPO age is the number of years since the founding year as reported on Form 990. The younger the NPO, the more likely the founder remains in charge. For each minimum wage event, I sort NPOs based on their average age in the pre-event period and define young NPOs as those with age below or equal to the sample median and all other NPOs as "old". I run the baseline DiD models in the two subsamples of old and young NPOs, reporting results in columns (7) and (8) of Table 2-6, respectively. The coefficient on Treat*Post is insignificant among old NPOs, but significantly negative among young NPOs, consist with the conjecture that CEOs in younger nonprofits have a higher likelihood of founder presence, are hence more likely to engage in labor donations.

For-profit Partitions

In Table 2-7, I partition the for-profit sample using the same categories as above, net assets, religiosity, social capital, and county population. In contrast to the results for NPOs presented in Tables 2-5 and 2-6, I do not observe any significant coefficients on *Treat*Post* in any of the subsamples. The lack of finding for for-profit organizations suggests that labor donation is unique to NPOs.

Conclusion

In this chapter, I investigated whether and how CEO compensation in nonprofit organizations (NPOs) responds to an exogeneous increase in labor cost resulting from state-level minimum wage hikes. I document a significant decline in the compensation of NPO

CEOs following these exogenous shocks. This decline is consistent with Stewardship theory, whereby the CEO views him/herself as the steward of the organization and thus takes steps to ensure the survival of his/her organization and the preservation of its mission. The act of taking a pay cut is consistent with labor donation, a manifestation of the Stewardship theory and a phenomenon previously observed by nonprofit researchers (e.g., Handy and Katz 1988; Preston 1989). The study provides causal evidence on labor donation in NPOs, as I examine the response of CEO compensation to an exogenous shock to NPOs' labor cost.

I then conducted cross-sectional analysis based upon partitions where I expect to identify those CEOs more likely to take pay reductions. I find, as expected, decreases in CEO pay are more likely in NPOs with fewer financial resources. I also employ several partition variables that proxy for CEOs' intrinsic motivation, finding significant declines in CEO compensation following significant minimum wage hikes in NPOs that are headquartered in smaller counties, counties with higher levels of religiosity, and in counties with greater levels of social capital. Finally, I find declines in compensation in younger NPOs, which I believe are more likely to be run by their founders.

In contrast, I do not find any significant impact on the compensation of CEOs of for-profit companies in the full sample or various aforementioned partitions I conduct for the NPO sample. Overall, while NPOs reduce their CEO compensation in response to an exogenous shock that increases labor costs, the same shock has no impact on the compensation of for-profit CEOs. Thus, the results provide support for Stewardship theory/labor donation in NPOs.

Table 2-1. State Minimum Wage Increase Events

This table reports the state and the year/month for each of the 21 significant minimum wage increase events used in the analysis. I also report the level (after the increase), the increase, and the percentage of increase of minimum wage, all of which are in 2018 constant dollars.

Event ID	State	Abbreviation	Year	Month	Min. Wage	Increase	Increase (%)
1	Connecticut	CT	2014	1	$9.24	$0.33	3.70
2	Connecticut	CT	2015	1	$9.70	$0.46	4.99
3	Massachusetts	MA	2015	1	$9.54	$1.05	12.31
4	Rhode Island	RI	2013	1	$8.37	$0.26	3.17
5	Rhode Island	RI	2015	1	$9.54	$1.05	12.31
6	Vermont	VT	2015	1	$9.70	$0.43	4.63
7	New Jersey	NJ	2014	1	$8.76	$0.93	11.90
8	New York	NY	2013	12	$8.64	$0.69	8.70
9	New York	NY	2014	12	$9.29	$0.65	7.55
10	Michigan	MI	2014	9	$8.65	$0.66	8.30
11	Minnesota	MN	2014	8	$8.49	$0.67	8.51
12	Minnesota	MN	2015	8	$9.54	$1.05	12.31
13	Nebraska	NE	2015	1	$8.48	$0.78	10.16
14	South Dakota	SD	2015	1	$9.01	$1.31	17.04
15	District of Columbia	DC	2014	7	$10.09	$1.18	13.23
16	District of Columbia	DC	2015	7	$11.13	$1.04	10.34
17	Delaware	DE	2014	6	$8.23	$0.40	5.12
18	Delaware	DE	2015	6	$8.74	$0.52	6.27
19	Maryland	MD	2015	7	$8.74	$1.05	13.60
20	West Virginia	WV	2015	1	$8.48	$0.78	10.16
21	Arkansas	AR	2015	1	$7.95	$0.25	3.27

Table 2-2. Summary Statistics

The sample in Panels A and B consists of 105,213 and 16,948 firm-year observations of non-profit and for-profit organizations, respectively, that fall within a (-3, 3) years window around the years in which minimum wage events occurred during 2013 to 2015. Panel A reports summary statistics on firm-level characteristics for NPOs, Panel B for for-profit firms, and Panel C presents summary statistics on state- and county-level variables.

Panel A. Firm-Level Characteristics for NPOs

	N	Mean	STD	P25	Median	P75
CEO Compensation (thousand $)	105,213	403.32	548.13	191.31	258.43	407.48
Net Income (million $)	105,213	1.51	3.99	-0.03	0.35	1.48
ROA (%)	105,213	3.53	12.87	-0.34	2.54	7.03
Operating Margin (%)	105,213	4.53	13.89	-0.39	2.99	8.28
Admin Efficiency (%)	105,213	85.25	8.10	81.03	86.64	90.71
Direct Donation (million $)	105,213	1.76	5.24	0.01	0.29	1.55
Gov Grant (million $)	105,213	1.94	7.37	0.00	0.00	0.80
PSR (million $)	105,213	18.72	27.73	2.83	9.14	21.47
NPO Competition	105,213	3.48	1.19	2.64	3.47	4.26
For-profit Competition	105,213	2.19	1.04	1.10	1.95	3.50
Total Assets (million $)	105,213	35.90	53.67	5.13	14.86	41.44
Board Size	105,213	16.69	9.90	10.00	14.00	21.00
Indep Ratio (%)	105,213	90.19	20.05	90.00	100.00	100.00
Unrestricted Cash (million $)	105,213	4.09	8.30	0.55	1.67	4.38
Ln(Total Compensation)	105,213	12.62	0.63	12.16	12.46	12.92
Ln(Net Income)	105,213	0.43	0.87	-0.04	0.30	0.91
Ln(Direct Donation)	105,213	0.58	0.73	0.01	0.26	0.93
Ln(Gov Grant)	105,213	0.45	0.79	0.00	0.00	0.59
Ln(PSR)	105,213	2.25	1.22	1.34	2.32	3.11
Ln(Total Assets)	105,213	2.80	1.29	1.81	2.76	3.75
Ln(Unrestricted Cash)	105,213	1.14	0.87	0.44	0.98	1.68

Panel B. Firm-Level Characteristics for For-profit Firms						
	N	Mean	STD	P25	Median	P75
CEO Compensation (thousand $)	16,948	7,229.92	7,738.16	2,906.53	5,300.33	9,257.27
Net Income (million $)	16,948	598.35	1,645.51	48.11	143.79	428.48
NPO Competition	16,948	0.09	0.57	0.00	0.00	0.00
For-profit Competition	16,948	2.17	1.14	1.10	1.95	3.18
Total Assets (million $)	16,948	15,716.86	62,614.60	1,110.09	3,164.00	9,512.43
Board Size	16,948	9.54	2.28	8.00	9.00	11.00
Indep Ratio (%)	16,948	85.59	7.27	83.33	87.50	90.00
Ln(Total Compensation)	16,948	15.38	0.87	14.88	15.48	16.04
Ln(Net Income)	16,948	4.82	1.37	3.89	4.98	6.06
Ln(Total Assets)	16,948	8.24	1.50	7.01	8.06	9.16

Panel C. State- and County-Level Characteristics						
	N	Mean	STD	P25	Median	P75
GDP per Capita (thousand $)	7,413	46.43	29.33	32.79	41.56	53.41
Population (thousand people)	7,413	191.36	379.17	27.62	64.32	181.58
Unemployment Rate (%)	7,413	6.07	2.37	4.30	5.70	7.60
Democrat Points	7,413	42.08	28.04	18.00	43.00	66.00
RTW Law	7,413	0.53	0.50	0.00	1.00	1.00
Ln(GDP per Capita)	7,413	3.75	0.40	3.49	3.73	3.98
Ln(Population)	7,413	11.17	1.42	10.23	11.07	12.11

Table 2-3. Effect of State Minimum Wage Increases on Nonprofit CEO Compensation

This table reports the effect of state minimum wage increases on nonprofit CEOs' total compensation during 2010 through 2018. I employ a difference-in-differences (DiD) design, where treated NPOs are those with headquarters located in the states that experience a significant minimum wage increase in a given year, and control NPOs are those that are located in the same census division as the treatment state but have no minimum wage increase or have only trivial minimum wage increases within the seven-year (-3, 3) window. The dependent variable is the natural logarithm of total compensation of a nonprofit CEO. *Treated* is a dummy variable that equals one for treated NPOs and zero for control NPOs. In column (1), *Post* is a dummy variable that equals one for the year when minimum wage increase occurs and the years afterward, and zero for the years before the minimum wage increases. In column (2), $Year^{-3}$ and $Year^{-2}$ are dummy variables that equal one if the sample year is three and two years prior to minimum wage increases, respectively, and zero otherwise. $Year^{+3}$, $Year^{+2}$, and $Year^{+1}$ are dummy variables that equal one if the sample year is three, two, and one years after minimum wage increases, respectively, and zero otherwise. All variable definitions are reported in the Appendix B. I include *Event*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Total Comp)*	*Ln(Total Comp)*
	(1)	(2)
Treated×Post	-0.024**	
	(0.019)	
Post	0.002	
	(0.768)	
$Treated \times Year^{-3}$		-0.006
		(0.569)
$Treated \times Year^{-2}$		-0.009
		(0.432)
Treated×Current		-0.021**
		(0.020)
$Treated \times Year^{+1}$		-0.026**
		(0.050)
$Treated \times Year^{+2}$		-0.024**
		(0.024)
$Treated \times Year^{+3}$		-0.036***
		(0.004)
$Year^{-3}$		0.007
		(0.612)
$Year^{-2}$		0.012
		(0.223)
Current		-0.007
		(0.435)
$Year^{+1}$		-0.008

		(0.602)
Year^{+2}		-0.021
		(0.254)
Year^{+3}		-0.033
		(0.230)
Ln(NI)	0.002	0.002
	(0.511)	(0.518)
Admin Efficiency	-0.001*	-0.001*
	(0.053)	(0.057)
Ln(Direct Donation)	0.004	0.004
	(0.543)	(0.557)
Ln(Gov Grants)	0.005	0.005
	(0.687)	(0.700)
Ln(PSR)	0.071***	0.071***
	(0.000)	(0.000)
NPO Competition	-0.004	-0.005
	(0.569)	(0.541)
FPO Competition	-0.005	-0.005
	(0.348)	(0.342)
Ln(Assets)	0.028**	0.028**
	(0.029)	(0.029)
Charitable	-0.006	-0.006
	(0.184)	(0.168)
Board Size	0.003***	0.003***
	(0.000)	(0.000)
Indep Ratio	-0.219***	-0.219***
	(0.000)	(0.000)
Ln(Unrestricted Cash)	-0.003	-0.003
	(0.323)	(0.318)
Ln(GDP per Capita)	0.129*	0.132*
	(0.055)	(0.053)
Ln(Population)	-0.003	-0.003
	(0.930)	(0.909)
Unemployment Rate	-0.008**	-0.009**
	(0.035)	(0.040)
Democrat Points	0.000*	0.000*
	(0.080)	(0.074)
RTW Law	0.007	0.004
	(0.640)	(0.780)
Event Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes

Year Fixed Effects	Yes	Yes
Observations	105,213	105,213
Adj. R-squared	0.924	0.924

Table 2-4. Minimum Wage Increases and CEO Compensation: For-Profits vs. Non-Profits

