Is Compassion a Good Career Move?

Nonprofit Earnings Differentials from Job Changes

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ABSTRACT

We explore the nonprofit earnings penalty. To separate the influence of demand and supply, we leverage workers who change employers in administrative tax data. The average nonprofit worker earns 5.5 percent less than the average for-profit worker. Supply-side factors (worker selection) contribute 80 percent of the nonprofit differential. The remaining 20 percent is from demand (a nonprofit penalty). Within-worker nonprofit variation generates several insights about the influence of nonprofits on the labor market. Nonprofits compress the wage distribution and reduce inequality among earners. Nonprofit penalties are much more pronounced in classic charities than in "commercial" nonprofits, which sometimes exhibit nonprofit premia.

> The overall economy has been expanding slowly, but at least one sector is vibrant: nonprofits, which have been growing at a breakneck pace. From 2001 to 2011, the number of nonprofits in the United States grew 25 percent while the number of for-profit businesses rose by half of 1 percent.
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> —Anna Bernasek, *New York Times* (Bernasek 2014)

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I. Introduction

Over the past half century, nonprofit organizations have proliferated in number, revenue, and employment (Leete 2001). In the past 20 years alone, the share of all workers employed by the nonprofit sector has increased by 40 percent (Friesenhahn 2016; Hirsh, MacPherson, and Preston 2018). The shift toward nonprofit employers may have consequences for the labor market if nonprofit firms affect the distribution of worker earnings (Rose-Ackerman 1996; Lakdawalla and Philipson 1999). On one hand, nonprofits may pay more because they must reinvest net earnings within the organization, encouraging the firm to distribute earnings internally in the form of higher wages (Pauly and Redisch 1973; Bishow and Monaco 2016); on the other hand, nonprofits can reduce wages if workers derive utility from participating in the mission of the nonprofit, eliciting a labor donation (Hansmann 1980; Preston 1989; Frank 1996). We evaluate these hypotheses by decomposing the nonprofit pay gap into demand- and supply-side factors.

Disentangling supply and demand in this setting is empirically challenging. Workers, for one thing, are not randomly assigned to employers. Even if random assignment were possible, the wage data used in previous studies are self-reported and contain considerable measurement error in earnings (Bound and Krueger 1991). Using administrative data, we demonstrate that these same records also have significant measurement error in nonprofit status, introducing bias that is hard to characterize, let alone quantify.

In this work, we address these challenges by bringing to bear full-population earnings and tax records from Florida. By focusing on workers who transition between for-profit and nonprofit employers, we account for unobserved, worker-specific traits to decouple the role of the supply- and demand-side factors driving nonprofit earning differences. The administrative data we use cover the full working population of Florida, and because the data are derived from tax records, there are strong incentives for wages and nonprofit status to be recorded accurately.

The data reveal that nonprofits pay 5.5 percent less, on average, with 80 percent of this differential explained by worker selection, and the remaining 20 explained by a nonprofit penalty. While the average nonprofit penalty is slight at just one percent, the penalty is much larger for high earners. The nonprofit penalty at the 95th percentile of the earnings distribution is 10 percent, ten times larger than average. This significant penalty may be the result of competitive labor-market forces in which nonprofit managers accept lower pay for greater influence over the direction of nonprofits (Glaeser 2002). Another possibility is the influence of regulations that sanction highly paid nonprofit managers and the boards that offer compensation eventually deemed "unreasonable."

Not all workers suffer a nonprofit penalty. Nonprofits pay a premium to workers in the bottom 25 percent of the earnings distribution, suggesting that nonprofits compress wages. If one applied the earnings compression we observe in nonprofits to the for-profit distribution, it would reduce income inequality, as measured by Gini coefficients, by 60 percent.

Several papers have estimated nonprofit penalties for individual industries (Borjas, Frech, and Ginsburg 1983; Weisbrod 1983; Goddeeris 1988; Preston 1988; Holtmann and Idson 1993; Roomkin and Weisbrod 1999; Leete 2001; Mocan and Tekin 2003;

