ESSAYS ON PUBLIC & HEALTH ECONOMICS

A Dissertation

by

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ABSTRACT

This dissertation examines three topics in the field of public and health economics using quasi-experimental methods. The first two topics focus on nonprofit institutions and charitable giving, and the last one is about the economics of reproductive health care.

First, in the section entitled “Effects of the Minimum Wage on the Nonprofit Sector", we explore how government policies such as minimum wage affect the nonprofit sector. Given the differences between nonprofits and for-profit firms, it is important to investigate how nonprofits respond to labor cost changes. We use data from the Bureau of Labor Statistics and the Internal Revenue Service linked to state minimum wages. We find a negative impact on employment, driven primarily by states with large statutory minimum wage increases.

In the section entitled “ Charitable Giving Responses to Education Budgets", we study the extent to which government spending on education crowds out private contributions to education. We use data from DonorsChoose.org, an online crowdfunding platform extensively used by public school teachers, and account for endogeneity and economic conditions that affect both spending and donations. We find evidence for crowd-out of private giving, driven by the demand side of the charitable giving market.

In the last section, we examine the long-run effects of exposure to legal changes in access to Reproductive Control Technology on women’s education and earnings. We use the Health and Retirement Study data and leverage variation in exposure to legal changes in access across cohorts born in the same states during the 1960s and 1970s. Our estimates suggest increases in levels of education and increases in the probability of working in a Social Security (SS)-covered job in women’s 20s and 30s.

DEDICATION

To my mother for all the love, care, and sacrifice.

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1. INTRODUCTION

The nonprofit sector has crucial impacts on the economy and society by providing essential health, education, and humanities services. In 2016, more than 1 million nonprofits were registered with the Internal Revenue Service (IRS) (NCCS, 2020). Nonprofit establishments can take various forms and vary in size, from large organizations operating internationally raising billions of dollars to entities running solely by volunteer workers on smaller scales. They work alongside governments to provide public goods, which can complement or substitute government expenditures, composing 5.4 percent of the United States' gross domestic product (GDP) (McKeever, 2018). Therefore, studying their impacts and performance has become of interests.

In addition, in recent years, the number of regulations enacted to improve the welfare of societies, in particular minorities, has been rising. Women, children, immigrants, and people of color are usually the main targeted groups. Hence, evaluating existing policies to identify their effectiveness has become the core of policy-relevant studies.

This dissertation employs quasi-experiment research designs to study charitable giving in the context of education, particularly the questions of why people give, what affects giving, and how government policies affect charities and pro-social institutions. On the one hand, this work explores the economics of volunteer activities, nonprofit organizations, and donors' behavior. On the other hand, it investigates policies that affect women's economic outcomes and labor force participation, such as reproductive health. This study examines how nongovernmental tools help provide essential services to improve welfare and evaluates government policies affecting minority and disadvantaged groups' well-being.

Section 2 studies nonprofit organizations and policies that impact their performance. It examines how changes in the minimum wage impact the nonprofit sector. The nonprofit sector's ability to absorb increases in labor costs differs from the private sector in a number of ways. We analyze how nonprofits are affected by changes in the minimum wage utilizing data from the Bureau of Labor Statistics and the Internal Revenue Service linked to state minimum wages. We

examine changes in reported employment and volunteering and other financial statements such as revenues and expenses. The results from both data sets show a negative impact on employment, driven primarily by states with large statutory minimum wage increases. We observe some evidence for a reduction in the number of nonprofit establishments but few consistent patterns in revenue, expenses, or the use of volunteers.

Section 3 investigates how voluntary contributions to education can substitute for government expenditures. We examine how changes in K-12 education budgets impact donations to teachers using data from DonorsChoose.org. We explore the impact of changes in budgets on donations and how teachers respond to those changes through their requests on the platform, allowing us to decompose crowd-out into its classic and indirect components. We estimate the impact of spending on donations as well as fundraising separately. Using a district-year panel and instruments to address the endogeneity of budgets, we find evidence for crowd-out of private giving. However, the magnitudes are fairly small in this setting and do not offset a large proportion of a budget change. These results are driven by entirely teachers' posting of requests, illustrating the importance of considering the demand side of the charitable giving market.

Section 4 evaluates policies that directly impact minority groups and women's outcomes, like reproductive health policies. We use the Health and Retirement Study and leverage variation in exposure to legal changes in access across cohorts born in the same states. Previous studies have shown that early legal access (ELA) to birth control pills provided greater autonomy in family planning, enabling women to obtain additional education, work experience, and occupational choice, ultimately generating a wage premium for some women. An alternative hypothesis is that access to abortion may be the driver behind changes in women's outcomes rather than access to the pill. We find positive effects on educational attainment that align with prior work but are not statistically significant. We also find positive effects on working in a Social Security-covered job in women's 20s and 30s, but no evidence of positive effects on women's earnings in their 50s.

2. EFFECTS OF THE MINIMUM WAGE ON THE NONPROFIT SECTOR

2.1 Introduction

In the private sector, labor cost increases induced by the minimum wage are borne by some combination of owners, through lower profits; consumers, through higher prices; and workers, through reductions in other margins of compensation or adjustments to employment. Given its structure, the nonprofit sector has fewer margins through which these cost increases can be borne. By definition, nonprofits do not disburse profits that can be reduced. Many nonprofits do not sell an output whose price can be increased, while others, like hospitals, serve a mix of paying and non-paying customers. Further, nonprofit firms tend to be concentrated in more labor-intensive industries, such as human services and health care (Bureau of Labor Statistics, 2019), and are therefore potentially more sensitive to increases in labor costs.

We analyze the impact of minimum wage increases on the nonprofit sector, which makes up approximately 5% of GDP in the United States (McKeever, 2018) and 10% of total U.S. private sector employment (Bureau of Labor Statistics, U.S. Department of Labor, 2021). Anecdotal evidence suggests that nonprofits struggle to pay increased minimum wages (Segedin, 2015). Surveys following the Seattle minimum wage increases reveal a tension between a desire for higher worker pay and the realities of budgeting for many nonprofits.[1] Higher labor costs may also have indirect effects for charities – given donor distaste for overhead costs (Meer, 2014), donations may fall as charities direct more resources towards their wage bill.

We utilize data from the Internal Revenue Service (IRS) e-filers database from 2011 to 2017, which includes reported employment, volunteering, and other financial statements and newly-released data from the Bureau of Labor Statistics (BLS) on the nonprofit sector. Our primary empirical approach follows that in Clemens and Strain (2017, 2018b, 2020b), who differentiate between inflation-based changes and legislative increases of different sizes in a

[1] In that survey, an executive from a large local nonprofit stated, "I am 100% behind people making better wages, but it is a significant amount of money that I do not know how we are going to make up over a long period of time." (Segedin, 2015)

difference-in-differences framework. For completeness, we also present estimates from a two-way panel fixed effects model.

Our results show a negative impact of state minimum wage changes on employment and the number of nonprofit establishments, driven by states with large statutory minimum wage increases. These effects are concentrated among the smallest nonprofits as measured by number of employees. We do not find any consistent effects on revenues, expenses, or the use of volunteers.

In Section 2.2, we provide a brief review of the literature on the nonprofit sector employment and the effects of minimum wage changes. Section 2.3 presents background on 501(c)3 organizations and the data we utilize. We present our specification and results in Section 2.4 and 2.5. We conclude in Section 2.6.

2.2 Literature Review

The nature of nonprofits' objective function has been a source of contention in this literature. Steinberg (1986) characterizes the extreme views as "budget maximization," in which the charity attempts to maximize the resources under its control, and "service maximization," in which the resources spent on charitable activities are maximized. The former view is associated with Tullock (1971) and Niskanen (2010), while the latter was laid out in Weinberg (1980); previously, Newhouse (1970) discussed nonprofit hospitals' objectives.[2]

The difference in objective functions can lead to wage differentials between the nonprofit and for-profit sector (Hirsch, Macpherson, and Preston, 2018). Under the "labor donation hypothesis," workers in the nonprofit sector accept lower wages as a donation to their employer (Preston, 1989; Handy and Katz, 1998; Narcy, 2011; Pennerstorfer and Trukeschitz, 2012; Jones, 2015; Cassar, 2019). Others suggest that nonprofits pay higher wages since they are not impacted by cost reduction incentives like for-profit firms and have higher incentives to employ better-quality workers and pay higher wages (Rutherford, 2015; Mocan and Tekin, 2003; Butler, 2008; Hirsch,

[2]Easley and O'Hara (1983), Duggan (2000), Duggan (2002), Preyra and Pink (2001), Leone and Van Horn (2005), and Lakdawalla and Philipson (2006), among others, explore the reasons for and behavior of nonprofit firms, particularly when there are for-profit counterparts in the same industry.

Macpherson, and Preston, 2018).

While nonprofits employ large numbers of low-wage workers, there has been little research on how the minimum wage affects those organizations.[3] More recently, there has been greater focus on margins of labor adjustment other than employment levels, such as hours of work (Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething, 2018; Clemens and Strain, 2020a), non-wage compensation (Kaestner and Simon, 2004; Long and Yang, 2016; Schumann, 2017; Clemens, Kahn, and Meer, 2018), the implicit effort contract and productivity (Obenauer and von der Nienburg, 1915; Ku, 2022), and other aspects of production function (Hirsch, Kaufman, and Zelenska, 2015). Others analyze broader margins of adjustment, such as capital-labor substitution (Sorkin, 2015; Aaronson, French, Sorkin, and To, 2018), labor-labor substitution (Luttmer, 2007; Horton, 2017; Clemens, Kahn, and Meer, 2021), profits (Bell and Machin, 2018), and prices (Harasztosi and Lindner, 2019). See a recent survey piece by Clemens (2021) for an in-depth discussion of these issues. Our data do not enable us to examine many of these margins, but expenditure data from the IRS, described in the next section, allow us to determine the effect on some of the operations of nonprofits.

2.3 Data

2.3.1 501(c)(3) Organizations

For an organization to be tax-exempt under section 501(c)(3) of the Internal Revenue Code, it must be organized and operated exclusively for exempt purposes and must not be operated for the benefit of private interests. In addition to being exempt from the corporate income tax, 501(c)(3) organizations benefit from the ability to solicit tax-deductible donations and exemption from sales and property taxes (Hirsch, Macpherson, and Preston, 2018).

501(c)(3) organizations are referred to as nonprofit organizations and are classified as either private foundations and public charities. The main distinction between the two is that public charities have an active program of fundraising and receive contributions from many sources,

[3]On the other hand, the literature on the minimum wage in the for-profit sector is vast. See Neumark and Shirley (2021) for a recent review.

usually from the general public or the government. Private foundations have a single primary source of funding (like endowment and gifts from one family or corporation) rather than the general public. Most private foundations do not accept donations and usually invest their principal funding and make grants to other charitable organizations (Internal Revenue Service, 2021).

A tax-exempt organization that has $50,000 or more in gross receipts is required to file its information to the IRS annually using Form 990, by the 15th day of the 5th month after the end of the organization's fiscal year. We limit our analysis to 501(c)(3) public charities that submitted Form 990 or Form 990-EZ, with the latter being a simplified version of the form.

2.3.2 The IRS E-Filer Database

The IRS has required large organizations with total assets of $10 million or more and approximately 245 employees or more to submit the Form 990 electronically since 2006 (Blackwood, Jayiyeola, and Pollak, 2013). Since 2010, all tax-exempt organizations have been permitted to submit their forms to the IRS electronically. The number of e-filers has been rising; Figure A.1 shows the percent of 501(c)(3) establishments that reported their Form 990 electronically between 2011-2017, with the numerator coming from our data and the denominator as the total number of 501(c)(3) public charities (Urban Institute, 2017).

We use a database of public charities organizations that submitted their forms electronically to IRS for fiscal years 2011-2017, which are posted by the IRS on Amazon Web Services.[4] [5] This is an unbalanced panel that potentially suffers from selection into e-filing, but nevertheless is the most comprehensive collection of data available; moreover, selection into e-filing would have to be systematically correlated with state minimum wage policies to bias our estimates. The data include Employer Identification Number (EIN), name, address, zip code, state, tax year, and total number of employees and volunteers as well as financial information such as total revenue, total expenses, net profit, and total assets.[6]

[4] see https://registry.opendata.aws/irs990 for more details.

[5] If an organization files an amended return, we keep the most recent data. In the case of duplicates, we kept the return with the largest reported financial variables.

[6] The data contain some clear misreporting, primarily in the number of employees and volunteers, likely due to data entry errors. We winsorize observations at the 99.9th percentile before aggregating. There is little qualitative

The IRS data sample covers 7 years and includes 357 state-year observations on a total of 1,689,308 establishment-years. Panel A of Table A.1 reports summary statistics at the state-year level. The average number of establishments per state-year is about 4,700, employing 288,000 workers and 643,000 volunteers.[7] The table also presents aggregate values for revenues, expenses, assets, and gross receipts in 2017 dollars.

2.3.3 Bureau of Labor Statistics Nonprofit Data

The Bureau of Labor Statistics (BLS) recently released data on the nonprofit sector, including employment, total wage bill, and the number of establishments. These data were created by merging existing Quarterly Census of Employment and Wages (QCEW) data with the IRS Exempt Organization Business Master File to identify 501(c)(3) organizations (Friesenhahn, 2016; Bureau of Labor Statistics, 2019).[8]

There are some differences between the IRS and the BLS datasets. The IRS database only includes 501(c)(3) public charities, while the BLS data contain 501(c)(3) entities, which they can be either public charities or private foundations. Moreover, since the BLS only includes organizations with an employee covered by unemployment insurance, it does not include nonprofits without paid workers – but it does collect information on organizations that did not e-file Form 990. The two datasets each have advantages and disadvantages.

The BLS data sample covers 7 years and includes 357 state-year observations, with an average of 5,496 nonprofit establishments per observation (Table A.1 Panel B).

2.3.4 Minimum Wage Data

The debate on the appropriate empirical specification for measuring the effects of the minimum wage is as contentious as the rest of that literature (Neumark, Salas, and Wascher, 2014). In a series of papers beginning with Clemens and Strain (2017), those authors show that there nearly

difference if winsorizing at the 99th or 99.5th percentile.

[7]The IRS allows nonprofits to report an estimate of the number of volunteers, so these numbers should be interpreted cautiously. For example, the American Cancer Society reported 3,000,000 volunteers in 2011 and a remarkably precise 1,388,169 volunteers in 2017.

[8]For more details on the data and its creation, see https://www.bls.gov/bdm/nonprofits/nonprofits.htm

no legislatively-driven state-level minimum wage increases between 2011 and 2013, following the Great Recession. This baseline period, following which a number of states increased their minimum wages, allowing for a classic difference-in-differences comparison. Following Clemens and Strain (2018a, 2020b,c), we classify states into four groups based on their minimum wage policy by the end of 2017: (1) those states with no change in minimum wage, (2) those with changes in the minimum wage driven by inflation-indexation provisions (indexer), (3) those that have enacted a small statutory increase in the minimum wage (less than $2), (4) those that have enacted a large statutory increase in the minimum wage ($2 or more). We use the date the first legislatively-driven change to designate the "post" period. The map in Figure A.2 shows states' policies.

Figure A.3 shows trends in the average minimum wage across the policy groups between 2011-2017. Before 2014, there is no meaningful change in the minimum wage across the policy groups, but the trends deviate after 2014. There is a sharp change in the trend for states with a large statutory increase in the minimum wage like California and Minnesota, and a smaller change for states enacting smaller statutory increases. There is a stable growth in the minimum wage trend in states that index their minimum wage to inflation. Following Clemens and Strain (2017), we exclude 2014 from the estimation, as it is a transition year for policy changes – many phased increases were begun or enacted in that year.

2.4 Empirical Framework

We use a standard difference-in-differences specification, interacting the policy groups described above with an indicator for the year of the first change.

$$Outcome_{st} = \alpha_0 + [After_{st} \times MWGroup_s]\alpha_1 + X_{st} + State_s + Year_t + \epsilon_{st} \tag{2.1}$$

The outcomes of interests are the total employment, volunteers, and the number of nonprofit organizations as well as a set of financial measures, described below. Variable $Outcome_{st}$ is the natural log of the outcome in state *s* and year *t*. $State_s$ and $Year_t$ are state and year fixed effects,

respectively. $After_{st} \times MWGroup_s$ is the interaction of state's policy group with an indicator for the time period after the first legislative policy change. We include additional time-varying state-level controls such as the log of per capita income, the Housing Price Index (HPI), and the Affordable Care Act (ACA) expansion.[9] α_1 is the coefficient of interest, which is an estimate of the causal effect of state minimum wage changes on the outcomes, under the identification assumption that nonprofit employment would have evolved similarly across the policy groups in the absence of the minimum wage changes. Figures A.4 and A.5 show total mean nonprofit employment relative to 2011 across policy groups using the IRS and BLS data, respectively.