This table reports a comparison of the effect of state minimum wage increases on CEO compensation in for-profit firms (columns (1) and (3)) and nonprofit organizations (columns (2) and (4)). I obtain data on for-profit CEO compensation from the ExecuComp database. The dependent variable is the natural logarithm of CEO total compensation, which is the sum of the salary, bonus, stock awards, option awards, non-equity incentives, change in pension value and non-qualified deferred compensation earnings, and other compensations. All variable definitions are reported in the Appendix 2. I include *Event*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Total Comp)*	*Ln(Total Comp)*
	(1)	(2)
	For-Profit	NPO
*Treat*Post*	0.008	-0.023**
	(0.582)	(0.015)
Post	0.012	0.002
	(0.179)	(0.764)
Ln(NI)	0.058***	0.004
	(0.000)	(0.277)
NPO Competition	0.069	-0.004
	(0.224)	(0.565)
FP Competition	-0.019	-0.005
	(0.596)	(0.401)
Ln(Assets)	0.196***	0.040***
	(0.000)	(0.000)
Board Size	-0.002	0.003***
	(0.787)	(0.000)
Indep Ratio	0.411*	-0.219***
	(0.059)	(0.000)
Ln(GDP per Capita)	-0.029	0.116*
	(0.919)	(0.069)
Ln(Population)	0.826**	0.002
	(0.016)	(0.934)
Unemployment Rate	-0.011	-0.008**
	(0.461)	(0.033)
Democrat Points	-0.001**	0.000*
	(0.023)	(0.053)
RTW Law	0.046	0.005
	(0.313)	(0.742)

Wald Test	2.96*	
	(0.085)	
Event Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	16,948	105,213
Adj. R-squared	0.807	0.924

Table 2-5. Minimum Wage Increases and Nonprofit CEO Compensation: Conditional on Net Assets

This table reports the effect of state minimum wage increases on nonprofit CEOs' compensation conditional on high vs. low level of net assets. Net assets are computed as total assets minus total liabilities, scaled by total assets. For each minimum wage event, I sort NPOs into two groups: those with low-net assets are the NPOs with pre-event average net assets below the sample median, and the rest are categorized as NPOs with high-net assets. The dependent variable is the natural logarithm of total compensation of a nonprofit CEO. All variable definitions are reported in the Appendix 2. I include *Event*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Total Comp)*	*Ln(Total Comp)*
	(1)	(2)
	Low-Net Assets	High-Net Assets
Treated×Post	-0.043***	-0.002
	(0.008)	(0.799)
Post	0.007	-0.003
	(0.564)	(0.731)
Ln(NI)	0.006	-0.002
	(0.124)	(0.678)
Admin Efficiency	-0.002	-0.001
	(0.100)	(0.400)
Ln(Direct Donation)	-0.000	0.006
	(0.995)	(0.448)
Ln(Gov Grants)	-0.005	0.013
	(0.653)	(0.346)
Ln(PSR)	0.073***	0.066***
	(0.000)	(0.000)
NPO Competition	0.000	-0.014
	(0.985)	(0.385)
FP Competition	-0.004	-0.006
	(0.487)	(0.488)
Ln(Assets)	0.019	0.047***
	(0.111)	(0.007)
Charitable	0.009	0.006
	(0.207)	(0.317)
Board Size	0.004***	0.002**
	(0.001)	(0.020)
Indep Ratio	-0.219**	-0.204***
	(0.017)	(0.001)

Ln(Unrestricted Cash)	-0.005	0.000
	(0.235)	(0.857)
Ln(GDP per Capita)	0.147*	0.106*
	(0.060)	(0.087)
Ln(Population)	-0.008	-0.002
	(0.784)	(0.965)
Unemployment Rate	-0.009*	-0.008
	(0.079)	(0.138)
Democrat Points	0.000	0.000
	(0.261)	(0.257)
RTW Law	-0.011	0.029
	(0.653)	(0.133)
Wald Test	6.59**	
	(0.010)	
Event Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	50,762	47,591
Adj. R-squared	0.917	0.922

Table 2-6. Minimum Wage Increases and NPO CEO Compensation: Conditional on Intrinsic Motivation

This table reports the effect of state minimum wage increases on nonprofit CEOs' total compensation conditional on intrinsic motivation, which is proxied by religiosity in columns (1) and (2), social capital in columns (3) and (4), county population in columns (5) and (6), and NPO age in columns (7) and (8). *Religiosity* is computed as total number of adherents reported by all denominations divided by total population, which is the ratio of religious people in the population of the county as defined by ARDA. I obtain county-level social capital data from the Northeast Regional Center for Rural Development (NRCRD), which are based on the surveys conducted in 1990, 1997, 2005, 2009, and 2014. Following Hoi, Wu, and Zhang (2019), I fill in the data for the missing years using the values in the preceding year in which data are available. *County population* is the population in the county where the NPO's headquarter is located. *NPO age* is the number of years since the founding year as reported on Form 990. For each minimum wage event, I sort NPOs based on the average religiosity measure, the average social capital score, the average county population, or the average NPO age across years in the pre-event period, and define Low-Religiosity (Low-Social Capital, Low-County Population, or Young) NPOs as those with such measure below or equal to the pre-event sample median, and High-Religiosity (High-Social Capital, High-County Population, or Old) NPOs as those with such measure above the pre-event sample median. The dependent variable is the natural logarithm of total compensation of a nonprofit CEO. All variable definitions are reported in the Appendix 2. I include *Event*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low-Religiosity	High-Religiosity	Low-Social Capital	High-Social Capital	Low-County Population	High-County Population	Young NPO	Old NPO
Treated×Post	-0.008	-0.040***	-0.014	-0.032***	-0.032**	-0.006	-0.031**	-0.017
	(0.467)	(0.009)	(0.303)	(0.005)	(0.031)	(0.514)	-0.039	-0.134
Post	-0.011	0.015	-0.005	0.009	0.003	0.001	0.003	0.001
	(0.141)	(0.229)	(0.581)	(0.391)	(0.749)	(0.936)	-0.735	-0.922
Ln(NI)	0.003	0.003	-0.000	0.007	0.006	-0.001	0.004	0.002
	(0.506)	(0.623)	(0.935)	(0.236)	(0.175)	(0.790)	-0.168	-0.647
Admin Efficiency	-0.002**	-0.001	-0.002*	-0.001	-0.001	-0.001**	-0.001	-0.001
	(0.012)	(0.621)	(0.052)	(0.262)	(0.173)	(0.049)	-0.134	-0.322
Ln(Direct Donation)	0.012	-0.007	0.002	0.003	-0.006	0.012***	-0.005	0.005

	(0.254)	(0.432)	(0.779)	(0.737)	(0.536)	(0.005)	-0.614	-0.644
Ln(Gov Grants)	-0.011	0.023*	0.001	0.012	0.021*	-0.011	-0.009	0.019
	(0.514)	(0.068)	(0.970)	(0.378)	(0.099)	(0.546)	-0.466	-0.244
Ln(PSR)	0.075***	0.066***	0.072***	0.073***	0.091***	0.051***	0.064***	0.081***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	0	0
NPO Competition	-0.008	-0.003	-0.003	-0.012	0.004	-0.014	0.001	-0.013
	(0.493)	(0.766)	(0.793)	(0.289)	(0.702)	(0.267)	-0.937	-0.266
FP Competition	-0.003	-0.009*	-0.007	-0.004	-0.010	-0.000	0.003	-0.017*
	(0.687)	(0.099)	(0.183)	(0.603)	(0.114)	(0.947)	-0.596	-0.052
Ln(Assets)	0.035	0.024**	0.044**	0.010	0.018	0.040**	0.028	0.035**
	(0.112)	(0.042)	(0.030)	(0.359)	(0.138)	(0.043)	-0.166	-0.022
Charitable	0.001	0.013	0.005	0.009	0.023***	-0.010**	-0.002	0.014*
	(0.911)	(0.145)	(0.444)	(0.242)	(0.002)	(0.038)	-0.807	-0.064
Board Size	0.002**	0.003***	0.002**	0.004***	0.002*	0.003***	0.004***	0.002*
	(0.019)	(0.007)	(0.031)	(0.001)	(0.069)	(0.007)	-0.006	-0.055
Indep Ratio	-0.178***	-0.242**	-0.229***	-0.177	-0.209**	-0.208***	-0.173***	-0.301***
	(0.001)	(0.018)	(0.000)	(0.115)	(0.012)	(0.001)	-0.003	-0.005
Ln(Unrestricted Cash)	-0.004	-0.001	-0.006*	0.002	-0.002	-0.004	-0.003	-0.002
	(0.223)	(0.727)	(0.087)	(0.583)	(0.603)	(0.127)	-0.489	-0.59
Ln(GDP per Capita)	0.146*	0.100	0.226**	0.074	0.152	0.105**	0.197**	0.046
	(0.052)	(0.238)	(0.025)	(0.154)	(0.116)	(0.011)	-0.029	-0.46
Ln(Population)	0.006	-0.008	0.000	-0.009			-0.01	0.01
	(0.860)	(0.786)	(0.994)	(0.768)			-0.778	-0.63
Unemployment Rate	-0.007	-0.010	-0.013**	-0.008	-0.005	-0.021***	-0.008*	-0.009*
	(0.223)	(0.141)	(0.038)	(0.147)	(0.311)	(0.005)	-0.079	-0.082
Democrat Points	0.000	0.000	-0.000	0.001*	0.000	0.000	0.001***	0
	(0.415)	(0.289)	(0.706)	(0.080)	(0.123)	(0.465)	-0.001	-0.65
RTW Law	-0.014	0.055**	-0.021	0.028***	0.030	-0.061***	0.006	0.012
	(0.333)	(0.012)	(0.229)	(0.003)	(0.140)	(0.006)	-0.719	-0.401

Wald Test	31.51***		9.61***		21.25***		5.58**	
	(0.000)		(0.002)		(0.000)		(0.018)	
Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,172	48,169	51,984	46,366	53,768	44,555	48,511	49,425
Adj. R-squared	0.920	0.919	0.923	0.915	0.917	0.923	0.927	0.908

Table 2-7: Minimum Wage Increases and For-Profit CEO Compensation: Conditional on Net Assets and Local Culture

This table reports the effect of state minimum wage increases on for-profit CEOs' total compensation conditional on net assets (columns (1) and (2)) and local culture (columns (3) and (4) for religiosity, columns (5) and (6) for social capital, and county population in columns (7) and (8)). Firms with low-net assets are the NPOs with pre-event average net assets below the sample median, and the rest are categorized as NPOs with high-net assets. *Net assets* is calculated as total assets minus total liabilities, scaled by total assets. Firms with Low-religiosity (Low-Social Capital or Low-County Population) are those whose average religiosity measure (average social capital score or county population) in the pre-event period below or equal to the sample median, and High-Religiosity (High-Social Capital or High-County Population) NPOs as those with such measure above the median. The dependent variable is the natural logarithm of total compensation of a for-profit CEO. All variable definitions are reported in the Appendix 2. I include *Event*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *State*-level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*	*Ln(Total Comp)*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low-Net Assets	High-Net Assets	Low-Religiosity	High-Religiosity	Low-Social Capital	High-Social Capital	Low-County Population	High-County Population
Treated×Post	0.015	0.015	0.010	0.008	0.015	0.010	-0.023	0.015
	(0.578)	(0.596)	(0.675)	(0.823)	(0.651)	(0.761)	(0.418)	(0.614)
Post	0.002	0.020	0.005	0.020	-0.001	0.026	0.026	0.005
	(0.903)	(0.298)	(0.738)	(0.377)	(0.941)	(0.228)	(0.161)	(0.799)
Ln(NI)	0.042**	0.060***	0.047***	0.054***	0.057***	0.038***	0.034**	0.068***
	(0.033)	(0.003)	(0.002)	(0.000)	(0.000)	(0.004)	(0.019)	(0.006)
NPO Competition	0.126	-0.018	0.010	0.634***	0.031	0.157	0.210	0.017
	(0.248)	(0.786)	(0.752)	(0.000)	(0.669)	(0.108)	(0.122)	(0.737)
FP Competition	0.024	-0.029	-0.059	0.040	0.007	-0.041	-0.029	0.010
	(0.615)	(0.605)	(0.149)	(0.444)	(0.900)	(0.320)	(0.493)	(0.872)
Ln(Assets)	0.326***	0.273***	0.325***	0.266***	0.318***	0.285***	0.278***	0.320***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Board Size	-0.012	0.003	-0.014	0.003	-0.010	0.000	-0.009	-0.000
	(0.401)	(0.781)	(0.228)	(0.840)	(0.394)	(0.988)	(0.506)	(0.994)
Indep Ratio	0.422	0.296	0.609*	0.085	0.452	0.221	0.435	0.229