Hirsch, Macpherson, and Preston 2018). We shed light on industry-specific nonprofit penalties, first, by presenting visual evidence that features the earning dynamics of workers transitioning between for-profit and nonprofit work in each industry. The estimates from this event-study approach demonstrate that the nonprofit penalty varies significantly from industry to industry. Workers face the most significant penalties when working in classic charitable organizations like legal aid (−13 percent) and religious employers (−10 percent). A few industries exhibit no nonprofit differential, including hospitals and nursing homes. In some industries, workers earn *more* in a nonprofit than in a for-profit, including in family services (3 percent), outpatient healthcare (4 percent), and childcare centers (5 percent), consistent with evidence suggesting nonprofit premia in some settings (Leete 2001; Bishow and Monaco 2016). We explore several industry-level explanations for varying penalties. Nonprofit penalties and premia are most strongly related to differences in worker fixed effects across nonprofit and for-profit sectors within industry, suggesting again the egalitarian influence of nonprofits on the distribution of wages. We find no evidence that the nonprofit wage differences across industries are related to differences in the competitive environment, employee misattribution of nonprofit status, or industry-specific differences in nonprofit utility.

It's useful to return to the broad misclassification of nonprofits in survey records to notice what it implies. That many employees do not know the nonprofit status of their employer seems to undermine a primary explanation for nonprofit existence: nonprofit legal status allows entrepreneurs to commit to providing quality and, thereby, gain market share. But if employees don't know that a firm is nonprofit, it's hard to imagine customers do. This suggests that nonprofit status is an information signal usually intended for deliberately informed donors, rather than paying customers or employees.

This work contributes to a long literature investigating the economic behavior of nonprofits (Arrow 1963; Newhouse 1970; Feldstein 1971; Baumol and Bowen 1965; Horwitz and Nichols 2007). We show that the survey data used to study this question in previous research contain significant measurement error in nonprofit designation (for example, at least half as many workers misclassify their status as there are nonprofit workers, greater than 4 percent of all respondents). This study is the first to resolve this issue using full-population, administrative panel data to account for individual worker differences and illuminate the magnitude of the nonprofit wage penalty in various settings. The size and scope of the data allow us to leverage a design-based approach to answer the question while providing clear visual evidence in event-study figures.

Our work compares most closely to Ruhm and Borkoski (2003) and later Hirsh, MacPherson, and Preston (2018), who use the Outgoing Rotation Group of the Current Population Survey to study workers who transition to or from nonprofit settings in survey data providing two observations, one year apart. Our primary contribution relative to these studies is that we (i) leverage administrative tax data, significantly reducing the scope for mismeasurement in both earnings and nonprofit status; (ii) study long panels of individuals changing jobs to carefully account for job-change dynamics; and (iii) harness the experience of several tens of thousands of workers who transitioned between nonprofit and for-profit employment to provide statistical clarity.

II. Background

To avoid a contradiction in terms, what is called "profit" in a typical setting is called "net earnings" in a nonprofit organization (revenues less cost). The essential characteristic of a nonprofit is that the organization is barred from distributing earnings to owners or managers, an institutional rule described by economists as the "nondistribution constraint" (Hansmann 1980). The primary economic rationale for the institutional feature is to mitigate concerns arising from information asymmetry. Should Jane donate money to charity, she cannot easily verify whether promised services were furnished to the indigent. If the charity were organized as a for-profit firm, its owner would be tempted to withhold promised services for personal gain. The nondistribution constraint blunts this incentive, allowing Jane to have greater confidence that her donation reaches the intended beneficiary. Similar information asymmetries exist in personal services (like assisted-living facilities, hospitals, daycares, and schools), in which the quality of care cannot easily be assessed by the patron. In many cases, the service recipient is unhelpful even in evaluating quality since the beneficiaries may be sedated, disabled, a child, or otherwise unable to determine the quality of care due to its technical nature, as is often the case when consumers seek medical treatment.

Jane can have confidence that the penalties imposed for violating the nondistributional constraint are quite exacting. Board members that approve a compensation package eventually deemed "unreasonable"[1] by the IRS are required to pay a fine equal to 10 percent of the overage (Internal Revenue Service 2016). In addition, the (overpaid) manager must repay the overage to the nonprofit, including interest, in addition to paying a 25 percent excise tax on the overpayment (Internal Revenue Service 2016). Under the uncertainty of this somewhat subjective rule, board members and managers may agree to lower levels of compensation to avoid censure and fine and potentially find nonpecuniary avenues to transfer utility. As an aside, this is one possible explanation for the sizeable nonprofit wage penalty we discover among the top percentiles of the wage distribution.