There has extensive discussion of the problems that staggered treatment timing and dynamic treatment effects can cause in these types of specifications, especially in the context of the minimum wage (Meer and West, 2016; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2021). Clemens and Strain (2021) show that their results on the impact of minimum wage changes are robust when using these new approaches. For completeness, we also estimate a panel fixed effects model (FE model) as Equation 4.2.

$$Outcome_{st} = \alpha_0 + [ln(MW)_{st}]\alpha_1 + X_{st} + State_s + Year_t + \epsilon_{st} \tag{2.2}$$

where s indexes states and t indexes years. $ln(MW)_{st}$ shows the log of the minimum wage in states s and year t, which is adjusted for the inflation using the CPI-Urban. In this specification, α_1 shows the elasticity of the outcome with respect to the minimum wage.

We also examine versions of Equation 4.2 using first differences, as well as longer differences up to six years to examine dynamics. But perhaps the simplest way to avoid issues of timing and dynamics is to examine long-run effects in the standard differences-in-differences model. We therefore also estimate versions of 4.1 using the baseline period of 2011-2013 and the final year of the sample, 2017.

[9]These data are collected from, respectively, the Bureau of Economic Analysis, the Federal Housing Finance Agency, and the Kaiser Family Foundation

2.5 Regression Estimates Results

2.5.1 Employment

We begin with the IRS data in Table A.2, reporting estimates from 4.1 in Panel A. Including controls, the effects are strongest for states with large increases in the minimum wage, with a roughly 6% decrease in employment relative to states without any minimum wage changes. But states with smaller minimum wage increases see little impact on employment, consonant with the results in Clemens and Strain (2021). States with inflation-indexed minimum wages also see a negative effect despite relatively small increases. However, Brummund and Strain (2020) argue that indexed increases have a substantially greater disemployment effect than similarly-sized nominal increases at the time of enactment. Nevertheless, we do not make too much of this finding, as the “indexer" group is small, its policy changes are early in the sample period, and, as seen below, the results do not replicate in the BLS sample. In Panel B, we report the results of estimates of 4.2, a standard two-way fixed effects specification. The estimate is negative and statistically significant; when time-varying controls are included, the elasticity of employment among nonprofits with respect to the minimum wage is -0.24 (s.e. = 0.11).

In Table A.3, we report corresponding estimates using BLS data. While smaller in magnitude, states with large statutory increases see a 2.7% (s.e. = 1.1%) decrease in employment relative to states that did not increase their minimum wage. Small statutory increasers see a negative but imprecise effect, while there is no meaningful impact on indexers. In the fixed effects estimates in Panel B, the estimated elasticity is -0.14 (s.e. = 0.05) when including time-varying controls.

These results give a strong indication that large statutory minimum wage increases reduce employment in the nonprofit sector. But as discussed above, dynamic effects and concerns about treatment timing are particularly prevalent in this literature. We examine several other specifications to explore these issues. In Table A.4, we present estimates from 4.1 excluding data for 2014-2016. This simply compares the 2011-2013 baseline to the endpoint of our sample period. The IRS and BLS data consistently show large negative effects of large statutory

minimum wage changes on employment relative to states that did not change their policy; -8.0% (s.e. = 4.2%) and -3.4% (s.e. = 1.8%), respectively. Small statutory increasers see smaller and negative but statistically insignificant reductions.

Table A.5 shows the results of estimates taking differences of 4.2 from one-year to six-year differences. The negative impact of the minimum wage increases as the time span rises, in line with the results in Meer and West (2016).

2.5.2 Establishments

We also examine the impact of minimum wage changes on the number of establishments. Table A.6 provides results. Recall that small nonprofits are less likely to e-file Form 990 and therefore less likely to be represented in the IRS data, while the BLS data will only include establishments with UI-covered employees. In estimates of 4.2, excluding 2014, the IRS data show little impact of small or large statutory increases on the number of establishments, though indexers show large decreases. The BLS data, on the other hand, show that states with large statutory increases have significant reductions in the number of establishment relative to non-changing states, with small and statistically insignificant negative impacts among small statutory increasers, and no differences for indexers. Dropping 2014-2016 to compare the baseline period to the endpoint shows a significant reduction in the number of establishments in states with large statutory increases relative to non-changing states in both the IRS and BLS data; the IRS data also show reductions for small increasers and indexers relative to non-changing states.

For completeness, we also report fixed effects estimates, which show elasticities of number of establishments with respect to the minimum wage of -0.19 (s.e. = 0.069) and -0.12 (s.e. = 0.055) for the IRS and BLS data, respectively.

2.5.3 Volunteering

One margin of adjustment in the nonprofit sector that is not available to for-profits is substitution towards volunteers. Form 990 reports a charity's estimated number of volunteers,

though, as discussed above, these figures are unreliable. Nevertheless, we report estimates of 4.1 on the log number of volunteers in a state in Table A.7. Perhaps unsurprisingly, no systematic pattern emerges, and all coefficients have large standard errors.

2.5.4 Expenses and Revenues

In Table A.8, we examine the effects of minimum wage increases on compensation costs. The IRS and BLS definitions of compensation differ. IRS Form 990 data includes salaries, benefits, and other compensation, while BLS compensation may vary from state to state depending on unemployment insurance laws. Generally, BLS compensation covers a broad definition of wages, but excludes most benefits such as health insurance. No strong patterns emerge, though the BLS results indicate that large statutory increases result in a reduction in the total wage bill, suggesting that the scale of employment reductions (including, perhaps, hours of work) more than offset the wage increases.

The IRS data include more detailed information on nonprofits' expenses and revenues. In Table A.9, we examine the impacts of the minimum wage on program services, grants, fundraising expenses, and total expenses.[10]

While large statutory increasers see negative effects on expenses across the board relative to non-changing states, none of the coefficients are statistically significant. Small statutory increasers see a decline in fundraising expenses, but it is difficult to draw strong conclusions from these results.

In Table A.10, we perform the same exercise for sources of revenue, again drawn from Form 990. These include contributions, program service revenue, investment income, other revenue, as well as total revenue.[11] As for expenses, no clear patterns emerge; we therefore refrain from

[10]Program services are activities that further the organization's purpose; grants include those made to organizations, governments, and individuals; fundraising expenses are those incurred in soliciting contributions; and total expenses include these as well as compensation costs and other expenses. See https://www.irs.gov/pub/irs-pdf/i990.pdf for more details.

[11]Contributions include those from private individuals, foundations, and governments; program service revenue is earned from operations related to the organization's purpose; investment income includes the net gains from sales of assets; other revenue includes unrelated business income; and total revenue covers all of the organization's annual revenues. See https://www.irs.gov/pub/irs-pdf/i990.pdf for more details.

drawing any conclusions on how minimum wage increases might affect nonprofits' revenues.

2.5.5 Effects by Charity Size and Type

We begin by examining the impact of the minimum wage on employment by the size of the establishment. We classify nonprofits by their size in their first appearance in the IRS data, keeping that categorization constant across years and binning to five groups, and aggregate employment to the state-year level. Figure A.6 shows the distribution of establishment size. Table A.11 shows these results. The smallest nonprofits, with three or fewer employees (including those that are entirely volunteer-run), are the most affected. Aggregate employment in this size bin is 27.2% (s.e. = 16.0%) lower in states with large statutory changes relative to non-changing states. Estimates for other size categories are negative but not statistically significant. We examine whether there is a greater prevalence of zero-employee nonprofits in the presence of higher minimum wages, but found no significant effects.

In Table A.12, we classify nonprofits into ten broad purpose groups using the NTEE-CC system, once again aggregating employment to the state-year level. The effects of large statutory increases are largest for charities focusing on the environment and animal welfare and smallest for health and international charities.

2.6 Discussion

We examine how the minimum wage affects the nonprofit sector using data from the IRS and BLS. The unusual nature of the lull in minimum wage policymaking from 2011 to 2013 allows us to define a baseline period and compare different policies in a straightforward manner.

We find that large statutory increases in the minimum wage reduces employment in nonprofits. Smaller statutory increases do not have strong effects, in line with the results in Clemens and Strain (2021), and providing more evidence that the effects on employment are nonlinear with respect to the size of minimum wage increases. We find suggestive evidence that large increases reduce the number of nonprofit organizations, and the employment effects seem strongest on the smallest nonprofits. There are no clear patterns of effects on other margins of adjustment in terms

of expenses and revenues.

3. CHARITABLE GIVING RESPONSES TO EDUCATION BUDGETS

3.1 Introduction

The relationship between government funding of and private contributions to public goods is of key importance in understanding the nature of altruism and policy towards charitable giving (Roberts, 1984; Warr, 1982; Bergstrom et al., 1986; Okten and Weisbrod, 2000; Hungerman, 2005).[1] Increased government spending may lead donors to give less, viewing taxation as a substitute for voluntary contributions – "classic" crowd-out – but charities may pull back on their fundraising efforts when receiving government funds – "indirect" crowd-out (Andreoni and Payne, 2003, 2011). Government grants can also have crowd-in effects, generally by serving as a signal of quality.[2] Further, local preferences and conditions influence spending by the government, charitable giving by individuals, and fundraising decisions by charities. The same people who elect policymakers or vote on budgets are those who make donations, making it difficult to determine the causal relationship (Payne, 1998). If crowd-out is significant in magnitude and primarily due to donors' responses, warm glow motivations may be less important (Andreoni, 1989, 1990; Ribar and Wilhelm, 2002).

In this paper, we examine how K-12 education budgets impact contributions to education, addressing the endogeneity issues inherent in estimating these relationships using instrumental variables. K-12 education is funded almost entirely through taxation and makes up a substantial portion of state and local budgets. How do voluntary contributions to educational services respond to government spending? Traditionally, fundraisers for schools have been local, generally organized by parent-led associations. As such, the relationship between these contributions and local education budgets is endogenous. These local donors often benefit directly from the contributions to the schools, as they are members of the community or have children in the

[1] See De Wit and Bekkers (2017) for a recent meta-analysis of this literature.

[2] Vesterlund (2003) and Eckel et al. (2005) suggest that third-party contributions can have an endorsement effect that increases contributions. Heutel (2014) finds that government grants crowd in private donations, particularly for younger charities, positing that the grant serves as a signal of quality. Bekkers and De Wit (2020) find that directly providing information about government budget cuts can lead to more donations.

school; the donations may be a form of consumption than as contributions to public goods.[3]

We construct a district-year panel by linking data from Donorschoose.org, an online platform for public school teachers to post projects for prospective donors, to data from the Department of Education on school budgets. We examine the impact of changes in budgets on donations, as well as how teachers respond to those changes through their requests on the platform, allowing us to decompose crowd-out into its classic and indirect components. The primary concern for identification is that variation in school budgets and charitable contributions are both affected by unobserved economic factors, which can also impact teachers' willingness to post requests.

We first address this problem by including state- or county-by-year fixed effects in addition to school district effects to control for shocks affecting a particular area in a given year. But these specifications may not fully account for district-year shocks that affect budgets, postings, and contributions, leading to spurious correlation. We instrument for per-pupil spending using the timing of school finance reforms (Jackson, Johnson, and Persico, 2015; Bayer, Blair, and Whaley, 2020) and a shift-share variable measuring the district's exposure to state funding (Deming and Walters (2017) and Dinerstein et al. (2015) use similar instruments in the higher education context). Since it is possible that a local crisis impacts both teachers' requests and donors' willingness to give, we instrument for requests using previous years' request activity by neighboring districts.

The DonorsChoose.org data have a number of advantages. Teachers' posts are easily linked to school districts and the sample size is large. Donations go to a specific project, which is fulfilled only if the requested threshold is met. Expenditures on fundraising are not a component of this platform, which precludes measuring their effectiveness. But since donations can only be made when a project is posted, fundraising requests are observable and there is a more direct link between the behavior of the recipient of the donation and the donor. Moreover, charities' incentives to reduce administrative expenses lead to underreporting of fundraising expenditures in administrative data (Krishnan, Yetman, and Yetman, 2006; Mayo, 2021).

Our instrumented results show that a 10 percent increase in per-pupil elementary-secondary

[3]See Andreoni (2006) for a discussion of the theory underlying voluntary contributions to charity.

spending leads to a 4.4 percentage point decrease in the likelihood that a teacher in that district posts a request in that fiscal year and around 40 percent decrease in the total amount requested by all teachers in that district. A 10 percent increase in per-pupil spending reduces the likelihood of any donation to a project by 4.1 percentage points and the total amount donated by 36 percent. Donations are very responsive to requests, with an 10 percent increase in total amount requested leading to about a 10 percent increase in donations, demonstrating the “power of the ask" in charitable giving (Andreoni and Rao, 2011; Andreoni, Rao, and Trachtman, 2017; Meer and Rosen, 2011). The effectiveness of requests in this context suggests that teachers are leaving a significant amount of donations on the table. Taken together, a $1,000 increase in per-pupil spending reduces donations by $628. But the amount requested by teachers is reduced by $1012; applying our estimates of the efficacy of these requests suggests that this reduces donations by $767 – that is, at least the entirety of the reduction in donations is drive by the endogenous response of teachers. While these magnitudes seem large, a $1,000 increase in per-pupil spending dwarfs this reduction in donations for the average-sized district – though it is more meaningful when compared to mean teacher out-of-pocket spending of $479 (Kim, 2021). We also only examine one source of education-related charitable giving – DonorsChoose.org – so the overall effects may be larger.

Recent evidence suggests that increases in education spending have positive effects on student outcomes, at least when that spending is reasonably well-targeted (Abott, Kogan, Lavertu, and Peskowitz, 2020; Card and Payne, 2002; Jackson, Johnson, and Persico, 2015; Jackson, Wigger, and Xiong, 2021; Lee and Polachek, 2018)[4]. Keppler et al. (0) show that funding from DonorsChoose.org increases student performance at the lowest-income schools.

The evidence is mixed on the response of private funding to changes in public education budgets, with some finding little evidence of a response (Jones, 2015; Nelson and Gazley, 2014; Milton, 2017), while others do find crowd-out (Grosskopf et al., 2020; Hungerman et al., 2019). If voluntary contributions increase in response to budget cuts, then the effects of those cuts may

[4]Jackson (2018) provides a review of the literature

be mitigated; however, depending on how those contributions are distributed, they may alleviate or exacerbate existing differences in resources.[5]

In the paper most similar to ours, Andreoni and Payne (2011) use tax filings by charities to decompose total crowd-out into classic and indirect components by estimating the impact of government grants on donations and fundraising separately, instrumenting with a set of variables for the political affiliation of the governor and congressional delegation. They also estimate the impact of fundraising on donations, instrumenting with variables for the financial health of the nonprofit organization. They find significant crowd-out, with a $1000 grant reducing giving by over $700, but this is entirely due to reduced fundraising effort; fundraising expenditures themselves are effective at increasing donations.

Differentiating between classic and indirect crowd-out demonstrates the importance of warm glow motivations in giving. We also contribute to the literature on crowdfunding in the context of charitable giving.[6]

3.2 Data and Empirical Strategy

3.2.1 Data

Information on project postings and donations come from DonorsChoose.org, an online platform for public school teachers to post projects and collect funding. Founded in 2000, more than 630,000 teachers have posted nearly 2 million projects for 40 million students on the site. The platform has attracted over $1 billion in donations from 4.9 million donors. Figure B.1 presents data on the growth of the organization.

Teachers select supplies from lists provided by vendors and writes a request that includes a discussion of student needs and the proposed use of the supply. Teachers also provide a photograph of their classroom. The request's page includes information about the school (such as its location and poverty level) and the project (such as its subject matter and the number of students reached).

[5] Kim (2021) shows that teachers in schools with larger minority populations tend to spend more out-of-pocket.