	(0.119)	(0.276)	(0.082)	(0.762)	(0.132)	(0.571)	(0.125)	(0.522)
Ln(GDP per Capita)	0.101	0.082	0.244	-0.020	-0.267	0.522*	0.385	-0.703**
	(0.743)	(0.848)	(0.306)	(0.956)	(0.428)	(0.082)	(0.267)	(0.014)
Ln(Population)	0.992**	0.744	1.575***	-0.349	0.832*	1.083	-0.009	-0.015
	(0.049)	(0.149)	(0.000)	(0.447)	(0.094)	(0.126)	(0.624)	(0.423)
Unemployment Rate	0.003	-0.017	-0.016	-0.020	-0.023	0.012	-0.002*	0.001
	(0.910)	(0.395)	(0.184)	(0.376)	(0.168)	(0.547)	(0.059)	(0.181)
Democrat Points	-0.001	-0.001**	-0.000	-0.002	-0.002*	0.000	0.074*	-0.073
	(0.222)	(0.038)	(0.989)	(0.164)	(0.058)	(0.765)	(0.057)	(0.169)
RTW Law	-0.028	0.109**	0.026	0.098	0.016	0.083*	11.137***	15.646***
	(0.413)	(0.031)	(0.536)	(0.233)	(0.895)	(0.082)	(0.000)	(0.000)
Wald Test	0.00		0.00		0.03		1.73	
	(0.994)		(0.964)		(0.864)		(0.189)	
Event Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,439	8,384	9,309	7,515	9,819	7,002	9,308	7,513
Adj. R-squared	0.824	0.804	0.814	0.833	0.808	0.836	0.822	0.808

Appendix B. Variable Definition

Variables	Description
State- or County-Level Characteristics Variable	
Annual State Maximum Min. Wage	The annual maximum of the daily state minimum wage obtained from Vaghul and Zipperer (2016), which is available at https://github.com/benzipperer/historicalminwage.
GDP per Capita	The county level Gross Domestic Product divided by the total population from the Bureau of Economic Analysis (BEA) Regional Economic Accounts at https://www.bea.gov/data/economic-accounts/regional.
Unemployment Rate	The county level unemployment rate from Local Area Unemployment Statistics Program through the Bureau of Labor Statistics at https://www.bls.gov/lau/.
Corporate Income Tax	The state-level maximum corporate income tax rate reported by the Tax Policy Center at https://www.taxpolicycenter.org/statistics/state-corporate-income-tax-rates.
RTW Law	An indicator variable that equals to 1 for years in which a state has Right-to-Work Law enacted, 0 otherwise, collected from the National Right-to-Work Committee at https://nrtwc.org/facts/state-right-to-work-timeline-2016/.
Democrat Points	An index of the partisan dominance of the state where the firm is headquartered. The raw data is obtained from The Green Papers website (http://www.thegreenpapers.com/G14/Comparative_Political_Party_Predominance.phtml), where they calculate their scores by tabulating the results of the General Elections in each state, assigning a weight for each election. The weights are: Presidential or Gubernatorial (20 points), Senate (15 points), U.S. House of Representatives and Houses of the State's legislature (10 points each). The website reports values ranging from 51 to 100, prefixed by a D or R to indicate which party is dominant in a state. I convert the score prefixed by R by subtracting it from 100, yielding an index value between 0 and 100, where the higher the score, the greater the power of the Democratic party in the state is.
Religiosity	Total number of adherents reported by all denominations divided by total population, which is the ratio of religious people in the population of the county as defined by the Association of Religion Data Archives (ARDA). https://www.thearda.com/Archive/Files/Descriptions/RCMSMGCY.asp
Social Capital	County-level social capital index is obtained from the Northeast Regional Center for Rural Development (NRCRD), which is based on surveys conducted in 1997, 2005, 2009, and 2014. Following Hasan, Hoi, and Zhang (2020), I fill in the data for the missing years

	using the value in the preceding year in which data are available. https://aese.psu.edu/nercrd/community/social-capital-resources
Non-Profit Organization Variable (as reported on Form 990 unless otherwise indicated)	
NI	Net income is the total revenue minus the total expenses.
Admin Efficiency	Admin Efficiency is calculated as one minus the ratio of management and general expense to total expense.
Gov Grant	The government grants received by the organization.
Direct Donation	The direct donations received by the organization.
PSR	The program service revenues.
NPO Competition	Log of frequencies of nonprofits headquartered within a given metropolitan statistical area (MSA) that are in the same industry classification, based on National Taxonomy of Exempt Entities (NTEE), and same size quartile.
FP Competition	Log of frequency of organizations in same industry/size quartile combination as COMPUSTAT full sample firm, where industry is measured using NAICS and size is measured by total assets.
Assets	The amount of year-end total assets.
Charitable	An indicator variable equals to 1 if the ratio of program service revenue to total revenue is below 90%, 0 otherwise
Board Size	The total number of total voting board members.
Indep Ratio	The ratio of the number of independent board members to total voting board members.
Unrestricted Cash	Total cash plus savings and temporary cash investments minus permanently and temporarily restricted net assets.
NPO Age	The number of years since the founding year.

CHAPTER 3

BOARD INTERLOCKS AND THE DIFFUSION OF INNOVATION

Introduction

The propagation of many corporate practices (e.g., governance practices, earnings management, financial disclosure, and tax avoidance) are often associated with interlocking directors (e.g., Bouwman, 2011; Brown, 2011; Chiu, Teoh, and Tian, 2013; Cai, Dhaliwal, Kim, and Pan, 2014; Jiang, Dhaliwal, Kim, and Pan, 2018). The prior literature has reached a consensus that firms that share interlocking directors learn from each other in making corporate decisions. Corporate innovation, as one of the most important aspects in firms' development, is also related to the network of management teams (Chang and Wu, 2021). While the literature often focuses solely on the overall innovation input and output *within* a firm, it cannot be overlooked that diffusion and generation of innovation are interlinked in a way that innovation efforts of external firms are stimulated by the diffusion of innovation ideas (Johansson, 2021). In this chapter, I investigate the role those interlocking directors play in facilitating the diffusion of innovation.

Since innovation typically involves the exploration of new and untested ideas, it is of critical importance for the board to provide timely monitoring and advice on the firm's strategies (Manso, 2011). When confronted with difficult decisions embedded with great uncertainties, typically the ones with research and development, managers may seek advice from directors with different experiences at other firms which have dealt with similar issues before. For instance, in 2004 and 2005, Apple Inc. (Apple) shared a common director, Mr. Jon Rubinstein, with Immersion Corporation (Immersion), which is a company that develops touch screens. When Apple was developing their first full touch screen iPhone (released in 2007), Immersion had been actively innovating in this field for years. It is plausible that Mr. Rubinstein may have shared his experience at Immersion when the board of directors at Apple requested guidance regarding the iPhone. Thus, there is evidence that boards do play an important role in advising business decisions (Schwartz-Ziv and Weisbach, 2013), and their involvement in the innovation process often includes conducting site visits to various facilities on the company's campus, such as research laboratories or quality control offices, and learning about product innovation (Klarner, Probst, and Useem, 2020). Such experiences with innovation processes at Immersion equipped Mr. Rubinstein with the knowledge Apple needed.[54] Given the huge number of patents filed in any field[55], interlocking directors have the advantage of being familiar with innovation projects on both interlocked firms, and, consequently, they have the knowledge to help the focal firm to identify the most relevant and contributory patents by guiding it to a particular patent pool related to the technology development in the interlocked firm, as opposed to having the focal firm to look for a needle in a haystack based on the universe of patents in a certain field. Therefore, interlocked directors can enhance innovation diffusion between interlocked firms. In this chapter, I explore the impact of board interlock on the diffusion of innovation.

I start by constructing the diffusion sample following Kostovetsky and Manconi (2020), where each focal firm is paired with firms that have cited the focal firm's patents in the sample period. Unlike common practices which firm can legally imitate, intellectual properties (IPs) are well-protected by laws, so innovation should diffuse in a way that firms cite each other's patents while filing for their own. Therefore, the diffusion of innovation is measured by the total number

[54] It is observable that Apple shared a common director, Mr. Rubinstein, with Immersion, but what happened inside the boards is hypothetical and only for illustration purpose.

[55] Even under a very specific category like touch screen technology (CPC code G06F3), there were as many as around 47 thousand patents granted before 2005 in the USPTO dataset. In the context of the Apple-Immersion example mentioned earlier, Mr. Rubinstein could help Apple easily identify the most relevant and contributory patents from Immersion to help improve Apple's own touch screen, so that Apple's management team do not have to go through all the 47 thousand patents. The number of patents under "G06F3" category has grown to about 560 thousand by 2021.

of citations the focal firm's patents receive from the paired citing firm in a given year (cross citations). I find that the number of citations received by the focal firm's patents is positively associated with the number of interlocking directors between the paired firms, suggesting a positive relationship between the board interlock and the diffusion of innovation. There is a 14.0% increase in the number of cross citations between the firms when the number of common directors increase from 0 to 1, all else equal.

To mitigate the endogeneity concern, I employ a difference-in-differences (DiD) design using the death or retirement of interlocking directors as external shocks to the board interlocks. The results show that the death or retirement of an interlocking director leads to a significant decrease in the number of cross citations after the event, compared to the death or retirement event of a director who is not interlocked with any other firm. This evidence is indicative that board interlocks have a positive effect on the diffusion of innovation. I also find that the effect is more pronounced for interlocking directors who have longer experience with R&D-intensive industries and with bigger professional and non-professional networks, suggesting that such directors are more effective in the diffusion process of innovation. In addition, the effect is only statistically significant when the directors have a higher compensation at the focal firm, suggesting that the director's financial incentive is also positively associated with the diffusion effect. Moreover, I find that board interlock enhances the diffusion of innovation across industries, however, it has no effect on within-industry diffusion. As one might expect, a larger number of board interlocks proportionally affects firms' overall innovation output.

Flow of innovation and adoption ideas are key components that affect the generation of innovation outcomes (Johansson, 2021). The findings up to this point all suggest that the flow of innovation is enhanced by interlocking directors, all the while maintaining that the stream of ideas steadily pass along the "assembly line" of innovation modeled by Johansson (2021). Thus, the innovation outcome should also be positively affected by board interlocks. To test this conjecture, the sample is aggregated to the firm-year level to examine the relationship between the number of board interlocks and the overall innovation outputs in a firm. Consistent with the findings by Helmers, Patnam, and Rau (2017), I find the firm-level innovation outputs are enhanced by board interlocks.

This chapter adds to the literature on the determinants of corporate innovation and the role of interlocking directors, by providing plausibly causal evidence that interlocking directors play an important role in the diffusion of innovation process. This chapter also contributes to the literature by showing director experience and incentive can affect their effectiveness and reach in the diffusion process and documenting the ease of diffusion by interlocking directors can ultimately enhance the corporate innovation outputs.

This chapter is closely related to Helmers, Patnam, and Rau (2017), which finds that board interlocks have significant positive effects on both R&D expenditure and the number of patents filed using datasets of Indian publicly traded companies. This chapter differs because I focus on U.S. publicly listed firms and I adopt a measure of innovation diffusion, i.e., cross citations, to quantify the flow of innovation through the channel of interlocks. Another recent related work by Chang and Wu (2021) studies whether board connectedness affects corporate innovation and shows a positive effect of board connection on innovation activities and quality. This chapter follows one of their identification strategies (which follows Fracassi and Tate (2012)) and uses the death or retirement of directors as exogenous shocks to the director network. The major difference between this chapter and Chang and Wu (2021) is that their focus is on the broad set of connections including current and past employment, educational background, and social (nonprofessional) activities, whereas mine only focuses on board interlocks, which allows me to take a closer look at this conduit, and again, I specifically measure the diffusion process.

The remainder of the chapter is organized as follows. I derive the hypotheses in Section 2. Section 3 describes the sample selection, data and variables. Section 4 presents the empirical results. Finally, Section 5 reminds readers of the benefits of strengthening board interlocks.