In exchange for the nondistribution constraint, the U.S. government grants nonprofit organizations an exemption from federal income taxation under the U.S. Internal Revenue Code section 501(c). Entrepreneurs can incorporate their organizations as nonprofits if they fit into one of several categories: traditional charities, religious communities, scientific organizations, education providers, and organizations that work to prevent child cruelty (section 501(c)3). Donations to these groups are tax deductible.[2] Nonprofit employees pay individual income taxes on their earnings, as they would if they were employed in for-profit institutions. Nonprofit employers are liable for payroll taxes that fund social insurance programs, but they do not pay federal or state income tax and do not pay property taxes—this is true in all 50 states (Lindblad 2019). In our setting in Florida, nonprofits are also exempt from paying sales and use taxes, but this is not the case everywhere.

1. The classification of a compensation package as "unreasonable" is somewhat subjective and determined by the IRS.

2. Hospitals often enjoy charitable/nonprofit status. This is a holdover from an era in which hospitals were charities that provided health services to the indigent (Hansmann 1980).

III. Data

A. Measurement Error of Nonprofit Status in Survey Data

Accurately gauging nonprofit differentials depends on reliable measures of nonprofit status. It is well known that survey data contain considerable measurement error in self-reported earnings arising from rounding, seam bias, imperfect memories, and intentional misrepresentation (Bound and Krueger 1991), in addition to selective reporting and top-coded earnings (Hirsh, MacPherson, and Preston 2018).[3] What has been unexplored is whether respondents accurately identify the nonprofit status of their employer when completing surveys like the Current Population Survey (CPS) or the American Community Survey (ACS). On one hand, an employer's nonprofit status is binary and stable, so it seems reasonable that employees may be able to reliably recall nonprofit status. On the other, employers may have little reason to communicate their tax status with workers.

To assess the prevalence of measurement error, we compare the nonprofit attribution in the ACS coverage of Florida with administrative employment records covering the same state. We reveal high rates of misidentification. In Table 1, we compare the nonprofit employment share in survey data to the nonprofit employment share in administrative records for several industries, focusing on those that have large nonprofit representation. In the administrative data, 72 percent of healthcare workers are employed at a nonprofit hospital; in survey records, however, only 43 percent of workers report working for a nonprofit, implying a misidentification rate of at least 40 percent. In the education sector, employees tend to make the opposite error: more than 12 percent of for-profit employees incorrectly respond that they work for nonprofits. These misidentification rates could be far higher since we are only able to ascertain *net* mismeasurement, not gross. For instance, should two individuals make opposite errors identifying their employers' tax status, we will detect no (net) measurement error, despite the fact that the nonprofit status of neither is correct.

The measurement problem poses difficulty for consistent estimation from survey records. From our administrative records, we can calculate a lower-bound for measurement error by summing the net error in each industry. We find that at least half as many workers as there are nonprofit employees misidentify their nonprofit status in the ACS over this period.

Measurement error of this magnitude, in the primary independent variable of interest, has likely led to significant statistical bias in estimates (Card 1996), a challenge addressed by the administrative tax records we use. We assess the potential bias in Online Appendix A and find that the estimates resulting from mismeasurement could either attenuate or exaggerate nonprofit differentials depending on the correlations between misreporting and income.

A careful reader may notice that this broad misidentification of nonprofit employment also poses a challenge to one compelling economic rationale for nonprofit existence. Entrepreneurs elect to originate nonprofits rather than for-profits to commit to—and

3. For instance, about 30 percent of working respondents in the CPS do not report their earnings (Hirsh, MacPhereson, and Preston 2018).

Table 1
Measurement Error in Nonprofit Status

	Within Industry Percent Nonprofit			
	ACS (1)	Admin (2)	Percentage Point Error (3)	% of Total Nonprofit Workers (4)
Hospitals	43	72	–29	40
Educational services	43	49	–6	19
Ambulatory healthcare services	12	14	–2	10
Social assistance	49	55	–6	8
Nursing and residential care facilities	25	27	–2	7
Religious and civic organizations	100	42	58	4
Recreation industries	5	12	–7	2
Credit and banking	15	7	7	2
Scientific and technical services	2	2	0	2
Utilities	8	12	–4	1

Notes: The first column is the percentage of reported nonprofit workers in each industry from the ACS Florida sample in 2010. The second column is the percentage of recorded nonprofit workers from the universe of Florida's UI records in 2010. The third column is the percentage point difference between Columns 1 and 2. The fourth column is the industry's share of all nonprofit workers according to UI records.

signal—quality in markets where quality is important but difficult for consumers to observe (Arrow 1963; Nelson and Krashinsky 1973; Hansmann 1980). At first appearance, our finding that many employees do not know the nonprofit status of their employer seems to undermine this explanation. After all, it is unlikely that customers would be better informed regarding a firm's nonprofit status than employees, since any information available to customers would, by the same avenues, also be available to workers. This suggests that nonprofit status is an information signal often intended for deliberately informed donors, rather than paying customers or employees.