[6] Crowdfunding platforms have been used to study the impact of social distance (Meer and Rigbi, 2013), the value of completing projects (Wash, 2013), competition among causes (Meer, 2017), donor distaste for overhead costs (Meer, 2014), the role of social networks and pressure (Castillo, Petrie, and Wardell, 2014, 2017), and other topics.

The request includes an itemized list of the materials requested, their price and quantity, and any additional charges. These projects are screened by the Donorschoose.org staff. Donors, whose gifts are tax-deductible, can browse, search, or filter projects. Figure B.2 shows the page of a representative project; the layout of the web page has changed several times over the history of the organization.

If a project reaches its goal, DonorsChoose.org purchases the materials and ships them directly to the teacher. If the project expires prior to being funded, donors have the option to have the funds returned to their account to select another project or to have DonorsChoose.org select a project for them. Projects that do not reach their goal generally expire after four months.

Data on projects, including National Center for Education Statistics ID number for the school, is available beginning in 2002. These consist of 1,715,764 projects posted by the end of 2018, of which 68.5% met their goal. The mean project amount requested (in 2017 dollars) is $791 with a median of $484. About 32 percent of projects request classroom supplies, with 18 percent requesting books and 30 percent requesting some form of technology. 83 percent of projects posted and 82 percent of dollars requested were from low-income schools, as defined by the percent qualifying for free and reduced-price lunch.

We aggregate the project data to the fiscal-year level, matching projects' posted dates to state fiscal years and summing amounts requested and donated within each district. We classify the amount requested to the fiscal year in which the project was posted, and the amount donated to the fiscal years in which those projects were funded.[7]

We link this to the Department of Education Common Core of Data (CCD), covering the 1995 to 2018 school years.[8] The sample begins with 409,108 observations. We exclude districts with fewer than 50 students enrolled, as is standard in the literature (Cellini et al., 2010) and drop those with missing ID numbers, leaving 380,090 observations. Dropping observations with missing

[7]We consider all donations, including those for projects that were not fully funded, as a measure of donor intent. The results are nearly identical when examining donations to projects that are entirely funded.

[8]Beginning in 2006, the Common Core of Data asks districts to report "gifts of cash or securities from private individuals or organizations." Using this outcome, we find evidence of crowd-out from increased K-12 spending, though the estimates are noisy. However, the lack of data on fundraising expenditures needed to compare classic and indirect crowd-out make this variable ill-suited for our purposes.

financial information leaves a final sample of 352,450 district-year observations representing 17,546 districts.[9] 21.7 percent of observations have at least one project posted (31 percent from the start of the DonorsChoose.org data in 2003); 81.2 percent of districts ever have a project posted. The data represent 1,572,790 individual projects are posted by 848,258 teachers, with 8,407,053 donations totaling 688.6 million dollars. Conditional on at least one project being posted in a district-year, the mean number of projects is 20.6, posted by 11.1 teachers. The mean amount raised in a district-year, conditional on any donations, is $9,600.03, with a median of $1,436.70. Nominal dollar amounts are adjusted to 2017 dollars. Table B.1 reports summary statistics.

Total expenditures include elementary-secondary expenditures (83.9 percent of the total), capital outlay expenditures (9.9 percent), payments to state or local governments, payments to different school systems, and interest paid. We focus on elementary-secondary expenditures because those expenditures directly affect operating the schools in the given school year. They include items such as salaries for school personnel, benefits, student transportation, school books, and materials. Figure B.3 shows per-pupil total expenditures, per-pupil elementary-secondary spending, and per-pupil capital expenditures in school districts between 1995 and 2018. We also extract the number of teachers in the district, the share of children living in poverty, and enrollment shares by race;[10] together these variables are available for 268,854 observations.

3.2.2 Empirical Approach

School spending is not randomly assigned. It is likely to be correlated with permanent and transitory economic conditions, as well as the underlying prosociality of a district's residents, which also impact charitable giving. We include district fixed effects and school district demographics in our specifications to control for the factors that may confound the relationship between spending and donations. Year effects account for macroeconomic conditions that affect

[9] Ideally, we would match the school-level DonorsChoose.org data with school-level funding data; however, financial data are only available at the district level.

[10] Estimated population ages 5-17 in poverty come from the U.S. Census Bureau's Small Area Income and Poverty Estimates program (SAIPE), and enrollment by race from the Rutgers University School Funding Fairness Database (Baker et al. (2016)).

the entire country, but do not account for time-varying shocks that affect only the region. For example, a localized recession could lead to both cuts in school spending and a reduction in donors' ability to make gifts. We include state-by-year fixed effects to capture this variation. Shocks at a more local level could still leave spurious correlation; we also estimate specifications including county-by-year fixed effects. But this approach does not fully account for time-specific factors within a district that could be driving the relationship between spending, fundraising requests, and donations. Below, we describe the set of instrumental variables we use to address this issue.

An advantage of the DonorsChoose.org data is that we can observe the demand for donations (as measured by projects posted by teachers) as well as the equilibrium outcome (projects funded and amounts donated). It is tempting to think of the amount donated as the supply of donations, but it is a function of both donors' intent and their opportunities – if there are no projects posted in a particular district, donors cannot give through the platform. By examining these outcomes separately, we can better determine whether teachers are responding to budget pressures separately from donors' behavior.

Of course, DonorsChoose.org is only one avenue for private contributions to education. Parent-teacher organizations raise significant amounts of money (Cope, 2019) and may serve as another conduit for funds. But the DonorsChoose.org platform, which allows us to examine teacher demand for financing and allows for donations from people not necessarily connected to the district, provides significant advantages in examining this question.

3.2.3 Specification

The Tobit is often used when there are many observations with outcomes equal to zero. However, this model suffers from tractability problems in the presence of fixed effects, is likely not appropriate when zeroes arise from corner solutions rather than true data censoring, and constrains the marginal effects on the extensive and intensive margins to be proportional to each other. This last issue is particularly problematic when considering the impact of, say, per-pupil spending, which may have different effects on the likelihood of a request receiving a donation and

the total amount received.

We use a single-hurdle model to first examine whether any project is posted (or receives a donation) in a given district-year and then separately estimate the effects on the intensive margin (the number of projects posted, the amount requested, or the amount received in donations). We then combine the results to find marginal effects on the unconditional means.[11]

In the first stage, we examine whether any projects have been posted or if any donation is made, as shown in Equation 3.1, which we estimate with a linear fixed effects model.

$$P(Y_{dst} > 0) = \alpha + \beta \cdot Exp_{dt} + \delta \cdot X_{dt} + \gamma_d + \mu_t + \eta_{st} + \epsilon_{dst} \tag{3.1}$$

Where d, s, and t index districts, states, and (fiscal) years, respectively. Exp_{dt} is the log of per-pupil expenditures in district d and year t. We also include the share of children ages 5 to 17 in poverty, the log of number of teachers in a district-year, and enrollment shares by race in X_{dt}. γ_d, μ_t, and η_{st} are district fixed effects, year fixed effects, and state-year (or county-year) fixed effects, respectively. Standard errors are clustered at the district level.

The second stage estimates effects on the intensive margin. The outcomes of interest for this specification, in Equation 3.2, are the log of the number of posted projects, log of the amount requested, and the log of the amount donated. We estimate this equation using a linear fixed effects model only on observations for which there is a nonzero outcome.

$$LogY_{dst} = \alpha + \theta \cdot Exp_{dt} + \delta \cdot X_{dt} + \gamma_d + \mu_t + \eta_{st} + \epsilon_{dst} \text{ if } Y_{dst} > 0 \tag{3.2}$$

Given the Equations 3.1 and 3.2, the coefficients of the interest are β and θ (respectively). The intensive margin effect cannot be taken as causal, though, because it reflects both a compositional change from the change in the sample due to the extensive margin effect as well as a behavioral effect on those whose extensive margin behavior does not change. That is, it consists of both a treatment effect and a change in the composition of the sample. However, these coefficients can be

[11] See Huck and Rasul (2011) and Meer (2011) for discussion on the use of this approach for charitable giving estimates.

be combined to find the marginal effect on the unconditional mean, with standard errors calculated using the delta method:

$$\frac{dP(Y_{dst} > 0)}{dLogExp_{dst}} \times E[LogY_{dst}|Y_{dst} > 0] + P(Y_{dst} > 0) \times \frac{dE[LogY_{dst}|Y_{dst} > 0]}{dLogExp_{dst}} \tag{3.3}$$

For the relationship between the amount requested and the amount given, we estimate specifications conditional on a request. No donations can be given through DonorsChoose.org without a request. As such, there are no observations for which there are positive donations but no requests.

$$LogDonations_{dst} = \alpha + \theta \cdot LogRequests_{dst} + \delta \cdot X_{dt} + \gamma_d + \mu_t + \eta_{st} + \epsilon_{dst} \text{ if } Requests > 0 \tag{3.4}$$

3.2.4 Instrumental Variables

These specifications include county-by-year fixed effects, district fixed effects, and district-level demographic variables to account for local conditions and factors that impact both giving and school expenditures. But it is possible that within a county, a school district's economic fortunes were trending downwards in a way that is not captured by our other controls, leading to both lower expenditure and reduced giving by its residents. Or a shock to the district may lead to spurious correlation: for example, a natural disaster could lead to greater giving and changes in government spending. Districts with lower levels of spending may be more likely to hire new teachers, who are more likely to be familiar with platforms like DonorsChoose.org.

It is difficult to rule out all such stories. But the use of instrumental variables which affect expenditures but are uncorrelated with district-specific shocks can assuage these concerns. We use versions of two sets of instrumental variables that have been used recently in the economics of education literature to instrument for per-pupil spending.[12]

[12]We considered using discontinuities around school budget votes as a source of identifying variation for changes

First, we adapt the school finance reform instruments used in Jackson et al. (2015) and Bayer et al. (2020), who argue that judicially-imposed reforms are an exogenous source of variations, and which increase per-pupil spending by more in low-income districts than higher-income ones.[13] The early reforms they study, beginning in the 1970s, are too far in the past to have appreciable impacts in our sample, yielding a weak first stage and imprecise and implausible estimates. We limited the sample to the seven reforms since 1995 (Vermont, Ohio, Michigan, Idaho, New York, South Carolina, and Oregon), interacted with base-year district spending quartile, and replicated those findings. Figure B.4 shows an event study graph of the impact of these reforms on per-pupil expenditures in a regression that includes year and district fixed effects. The F-statistics for the first stages are 33 and 44 on the extensive margin for the specifications without and with additional controls, respectively, and 22 and 25 on the intensive margin.

We also follow Deming and Walters (2017), who use a shift-share instrument for higher education expenditures, interacting an institution's appropriations revenue share in an initial year with the current year's total state appropriations (on a per-college-aged-population basis). In a similar vein, we interact a district's share of its revenues coming from state appropriations in the first year it appears in our sample with current state appropriations divided by the number of children between the ages of 5 and 18, as shown below.

$$Z_{d,t} = \left(\frac{District's\ State\ Revenue_{d,t=1}}{District's\ Total\ Revenue_{d,t=1}}\right) \times \left(\frac{State\ Revenue_{s(d),t}}{Pop\ ages\ 5-18_{s(d),t}}\right) \tag{3.5}$$

If state appropriations increase, districts that are more reliant on state revenue in the baseline year are more likely to have revenue increases. But this measure will not be related to changes in the district's circumstances, which are more likely to be correlated with unobserved factors that

in charitable giving, using data from New Jersey and New York. We found little impact on per-pupil spending and the results were sensitive to specification. We also follow Baron (2019), who finds that operational referenda in Wisconsin increase per-pupil expenditures, replicating his finding. However, the relatively small sample size of DonorsChoose.org projects posted in Wisconsin in the relevant time periods yields noisy estimates.

[13] We also replicated the results in Brunner et al. (2022), which use the construction of wind farms to proxy for increases in revenues. However, as Brunner et al. (2022) explain, these revenues are more likely to be used for capital expenditures (and, in some cases, are required to be used as such). As they did, we found little first-stage effect on elementary-secondary expenditures, making this approach unsuitable for our purposes.

also impact project postings and donations. Since increases in state-level spending on education are expected to increase spending in districts that are more reliant on state aid, we expect this measure to have a positive coefficient in the first stage estimates. As shown in Columns 1 and 3 of Tables B.4 and B.5 – which also include the school finance reform indicators – it does, in both the extensive and intensive margins, and is precisely estimated.

Using these instruments in our specifications for donations and requests comes at a cost. Both are determined at the state-year level. The school finance reform variables are a function of a district-specific factor (the district's resource quartile) multiplied by a state-year function. The shift-share instrument is similarly composed of a district-level factor (reliance on state revenue in the baseline year) multiplied by a state-year function. As such, including state- or county-by-year effects in the instrumented specifications leaves little identifying variation.[14]

When estimating the effect of the amount requested on the amount donated, we instrument using the amount and number of projects posted by neighboring school districts.[15] We use school district boundary information from the Education Demographic and Geographic Estimates (EDGE) Program and determine the neighboring districts based on the 1995 geographic estimates (TIGER/Line®Files, 1995; Geverdt, 2018). For these specifications, we can include state-by-year or county-by-year fixed effects. As seen in Table B.6, these instruments tend to be statistically significant; in practice, the relationship between amount requested and amount donated is similar with and without instrumental variables. The set of instrumental variables used does not greatly affect the estimates.

As a further check, we examine the relationship between a given year's amount requested and the previous and following year's amount given. If this latter relationship is strong, it would suggest that unobserved trends are driving teachers' posting behavior and donors' giving. In a specification with district and county-by-year fixed effects, previous year's giving has a small impact on this

[14]Recent work by Goldsmith-Pinkham et al. (2020) indicates that the shift-share approach is equivalent to using the initial shares as instruments, weighting by the overall shift over time. While we cannot verify that this exclusion restriction holds, the baseline year for most districts is eight years before the availability of DonorsChoose.org; as such, the shares are more likely to reflect these initial conditions and be excludable from the second stage.

[15]We sum these variables among all neighboring districts, adding 1 before taking logs.

year's amount requested, with an elasticity of 0.051 (s.e. = 0.01); the following year's giving has no effect, with a coefficient of 0.0025 (s.e. = 0.016).

3.3 Results

3.3.1 Baseline Specifications

3.3.1.1 Teacher Postings

We begin with the demand side – that is, postings by teachers. Changes in expenditures are generally more salient to teachers than to parents. Further, teachers can post a request irrespective of the desire to donate. This response, therefore, gives a measure of the need perceived by teachers.[16]

Columns (1) through (4) of Panel A of Table B.2 shows the impact of per-pupil elementary-secondary expenditures on the likelihood that any project is posted in the district in a given year, including state-by-year and district fixed effects. Column (1) shows that a 10 percent increase in those expenditures leads to a 0.6 percentage point decrease in the likelihood of a posted project (standard error = 0.075 percentage points).

Adding county-by-year fixed effects in Column (2), to account for local economic and political shocks that could impact both budgets and giving, reduces this effect to -0.47 percentage points for a 10 percent change in expenditures. Columns (3) and (4) add a set of control variables for the share of students in poverty, the log of the number of teachers, and enrollment shares by race to the specifications in Columns (1) and (2), respectively. This reduces the sample somewhat due to missing data. In the specification with state-by-year effects, a 10 percent increase in the per-pupil elementary-secondary budget reduces the likelihood of a posting by 0.53 percentage points (s.e. = 0.11), while with county-by-year effects, the reduction is 0.38 percentage points (s.e. = 0.13).[17]

[16]Of course, we cannot reject the possibility that teachers are responding to a stated desire to give by potential donors; for example, a parent may suggest to his or her child's teacher that the teacher post a request to allow for tax-deductible directed giving to that classroom. Note that donations can come from anywhere. Meer (2017) shows that general geographic proximity has an effect on donor preferences, but many donations are given to schools outside of the area in which the donor lives.

[17]The change between the first two columns and the second two is driven by the inclusion of the controls. Estimating the more parsimonious specification on the limited sample in Columns (3) and (4) yields results similar to those in (1) and (2), respectively.

Panel B examines the intensive margin of the number of dollars requested by teachers, conditional on a project being posted. Increased budgets are associated with reduced posting on the intensive margin, though these estimates are imprecise and cannot be interpreted causally since they consist of both a treatment effect and a change in the composition of the sample. But in Panel C, we combine these effects. Combining the negative effects on the extensive margin and intensive margins, we find an elasticity of -0.31 (s.e. = 0.094) for the specification in Column (4). We conclude, therefore, that teachers are responsive to changes in educational budgets; they reduce their efforts to raise external funds in the face of higher budgets. These results are similar in spirit with those in Andreoni and Payne (2003) and Andreoni and Payne (2011), who find a significant reduction in fundraising expenditures in response to government grants.