Hypothesis Development

Board interlocks are a credible and low-cost channel for disseminating information and communicating (Haunschild, 1993), for managing uncertainty, and for gaining access to diverse skills and resources for directors (Salancik and Pfeffer, 1978). Moreover, as mentioned above, the managerial role (Schwartz-Ziv and Weisbach, 2013) and active involvements in the innovation process (Klarner, Probst, and Useem, 2020) ensure the interlocking director can act as an effective conduit for the diffusion of innovation.

Hypothesis 1. A focal firm's patents are more likely to be cited by the patents from firms that are director interlocked to the focal firm.

One strand of literature shows directors' technical or industry-specific experience can improve firm performance. For instance, independent directors' industry expertise is found to positively affect firm value (Drobetz, Meyerinck, Oesch, and Schmid, 2018). Dass, Kini, Nanda, Onal, and Wang (2014) explain that a higher fraction of directors with experiences from supplier or customer industries is associated with a higher firm value. Wang, Xie, and Zhu (2015) show that directors' industry experiences in various committees can affect corporate practices in many ways. For instance, directors on an audit committee with industry experience reduce earnings management and the probability of financial fraud; compensation committee industry experience reduces CEO excess compensation; higher fraction of industry experts on the entire board is associated with higher CEO turnover-performance sensitivity and higher acquirer returns from diversifying acquisitions. Directors with research background can better advise the management team on R&D projects (Xie, Xu, and Zhu, 2021). Moreover, a wealth of technical knowledge from interlocking directors' experiences increases their influence and ability to spur knowledge diffusion from an outside firm (Shropshire, 2010). Therefore, I expect the directors who have been exposed to more research projects can better facilitate the diffusion of innovation. I measure the director experience by the number of years the director has worked (being employed or serving on a board) in research and development intensive industries, and hypothesize this measure is positively related to the effectiveness of the diffusion.

Hypothesis 2. The diffusion of innovation is stronger when the interlocking director has more technical experience.

Masulis and Mobbs (2014) find that directors with multiple directorships distribute their effort unequally based on the directorship's relative prestige. Social identity theorists suggest that an interlocking director is more likely to search for opportunities to inform and contribute to the focal firm when their position in the board is perceived as more prominent (Shropshire, 2010). Moreover, there is evidence that corporate directors perform for pay (Yermack, 2004; Adams and Ferreira, 2008). Therefore, I expect the diffusion effect is stronger when the director's incentive is more aligned with the focal firm.

Hypothesis 3. The diffusion of innovation is stronger when the interlocking director's incentive is more aligned with the focal firm.

The prior literature has built up a common ground that an effective conduit for information diffusion and resource exchange between firms plays an important role in enhancing overall economic benefits (Cohen, Frazzini, and Malloy, 2008). In my context, interlocking directors can become information and resource transmission conduits, which benefits all parties involved and can offer a broader knowledge base. Moreover, the interlocking directors can serve as ambassadors and facilitate the diffusion of innovation, with an increase of cross-citation. Greater information traffic through professional and social networks enables directors to better assist top management in adjusting investment portfolios and designing competitive investment strategies that create first-mover advantage (Baldwin, 1982). The prior literature collectively points out that the overall innovation outcome is positive related to the number of interlocks.

Hypothesis 4. Director interlocks enhance firm innovation.

Data and Methodology

Sample Selection and Data Sources

I obtain information on individual directors and executives from BoardEx, which collects data on more than 14,000 public and private companies in Europe and the United States. In this study, I focus on U.S. publicly traded firms. For each firm, BoardEx provides a list of directors with biographical data including the director's name, date of birth, date of death, job title, role description, whether this director is independent or not, and the beginning and ending dates for each past and current position, etc. The sample period starts in 2000, when the BoardEx algorithm became more reliable. The financial information is from Compustat and I exclude firms in the utility (SIC from 4900 to 4999) and financial (SIC from 6000 to 6999) industries. Institutional ownership data is from the Thomson Reuters 13-F filing dataset. Both Compustat and 13-F filing datasets are matched with firms in BoardEx using CUSIP.

The patents and citation data are retrieved from the U.S. Patent and Trademark Office (USPTO) bulk data files for the period from 2000 to 2017[56] and is then linked to Compustat using

[56] In untabulated tests, observations in 2016 are dropped to mitigate a truncation issue with the dataset (Gu, Kaviani, Li, Maleki, and Mao 2021), and the results remain qualitatively robust.

the match file provided by Kogan, Papanikolaou, Seru, and Stoffman (2017).[57] For each patent, the dataset includes detailed data on any other patents it cites, which lets me construct the measure of the diffusion of innovation. Following Kostovetsky and Manconi (2020) and Kogan, Papanikolaou, Seru, and Stoffman (2017), each cited firm *i* in BoardEx database from 2000 to 2017 is matched to a potential citing firm *j* if, at any point during the sample period, *j*'s patents cite *i*'s, where *i* does not equal to *j*, or in another words, all self-citations are excluded. The final sample consists of 1,090,084 firm i-firm j-year level observations.

The baseline measure of innovation diffusion is calculated as the natural logarithm of 1 plus the number of citations that patents of firm *j* cite the patents assigned to firm *i* at a given year *t*:

$$\text{Ln}\ (Cross\ Cite_{i,j,t}) = \text{Ln}\ [1 + \#citations(i,j,t)] \tag{6}$$

which serves as the dependent variable in the baseline results, whereas the key interest variable on the right-hand side is board interlocks, which is defined as the natural logarithm of 1 plus the number of directors who serve on both boards of firm *i* and *j* at a given year *t*:

$$\text{Ln}\ (Interlock_{i,j,t}) = \text{Ln}\ [1 + \#Interlocked\ Directors(i,j,t)] \tag{7}$$

Following prior literature, I control for the following firm characteristics that affect corporate innovation, which are gathered from Compustat, BoardEx, and Thomson Reuters 13-F datasets: research and development intensity (*R&D*) calculated as R&D expenses over total assets; leverage ratio (*Leverage*); ratio of cash and short-term investments to total assets (*CHE*); ratio of capital expenditure to total assets (*CAPX*); asset tangibility (*PP&E*) defined as property, plant, and equipment over total assets; firm size (*Size*) calculated as the natural logarithm of 1 plus the book value of total assets; growth opportunities (*Tobin's Q*); profitability (*ROA*); total number of patents

[57] https://github.com/KPSS2017/Technological-Innovation-Resource-Allocation-and-Growth-Extended-Data

filed (and eventually granted) by the focal firm (*Patent*); number of board members (*Board Size*); busyness of the board (*Busy Board*) defined as the fraction of busy independent directors on the board, where an independent director is defined as busy if the director holds at least three outside directorships following Fich and Shivdasani (2006); fraction of independent directors on the board (*Board Independence*); and percentage of a firm's outstanding shares held by institutional investors (*Institutional Ownership*). All variables are winsorized at the 1st and 99th percentiles. Variable definitions are detailed in Appendix C.

Table 3-1 Panel A shows the summary statistics of the sample of the 1,090,084 pairs of firm *i*-firm *j* in a year. On average, a firm receives 1.82 citations from a paired firm in a year. The number of interlocking directors between a pair of firms *i* and *j* varies from zero (lowest) to eight (highest). The mean value of *Interlock* is 0.01, suggesting that, in a given year, the likelihood of firm *i* having an interlocking director with firm *j* (conditional on firm *j*'s patents having ever cited firm *i*'s patents throughout the sample period) is 1%. Nevertheless, the chance of a firm having an interlocking director with another firm is 81%, which is much higher. This indicates that director interlocks are not rare events. On average, firms have $4.4 billion total assets, about 6.8% of which is contributed to research and development; the leverage is approximately 20.6% of the total assets; an average firm holds 21.2% of total assets as cash or equivalent, and 18.7% as property, plant and equipment; the capital expenditure is around 3.7% of the total assets. The Tobin's Q is 2.11, and the return on asset is 2.7% for an average firm. On average, there are 10 directors on a firm's board, of which 23.1% are busy independent directors, and 85.6% are independent. The mean percentage of shares held by institutional investors is 78.1%. The annual average number of patents granted to a firm is 172, while the median is only 19, indicating the sample is highly skewed towards firm pairs in which the focal firm produces relatively more patents. This lopsidedness is partly due to the sample construction method, which matches each cited firm i in the BoardEx database from 2000 to 2017 to a potential citing firm *j* if, at any point during the sample period, *j*'s patents cite i's. A cited firm with more patents will be paired with more potential citing firms, since more patents will increase the number of citations and, therefore, the number of citing firms (which are identified as potential citing firms). Due to the same reason, when the sample is aggregated to firm-year level (presented in Panel B of Table 3-1), the average number of patents drops to 8 per year, while the descriptive statistics of other variables remain comparable to the diffusion sample.

Empirical Strategy

To examine and establish the relationship between board interlocks and the diffusion of innovation, I estimate the following model as the baseline:

$$\text{Ln}\ (Cross\ Cite_{i,j,t+1}) = \alpha + \beta_1 \text{Ln}\ (Interlock_{i,j,t}) + Firm\ Controls_{i,t} + \varepsilon_{i,t} \tag{8}$$

where *i* denotes the cited firm, *j* denotes the citing firm, and *t* represents year. The dependent and key independent variables are defined in Equations (6) and (7), respectively. Firm control variables are described in section 3.1. All other right-hand-side variables are lagged by one year following prior research. I include *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects as the main set of fixed effects, and in some specifications, I show that the results are robust to using *Cited Firm*-, *Citing Firm*-, and *Year*-fixed effects. The standard errors are clustered at *Cited Firm* × *Citing Firm*-level in the most stringent models.

To mitigate the potential omitted variable and reverse causality issues, I follow prior literature (e.g., Chang and Wu, 2021; Fracassi and Tate, 2012) to employ a difference-in-difference (DiD) design using the departure (death or retirement) of directors as the plausibly exogenous variation in board interlocks to infer causal effects rather than just correlation. Retirement is defined as the director leaving the firm at or above 65 years old. The underlying assumption for the exogeneity of such departure events is that they are not driven by the innovation diffusion or production. Death is a solid setting in this regard since the failure of innovation diffusion is hardly lethal for directors. Retirements, on the other hand, may face some challenges. One issue is that even when firms have retirement ages they can waive them, and therefore the ultimate retirement might be determined by firm performance or the director's ability to monitor and advice on various issues including innovation. If the retirement is indeed a consequence of the director's weak ability to diffuse, advice or monitor on the innovation projects, I should expect an increase in the innovation diffusion or outputs after the retirement. However, in later section the results show the opposite. Moreover, in the robustness test section, I match treatment and control events using propensity score matching (PSM), and the criteria take directors' ability to facilitate innovation diffusion into account. The results remain the same qualitatively in this robustness test. Another issue for the retirement is that it is fairly easy to anticipate, and therefore the board may have arrangement to replace the retired director with a similar interlock. I empirically investigate whether this issue would affect the results. After the departure of directors, only a small portion (around 25% in the sample) of the firms bring in new directors to fill the position. After excluding the events when firms bring in new directors within two years after director departures, I obtain consistent results as documented in the robustness test section.

There are 136 deaths and 1,611 retirements identified in the sample. Of the deceased (retired) directors, 20 (229) directors had at least one interlock, which are considered as treatment events, and the remaining retirements and deaths are considered as control events. Thus, the treatment events are those of death and retirement of interlocking directors. Control events are those of death and retirement of director without any patent? interlock (non-interlocking director). There could be multiple departure events within a firm, and each event is included in the sample as an independent event if there is no other type (i.e., treatment or control) of events within the [-3, +3] seven-year event window. For instance, if there is an interlocking director who passed away in 2012 (a treatment event), and a non-interlocking director who retired in 2014 (a control event), then neither of these two events would be included in the panel.