B. Data Construction

We obtained employer–employee matched administrative data for the full population of workers and employers in Florida for 2003–2012, and we link two large registers using identification numbers for workers and firms. The data include total earnings at each job in every quarter for the universe of legitimate workers.[4] Because the administrative

4. The data cover all businesses, nonprofit organizations, state or local government employers, and Indian tribal units that either have a yearly payroll exceeding $1,500 or have at least one employee working at least a portion of one day during any 20 weeks of the year (Florida 2012).

earnings records are based on firms' reports used to calculate unemployment insurance tax liabilities and benefits, they are subject to audit and are thus unlikely to contain significant measurement problems. Moreover, whereas survey data give rise to measurement error in the primary independent variable of interest, the records we use to code "nonprofit" capture the firm's official legal status.[5,6] The firm identification number in the wage records allows us to link worker wages to firm information, including administrative records of their nonprofit status and detailed industry codes (NAICS).

Our main analysis centers on the earning dynamics of those who transition from for-profit to nonprofit work, while accounting for the wage evolution common to workers moving between for-profit employment. To focus the analysis on relevant individuals, we limit the data to those earnings observations in which employees were working for for-profits (those with the legal classification of c-corporation or s-corporation in the employer tax data) and those working for nonprofits (those classified as not-for-profits in the tax data). To generate a panel of worker wages for each individual, we include only the highest wage record for each worker in a given quarter when a worker has multiple jobs at one time. We drop wage records in which the employee earns less than what they would earn if they were employed full-time at the minimum wage to concentrate the analysis on similar employment arrangements, similar to Song et al. (2019). Some workers appear to change jobs frequently. We remove work spells with fewer than six quarters, limiting the analysis to those who have at least a year and a half of work experience both before and after a job-change "event."[7] Several workers present more than one event. To leverage all the available variation, we stack events so that a given worker's wage evolution at a given employer may function as the pre-job-change earnings in one event and the post-job-change earnings in a separate event.

Once the records are narrowed to workers who change jobs, with at least a year and a half of tenure before and after a move, the analytic sample includes 92,429 transitions to nonprofits from for-profit firms and 66,928 transitions the other way, with 18,838 individuals transitioning in both directions at different times.[8] In total, we leverage the wage dynamics of 178,195 nonprofit-to-for-profit job transitions. In the primary specification, we use 1,596,220 within-sector transitions to control for the wage dynamics general to job changes. In Table 2, we present summary statistics for average quarterly earnings in each industry by nonprofit status.

Although these data are complete and detailed, they have important limitations that bear mention. First, the analyst has no direct information with which to compare the type or difficulty of work required in each employment setting (such as hours, work requirements, or nonwage benefits), potentially missing important nonmonetary compensation

5. Similarly, governments have a strong incentive to make sure that firms do not erroneously report their tax-exempt status.

6. See Salamon and Sokolowski (2005) for more information on how states collect wage records.

7. In Online Appendix Figure 2, we present estimates while varying the data restrictions. The results are quite robust, and the ratio of the nonprofit penalty to the nonprofit differential is constant.

8. The results are robust to other exclusion thresholds, as shown in Online Appendix Figure 2. Notice that two-thirds (66 percent) of transitions between the sectors are from for-profits into nonprofits, with only one-third flowing the other direction. This suggests that nonprofits are preferred by the marginal worker, potentially for noncompensation factors that make them more attractive, including less demanding, more laid-back work environments.