3.3.1.2 Donations

In Columns (5) through (8) of Table B.2, we turn to the effect on donors' contributions. Panel A shows the effects on the likelihood that any donations are made. Column (8), which includes both controls and county-by-year effects, shows that a 10 percent increase in expenditures leads to a 0.27 percentage point decrease in the likelihood of a donation (s.e. = 0.13 percentage points).

Panel B shows the intensive margin effect on the amount donated, which are are close to zero and imprecisely estimated. Combining the effects in Panel C yields an elasticity of -0.20 (s.e. = 0.091) in Column (8).

The similarity of this elasticity to that of the amount requested suggests that the crowding out we observe is primarily due to teachers' reduced posting of projects.

3.3.1.3 Fundraising Effectiveness

Finally, we estimate the impact of requests on donations. This specification differs from those above since donations can only be made in response to a request. Table B.3 shows similar results across all the columns, with a 10 percent increase in the amount requested associated with a roughly 9 percent increase in donations. While this is not directly comparable to the effect of fundraising expenditures in other work, it is line with the findings that charities are not revenue maximizers –

that is, it appears that teachers could raise more funds by posting more projects.

3.3.1.4 Estimates of Classic and Indirect Crowd-Out

These estimates can be combined to decompose the total change in crowd-out into its classic and indirect components. Using the specifications that include controls and county-by-year effects, a $1,000 increase in per-pupil expenditures yields a total decrease in donations of $35, measured at the means of annual district-level expenditures and donations. The amount requested declines by $78, yielding a reduction in donations of $49. Indirect crowd-out therefore accounts for the entirety of the reduction in donations, with a small but statistically insignificant amount of crowd-in as the direct effect of increased K-12 spending.

Though the elasticities that generate them are precisely estimated, these values are even smaller in magnitude than they appear. The average district-year observation has an enrollment of 3255 students, meaning that a $1,000 increase in per-pupil expenditures, taken as a whole, dwarfs the reduction in donations. But these estimates do not account for potential endogeneity issues, to which we turn in the next section.

3.3.2 Instrumented Specifications

3.3.2.1 Teacher Postings

We begin again with the demand side, examining the effect on teacher postings. The instrumented specifications include year and district fixed effects, since most of the variation in the instruments is at the state-year level. The effects on the extensive margin are much larger; Table B.4 shows that a 10 percent increase in per-pupil elementary-secondary expenditures reduces the likelihood of any project being posted by 4 percentage points. Panel B shows that the effects on the intensive margin are also negative, with a coefficient of -0.94 (s.e. = 0.23) – again, note that one cannot draw causal conclusions from these estimates. The inclusion of controls does not change the coefficients very much. Combining the estimates yields an unconditional elasticity of -4.0 (s.e. = 0.30). This estimate appears quite large, perhaps implausibly so, but it compares per-pupil expenditures to overall donations; we benchmark the estimates below for a more clear

interpretation.

3.3.2.2 Donations

Turning to the amount donated, reported in Table B.5, we once again see that the effects of changing K-12 budgets are similar to the effects on postings. The impact of a 10 percent increase in per-pupil elementary-secondary spending on the extensive margin is about -4 percentage points (s.e. = 0.3 percentage points). On the intensive margin, the coefficient is -1.5 (s.e. = 0.29). Combining the effects yields an estimated elasticity of -3.6 (s.e. = 0.29).

3.3.2.3 Fundraising Effectiveness

Since there is within state-year (and county-year) variation in the instruments used for the amount requested, we report specifications that include those additional fixed effects in Table B.6. In practice, the coefficients do not change much when these finer controls are included. Much like the uninstrumented estimates, the elasticity of donations with respect to requests is close to 1, suggesting that teachers could raise more money by posting more projects.

3.3.2.4 Estimates of Classic and Indirect Crowd-Out

Using the instrumented results, we decompose total crowd-out into the classic and indirect effects. A $1,000 increase in per-pupil expenditures yields a total decrease in donations of $628, measured at the means of annual district-level expenditures and donations. The amount requested declines by $1012, yielding a reduction in donations of $767. As above, indirect crowd-out accounts for the entirety of the reduction in donations, with potentially a small amount of crowd-in from increased spending.

The magnitudes here are much larger than those in Section 3.3.1.4, but still small compared to the total expenditure change for the average district. A $1,000 increase in per-pupil elementary-secondary expenditures is about $3.2 million at the mean, compared to a reduction of $628 in donations. However, relative to a baseline of teacher out-of-pocket spending, or the funds districts allocate towards the types of activities that DonorsChoose.org funds, these effects are larger.

3.3.3 Additional Results

3.3.3.1 Donor Location

Examining the response of local and non-local donors to changes in changes in elementary-secondary expenditures provides suggestive evidence on the degree to which shocks to local preferences that affect both giving behavior and K-12 funding are a concern. About 85 percent of the dollars donated are associated with observations that have the donor's state available.[18] In-state donors are somewhat more responsive to changes in expenditures than out-of-state donors, with an instrumented elasticity of -3.3 (s.e. = 0.25) as compared to -2.8 (s.e. = 0.26).

However, this finding should not be taken as definitive. Expenditures are likely more salient to locals, but states are fairly large geographic areas. And ultimately, given the evidence that changes to teachers' posting behavior drive the results – and the small role that classic crowd out plays – this is not surprising.

3.3.4 Project Subject and Resource Type

We examine how the responsiveness for teachers' requests for funds varies by the subject and resource type. Each project is assigned one of 31 categories as their primary subject matter, such as "Mathematics," "Literature & Writing," "Mental Health," " Special Needs," and so on. We classify these as "Academic," "Enrichment/Extracurricular," "Support," and "Other." Further, projects are assigned to one of 18 categories of resource types, such as "Art Supplies," "Books," "Food, Clothing, & Hygiene," and "Musical Instruments." We classify these as "Classroom Supplies," "Enrichment," "Technology," and "Basic Needs/Other." We then estimate our instrumented specification separately for each category type.

Requests for and donations to projects focusing on Academic subjects and Classroom Supplies and Technology resources are the most responsive to changes in budgets. Enrichment (both in terms of subject and resources) and other types of projects tend to be less responsive. Without

[18] ZIP codes are available for far fewer observations, so we focus on state.

making too much of these patterns, they suggest that teachers are funding core needs through DonorsChoose.org. That is, the results are consistent with enrichment-type activities being less affected by marginal changes in budgets and more often in need of external support.

3.4 Discussion & Conclusion

We examine how K-12 education budgets impact private giving to education. Using rich data from DonorsChoose.org, we show that teachers respond to changes in school expenditures by reducing both the likelihood of making a request for funds and the overall amount requested. This, in turn, reduces contributions.

We show that private contributions can counteract changes in government spending, though the magnitudes we find are small relative to overall education spending. But our primary contribution is shedding light on the nature of altruism. If donors are driven by pure altruism, that is, a simple desire to see public goods provided irrespective of their own actions, theory suggests that they will reduce their donations in the face of government spending. Taken on their own, the effect of expenditures on donations would suggest that this is the case. But by examining the impact of expenditures on teachers' requests, and of requests on donations themselves, we show that this effect is entirely driven by endogenous responses on the part of the teachers. This shows the importance of considering the demand side of the charitable giving and nonprofits' objective functions.

4. LEGAL ACCESS TO REPRODUCTIVE CONTROL TECHNOLOGY, WOMEN'S EDUCATION, AND EARNINGS APPROACHING RETIREMENT*

4.1 Introduction

The landscape for reproductive health care in the United States has undergone massive changes in recent years. In 2017, the set of employers and insurers who are exempt from the Affordable Care Act's contraceptive coverage mandate was broadened to include those with moral objections. In 2019, Title X rules were changed to deny funding to family-planning providers that refer patients for abortion, which could restrict women's access to both contraception and abortion care. At the same time, several states, including Delaware, Massachusetts, South Carolina, and Washington, have launched major initiatives to expand access to the full range of contraceptives, including intrauterine devices and implants, which can be difficult for some women to obtain because of costs and a lack of trained providers. A variety of state restrictions have made it harder for women to access abortion, including restrictions that have caused abortion clinics to close. Telemedicine for consultation and/or medication abortion has expanded access in some states. Questions about the economic effects often come up when the desirability of such policies is discussed. The economic effects are relevant to considering the merits of subsidizing access and to considering the costs imposed by regulations that limit access.

What do historical changes in contraception and abortion access tell us about the long-run effects of such changes? In this study we investigate this question using data from the Health and Retirement Study and an identification strategy that leverages variation in exposure to legal changes in access across cohorts born in the same states during the 1960s and 1970s. We follow the methodology of Bailey et al. (2012) (hereafter "BHM") who used the National Longitudinal Survey of Young Women and documented significant increases in contraception use at ages 18-20 associated with unmarried women's ability to consent for contraception at such ages. They

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also documented increased educational attainment and increased earnings in women's 30s and 40s associated with this confidential access to contraception. Our analysis revisits the effects on education and earnings. We also investigate the sensitivity of the estimated effects to the legal coding and control variables used in Myers (2017)'s study of the effects on fertility and marriage.

The results for educational attainment align with prior work but are not statistically significant. The results for earnings indicate increases in the probability of working in a Social Security (SS) covered job in women's 20s and 30s associated with early access to contraception and abortion, but we find no evidence of positive effects on women's earnings in their 50s.

4.2 Data and Methodology

Our analyses use restricted-use data from the Health and Retirement Study (HRS).[1] HRS is a longitudinal survey of Americans over age 50 and their spouses. The study interviews approximately 20,000 respondents every two years on subjects like employment, health care, housing, assets, pensions, and disability. We use restricted-use data from HRS that includes individuals' earnings histories from 1951-2013 based on information provided by the Social Security Administration. The HRS has collected information on six groups of birth cohorts across multiple survey waves since they began conducting surveys in 1992. Our analysis of educational outcomes follows the approach used in Goldin and Katz (2002), Bailey (2006), Bailey (2009), Guldi (2008), Hock (2008), and Myers (2017) who analyze the effects of legal access to contraception and abortion on women's marital and fertility outcomes using within-state-across-cohort variation. Following Myers (2017), our analysis of education focuses on women born 1935-1958 and considers two measures of access to each reproductive control method (contraception and abortion): the method being legal and young unmarried women being able to provide legal consent ("pill consent" or *PiCon*, "abortion consent" or *AbCon*), and the method being legal but young unmarried women not being able to provide legal consent ("pill legal" or *PiLeg*, "abortion legal" or *AbLeg*). We measure a woman's exposure to legal access

[1]The HRS (Health and Retirement Study) is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan.

based on the legal circumstances in her state of residence between the ages 18-20, allowing variables to range from zero to one for the proportion of years of legal access during these years. We infer a woman's state of residence at these ages based on her state of residence at age 10 for the vast majority of women for whom this is available and based on state of birth for the remainder. Our regression model, identical to Myers (2017), is as follows:

$$Ed_{ics} = PiLeg_{cs}\gamma + PiCon_{cs}\beta + AbLeg_{cs}\theta + AbCon_{cs}\delta + \eta_c + \psi_s + X_{ics}\lambda + \epsilon_{ics} \tag{4.1}$$

where Ed_{ics} measures the educational attainment for woman i born in cohort c who lived in state s as a youth, the legal access measures are as defined above, η_c are cohort fixed effects, ψ_s are state fixed effects, and X_{ics} includes a rich set of additional controls including state-linear cohort trends.[2] In constructing standard error estimates, we allow the error term ϵ_{ics} to be correlated across cohorts from the same state. In addition to reporting estimates based on Myers (2017)'s legal coding, we also report estimates that use BHM's legal coding for contraception access.[3]

Our analysis of women's economic outcomes across the life-cycle follows BHM. This methodology also leverages variation in access across cohorts of women from the same state but focuses on variation in young women's ability to provide consent to access contraception and extends the model to assess the effects on women's outcomes that are measured at different ages. Specifically, we estimate

$$\begin{aligned} Y_{iacs} = &\sum_g \beta_g PiCon_{cs} D_{g(a)} + \sum_g \gamma_g EAA_{cs} C50_c D_{g(a)} + \\ &\sum_g \theta_g PiCon_{cs} EAA_{cs} C50_c D_{g(a)} + \delta lnDist_s C50_c + \\ &\gamma_{g(a)} + \theta_s + \psi_c + \epsilon_{iacs} \end{aligned} \tag{4.2}$$

[2]The additional control variables include race, ethnicity, the interaction of "early pill legal" and "abortion legal" and the interaction of "early pill legal" and "early abortion legal." They also include exposure (measured as the fraction of years from age 18-20) to: state abortion reforms, which were enacted in 13 states prior to *Roe vs. Wade* and permitted abortion under limited circumstances; state policy permitting no-fault divorces; state equal pay law prior to the enactment of federal legislation in 1963; and state fair employment practices act (FEPA) prohibiting racial discrimination in hiring, discharge, and compensation.

[3]BHM's coding is based on Bailey et al. (2011).

where g corresponds to 5-year age groups (20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55+), $D_{(g(a))}$ is an indicator if an observation is in age group g based on its corresponding age a, EAA_{cs} is an indicator for early legal access to abortion (defined as residing in an early-legalizing state[4] before age 21), $C50_c$ is an indicator for being born in 1950 or later and thus potentially being affected by abortion legalization before age 21 for women residing in an early legalizing states, and the other variables are defined as in Equation 4.1. For this analysis we follow BHM by considering women born no later than 1954.[5]

Two notable difference between the models characterized by Equation 4.1 and Equation 4.2 are that the latter model: (i) does not distinguish between legal access to abortion and minors' ability to consent for abortion and (ii) does not consider the degree to which there may be effects of legal access when these women are themselves older. We intend to examine these possibilities in future work. In this study, we replicate BHM, extend the analysis to consider effects at older ages, and we examine the sensitivity of the estimates to using legal coding and additional control variables based on Myers (2017).[6] When we do so, we measure early abortion access when women were age 18-20 based on whether unmarried of such ages could consent to abortion according to Myers (2017)'s coding.

4.3 Results

4.3.1 Educational Attainment

Table C.1 reports our estimated effects on years of education (up to 17) based on Equation 4.1. Consistent with estimates reported in BHM, and previously in Goldin and Katz (2002) and Hock

[4]Early-legalizing states are states that legalized in 1969-1971: Alaska, California, the District of Columbia, Hawaii, New York, and Washington.

[5]BHM was restricted to using data on from the 1943-1954 cohorts because those were the cohorts covered by the National Longitudinal Survey of Young Women (NLS-YW), which was first conducted in 1968 and focused on 5,159 women ages 14 to 24 at the time. The results reported in the tables in this paper are based on an expanded set of cohorts, 1930-1954. These results are consistent with our analysis of the 1943-1954 cohorts, which produce estimates that are slightly smaller in magnitude but with much larger standard errors.

[6]These additional control variables include indicators for the race and ethnicity of the respondent, state-linear cohort trends, and measures of the fraction of years of exposure (from age 18-20) to: state abortion reforms and consent to state abortion reforms (enacted in 13 states prior to Roe vs. Wade and permitted abortion under limited circumstances); state policy permitting no-fault divorces; state equal pay law prior to the enactment of federal legislation in 1963; and state fair employment practices act (FEPA) prohibiting racial discrimination in hiring, discharge, and compensation.

(2008), our estimates suggest that both legal access and being able to consent for contraception from age 18-20 is associated with increased levels of education. With that said, we note that these estimates are only marginally statistically significant when we use BHMs' coding of legal access to contraception (Column 1) and that the estimates are somewhat smaller and are not statistically significant when we use Myers (2017)' coding (Column 2). Our analysis of black women also suggests positive effects of greater legal access to reproductive control technology, and to legal access to abortion in particular (columns 3 and 4).