The DiD estimation is based on a sample with a [-3, +3] seven-year window around the departure event, and firms are required to have at least one year of observation before and after the year of departure to be included in the sample. The sample for DiD estimation consists of 301,213 firm *i*-firm *j*-year observations.[58] There are 4,735 observations stemming from the 249 treatment events, with an average of 2.5 firm pairs, and averaging 6 observations per firm pair. When an interlocking director leaves a firm, this linkage between firms (i.e., interlock) is broken, so the firm pairs with severed interlocks are affected by the treatment event. If an interlocking director serves on two firms A and B, when she departs, firm pair A-B and B-A will be the treatment firm pairs, i.e., the firm *i*-firm *j* pairs that are affected by treatment events. In the DiD sample, a departed director serves at most on 3 firms, and in these rare cases, there are 6 treatment firm pairs.[59] Consequently, the mean number of firm pairs with a treatment event is 2.5. A typical firm *i* in the sample has 33 other firms that have cited its patents at least once, so the average director departing a control firm generates 33 firm pairs for the control sample.

By constructing the treatment and control subsamples in this way, the DiD approach can isolate the effects of the severance of board interlocks from any potential effects of director death or retirement, by estimating the difference effects around two types of firm pairs, which were 1)

[58] The large sample size results from the control group. For the 1,498 control events, on average an event has around 33 firm *i*-firm *j* pairs, and each pair averages 6 years of data, consequently the control group consists of 297,478 firm pair-year observations.
[59] Suppose a director is interlocked with three firms A, B, and C, and assume this director departs deceased. Then the treatment firm pairs in this case would be A-B, B-A, A-C, C-A, B-C, and C-B.

previously board interlocked firm pairs but no longer due to director departure; and 2) firm pairs that experienced director departure in a focal firm but are not patent related with any firms throughout the event window.

$$\text{Ln}\ (Cross\ Cite_{i,j,t+1}) = \alpha + \beta_1 Treat_{i,j} \times Post_{i,j,t} + \beta_2 Post_{i,j,t} + Firm\ Controls_{i,t} + \varepsilon_{i,t} \quad (9)$$

where *i* denotes the cited firm, *j* denotes the citing firm, and *t* represents year. The dependent variable is defined in Equation (6). Treated is a dummy variable equals one if the focal firm has a deceased or retired director who is interlocked with other patent-related firm(s) at the time of the event, and zero if the firm has a deceased or retired director who is not interlocked with any patent-related firm. Post is a dummy variable equals one for years after the director's death or retirement, and zero otherwise. Firm control variables are described in section 3.1. All other right-hand-side variables lag by one year following prior literature. I include *Director*- fixed effects, *Cited Firm* × *Citing Firm*-fixed effects, and *Year*-fixed effects. Treated alone is not included as an independent variable since it would be absorbed by the fixed effects. The standard errors are clustered at *Cited Firm* × *Citing Firm*-level.

To test for the parallel trend assumption, I estimate the following model:

$$\ln\left(Cross\ Cite_{i,j,t+1}\right) = \alpha + \beta_{-3} \times Treat_{i,j} \times Year_{i,j,t}^{-3} + \beta_{-2} \times Treat_{i,j} \times Year_{i,j,t}^{-2} + \beta_0 \times Treat_{i,j} \times Current_{i,j,t} + \beta_1 \times Treat_{i,j} \times Year_{i,j,t}^{+1} + \beta_2 \times Treat_{i,j} \times Year_{i,j,t}^{+2} + \beta_3 \times Treat_{i,j} \times Year_{i,j,t}^{+3} + \gamma_{-3} Year_{i,j,t}^{-3} + \gamma_{-2} Year_{i,j,t}^{-2} + \gamma_0 Current_{i,j,t} + \gamma_1 Year_{i,j,t}^{+1} + \gamma_2 Year_{i,j,t}^{+2} + \gamma_3 Year_{i,j,t}^{+3} + Firm\ Controls_{i,t} + \varepsilon_{i,t} \quad (10)$$

where $Year_{i,t}^{-3}$ and $Year_{i,t}^{-2}$ are dummy variables that equal one if the year is three or two years prior to the event, and zero otherwise. $Current_{i,t}$ is a dummy variable that equals one in the year when the event occurs, and zero otherwise. $Year_{i,t}^{+1}$, $Year_{i,t}^{+2}$, and $Year_{i,t}^{+3}$ are dummy variables that equal one if the year is one, two, or three years after the event, respectively, and zero otherwise. The omitted time dummy is $Year_{i,t}^{-1}$, the year immediately before the director departure. β_n captures the average change in the number of cross-citations between firm pairs in treated and control events during year n, relative to year −1. If the number of cross-citation has similar pre-trends in the treated and control groups, then β_{-3} and β_{-2} would be statistically insignificant. The model specification mimics Equation (9), with only the post dummy changing to separate year dummies.

Main Results

Effect of Board Interlock on Diffusion of Innovation

Table 3-2 reports the relationship between board interlock and the diffusion of innovation, where the dependent variable is the natural logarithm of 1 plus the number of times that patents of firm *j* cite the patents assigned to firm *i* at a given year *t*, and the key independent variable is defined as the natural logarithm of 1 plus the number of directors who serve on both boards of firm *i* and *j* at a given year *t*. The model is specified in Equation (8), and the results establish a baseline relationship as conjectured in Hypothesis 1.

Column (3) of Table 3-2 presents the most stringent model among the three, which includes *Board Size*, *Board Independence*, and *Institutional Ownership* as additional control variables compared to column (1) and includes tighter fixed effects as compared to column (2). All models show a positive and significant association between board interlock and the diffusion of innovation. In column (3), the coefficient on *Ln(Interlock)* is 0.90, that is a 1% increase in the number of interlocking directors is associated with 0.09% increase in the number of cross citations between firm *i* and *j*. Alternatively, the loss of a board interlock creates a 14.0% decrease in the average number of cross citations between firm *i* and *j*, which appears economically large (e.g., Duchin, Ozbas, and Sensoy (2010); Tian and Wang (2014); Chang, Fu, Low, and Zhang (2015); Chang and Wu (2021)). [60]

[60] Since d[Ln(1+y)]/d[Ln(1+x)] = [(1+x)/(1+y)]dy/dx, then dy = d[Ln(1+y)]/d[Ln(1+x)]×[(1+y)/(1+x)]dx = β_1[(1+y)/(1+x)]dx, where dy is the change in cross citation, and dx is the change in number interlocks between firm *i*

Although the positive relationship in Table 3-2 is consistent with Hypothesis 1, the endogeneity issue is not properly controlled by the model. Firm pairs that have extensive innovation diffusion might desire a common director to better facilitate the diffusion process (an example of reverse causality concern). Alternatively, there could be omitted firm or board characteristics affecting both the number of common directors and innovation diffusion. To try to mitigate these issues, I employ a DiD by estimating Equations (9) and (10). The estimation results are presented in Table 3-3.

In Table 3-3, columns (1), (3), and (5) present the DiD estimations specified by equation (4) with each followed by the corresponding dynamic Equation (10) estimates to test for the parallel trend assumption, i.e., columns (2), (4), and (6) present estimation results of Equation (10). The DiD estimates are consistent with Hypothesis 1, and the parallel trend assumption is validated. As in Table 3-2, columns (5) and (6) of Table 3-3 show the most stringent models. In column (5), the coefficient-estimate on *Treated*×*Post* is -0.053, and is statistically significant at 10% level, which indicates that the death or retirement of an interlocking director leads to a 5.3% drop in the cross citations between firm *i* and *j*, compared to the firm pairs with death or retirement of non-interlocking directors. This drop suggests that an adverse event (director's departure due to death or retirement) that severs the board interlock leads to a decrease in the diffusion of innovation, or in other words, the presence of a board interlock has a positive effect on the diffusion of innovation, which is consistent with Hypothesis 1. The parallel trend assumption is verified by the results in columns (2), (4), and (6), which show that there is no significant coefficient estimate for the year

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and *j*. To interpret the economic magnitude, assume dx = 1-0 =1, i.e., the number of interlocks increases from 0 to 1. Then the change in cross citations (dy) from its mean value (1.82) is then equal to 0.09×[(1+1.82)/(1+0)]×1=0.254, so in percentage terms it is 0. 254/ 1.82 = 14.0%.

prior to the director departure events, and the effects only become significant two years after the unforeseen events.

Diffusion of Innovation: Conditional on Directors' Technical Experience

The results in Table 3-3 suggest that interlocking directors can diffuse innovations between firms. In this subsection, I test Hypothesis 2 and investigate whether interlock directors' past technical experience affects how well they promote innovation diffusion. For a firm that is within an R&D-intensive industry, the board's agenda is likely associated with innovation projects more often than firms in industries where R&D is less relevant. Thus, if an interlocking director has been working longer in such industries, she will accumulate more experiences related to technological development, which qualifies her to identify the most beneficial technology for a firm since the topic is in her field of expertise and research. Therefore, I should find the diffusion effects to be stronger for directors with greater experience in R&D intensive industries.

To test this conjecture, I measure the directors' technical experience as following. For each year of data, I sorted two-digit SIC coded industries into R&D-intensive versus non-R&D-intensive industries. R&D-intensive industries are those whose average ratio of total R&D expenses to total assets is in the top decile of sample firms in a year. I then count the total number of years that a director has worked in any R&D-intensive industries before the death and retirement of said directors. I define *Long Exp in RD Intensive Ind* as a dummy variable equals one for directors with total number of years working in R&D-intensive industries above the sample median, and zero otherwise. The rest are indicated by *Short Exp in RD Intensive Ind*. Only the coefficient on *Long Exp in RD Intensive Ind×Treated×Post* is significant in column (1) of Table 3-4, suggesting that directors with long R&D-intensive industry experience are better at diffusing innovation.[61] These results are consistent with Hypothesis 2.

Diffusion of Innovation: Conditional on Directors' Network Size

Besides the historical experiences accumulated by directors in their own careers, their broader social connections also enhance the directors' contribution to innovation (Chang and Wu, 2021). Broader social networks can grant directors wider exposure to the technological development, industry insights, and information regarding peer firms, etc.; this specialized knowledge helps directors identify and assess the high-risk projects and provide more valuable counsel. Therefore, I explore next whether interlocking directors with a bigger network diffuse innovation more effectively. To do so, I measure the network size by the number of individuals with whom the director overlaps through employment, education, and other activities. The sample is then sorted based on the average director's network size in the pre-event period, and big network size is defined as those with such measure above the sample median, small network size as those with such measure below or equal to the sample median. *Big Network* is defined as a dummy variable equals one for directors with such measure above the sample median, and zero otherwise. *Small Network* indicates the remaining directors. In column (2) of Table 3-4, only the coefficient on *Big Network*×*Treated*×*Post* is significant, suggesting directors with bigger networks are more effective in diffusing innovation.

Diffusion of Innovation: Conditional on Directors' Incentive

To further investigate whether the effectiveness of innovation diffusion varies based on director types, I next examine another director characteristic, which is the incentive associated with personal wealth. The results are reported in Table 3-5. One confounding nature of the

[61] Dummy variables *Long Exp in RD Intensive Ind* and *Short Exp in RD Intensive Ind* are not included in this and subsequent models since they include *Director*-fixed effects that are time invariant for each director.

interlocking director is that they serve on multiple (at least two, by definition) boards, and such directors tend to distribute their efforts unequally based on personal incentives (Yermack, 2004; Adams and Ferreira, 2008; Shropshire, 2010; Masulis and Mobbs, 2014). Hypothesis 3 predicts that the diffusion effects are more prominent when the director's incentive is more aligned with the focal firm. To test for this conjecture, I examine whether the effects of board interlock on the diffusion of innovation is different based on director incentive, which is proxied by director wealth delta, i.e., the change in director's wealth in the company for each 1% change in the stock price, as reported by BoardEx. The sample is sorted based on the average director wealth delta in the pre-event period, and *High Delta* is defined as a dummy variable equals one for directors with such measure above the pre-event sample median, and zero otherwise. *Low Delta* indicates the rest directors. Taking the results in Table 3-5, the diffusion effect is more pronounced when the director has stronger incentives aligned with the focal firm, which is consistent with Hypothesis 3.

Diffusion of Innovation: Within vs. Cross Industry

In this section, I investigate to what extent board interlock spurs knowledge diffusion within and across industries. As firms expand their knowledge space across industries to unfamiliar areas, they face greater information search costs, which can be reduced by board interlocking directors through their guidance and advice. Therefore, the marginal benefit of board interlocking directors is larger for such firms. In contrast, some firms have already developed expertise in their own industries and are familiar with various technologies within it. Thus, firms will benefit less from board interlocking directors with respect to same industry knowledge diffusion. As a result, I propose that interlocking directors facilitate the cross-industry diffusion of innovation to a greater extent than they help the within-industry diffusion.