Table 2
Industry Composition & Nonprofit Earnings Differences

Industry	Average Quarterly Earnings ($) Overall (1)	For-Profits (2)	Nonprofits (3)	Nonprofit Differential (4)	Share Nonprofit (5)
All industries	11,394	11,476	10,633	0.07	0.10
Health and human services					
Outpatient healthcare (621)	13,103	13,449	11,739	0.13	0.20
Doctor's offices (621,111)	16,256	16,086	17,618	–0.10	0.11
Hospitals (622)	11,352	11,206	11,413	–0.02	0.71
Nursing facilities (623)	7,824	7,740	7,900	–0.02	0.53
Social services (624)	7,321	6,969	7,505	–0.08	0.66
Childcare (62,441)	5,873	5,438	6,652	–0.22	0.36
Education (611)	10,522	10,380	10,646	–0.03	0.53
Finance and management					
Law offices (54,111)	16,173	16,235	9,781	0.40	0.01
Banking & credit (522)	13,225	13,443	8,751	0.35	0.05
Investments (523)	25,460	24,567	40,427	–0.65	0.06
Insurance (524)	12,936	12,938	11,058	0.15	0.00
Administration (561)	9,995	9,993	10,853	–0.09	0.00
Utilities (221)	18,545	18,837	11,164	0.41	0.04
Classic charities					
Religious organizations (8,131)	8,541	7,939	8,659	–0.09	0.84
Grant-making foundations (8,132)	10,194	11,084	10,157	0.08	0.96
Social advocacy (8,133)	8,605	9,687	8,403	0.13	0.84
Civic organizations (8,134)	9,220	9,766	8,998	0.08	0.71

Notes: Summary statistics are calculated using the sample which includes all workers for the years 2003–2012. No sample restrictions are imposed. "Share nonprofit" indicates the share of industry workers which are employed by a nonprofit firm.

differences.[9] Second, the data do not allow the researcher to see whether job changes coincide with shocks to human capital, for instance the onset of a debilitating medical condition or the occurrence of a life-changing accident. We expect these events to be uncommon and second order.[10]

9. To get a measure of job-type utility, we measure how long workers tend to stay in a given job category. Whether duration depends primarily on labor supply (worker utility) or labor demand (job security), the measure reflects an important indicator of how attractive the job is.
10. Another limitation is that the data do not include demographic information on workers, so we cannot evaluate sorting or penalties by gender.

IV. Empirical Methodology and Results

The ideal design to measure the nonprofit earnings penalty would be to randomize workers to sectors, for-profit or nonprofit. Absent such an experiment, researchers have sought to compare workers with similar observable characteristics across sectors (Preston 1989; Leete 2001). These cross-sectional designs can provide insight but are unable to fully resolve the underlying concern that nonprofit workers may be different in unobserved dimensions, principally those related to productivity. To address this fundamental issue, we adopt two primary strategies. The simplest is a within-worker comparison in which we compare a given worker's earnings at a nonprofit to their earnings at a for-profit firm using worker-level fixed effects. The second follows a generalized difference-in-difference (DiD) approach that explicitly adjusts for the earnings dynamics of job changes.

A. Visual Evidence from Event Studies

In addition to contributing within-worker variation and administrative data, we shed new light by presenting visual evidence of the nonprofit penalty using event studies of workers who changed jobs. In each job-change event in the data, we denote $t=0$ the quarter in which the individual begins their new job and index all other quarters relative to it. In the baseline specification, we include six quarters leading up to the job change and 12 quarters after the event.[11] We denote W^q_{iste} the log earnings of individual i, in year–quarter s, at event time t, in the dynamics of event type q, which describes the type of employment change. There are four event types possible: for-profit-to-for-profit transitions (P→P), for-profit-to-nonprofit transitions (P→NP), nonprofit-to-nonprofit transitions (NP→NP), or nonprofit-to-for-profit transitions (NP→P). The primary event studies we present compare for-profit-to-nonprofit transitions (P→NP) with for-profit-to-for-profit ones (P→P) because they provide many treatment and control events (relatively few events originate from nonprofits). We also present the event-study figure for transitions originating in nonprofits in Online Appendix Figure 1. We run the following regression separately for each event type:

$$(1) \quad W^q_{iste} = \sum_{j \neq -1} \alpha_j \times 1[j=t] + \sum_y \beta_y \times 1[y=s] + \gamma_{i(e)} + \varepsilon^q_{iste}$$

where we include a full set of event–time dummies (α), year–quarter dummies (β), and individual fixed effects (γ) that account for the average earnings of an individual. In some specifications, we include individual–event specific fixed effects that account for the worker's earnings around the time of a given event and thus controls more flexibly for evolving human capital. We omit the event–time dummy at $t=-1$, so the event–time coefficients measure earnings relative to the quarter just before a job change. By including year–quarter dummies, we control nonparametrically for time trends including those arising from the business cycle. We can identify each dummy set because there is variation in event time driven by the variation in time when a given worker changes jobs. Throughout the analysis, standard errors are clustered at the worker level, which we view as suitably conservative.