4.3.2 Earnings

We examine earnings using two types of data available in the HRS. Earnings based on social security (SS) records and earnings based on HRS surveys. The former has the advantage of a large sample size covering a very broad set of age groups; however, it will vastly understate earnings for women working in jobs that are not covered by SS. For this reason, we use this measure simply to evaluate whether a woman had any earnings in a SS-covered job in a given year, which is measured without error.[7] In 1981, 90 percent (98 million) of all wage and salary workers and 62 percent (13 million) of workers in the public sector were covered under SS (Heeringa and Connor, 1995). We use HRS's survey-based measure of earnings to evaluate women's earning levels in their 50s.[8]

Table C.2 reports the estimated effects on whether a woman is working in a SS-covered job. Column 1 shows the results following BHM's methodology and Column 2 shows the results using Myers (2017)' coding and the additional control variables described in Footnote 6. As a whole, these estimates indicate that early legal access to contraception increased women's probability of working in a SS-covered job, particularly in their late 20s and early 30s. While any such effects may reflect increased labor force participation, it could also arise from substitution from SS-uncovered jobs to SS-covered jobs.

The results also indicate that gaining early legal access to abortion is similarly associated with

[7]If instead evaluated earnings levels based on this measure, it could cause us to understate the economic benefits of legal access to reproductive control technology if such access led women into higher paying jobs that are not covered by Social Security.

[8]The analysis includes younger women but we only report estimates for women in their 50s, because younger women are only included in the HRS if they are married to someone who is older than 50.

an increased probability of working in a SS-covered job. The estimates again suggest effects for women in their 20s and early 30s. As discussed above, an important caveat to these results is that the estimates could be picking up long-run effects of the conditions when a woman was 18-20 or the effects of having access at older ages.[9] Table C.3 shows estimates focusing on the log of women's hourly wages. As a whole, the estimated effects on this outcome indicate no statistically significant effects on women's earnings in their 50s. These results are not inconsistent with BHM who find positive effects of early access to the pill when women were in their 30s and 40s. We also do not find evidence of statistically significant positive effects if we evaluate hourly wages (not taking the logarithm), hourly wages excluding zeroes, weekly wages (taking the logarithm or not, excluding zeroes or not), or if we restrict the sample to the 1943-1954 cohorts (as in BHM).

4.4 Conclusions

Given major gaps in access to contraception and abortion care, understanding the economic effects of such access is likely to continue to be relevant to policy. In this paper, we build on the knowledge base by evaluating how changes in access resulting from policy changes in the 1960s and 1970s affected educational attainment and women's very-long run earnings. We hope that future work will go deeper in assessing the robustness of these results.

[9]Estimated effects of both contraception access and abortion access are slightly smaller in magnitude with much larger standard errors if we instead analyze the 1944-1954 cohorts (like BHM) instead of the 1930-1954 cohorts.

5. SUMMARY AND CONCLUSIONS

This dissertation employs quasi-experiment research designs in public and health economics to study charitable giving and pro-social institutions as well as the economic effects of reproductive health care on women's outcomes.

Section 2 shows that the minimum wage changes can impact the nonprofit sector. In the states with large statutory minimum wage increases, the disemployment effect is more significant. We explore other margins of adjustments and find suggestive evidence that the number of establishments reduces with large increases in the minimum wages. However, there is no clear impact on finances.

In Section 3, we use rich data from DonorsChoose.org and show government spending crowds out private contributions to education. This section sheds light on the importance of the demand side of charitable giving, showing that teachers reduce fundraising efforts as government spending increases, which reduces contributions.

Section 4 provides evidence on how access to contraception and abortion care impact women's economic outcomes in the long term. We find a reduction in educational attainment and earnings after exposure to policy changes in the 1960s and 1970s, demonstrating the costs imposed by regulations that limit access.

This dissertation provides empirical evidence on how policies affect the nonprofit sector and donors' behavior. Given the significant role of nonprofits in society, exploring how they respond to changes in policies like minimum wage would be useful to understand how the cost increases can be borne in this sector. In addition, the findings also shed light on the nature of altruism by providing evidence of indirect crowd-out. Lastly, evaluating the economic effects of access to reproductive health care would be crucial from a policy standpoint and lead to opportunities to design more effective policies.

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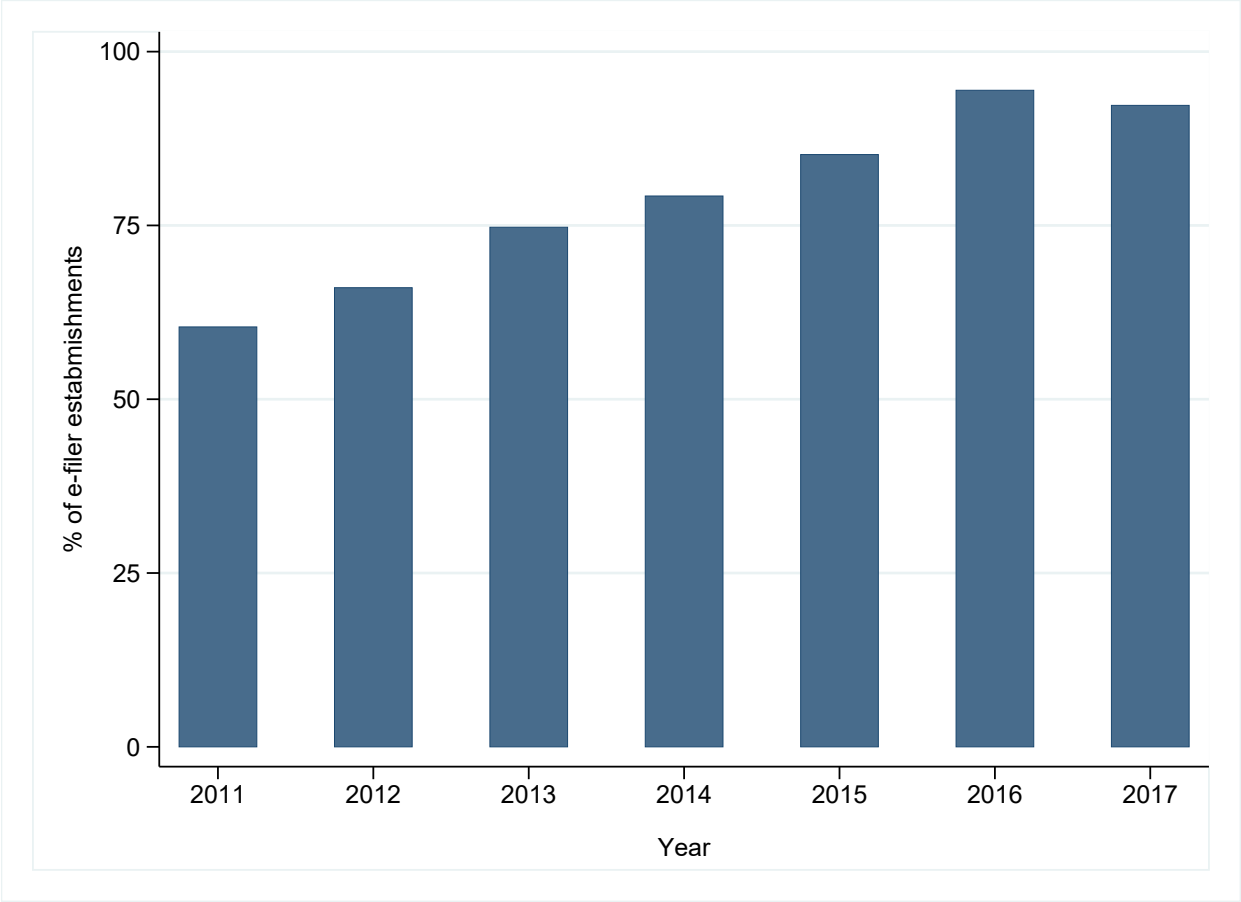
APPENDIX A

FIGURES AND TABLES FOR SECTION TWO

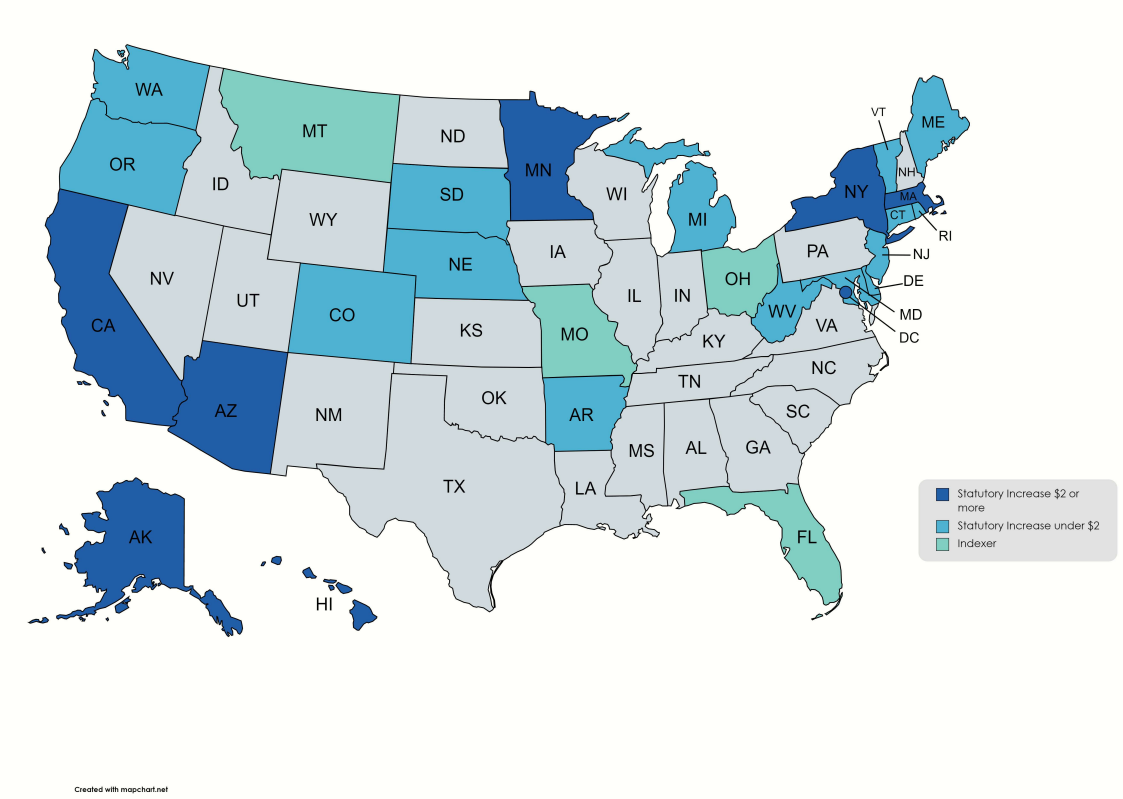
A.1 Figures

Figure A.1: Percent of public charities using e-filing



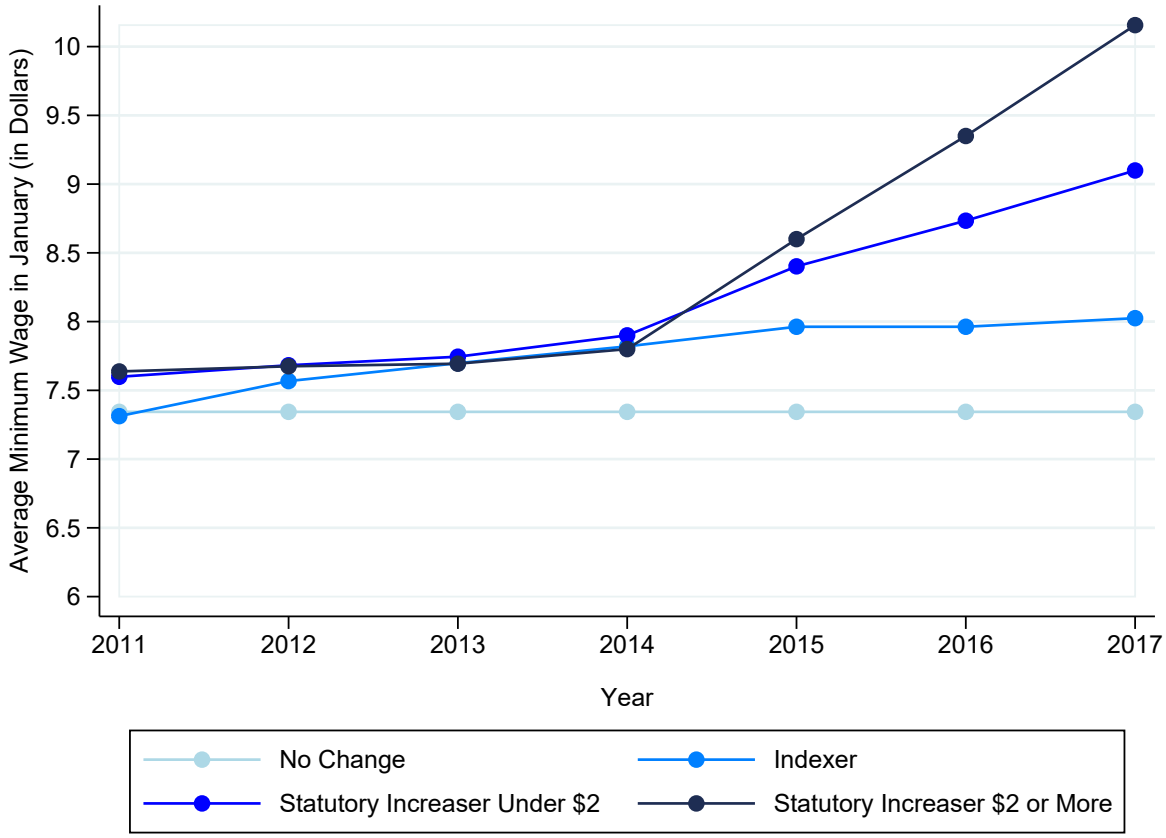
Notes: The numbers are calculated by authors using data from the IRS Forms 990 filed electronically by 501(c)(3) public charities, posted on Amazon Web Services and Urban Institute (2017).

Figure A.2: Minimum wage policy by state



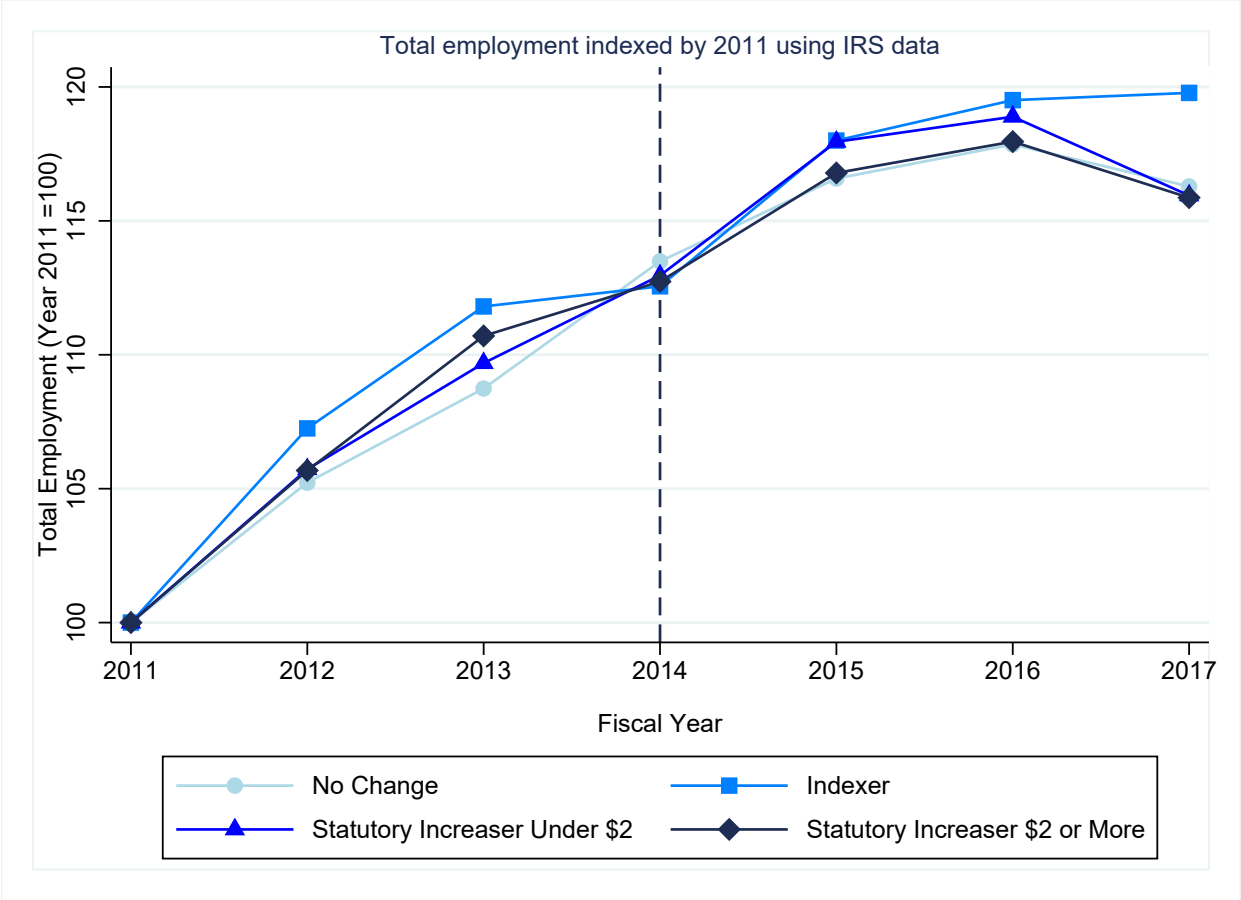
Notes: The map was created by authors with mapchart.net using information from Clemens and Strain (2018a, 2020b,c).