To test this conjecture, I define *Across Ind FF12* as a dummy variable that equals one for cross-industry pairs based on Fama-French 12 industry classification, and zero otherwise. *Within Ind FF12* indicates the within-industry pairs. The results are presented in column (1) of Table 3-6. Columns (2) and (3) are similar to column (1), except that the industries are defined by Fama-French 49 industry and two-digit SIC-coded industry, respectively. The results are consistent with the expectation that interlocking directors facilitate the cross-industry diffusion of innovation to a greater extent.

Effects of Board Interlock on Firm Level Innovation

Interlocking Directors and Firm Innovation: OLS Regressions

Now I turn to the firm-level overall effects to test for the Hypothesis 4. I first examine the relationship between innovation output and the total number of interlocks within a firm. To do so, I aggregate the firm-pair sample into firm-year sample, which consists of 44,939 firm-year observations; summary statistics are reported in panel B of Table 3-1. I count the total number of firms that share one or more common directors with a given firm in a year, which is a firm-level measure of board interlock, labelled *Interlock Sum*.[62] I then estimate the following OLS regression model:

$$\text{Ln}\ (Innovation\ Variable_{i,t+1}) = \alpha + \beta_1 \text{Ln}\ (Interlock\ Sum_{i,t}) + Firm\ Controls_{i,t} + \varepsilon_{i,t} \tag{11}$$

where i denotes firm, and t represents year. Firm control variables follow the previous models and are described in section 3.1. All the other right-hand-side variables lag one year following prior research. I include *Firm*- and *Year*-fixed effects, and the standard errors are clustered at the *Firm*-

[62] If there are multiple common directors between two firms, they still count as one towards *Interlock Sum*. In an untabulated test, these multiple common directors are counted independently, i.e., if there are n common directors between two firms, *Interlock Sum* will be added up as n, instead of one. The results remain unchanged qualitatively.

level. The regression results are reported in Table 3-7. The innovation variable is the natural logarithm of one plus the total number of patents, and the natural logarithm of one plus the number of citations per patent in columns (1) and (2), respectively.

I find that the total number of board interlocks in a firm is significantly positively related to patent counts and citation counts, supporting Hypothesis 4. Nevertheless, the OLS model might suffer from an endogeneity issue. Therefore, I revise the DiD model to examine the impact of board interlocks on firm-level innovation output. I estimate the following DiD regression:

$$\mathrm{Ln}\left(Innovation\ Variable_{i,t+1}\right) = \alpha + \beta_1 Treat_{i,t} \times Post_{i,t} + \beta_2 Post_{i,t} + Firm\ Controls_{i,t} + \varepsilon_{i,t} \quad (12)$$

where i denotes firm, and t represents year. *Treated* and *Post* are defined in the same way as in Equation (9). I include *Director*-, *Firm*-, and *Year*-fixed effects.[63] The firm level DiD sample consists of 8,395 observations.

The estimation results are presented in Table 3-8. Columns (1) and (2) shows that the death or retirement of a board interlocking director leads to a significant drop in the total number of patents and the citation counts of each patent, respectively, compared to firms with deaths or retirements of non-interlocking directors. This data can be interpreted as board interlocks having a positive effect on firms' innovation output.

Firm Innovation: Conditional on Director's Technical Experience

Parallel to the Table 3-4 in subsection 4.2, which presents the results for the heterogeneous effects of board interlocks on the diffusion of innovation based on the directors' experience, I conduct similar cross-sectional tests for the firm level innovation output. The regression results

[63] Just as earlier, there could be multiple departure events within a firm, and each event is treated as an independent event. If there is another type (i.e., treatment or control) of event within the [-3, +3] seven-year event window, then neither of the two is included in the sample.

are presented in Table 3-9. The results in Table 3-4 show interlocking directors with longer experience in R&D-intensive industries can better facilitate the diffusion of innovation, and the firm level innovation outputs are plausibly enhanced by the ease of information dissemination. Conversely, I should expect to see that the decrease in innovation outputs is only statistically significant after the departure (due to death or retirement) of interlocking directors who have worked in R&D-intensive industries longer and who have relatively bigger networks. The firm level innovation output, measured by both the number of patents and citations, is negatively affected by the adverse shock of director departures who have worked in R&D-intensive industries longer, and directors who have relatively bigger networks. This evidence indicates that the effects of board interlocks on firm level innovation output are greater when the interlocking director is more experienced.

Robustness Tests

To further illustrate that the diffusion effects are robust to different sample construction method, I implement two more tests and present the results in Table 3-10. In column (1), I drop the events when firms bring in new directors within two years after the director departure, and the results remain qualitatively similar, indicating that departure events have material effects on the diffusion of innovation and the effects are not driven by director rotation. After the departure of directors, only a small portion (around 25% in the sample) of the firms bring in new directors to fill the position. After excluding the events when firms bring in new directors within two years after director departures, I obtain consistent results. Fracassi and Tate (2012) also argue that firms cannot find an immediate replacement for a departing director with the same connectedness. Therefore, the departure events are plausibly exogenous.

In previous results illustrated in Table 3-3, *Director*-fixed effects are included to rule out the possibility that the effects are driven by unobservable, time-invariant director characteristics. On top of that, propensity score matching (PSM) is employed to better control for the concern that the interlocking directors are invited on both boards due to their superior ability to facilitate the diffusion of innovation. The results are presented in column (2) and (3) of Table 3-10, where each treatment event is matched with a control event based on the nearest-neighbor matching criteria without replacement: the pre-event average of directors' age, number of years worked in R&D-intensive industries, wealth delta associated with the focal firm, and network size. In column (3), I replace the *Director*-fixed effects with *PSM Pair*-fixed effects to make sure the estimation is within pairs of treatment and control events based on PSM, i.e., the departed interlocking director has similar ability to diffuse innovation.

Conclusion

This chapter documents a causal positive effect of board interlocks on the diffusion of innovation by firstly showing that the number of citations received by the focal firm's patents is positively associated with the number of interlocking directors between the firm pair. The increase in the number of common directors from 0 to 1 is associated with a 14.0% increase in the number of cross citations between the firm pair, all else equal. Then, to mitigate endogeneity concerns, a DiD design is employed using the departure (death or retirement) of interlocking directors as plausibly exogeneous shocks to the board interlocks. The results show that the death or retirement of an interlocking director leads to a significant decrease in the number of cross citations afterwards, compared to the death or retirement event of a director who is not interlocked with any other firm, indicating that board interlocks have a causal positive effect on the diffusion of innovation. The effect is more pronounced for interlocking directors who have longer experience with research-and-development-intensive industries, have bigger networks, and have larger compensation delta associated with the focal firm. I also found that the diffusion effects are only significant for cross-industry firm pairs since firms generally face greater information search costs when expanding into unfamiliar areas. Last but not least, the firm-level innovation outputs are positively affected by the number of board interlocks. The results collectively show that interlocking directors play an important role in diffusing innovation, which can also stimulate the firm's overall innovation outcome.

Table 3-1. Summary Statistics

The sample in Panel A consists of 1,090,084 firm *i*-firm *j*-year observations for analyzing innovation diffusion between firm pairs. Panel B reports summary statistics for the panel of firms used for the firm-level analysis.

Panel A. Firm-Pair Sample						
	N	Mean	SD	p25	p50	p75
Cross Cite	1,090,084	1.82	7.06	0.00	0.00	1.00
Interlock	1,090,084	0.01	0.11	0.00	0.00	0.00
R&D	1,090,084	0.07	0.08	0.02	0.04	0.10
Leverage	1,090,084	0.21	0.17	0.06	0.19	0.30
CHE	1,090,084	0.21	0.19	0.07	0.15	0.30
CAPX	1,090,084	0.04	0.03	0.02	0.03	0.05
PP&E	1,090,084	0.19	0.14	0.09	0.14	0.26
Size	1,090,084	22.20	2.19	20.71	22.28	23.88
Tobin's Q	1,090,084	2.11	1.11	1.37	1.79	2.47
ROA	1,090,084	0.03	0.15	0.01	0.06	0.10
Patents	1,090,084	171.63	417.87	1.00	19.00	121.00
Board Size	1,090,084	9.89	2.55	8.00	10.00	12.00
Busy Board	1,090,084	0.23	0.16	0.11	0.22	0.33
Board Independence	1,090,084	0.86	0.07	0.83	0.88	0.91
Institutional Ownership	1,090,084	0.78	0.20	0.67	0.81	0.95
Panel B. Firm Panel Sample						
	N	Mean	SD	p25	p50	p75
Patent	44,939	7.54	34.23	0.00	0.00	0.00
Cite	44,939	66.20	292.78	0.00	0.00	0.00
Interlock	44,939	4.96	4.79	1.00	4.00	7.00
R&D	44,939	0.07	0.14	0.00	0.00	0.07
Leverage	44,939	0.22	0.25	0.01	0.17	0.34
CHE	44,939	0.22	0.24	0.04	0.13	0.32
CAPX	44,939	0.05	0.06	0.01	0.03	0.06
PP&E	44,939	0.25	0.24	0.06	0.16	0.36
Size	44,939	20.02	2.13	18.54	20.06	21.48
Tobin's Q	44,939	2.19	1.79	1.19	1.61	2.45
ROA	44,939	0.05	0.20	0.02	0.10	0.16
Board Size	44,939	8.09	2.22	7.00	8.00	9.00
Busy Board	44,939	0.14	0.15	0.00	0.11	0.22
Board Independence	44,939	0.81	0.10	0.75	0.86	0.89
Institutional Ownership	44,939	0.72	0.30	0.53	0.83	1.00

Table 3-2. Relationship between Board Interlocks and Diffusion of Innovation

This table reports the relationship between board interlocks and the diffusion of innovation. The sample consists of each possible annual pair of firms during the period 2000 to 2017. The dependent variable is the natural logarithm of 1 plus the number of citations that patents of firm *j* cite the patents assigned to firm i at a given year *t*. The key independent variable is the natural logarithm of 1 plus the number of directors who serve on boards of both firm *i* and *j* in a given year *t*. In column (1) and (2), I include *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects, whereas the governance control variables (i.e., Board Size, Busy Board, Board Independence, and Institutional Ownership) are included in column (2), but not in column (1). In column (3), the model includes *Cited Firm* × *Citing Firm*- and *Year*-fixed effects, and the governance control variables are also included. The dependent variables are from year *t+1*, and all other variables are from year *t*. All variable definitions are reported in Appendix 3. *P*-values are in parentheses with standard errors clustered at the *Cited Firm*- and *Citing Firm*-levels in column (1) and (2), and at *Cited Firm* × *Citing Firm*-level in column (3). ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*
	(1)	(2)	(3)
Ln(Interlock)	0.181***	0.180***	0.090***
	(0.000)	(0.000)	(0.000)
R&D	0.213***	0.205***	0.367***
	(0.000)	(0.001)	(0.000)
Leverage	-0.023	-0.023	-0.057***
	(0.181)	(0.164)	(0.000)
CHE	0.017	0.017	0.048***
	(0.248)	(0.235)	(0.000)
CAPX	-0.155*	-0.147*	0.116**
	(0.081)	(0.093)	(0.040)
PP&E	0.164***	0.165***	-0.075***
	(0.000)	(0.000)	(0.008)
Size	0.045***	0.043***	0.025***
	(0.000)	(0.000)	(0.000)
Tobin's Q	-0.004*	-0.004*	-0.008***
	(0.081)	(0.095)	(0.000)
ROA	0.019	0.017	-0.003
	(0.214)	(0.245)	(0.692)
Ln(Patent)	0.031***	0.031***	0.046***
	(0.000)	(0.000)	(0.000)
Board Size		0.002	-0.001
		(0.268)	(0.351)
Busy Board		0.030**	0.010
		(0.047)	(0.305)
Board Independence		0.033	0.021
		(0.228)	(0.329)
Institutional Ownership		0.014	0.014
		(0.477)	(0.228)
Cited Firm F.E.	Yes	Yes	No