11. Because the data are selected so as to require at least six continuous quarters of employment, the panel is not fully balanced after Quarter 6 after the job change.

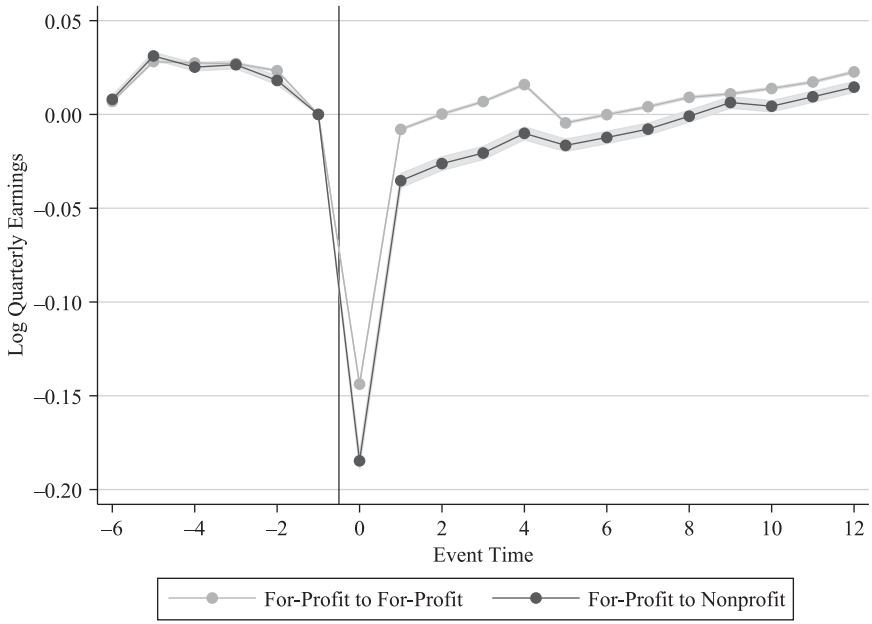

Figure 1

Event Study—Job Changes from For-Profit Employers

Notes: We plot the event–time dummies for workers who changed jobs between 2003 and 2012 and held the previous and new job for at least six quarters (18 months). After $t = 6$, the results derive from an unbalanced panel. Controls include a full set of event–time dummies, year–quarter dummies, and event-specific dummies, a refinement of worker fix effects.

We plot the resulting α values from these models to illustrate the dynamics of job changes and present visually how nonprofit compensation differs, conditional on worker unobservables via fixed effects. In Figure 1, we see the earnings evolution of employees who started in for-profit firms and changed jobs. The light gray evolution reflects the earning dynamics of workers transitioning from for-profit firms to another for-profit firm. This gray line provides a baseline for how we might expect earnings to evolve for workers who change jobs, but not sectors. Workers earn slightly less in the quarter they depart and the quarter they begin a new job but maintain relatively constant wages before and after the job change. The nearly 20 percent dip in earnings in the first quarter of the new job is an artifact of the quarterly nature of the data. Unless all workers begin their employment on the first day of a quarter, quarterly earnings records will reveal lower earnings at a new job since the worker registered earnings for only a part of the quarter. The fact that earnings do not increase substantially over time is the result of the control strategy in which we account for year–quarter specific fixed effects that absorb the typical time-driven increases in earnings workers experience. The event-study figure suggests nonprofits pay a modest earnings penalty that attenuates over time. Over the

three-year post period, the average nonprofit penalty is 0.9 percent.[12] Visually, workers entering nonprofits converge to the earnings of those entering for-profits. In order to compare like estimates, throughout the empirical exercises we restrict the sample to those observations used in the event studies.

B. Estimating the Nonprofit Penalty

One concern with comparing pre- and post-change earnings is that job changes may be related to changes in roles or status that could bias estimates if, for instance, job changes tend to occur as the result of layoffs or promotion. To address this issue, we adopt a generalized difference-in-difference approach that leverages the sharp changes in sector that take place when workers leave the for-profit sector for nonprofit employment, while controlling for the dynamics that exist for job-changes within the for-profit sector, essentially adapting the event studies presented in the previous subsection to produce estimates of the nonprofit penalty. This method compares the dynamics of workers transitioning to nonprofits to the natural evolution of earnings as workers change jobs within the for-profit sector. Although job changes are not exogenous, the job-change event generates a sharp change in employer that is arguably orthogonal to unobserved determinants of wage outcomes (experience, health, ability, etc.), which likely evolve smoothly over time.