Figure A.3: Average minimum wage by policy groups



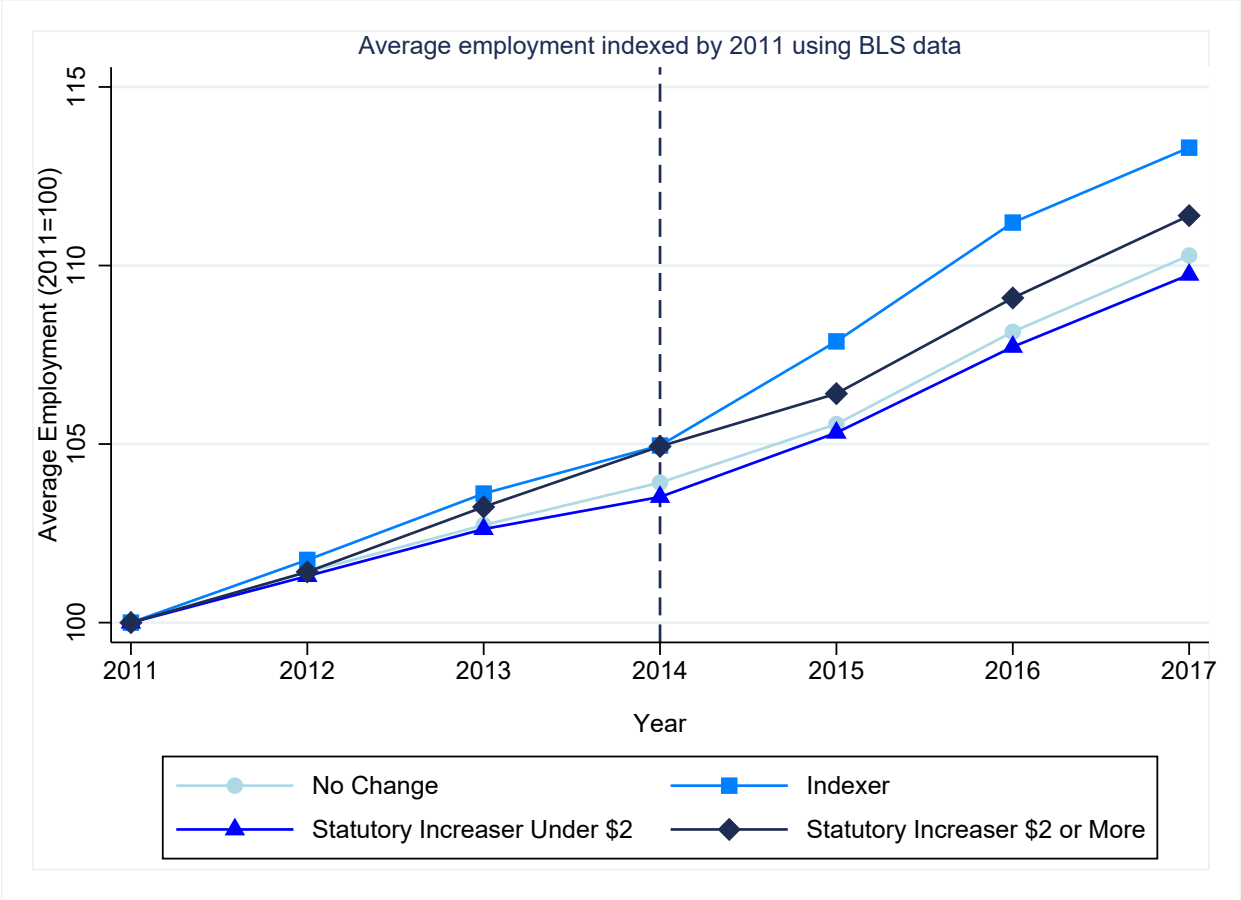
Note: The numbers are calculated by authors using data from Clemens and Strain (2018a, 2020b,c).

Figure A.4: Nonprofit employment by policy group (IRS data)



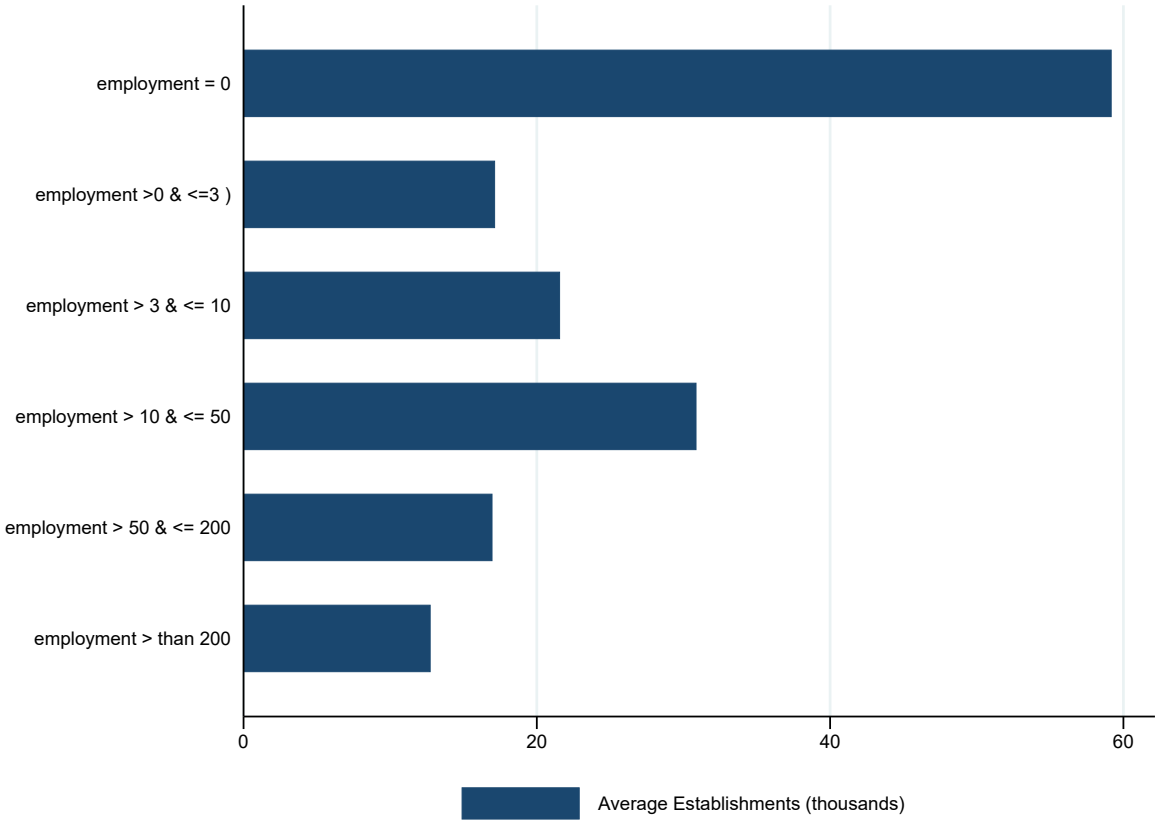
Notes: The numbers are calculated by authors using data from the IRS Forms 990 filed electronically by 501(c)(3) public charities, posted on Amazon Web Services.

Figure A.5: Nonprofit employment by policy group (BLS data)



Notes: The numbers are calculated by authors using data from Bureau of Labor Statistics (2019).

Figure A.6: Nonprofit establishments by employment (2011-2017)



Notes: The numbers are calculated by authors using data from the IRS Forms 990 filed electronically by 501(c)(3) public charities, posted on Amazon Web Services.

A.2 Tables

Table A.1: Summary statistics across states for 2011-2017

Panel A: IRS Data	Mean	Std. Dev.	Median
Number of establishments	4732	5265	3014
Total employment	288351	318675	177139
Total volunteers	643148	651335	437856
Total revenues (billions)	29.13	33.13	16.55
Total expenses (billions)	27.42	31.24	15.71
Total assets (billions)	50.38	56.15	28.22
Total gross receipts (billions)	35.50	40.85	19.48
Panel B: BLS Data	Mean	Std. Dev.	Median
Employment	231700	260244	140682
Total wages (billions)	11.45	13.86	6.35
Establishments	5496	6317	3314

Number of observations is 357. Individual observations in Panel A are winsorized at the 99.9th percentile prior to aggregating to the state-year level. Financial variables are indexed to $2017 using the CPI-Urban.

Table A.2: Effects of minimum wage policy on employment (IRS data)

Panel A: DiD Model	Log employment (1)	Log employment (2)
Indexer x Post	-0.034**	-0.027**
	(0.014)	(0.013)
Small Statutory Increase x Post	-0.021	-0.015
	(0.018)	(0.021)
Large Statutory Increase x Post	-0.035	-0.062**
	(0.023)	(0.027)
Observations	306	306
Panel B: FE Model		
Log-MW	-0.156*	-0.240**
	(0.088)	(0.112)
Observations	357	357
Time & state FE	Yes	Yes
Time Controls	No	Yes
Cluster	State	State

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on employment using the IRS e-filers database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Column (2) includes the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.3: Effects of minimum wage policy on employment (BLS data)

Panel A: DiD Model	Log employment (1)	Log employment (2)
Indexer x Post	-0.001	0.004
	(0.006)	(0.006)
Small Statutory Increase x Post	-0.013	-0.013
	(0.011)	(0.008)
Large Statutory Increase x Post	-0.002	-0.027**
	(0.009)	(0.011)
Observations	306	306
Panel B: FE Model		
Log-MW	-0.055	-0.138***
	(0.055)	(0.051)
Observations	357	357
Time & state FE	Yes	Yes
Time Controls	No	Yes
Cluster	State	State

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on employment using the BLS nonprofit establishment database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Column (2) includes the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.4: Effects of minimum wage policy on employment (Excluding 2014-2016)

Log employment	**IRS Data**		**BLS Data**	
	(1)	(2)	(3)	(4)
Indexer x Post	-0.022	-0.018	0.004	0.006
	(0.014)	(0.014)	-0.006	-0.005
Small Statutory Increase x Post	-0.020	-0.023	-0.007	-0.016
	(0.025)	(0.033)	-0.015	-0.011
Large Statutory Increase x Post	-0.042*	-0.080*	0.003	-0.034*
	(0.025)	(0.042)	-0.014	-0.018
Observations	204	204	204	204
Time & state FE	Yes	Yes	Yes	Yes
Time Controls	No	Yes	No	Yes
Cluster	State	State	State	State

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on employment using the IRS e-filer (Columns 1 and 2) and BLS nonprofit establishment (Columns 3 and 4) databases. Data for years 2014-2016 are excluded. Columns (2) and (4) include the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.5: Differenced estimates of minimum wage on employment

Log employment	1 Year	2 Year	3 Year	4 Year	5 Year	6 Year
Panel A: IRS Data						
Log-MW	-0.171***	-0.205***	-0.213**	-0.279**	-0.269	-0.287
	(0.059)	(0.069)	(0.085)	(0.129)	(0.187)	(0.268)
Panel B: BLS Data						
Log-MW	-0.059*	-0.117**	-0.154***	-0.167***	-0.153**	-0.111
	(0.033)	(0.045)	(0.051)	(0.056)	(0.063)	(0.069)
Observations	306	255	204	153	102	51

* p<0.1 ** p<0.05 *** p<0.01

This table shows differenced estimates of the log of the minimum wage the log of employment (2011-2017). Each column increases the span of the difference taken. Panel A uses the IRS e-filers database and Panel B uses the BLS nonprofit establishment data. The minimum wage is indexed to the inflation using the CPI-Urban. All columns include time fixed effects and differenced log of income per capita, house price index, and ACA expansions; state effects are eliminated by the differencing. Standard errors are clustered at the state level.

Table A.6: Effects of minimum wage policy on establishment counts

Log establishment	**IRS Data**		**BLS Data**	
Panel A: DiD Model	(1)	(2)	(3)	(4)
Indexer x Post	-0.057***	-0.052**	-0.005	-0.000
	(0.019)	(0.024)	(0.009)	(0.011)
Small Statutory Increase x Post	-0.011	-0.022	-0.013	-0.017
	(0.020)	(0.018)	(0.012)	(0.011)
Large Statutory Increase x Post	0.014	-0.019	-0.026**	-0.054***
	(0.037)	(0.028)	(0.013)	(0.009)
Observations	306	306	306	306
Panel B: FE Model				
Log-MW	-0.054	-0.193***	-0.053	-0.124**
	(0.096)	(0.069)	(0.060)	(0.055)
Observations	357	357	357	357
Time & state FE	Yes	Yes	Yes	Yes
Time Controls	No	Yes	No	Yes

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on employment using the IRS e-filer (Columns 1 and 2) and BLS nonprofit establishment (Columns 3 and 4) databases. Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Columns (2) and (4) include the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.7: Effects of minimum wage policy on volunteers

Panel A: DiD Model	Log volunteers (1)	Log volunteers (2)
Indexer x Post	-0.027	-0.016
	(0.072)	(0.068)
Small Statutory Increase x Post	0.050	0.039
	(0.045)	(0.049)
Large Statutory Increase x Post	-0.016	-0.076
	(0.066)	(0.060)
Observations	306	306
Panel B: FE Model		
Log-MW	-0.104	-0.318
	(0.195)	(0.229)
Observations	357	357
Time & State FE	Yes	Yes
Time Controls	No	Yes
Cluster	State	State

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on volunteers using the IRS e-filers database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Column (2) includes the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.8: Effects of minimum wage on compensation

Log compensation	**IRS Data**		**BLS Data**	
Panel A: DiD Model	(1)	(2)	(3)	(4)
Indexer x Post	-0.009	-0.004	0.009	0.014**
	(0.021)	(0.021)	(0.008)	(0.007)
Small Statutory Increase x Post	-0.001	0.016	0.001	0.005
	(0.027)	(0.029)	(0.012)	(0.010)
Large Statutory Increase x Post	-0.019	-0.029	-0.002	-0.019*
	(0.026)	(0.035)	(0.008)	(0.011)
Observations	306	306	357	357
Panel B: FE Model				
Log-MW	-0.097	-0.114	-0.025	-0.083
	(0.119)	(0.149)	(0.062)	(0.061)
Observations	357	357	357	357
Time & state FE	Yes	Yes	Yes	Yes
Time Controls	No	Yes	No	Yes

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on log compensation using the IRS e-filer (Columns 1 and 2) and BLS nonprofit establishment (Columns 3 and 4) databases. Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Columns (2) and (4) include the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.9: Effects of minimum wage policy on expenses

Log	(1) Program Services	(2) Grants	(3) Fundraising Expenses	(4) Total Expenses
Panel A: DiD Model				
Indexer x Post	0.003	0.050	0.020	-0.005
	(0.016)	(0.064)	(0.053)	(0.016)
Small Statutory Increase x Post	0.012	-0.042	-0.061**	0.013
	(0.033)	(0.037)	(0.023)	(0.032)
Large Statutory Increase x Post	-0.020	-0.065	-0.049	-0.018
	(0.030)	(0.042)	(0.031)	(0.032)
Observations	306	306	306	306
Panel B: FE Model				
Log-MW	-0.125	-0.417**	-0.305***	-0.118
	(0.140)	(0.183)	(0.100)	(0.143)
Observations	357	357	357	357
Time & state FE	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes
Cluster	state	state	state	state