Citing Firm F.E.	Yes	Yes	No
Cited Firm × *Citing Firm* F.E.	No	No	Yes
Year F.E.	Yes	Yes	Yes
Observations	1,090,084	1,090,084	1,090,084
Adj. R-squared	0.200	0.200	0.541

Table 3-3. Effects of Board Interlocks on Diffusion of Innovation

This table reports the effects of board interlocks on the diffusion of innovation during 2000 through 2017. The sample consists of firm pairs that are identified as in either treatment or control events under the difference-in-differences (DiD) setting, where the treatment events are those of death and retirement of interlocking directors; control events are those of death and retirement of director without any patent-related interlocks. The DiD estimation is based on a sample with a [-3, +3] seven-year window around the departure event. The dependent variable is the natural logarithm of 1 plus the number of citations that patents of firm *j* cite the patents assigned to firm *i* at a given year *t*. Treated is a dummy variable equals one if the firm has a deceased or retired director who is interlocked with other firm(s) at the time of the event, and zero if the firm has a deceased or retired director who is not interlocked with any firm. Post is a dummy variable equals one for years after the director's death or retirement, and zero otherwise. $Year^{-3}$ and $Year^{-2}$ are dummy variables that equal one if the year is three or two years prior to the event, respectively, and zero otherwise. Current is a dummy variable that equals one in the year when the event occurs, and zero otherwise. $Year^{+1}$, $Year^{+2}$, and $Year^{+3}$ are dummy variables that equal one if the year is one, two, or three years after the event, respectively, and zero otherwise. In columns (1) – (4), I include *Director*-, *Cited Firm*-, *Citing Firm*-, and *Year*-fixed effects, whereas the governance control variables (i.e., Board Size, Busy Board, Board Independence, and Institutional Ownership) are included in columns (3) and (4), but not in columns (1) and (2). In columns (5) and (6), the model includes *Director*-, *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects, and the governance control variables are included. The dependent variables are from year *t+1*, and all other variables are from year *t*. All variable definitions are reported in Appendix 3. *P*-values are in parentheses with standard errors clustered at *Cited Firm*- and *Citing Firm*-levels in column (1) – (4), at *Cited Firm* × *Citing Firm*-level in column (5) and (6). ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	-0.055*		-0.054*		-0.053*	
	(0.066)		(0.074)		(0.052)	
Post	0.001		0.002		-0.001	
	(0.802)		(0.690)		(0.711)	
Treated×Year^{-3}		-0.019		-0.019		-0.020
		(0.714)		(0.703)		(0.553)
Treated×Year^{-2}		0.026		0.026		0.023
		(0.413)		(0.430)		(0.482)
Treated×Current		-0.024		-0.023		-0.026
		(0.503)		(0.521)		(0.442)
Treated×Year^{+1}		-0.038		-0.037		-0.035
		(0.525)		(0.538)		(0.423)
Treated×Year^{+2}		-0.117**		-0.116**		-0.118***

		(0.014)		(0.015)		(0.007)
Treated×*Year*$^{+3}$		-0.049		-0.048		-0.048
		(0.420)		(0.424)		(0.329)
Year^{-3}		0.208***		0.214***		0.068**
		(0.000)		(0.000)		(0.019)
Year^{-2}		0.103***		0.106***		0.034**
		(0.000)		(0.000)		(0.019)
Current		-0.101***		-0.102***		-0.032**
		(0.000)		(0.000)		(0.031)
Year^{+1}		-0.196***		-0.200***		-0.058**
		(0.000)		(0.000)		(0.047)
Year^{+2}		-0.300***		-0.306***		-0.091**
		(0.000)		(0.000)		(0.037)
Year^{+3}		-0.399***		-0.407***		-0.120**
		(0.000)		(0.000)		(0.038)
R&D	0.012	0.014	-0.001	0.001	0.000	-0.000
	(0.862)	(0.836)	(0.991)	(0.989)	(0.994)	(0.996)
Leverage	-0.027	-0.027	-0.026	-0.026	-0.028*	-0.029*
	(0.265)	(0.283)	(0.286)	(0.303)	(0.072)	(0.065)
CHE	-0.014	-0.014	-0.015	-0.015	-0.013	-0.014
	(0.512)	(0.548)	(0.479)	(0.514)	(0.355)	(0.324)
CAPX	-0.025	-0.026	-0.032	-0.033	-0.027	-0.028
	(0.804)	(0.847)	(0.749)	(0.804)	(0.680)	(0.666)
PP&E	0.061	0.064	0.062	0.066	0.015	0.021
	(0.392)	(0.385)	(0.366)	(0.356)	(0.651)	(0.534)
Size	0.028***	0.028***	0.024***	0.023***	0.020***	0.020***
	(0.002)	(0.002)	(0.005)	(0.006)	(0.000)	(0.000)
Tobin's Q	0.001	0.001	0.001	0.001	0.000	0.000
	(0.601)	(0.628)	(0.612)	(0.652)	(0.868)	(0.892)
ROA	-0.006	-0.006	-0.006	-0.006	-0.012	-0.012
	(0.722)	(0.730)	(0.707)	(0.719)	(0.270)	(0.269)
Ln(Patent)	0.021***	0.021***	0.021***	0.021***	0.023***	0.023***

	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Board Size			0.003	0.003	0.002*	0.002**
			(0.205)	(0.157)	(0.074)	(0.043)
Busy Board			0.010	0.010	0.017	0.016
			(0.558)	(0.576)	(0.173)	(0.185)
Board Independence			0.079**	0.078**	0.068***	0.070***
			(0.036)	(0.043)	(0.005)	(0.004)
Institutional Ownership			0.026	0.028	0.026**	0.027**
			(0.175)	(0.173)	(0.027)	(0.024)
Director F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Cited Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Citing Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Cited Firm × *Citing Firm* F.E.	No	No	No	No	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	301,213	301,213	301,213	301,213	301,213	301,213
Adj. R-squared	0.211	0.211	0.211	0.211	0.626	0.626

Table 3-4. Board Interlocks and Diffusion of Innovation: Conditional on Director's Technical Experience

This table reports the effects of board interlocks on the diffusion of innovation conditional on director experience, which is proxied by director experience in research and development intensive industries in columns (1), and director network size in columns (2). Every year, I sort two-digit SIC coded industries into R&D intensive vs. non-R&D intensive industries. R&D intensive industries are those whose average ratio of total R&D expenses to total assets across all firms is in the top decile of the sample in a year. I then count total number of years that a director has worked in any R&D intensive industries before the death and retirement of directors. I define Long Exp in RD Intensive Ind as a dummy variable equals one for directors with total number of years working in R&D intensive industry above the sample median, and zero otherwise. The rest are indicated by Short Exp in RD Intensive Ind. Director network size is from BoardEx, which is the number of individuals with whom the director overlaps through employment, other activities, and education. The sample is sorted based on the average director network size in the pre-event period, and Big Network is defined as a dummy variable equals one for directors with such measure above the sample median, and zero otherwise. Small Network indicates the rest of directors. The dependent variables are from year *t+1*, and all other variables are form year *t*. All variable definitions are reported in Appendix 3. I include *Director*-, *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at *Cited Firm* × *Citing Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Cross Cite)*	*Ln(Cross Cite)*
	(1)	(2)
Long Exp in RD Intensive Ind×Treated×Post	-0.068**	
	(0.042)	
Short Exp in RD Intensive Ind×Treated×Post	-0.010	
	(0.810)	
Big Network×Treated×Post		-0.082**
		(0.015)
Small Network×Treated×Post		0.018
		(0.806)
Post	-0.001	-0.001
	(0.711)	(0.707)
R&D	0.000	0.000
	(0.995)	(1.000)
Leverage	-0.028*	-0.028*
	(0.072)	(0.072)
CHE	-0.013	-0.013
	(0.354)	(0.358)
CAPX	-0.027	-0.027
	(0.679)	(0.680)
PP&E	0.015	0.015
	(0.647)	(0.644)
Size	0.020***	0.020***
	(0.000)	(0.000)

Tobin's Q	0.000	0.000
	(0.868)	(0.871)
ROA	-0.012	-0.012
	(0.269)	(0.270)
Patent	0.023***	0.023***
	(0.000)	(0.000)
Board Size	0.002*	0.002*
	(0.073)	(0.075)
Busy Board	0.017	0.017
	(0.173)	(0.171)
Board Independence	0.068***	0.068***
	(0.005)	(0.005)
Institutional Ownership	0.026**	0.026**
	(0.027)	(0.028)
Director F.E.	Yes	Yes
Cited Firm × *Citing Firm* F.E.	Yes	Yes
Year F.E.	Yes	Yes
Observations	301,213	301,213
Adj. R-squared	0.626	0.626

Table 3-5. Board Interlocks and Diffusion of Innovation: Conditional on Director Incentive

This table reports the effects of board interlocks on the diffusion of innovation conditional on director incentive, which is proxied by director wealth delta, i.e., the change in director's wealth in the company for each 1% change in the stock price, as reported by BoardEx. The sample is sorted based on the average director wealth delta in the pre-event period, and High Delta is defined as a dummy variable equals one for directors with such measure above the pre-event sample median, and zero otherwise. Low Delta indicates the remaining directors. The dependent variables are from year *t+1*, and all other variables are from year *t*. All variable definitions are reported in the Appendix 3. I include *Director*-, *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at *Cited Firm* × *Citing Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Cross Cite)*
	(1)
High Delta×Treated×Post	-0.072**
	(0.040)
Low Delta×Treated×Post	-0.012
	(0.749)
Post	-0.001
	(0.708)
R&D	0.000
	(0.992)
Leverage	-0.028*
	(0.072)
CHE	-0.013
	(0.360)
CAPX	-0.027
	(0.681)
PP&E	0.015
	(0.651)
Size	0.020***
	(0.000)
Tobin's Q	0.000
	(0.868)
ROA	-0.012
	(0.270)
Patent	0.023***
	(0.000)
Board Size	0.002*
	(0.075)
Busy Board	0.017
	(0.173)
Board Independence	0.068***
	(0.005)
Institutional Ownership	0.026**
	(0.028)

Director F.E.	Yes
Cited Firm × *Citing Firm* F.E.	Yes
Year F.E.	Yes
Observations	301,213
Adj. R-squared	0.626

Table 3-6. Board Interlocks and Diffusion of Innovation within vs. Cross Industries

This table reports the heterogeneous effects of board interlocks on the diffusion of innovation between firms within the same industry vs. across industries. In column (1), *Across Ind FF12* is a dummy variable equals one for cross-industry pairs based on Fama-French 12 industry, and zero otherwise. *Within Ind FF12* indicates the within-industry pairs. Columns (2) and (3) are similar to column (1), except that the industry are defined by Fama-French 49 industry and two-digit SIC coded industry, respectively. The dependent variables are from year *t+1*, and all other variables are from year *t*. All variable definitions are reported in the Appendix 3. I include *Director*-, *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at *Cited Firm* × *Citing Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*
	(1)	(2)	(3)
Across Ind FF12×Treated×Post	-0.053*		
	(0.068)		
Within Ind FF12×Treated×Post	-0.053		
	(0.347)		
Across Ind FF49Treated×Post		-0.049*	
		(0.074)	
Within Ind FF49×Treated×Post		-0.077	
		(0.433)	
Across Ind SIC2×Treated×Post			-0.058*
			(0.056)
Within Ind SIC2×Treated×Post			-0.034
			(0.605)
Post	-0.001	-0.001	-0.001
	(0.711)	(0.711)	(0.710)
R&D	0.000	0.001	0.000
	(0.994)	(0.990)	(0.998)
Leverage	-0.028*	-0.028*	-0.028*
	(0.072)	(0.072)	(0.072)
CHE	-0.013	-0.013	-0.013
	(0.355)	(0.355)	(0.355)
CAPX	-0.027	-0.027	-0.027
	(0.680)	(0.680)	(0.680)
PP&E	0.015	0.015	0.015
	(0.651)	(0.650)	(0.651)
Size	0.020***	0.020***	0.020***
	(0.000)	(0.000)	(0.000)
Tobin's Q	0.000	0.000	0.000
	(0.868)	(0.867)	(0.868)
ROA	-0.012	-0.012	-0.012
	(0.270)	(0.271)	(0.269)
Patent	0.023***	0.023***	0.023***