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on the log of real expense by category using the IRS e-filers database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Each column includes the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.10: Effects of minimum wage policy on revenues

Log	(1) Contributions	(2) Program Service Revenue	(3) Investment Income	(4) Other Revenue	(5) Total Revenue
Panel A: DiD Model					
Indexer x Post	-0.021	-0.018	0.026	-0.049	-0.020
	(0.027)	(0.020)	(0.039)	(0.052)	(0.016)
Small Statutory Increase x Post	-0.012	0.020	0.015	-0.044*	0.012
	(0.025)	(0.034)	(0.046)	(0.024)	(0.032)
Large Statutory Increase x Post	-0.053	-0.006	0.044	-0.028	-0.038
	(0.045)	(0.051)	(0.048)	(0.047)	(0.033)
Observations	306	306	306	306	306
Panel B: FE Model					
Log-MW	-0.310*	-0.038	0.239	-0.259	-0.149
	(0.166)	(0.175)	(0.200)	(0.185)	(0.146)
Observations	357	357	357	357	357
Time & state FE	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes
Cluster	state	state	state	state	state

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on the log of real revenue by category using the IRS e-filers database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Each column includes the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

Table A.11: Effects of minimum wage policy on employment by size

Size Log - employment	Employment <= 3	Employment > 3 & <= 10	Employment > 10 & <= 50	Employment > 50 & <= 200	Employment > than 200
Panel A: DiD Model					
Indexer x Post	-0.178	-0.049	-0.016	-0.047	-0.007
	(0.113)	(0.082)	(0.045)	(0.033)	(0.018)
Small Statutory Increase x Post	-0.150	-0.041	-0.022	-0.057**	0.008
	(0.137)	(0.056)	(0.030)	(0.028)	(0.025)
Large Statutory Increase x Post	-0.272*	-0.026	-0.059	-0.058	-0.035
	(0.160)	(0.052)	(0.049)	(0.042)	(0.028)
Observations	306	306	306	306	306
Panel B: FE Model					
Log-MW	-1.684***	-0.278	-0.163	-0.391***	-0.084
	(0.557)	(0.193)	(0.126)	(0.141)	(0.131)
Observations	357	357	357	357	357
Time & state FE	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes
Cluster	state	state	state	state	state

* p<0.1 ** p<0.05 *** p<0.01

This table reports estimate results of the impact of the minimum wage on the log of employment using the IRS e-filers database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Entity size category is fixed at its size in the first appearance in the sample. All columns include control variables as the log of income per capita, house price index, and ACA expansions. Standard errors are clustered at the state level.

Table A.12: Effects of minimum wage policy on employment by charity purpose type

Charity group Log - employment	1	2	3	4	5	6	7	8	9
Panel A: DiD Model									
Indexer x Post	-0.120***	-0.067***	-0.022	-0.049**	0.025**	-0.017	0.047	-0.372***	-0.197
	(0.037)	(0.017)	(0.037)	(0.022)	(0.012)	(0.110)	(0.107)	(0.119)	(0.155)
Small Statutory Increase x Post	-0.051	-0.081***	0.016	0.014	-0.037	-0.004	-0.028	-0.051	-0.009
	(0.075)	(0.025)	(0.069)	(0.042)	(0.029)	(0.083)	(0.054)	(0.077)	(0.117)
Large Statutory Increase x Post	-0.104**	-0.105***	-0.235**	-0.049	-0.099***	-0.009	-0.070	-0.091	0.170
	(0.047)	(0.034)	(0.093)	(0.049)	(0.025)	(0.117)	(0.058)	(0.105)	(0.479)
Observations	306	306	306	306	306	299	306	306	216
Panel B: FE Model									
Log-MW	-0.738***	-0.456***	-0.532	-0.141	-0.355***	-0.771	-0.255	-0.317	-0.314
	(0.174)	(0.134)	(0.373)	(0.204)	(0.115)	(0.558)	(0.239)	(0.346)	(1.076)
Observations	357	357	357	357	357	351	357	357	254
Time & State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	state	state	state	state	state	state	state	state	state

* p<0.1 ** p<0.05 *** p<0.01

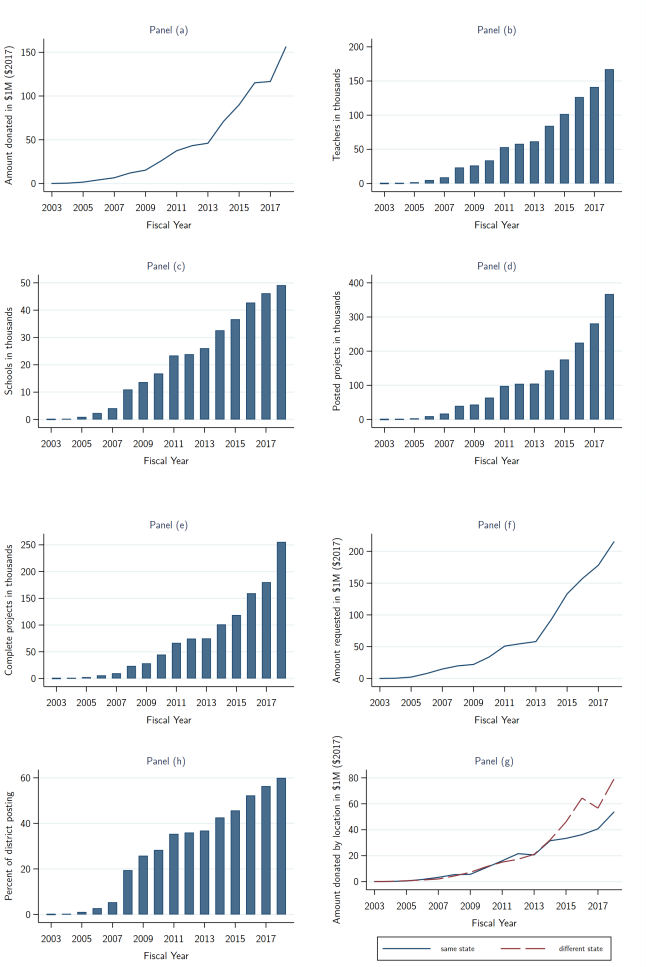
This table reports estimate results of the impact of the minimum wage on the log of employment using the IRS e-filers database (2011-2017). Panel A shows difference-in-differences estimates, excluding 2014, for state policy group interacted with an indicator for the period after the first legislative change. Panel B shows panel fixed effects estimates using the log of the real minimum wage. Each column shows employment by charity type as categorized by the National Taxonomy of Exempt Entities (NTEE): 1) Arts, culture, and Humanities 2) Education 3) Environment and Animal Welfare 4) Health 5) Human Services 6) International and Foreign Affairs 7) Public and Social Benefit 8) Religion-Related 9) Mutual/Membership Benefit. Each column includes the log of income per capita, house price index, and indicators for expansion of the Affordable Care Act in that state. Standard errors are clustered at the state level.

APPENDIX B

FIGURES AND TABLES FOR SECTION THREE

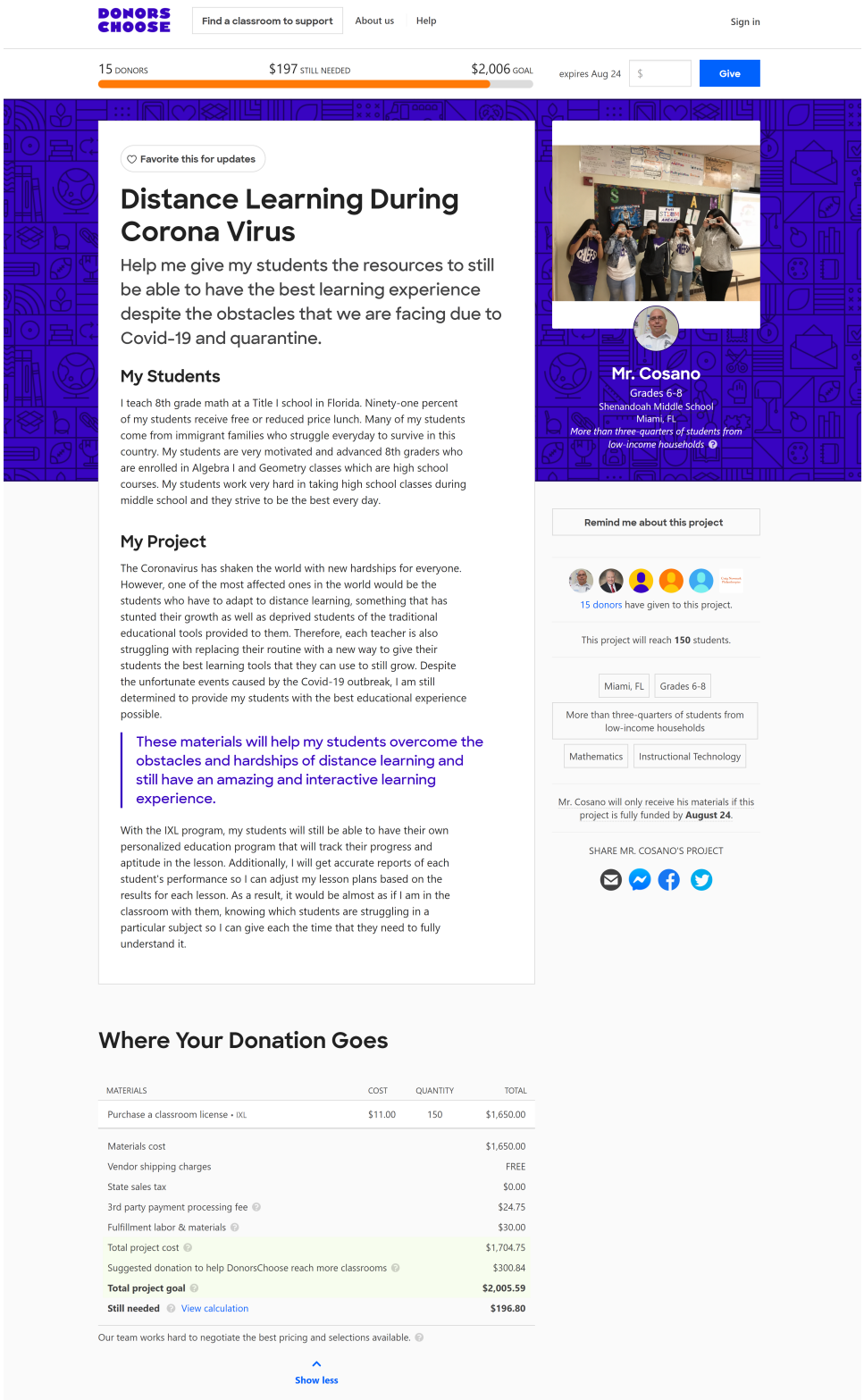
B.1 Figures

Figure B.1: Some characteristics of the DonorsChoose.org data (2003-2018).



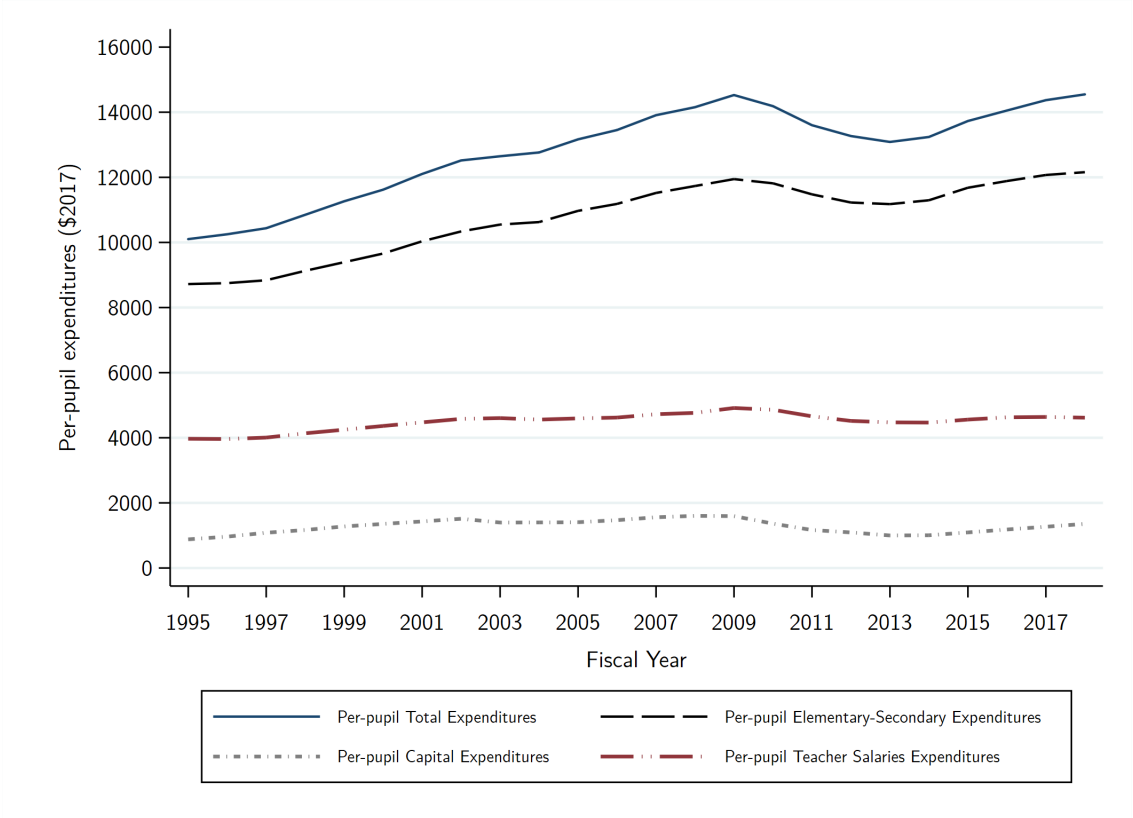
Notes: The numbers are calculated by authors using data from DonorsChoose.org.

Figure B.2: Sample DonorsChoose.org project posting.



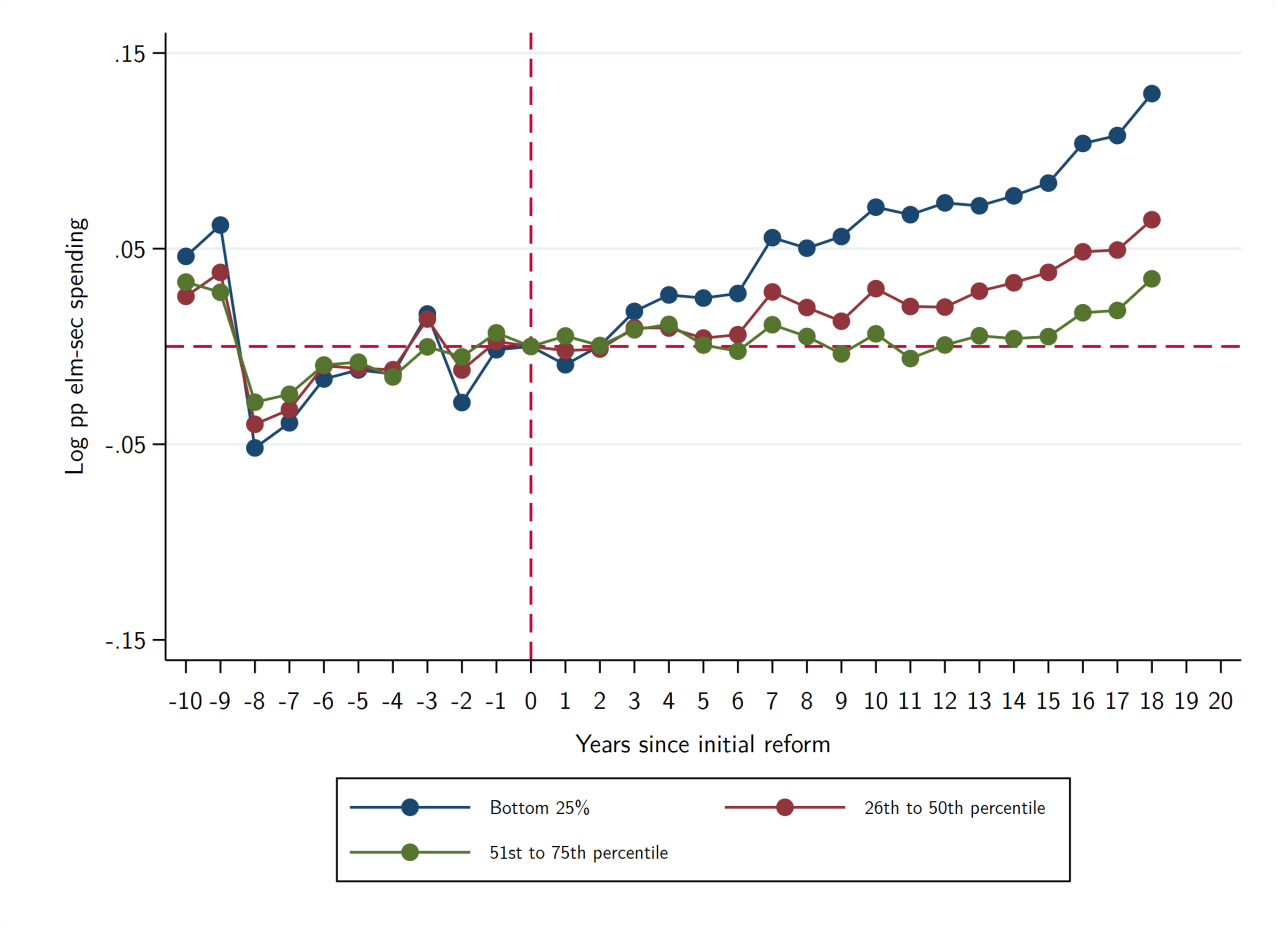
Notes: This picture was retrieved from DonorsChoose.org website.