	(0.000)	(0.000)	(0.000)
Board Size	0.002*	0.002*	0.002*
	(0.074)	(0.073)	(0.074)
Busy Board	0.017	0.017	0.017
	(0.173)	(0.173)	(0.173)
Board Independence	0.068***	0.068***	0.068***
	(0.005)	(0.005)	(0.005)
Institutional Ownership	0.026**	0.026**	0.026**
	(0.027)	(0.027)	(0.027)
Director F.E.	Yes	Yes	Yes
Cited Firm × *Citing Firm* F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
Observations	301,213	301,213	301,213
Adj. R-squared	0.626	0.626	0.626

Table 3-7. Board Interlocks and Firm Level Innovation Output: OLS Regressions

This table reports the relationship between total number of board interlocks and the innovation output in a firm. The sample is aggregated to firm-year level, and the summary statistics is presented in Table 1 Panel B. The dependent variables are from year *t+1*, and all other variables are form year *t*. *Ln(Patent)* is the natural logarithm of the total number of patents filed (and eventually granted) by the firm in a given year. *Ln(Cite)* is the natural logarithm of the total number of citations received by all patents of the firm in a given year. I include *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Patent)*	*Ln(Cite)*
	(1)	(2)
Ln(Interlock Sum)	0.046***	0.036**
	(0.002)	(0.038)
R&D	0.019	-0.071
	(0.682)	(0.230)
Leverage	-0.013	0.005
	(0.447)	(0.721)
CHE	0.085*	0.163***
	(0.076)	(0.004)
CAPX	-0.209***	0.007
	(0.008)	(0.941)
PP&E	0.416***	0.312***
	(0.000)	(0.000)
Size	0.084***	-0.010
	(0.000)	(0.542)
Tobin's Q	0.001*	0.003**
	(0.083)	(0.016)
ROA	-0.010	0.008
	(0.134)	(0.299)
Ln(Patent)		1.179***
		(0.000)
Board Size	-0.016***	-0.022***
	(0.003)	(0.000)
Busy Board	-0.222***	-0.218***
	(0.006)	(0.006)
Board Independence	0.231***	0.173*
	(0.004)	(0.065)
Institutional Ownership	-0.029	-0.122***
	(0.343)	(0.000)
Firm F.E.	Yes	Yes
Year F.E.	Yes	Yes
Observations	44,939	44,939
Adj. R-squared	0.739	0.816

Table 3-8. Board Interlocks and Firm Level Innovation Output: A DiD Approach

This table reports the effects of board interlocks on innovation output. The sample is derived from the firm-year level data in Table 7. The firms in this sample are either identified in the treatment or control events under the DiD setting, where the treatment events are those of death and retirement of interlocking directors; control events are those of death and retirement of director without any interlock. The DiD estimation is based on a sample with a [-3, +3] seven-year window around the departure event. *Treated* is a dummy variable equals one if the firm has a deceased or retired director who is interlocked with other firm(s) at the time of the event, and zero if the firm has a deceased or retired director who is not interlocked with any firm. *Post* is a dummy variable that equals one for years after the director's death or retirement, and zero otherwise. The dependent variables are from year *t+1*, and all other variables are from year *t*. All variable definitions are reported in Appendix 3. I include *Director*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Patent)*	*Ln(Cite)*
	(1)	(2)
Treated×Post	-0.126***	-0.126**
	(0.000)	(0.021)
Post	0.096***	0.112**
	(0.001)	(0.036)
R&D	-0.065	-0.135
	(0.574)	(0.546)
Leverage	-0.041	0.019
	(0.585)	(0.889)
CHE	0.201**	0.344**
	(0.020)	(0.029)
CAPX	0.283**	0.115
	(0.040)	(0.629)
PP&E	0.277**	0.537***
	(0.010)	(0.005)
Size	-0.004	0.000
	(0.878)	(0.997)
Tobin's Q	-0.009	-0.000
	(0.172)	(0.998)
ROA	0.004	0.024
	(0.904)	(0.721)
Ln(Patent)		0.642***
		(0.000)
Board Size	-0.007	-0.025**
	(0.428)	(0.049)
Busy Board	-0.077	-0.086
	(0.445)	(0.554)
Board Independence	-0.074	-0.024
	(0.569)	(0.914)
Institutional Ownership	0.005	-0.052

	(0.911)	(0.527)
Director F.E.	Yes	Yes
Firm F.E.	Yes	Yes
Year F.E.	Yes	Yes
Observations	8,395	8,395
Adj. R-squared	0.669	0.659

Table 3-9. Board Interlocks and Firm Level Innovation Output: Conditional on Director's Technical Experience

This table reports the effects of board interlocks on the firm-level innovation output conditional on director experience, which is proxied by director experience in research and development intensive industries in columns (1) – (4), and director network size in columns (5) – (8). Every year, I sort two-digit SIC-coded industries into R&D- intensive vs. non-R&D-intensive industries. R&D-intensive industries are those whose average ratio of total R&D expenses to total assets across all firms is in the top decile of the sample in a year. I then count the total number of years that a director has worked in any R&D-intensive industries before the death or retirement of directors. I define *Long Exp in RD Intensive Ind* as a dummy variable equals one for directors with total number of years working in R&D intensive industry above the sample median, and zero otherwise. The rest are indicated by *Short Exp in RD Intensive Ind*. Director network size is from BoardEx, which is the number of individuals with whom the director overlaps through employment, other activities, and education. The sample is sorted based on the average director network size in the pre-event period, and *Big Network* is defined as a dummy variable equals one for directors with such measure above the sample median, and zero otherwise. *Small Network* indicates the rest of directors. The dependent variables are from year *t+1*, and all other variables are form year *t*. All variable definitions are reported in the Appendix 3. I include *Director*-, *Firm*- and *Year*-fixed effects in all models. *P*-values are in parentheses with standard errors clustered at the *Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Patent)*	*Ln(Cite)*	*Ln(Patent)*	*Ln(Cite)*
	(1)	(2)	(3)	(4)
Long Exp in RD Intensive Ind×Treated×Post	-0.255***	-0.246***		
	(0.000)	(0.000)		
Short Exp in RD Intensive Ind×Treated×Post	-0.020	-0.033		
	(0.549)	(0.585)		
Big Network×Treated×Post			-0.147***	-0.132*
			(0.004)	(0.052)
Small Network×Treated×Post			0.016	-0.010
			(0.689)	(0.868)
Post	0.097***	0.114**	0.048*	0.066
	(0.001)	(0.032)	(0.074)	(0.161)
R&D	-0.068	-0.138	-0.065	-0.133
	(0.571)	(0.542)	(0.579)	(0.555)
Leverage	-0.045	0.013	-0.040	0.019
	(0.536)	(0.921)	(0.589)	(0.887)
CHE	0.176**	0.324**	0.199**	0.343**
	(0.040)	(0.039)	(0.022)	(0.030)
CAPX	0.317**	0.147	0.275**	0.108
	(0.019)	(0.531)	(0.046)	(0.651)
PP&E	0.259**	0.524***	0.275**	0.537***
	(0.019)	(0.006)	(0.013)	(0.005)
Size	-0.004	0.001	-0.005	-0.000
	(0.889)	(0.988)	(0.860)	(0.995)
Tobin's Q	-0.009	0.000	-0.010	-0.001
	(0.185)	(0.993)	(0.135)	(0.944)

ROA	0.006	0.025	0.006	0.025
	(0.859)	(0.703)	(0.874)	(0.707)
Patent		0.629***		0.641***
		(0.000)		(0.000)
Board Size	-0.006	-0.024*	-0.007	-0.025**
	(0.474)	(0.055)	(0.432)	(0.049)
Busy Board	-0.109	-0.117	-0.079	-0.084
	(0.272)	(0.421)	(0.438)	(0.564)
Board Independence	-0.087	-0.036	-0.062	-0.014
	(0.499)	(0.873)	(0.633)	(0.950)
Institutional Ownership	0.010	-0.046	0.006	-0.051
	(0.827)	(0.570)	(0.898)	(0.528)
Director F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Observations	8,395	8,395	8,395	8,395
Adj. R-squared	0.674	0.660	0.670	0.659

Table 3-10. Robustness Tests

This table reports the effects of board interlocks on the diffusion of innovation during 2000 through 2017 employing different methods to construct the sample. Compared to the DiD sample in Table 3-3, in column (3) of the table below, I drop the events when firms bring in new directors within two years after the departure. In column (2) and (3), I construct the sample using propensity score matching, where each treatment event is matched with a control event based on the nearest-neighbor matching criteria without replacement: the pre-event average of directors' age, number of years have worked in R&D intensive industries, wealth delta associated with the focal firm, and network size. In columns (1) and (2), I include *Director*-, *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects. In column (3), I include *PSM Pair*-, *Cited Firm* × *Citing Firm*-, and *Year*-fixed effects. The dependent variables are from year *t+1*, and all other variables are from year *t*. All variable definitions are reported in the Appendix A. *P*-values are in parentheses with standard errors clustered at *Cited Firm* × *Citing Firm*-level. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Ln(Cross Cite)*	*Ln(Cross Cite)*	*Ln(Cross Cite)*
	(1)	(2)	(3)
Treated×Post	-0.061**	-0.084*	-0.098**
	(0.039)	(0.093)	(0.049)
Treated			0.193
			(0.504)
Post	-0.003	0.010	0.001
	(0.441)	(0.434)	(0.918)
R&D	-0.029	-0.089	0.311
	(0.595)	(0.746)	(0.310)
Leverage	-0.023	-0.207***	-0.195**
	(0.225)	(0.009)	(0.019)
CHE	-0.026	-0.055	-0.024
	(0.150)	(0.452)	(0.753)
CAPX	0.021	-0.212	0.049
	(0.788)	(0.553)	(0.902)
PP&E	-0.017	0.559**	0.258
	(0.665)	(0.010)	(0.393)
Size	0.027***	0.063**	0.102**
	(0.000)	(0.027)	(0.018)
Tobin's Q	-0.002	0.005	0.015
	(0.480)	(0.579)	(0.113)
ROA	-0.034***	-0.165*	-0.151*
	(0.008)	(0.067)	(0.100)
Patent	0.024***	0.032***	0.037***
	(0.000)	(0.000)	(0.000)
Board Size	0.000	0.011***	0.012***
	(0.885)	(0.002)	(0.001)
Busy Board	0.011	0.046	0.057
	(0.432)	(0.220)	(0.136)
Board Independence	0.084***	0.263**	0.225*

	(0.003)	(0.039)	(0.079)
Institutional Ownership	0.023	0.071	0.048
	(0.103)	(0.488)	(0.691)
Director F.E.	Yes	Yes	No
PSM Pair F.E.	No	No	Yes
Cited Firm × *Citing Firm* F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
Observations	226,063	36,251	36,251
Adj. R-squared	0.648	0.751	0.750

Appendix C. Variable Definition

Variables	Description
Cross Cite	The number of citations that patents of firm *j* make, at a given year *t*, of patents assigned to firm *i*.
Interlock	The number of directors who serve on both boards of firm *i* and *j* at a given year *t*.
R&D	The ratio of research and development expenditures to total assets.
Leverage	The ratio of the sum of short- and long-term debt to total assets.
CHE	The ratio of cash and short-term investments to total assets.
CAPX	The ratio of capital expenditure to total assets.
PP&E	The ratio of property, plant, and equipment to total assets.
Size	The natural logarithm of one plus total asset.
Tobin's Q	The ratio of book value of assets minus book value of equity plus market value of equity to total assets.
ROA	The ratio of operating income before depreciation to total assets.
Board Size	Total number of directors on a board.
Busy Board	The ratio of the number of busy independent directors to total number of directors on a board. An independent director is defined as busy if the director holds at least three outside directorships following Fich and Shivdasani (2006).
Board Independence	The ratio of number of independent directors to total number of directors on a board.
Institutional Ownership	Percentage of the outstanding shares held by institutional investors.
Patent	Total number of patents filed (and eventually granted) each year.
Cite	Total number of citations received by all patents of the firm each year.
Interlock Sum	The number of other firms that share common directors with the firm in given year *t*.

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