Figure B.3: Per-pupil expenditures in 2017 dollars in school districts (1995-2018).



Notes: The numbers are calculated by authors from the Common Core of Data.

Figure B.4: SFR effect - changes in elementary-secondary school spending



Notes: This figure shows an event study graph of the change in elementary-secondary school spending before and after court-mandated school finance reforms that occurred between 1995 and 2010. The event time indicators are interacted with the base year spending quartiles. Each series represent difference in the log of elementary-secondary school spending in the associate quartile compared to the omitted category (the highest-spending quartile) before and after the reforms. The regression includes year and district fixed effects. Source: Bayer et al. (2020).

B.2 Tables

Table B.1: Summary statistics

Panel A - unconditional				
District Demographics	Mean	Std. Dev.	Median	Observations
Fall Enrollment	3255.40	14138.66	1013.00	352450
Total Teachers	203.50	816.63	70.70	339836
Frac. White Enrollment	0.73	0.29	0.86	349781
Frac. Black Enrollment	0.10	0.21	0.01	347044
Frac. Hispanic Enrollment	0.12	0.20	0.03	348900
Frac. Children In Poverty (Ages 5 to 17)	0.16	0.10	0.15	274595
Any Project Posted	0.22	0.41	0.00	352450
Any Donation Received	0.20	0.40	0.00	352450
Number of Schools with Posted Projects	0.90	6.79	0.00	352450
District Finance Data ($2017)				
Per-pupil Revenues	13729.11	9261.62	11876.37	352450
Per-pupil Total Expenditures	13719.36	9526.25	11767.35	352450
Per-pupil Elementary-Secondary Expenditures	11505.96	6804.54	10108.61	352450
Per-pupil Capital Expenditures	1211.11	2865.37	504.15	352450
Per-pupil Teacher Salaries Expenditures	4588.61	2425.82	4172.03	352450
Panel B - conditional on any posting				
Posting ($2017)	Mean	Std. Dev.	Median	Observations
Number of Teachers with Posted Projects	11.10	63.05	2.00	76448
Number of Posted Projects	20.57	137.42	3.00	76448
Amount Requested by Teachers	12706.35	89676.53	2015.91	76448
Per-pupil Private Contributions to Districts	48.76	341.21	0.00	196909
Panel C - conditional on any donation				
Donations ($2017)	Mean	Std. Dev.	Median	Observations
Number of Complete Projects	14.93	99.67	2.00	71728
Number of Donations	117.21	1073.02	18.00	71728
Amount Donated	9600.03	67922.86	1436.70	71728
Amount Donated within the Same State	3557.14	32131.54	476.46	71728
Amount Donated by a Different State	4714.58	32634.42	610.93	71728

Authors' calculations.

Table B.2: Impact of per-pupil elementary-secondary expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Postings				Donations			
Panel A: Extensive Margin	Any posted project				Any giving			
Log pp spending	-.063	-.047	-.053	-.038	-.058	-.041	-.044	-.027
	(.0075)	(.0081)	(.011)	(.013)	(.0075)	(.0081)	(.011)	(.013)
N	352450	326871	265586	242367	352450	326871	265586	242367
Panel B: Intensive Margin	Log requested amount				Log amount received			
Log pp spending	-.08	-.072	-.033	-.042	-.046	-.067	.019	-.0045
	(.049)	(.054)	(.083)	(.11)	(.06)	(.065)	(.1)	(.13)
N	74789	61016	63899	49646	69885	56362	59685	45704
Panel C: Combined Effects	Panel A and B				Panel A and B			
Log pp spending	-.51	-.38	-.43	-.31	-.43	-.32	-.32	-.20
	(.059)	(.059)	(.093)	(.094)	(.056)	(.056)	(.089)	(.091)
N	352450	326871	265586	242367	352450	326871	265586	242367
Year and District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
State-Year FE	Yes	No	Yes	No	Yes	No	Yes	No
County-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors in parentheses.

This table shows the impact of per-pupil elementary-secondary expenditures on project postings and donations for years 1995-2018. Expenditures and amount requested (received) are in constant 2017 dollars. Panel A shows the results for the extensive margin (if a project posted and if a project receives any donation), while Panel B shows the intensive margins (amount requested and donated). Panel C provides the marginal effect of unconditional mean for the associated panels. We show result for posting outcomes in columns 1-4 and contributions in columns 5-6. Columns 1, 2, 5, and 6 show the results including no controls, while columns 3, 4, 7, and 8 include covariates as share of children in poverty, enrollment shares by race, and log number of teachers. All odd columns include state-year FEs while we include county-year FEs in even columns.

Table B.3: Impact of fundraising effort on donations

	(1)	(2)	(3)	(4)
	Log amount received			
Log amount requested	.92	.91	.92	.90
	(.0042)	(.0051)	(.0046)	(.006)
N	71946	58494	59839	45864
Year and District FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
State-Year FE	Yes	No	Yes	No
County-Year FE	No	Yes	No	Yes

Standard errors in parentheses.

This table shows the impact of fundraising effort on donations for years 1995-2018. Donations are in constant 2017 dollars. Columns 1 and 2 show the results including no controls, while columns 3 and 4 includes covariates as a share of children in poverty, enrollment shares by race, and log number of teachers. Columns 1 and 3 includes state-year FEs while we include county-year FEs in columns 2 and 4.

Table B.4: Two-stage least squares estimates of the effects of per-pupil elementary-secondary spending on project postings

	First stage	Second stage	First stage	Second stage
Panel A: Extensive Margin	Log pp spending	Any posted project	Log pp spending	Any posted project
	(1)	(2)	(3)	(4)
Budget shock instrument	.064		.079	
	(.0024)		(.0023)	
Log pp spending		-.42		-.44
		(.035)		(.033)
F-statistic for instruments	33.42	145.11	44.40	192.80
N	348012	348012	265503	265503
Panel B: Intensive Margin	Log pp spending	Log requested amount	Log pp spending	Log requested amount
Budget shock instrument	.083		.093	
	(.0024)		(.0023)	
Log pp spending		-.91		-.94
		(.24)		(.23)
F-statistic for instruments	21.98	14.83	25.01	20.20
N	73291	73291	63878	63878
Panel C: Combined Effects		Panel A and B		Panel A and B
Log pp spending		-3.80		-4.00
		(.33)		(.30)
N		348012		265503
Year and District FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
State-Year FE	No	No	No	No
County-Year FE	No	No	No	No

Standard errors in parentheses.

This table reports two-stage least squares estimates of the impact of per-pupil elementary-secondary expenditures on project postings for years 1995-2018. In the first stage, we regress each district's log per-pupil elementary-secondary spending on constructed instruments as the budget shock and school finance reforms after 1995. The second stage regresses posting outcomes on predicated spending from the first stage. Expenditures and amount requested are in constant 2017 dollars. Panel A shows the results for the extensive margin (if a project posted), while Panel B shows the intensive margin as the amount requested by teachers. Panel C presents the marginal effect of unconditional mean for the associated panels. Columns 1 and 2 show the results including no controls, while columns 3 and 4 include covariates as share of children in poverty, enrollment shares by race, and log number of teachers. All Columns include year fixed effects and district fixed effects.

Table B.5: Two-stage least squares estimates of the effects of per-pupil elementary-secondary spending on donations

	First stage	Second stage	First stage	Second stage
Panel A: Extensive Margin	Log pp spending	Any giving	Log pp spending	Any giving
	(1)	(2)	(3)	(4)
Budget shock instrument	.064		.079	
	(.0024)		(.0023)	
Log pp spending		-.39		-.41
		(.035)		(.033)
F-statistic for instruments	33.42	123.56	44.40	185.03
N	348012	348012	265503	265503
Panel B: Intensive Margin	Log pp spending	Log amount received	Log pp spending	Log amount received
Budget shock instrument	.084		.094	
	(.0024)		(.0024)	
Log pp spending		-1.60		-1.50
		(.30)		(.29)
F-statistic for instruments	21.12	29.69	24.14	20.90
N	68469	68469	59666	59666
Panel C: Combined Effects		Panel A and B		Panel A and B
Log pp spending		-3.40		-3.60
		(.31)		(.29)
N		348012		265503
Year and District FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
State-Year FE	No	No	No	No
County-Year FE	No	No	No	No

Standard errors in parentheses.

This table reports two-stage least squares estimates of the impact of per-pupil elementary-secondary expenditures on donations for years 1995-2018. In the first stage, we regress each district's log per-pupil elementary-secondary spending on constructed instruments as the budget shock and school finance reforms after 1995. The second stage regresses donations on predicated spending from the first stage. Expenditures and amount donated are in constant 2017 dollars. Panel A shows the results for the extensive margin (if a project receives any donation), while Panel B shows the intensive margin as the amount donated. Panel C presents the marginal effect of unconditional mean for the associated panels. Columns 1 and 2 show the results including no controls, while columns 3 and 4 include covariates as share of children in poverty, enrollment shares by race, and log number of teachers. All Columns include year fixed effects and district fixed effects.

Table B.6: Two-stage least squares estimates of the effects fundraising effort on donations

	First stage	Second stage	First stage	Second stage
	Log amount requested	Log amount donated	Log amount requested	Log amount donated
	(1)	(2)	(3)	(4)
Log amount requested		1.00		1.20
		(.058)		(.16)
Log amount of neighbors'	-.045		-.0083	
requests in t-1	(.0046)		(.0068)	
Log number of neighbors'	.16		-.051	
posted projects in t-1	(.012)		(.017)	
F-statistic for instruments	93.77	100.67	15.00	14.78
N	58465	58465	44540	44540
Year and District FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	No	No
County-Year FE	No	No	Yes	Yes

Standard errors in parentheses.

This table shows the impact of amount requested on amount donated for years 1995-2018. It shows the 2SLS estimates using instruments as amount requested and number of posted project by neighboring districts at t-1. Columns 1 and 2 include state-by-year fixed effects, while the other columns include county-by-year fixed effects Donation and request amounts are in constant 2017 dollars. All the columns include covariates as a share of children in poverty, enrollment shares by race, and log number of teachers.

APPENDIX C

FIGURES AND TABLES FOR SECTION FOUR

C.1 Tables

Table C.1: Effects of the Pill and abortion on years of education

	Full Sample		Blacks	
Contraception coding:	BHM (2012)	Myers (2017)	BHM (2012)	Myers (2017)
	(1)	(2)	(3)	(4)
Pill consent	0.3677	0.203	0.6627	0.3537
	(0.2157)	(0.1782)	(0.4279)	(0.5379)
Pill legal	0.2488	0.2282	0.0722	0.0288
	(0.1384)	(0.1548)	(0.324)	(0.4031)
Abortion consent	-0.3104	-0.3837	0.7801	0.6052
	(0.3482)	(0.318)	(0.5444)	(0.5454)
Abortion legal	-0.2276	-0.2724	1.4631	1.3402
	(0.2665)	(0.2704)	(0.349)	(0.344)
Observations	9390	9390	2095	2095

Notes: The table reports coefficients and robust to heteroskedasticity clustered at the state-level standard errors in parenthesis. The dependent variable is years of education up to a maximum of seventeen. Pill (abortion) consent measures the proportion of years from ages 18 to 20 in which the pill (abortion) was legally available and allowed minors to legally consent for them. Pill (abortion) legal and abortion legal measures the proportion of years from ages 18 to 20 in which the pill (abortion) was legally available but unmarried minors of these ages could not consent. See the text, including Footnote 2, for additional details on the models.

Table C.2: Effects of the Pill and abortion on working in a social security covered job

Legal coding:	BHM (2012)	Myers (2017)
	(1)	(2)
Pill consent x age 20-24	0.037	0.025
	(0.018)	(0.017)
Pill consent x age 25-29	0.076	0.055
	(0.019)	(0.024)
Pill consent x age 30-34	0.044	0.054
	(0.019)	(0.018)
Pill consent x age 35-39	0.017	0.027
	(0.015)	(0.019)
Pill consent x age 40-44	0.011	0.011
	(0.018)	(0.02)
Pill consent x age 45-49	-0.009	-0.003
	(0.02)	(0.017)
Pill consent x age 50-54	-0.043	-0.022
	(0.02)	(0.024)
Pill consent x age 55+	0.042	0.065
	(0.022)	(0.022)
EAA x age 20-24	0.053	0.042
	(0.018)	(0.017)
EAA x age 25-29	0.138	0.070
	(0.026)	(0.04)
EAA x age 30-34	0.056	0.049
	(0.033)	(0.036)
EAA x age 35-39	0.015	0.021
	(0.017)	(0.016)
EAA x age 40-44	-0.044	-0.022
	(0.043)	(0.027)
EAA x age 45-49	-0.098	-0.061
	(0.02)	(0.018)
EAA x age 50-54	-0.045	-0.036
	(0.036)	(0.023)
EAA x age 55+	0.044	0.079
	(0.068)	(0.052)
Observations	305877	305877

Notes: The table reports coefficients and standard errors robust to heteroskedasticity clustered at the state-level in parenthesis. The sample includes 7,608 unique women. The dependent variable is an indicator variable that takes value of one if the respondent showed zero earnings in the Social Security information. This information comes from the SSA supplement to the HRS. Pill consent is equal to one if a woman would have could legally consent for contraception before age 21 in her state of residence as a youth. EAA represents early access to abortionin the first column it is equal to one if a woman lived in an early-legalizing state before age 21 and in the second column and it is equal to one if a woman could legally consent to having an abortion before age 21. See the text, including Footnote 6, for additional details on the models.

Table C.3: Effects of the Pill and abortion on log of real hourly wage of the previous year

Legal coding:	BHM (2012)	Myers (2017)
	(1)	(2)
Pill consent x age 50-54	0.018	0.014
	(0.049)	(0.049)
Pill consent x age 55+	-0.029	-0.032
	(0.034)	(0.041)
EAA x ages 50-54	-0.0056	-0.031
	(0.083)	(0.105)
EAA x ages 55+	-0.077	-0.146
	(0.094)	(0.066)
Observations	24907	24907

Notes: The table reports coefficients and standard errors robust to heteroskedasticity clustered at the state-level in parenthesis. The sample includes 6,533 unique women. The dependent variable is the log the real hourly wage (2000s dollars) of the previous year. Observations with zero wages are excluded from these estimations. ELA is equal to one if a woman would have could legally consent for contraception before age 21 in her state of residence as a youth. EAA represents early access to abortionin the first column it is equal to one if a woman lived in an early-legalizing state before age 21 and in the second column and it is equal to one if a woman could legally consent to having an abortion before age 21. See the text, including Footnote 6, for additional details on the models.