To implement the generalized DiD estimate, we denote $t=0$ the quarter in which the individual begins their new job and index all other quarters relative to it. In the baseline specification, we concentrate on quarters close to the event, including six quarters leading up to the job change and 12 quarters after the event. Denoting W_{iste} the log-earnings of an individual in year–quarter s, at event time t, as part of event e. The primary estimates we present make within-worker comparisons among those who shift between sectors while using workers who transitioned between for-profit employers as a comparison:

$$(2) \quad W_{iste} = \rho NP_{iste} + \sum_{j \neq -1} \alpha_j \times 1[j=t] + \sum_y \beta_y \times 1[y=s] + \Gamma \mathbf{X} + \gamma_{i(e)} + \varepsilon_{iste}$$

We include a full set of event–time dummies (α), year–quarter dummies (β), and, importantly, personal dummies (γ) or finer dummies designating each individual event a person engages.[13] The coefficient on NP, ρ, captures the average nonprofit penalty. The vector $\mathbf{X}$ represents various controls; in the preferred specification, we include county fixed effects since nonprofit employers tend to locate in counties with higher earnings. In the main results, we present a specification that includes industry fixed effects to evaluate whether the nonprofit penalty appears primarily within or across industries.

Table 3 presents the main results. In the cross-section, nonprofit workers earn 6.9 percent less than for-profit workers employed at the same time. Including county-level controls (that is, county fixed effects) reduces this cross-sectional difference by a

12. We also present the event study using workers who engaged in an NP → P transition (using other workers transitioning from NP employment as a control group), generating a figure corollary to Figure 1 in Online Appendix Figure 1. The DiD estimate from the figure implies a similar nonprofit penalty of 1.36 percent.

13. Note that event fixed effects are a refinement of individual fix effects that allow for an individual to be earning different amounts at different points in their career.

Table 3
Nonprofit Differential Estimates

	Log Earnings					
	Cross-Sectional Difference			Within-Worker Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
Nonprofit	–0.069***	–0.053***	–0.055***	–0.012***	–0.010***	–0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year–quarter FE	X	X	X	X	X	X
County FE		X	X	X	X	X
Event–time FE			X	X	X	X
Worker FE				X		
Event FE					X	X
Industry FE						X
R^2	0.01	0.02	0.03	0.81	0.82	0.82
Observations	26,919,859	26,919,859	26,919,859	26,919,859	26,919,859	26,919,859

Notes: Table is based on the estimation of Equation 2 where the dependent variable is log quarterly earnings. All sample restrictions described in Section III are imposed. Columns 1–3 provide estimates without controlling for person fixed effects (FE). Columns 4 includes a worker fixed effect, while Columns 5 and 6 include fixed effects for events, allowing a worker with multiple events a separate fixed effects for each event. Industries are grouped by three-digit NAICS codes. This table leverages 1,336,205 unique workers and 1,568,483 unique job-change events. Significance: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

quarter. When we include worker fixed effects, we find that 78 percent of the cross-sectional difference is explained by worker differences (compare Columns 3 and 4).[14] Event dummies attenuate the difference slightly more than worker effects, suggesting that cross-sectional nonprofit differences are, in part, a product of life-cycle earnings differences (compare Columns 4 and 5). Finally, when we include industry fixed effects (three-digit NAICS classifiers), the nonprofit penalty attenuates little, just 10 percent, suggesting that the remaining nonprofit penalty exists primarily within industry.

The results tend to suggest smaller cross-sectional differences in compensation between nonprofits and for-profits than those registered in past studies. We register a 5.5 percent cross-sectional difference, whereas previous studies suggest somewhat larger gaps. Preston (1989) reports differences ranging from 0 to 32 percent, and Leete (2001)

14. The fraction explained by selection depends somewhat on the order in which other controls are added to the specification. To assess the role of other covariates, we reestimate the nonprofit gap (nonprofit differences without worker fix effects) and the nonprofit penalty (coefficient on nonprofit once worker fix effects are added to the base specification) with every combination of controls. The ratio of these two estimates form the share of the nonprofit gap attributable to a demand-side nonprofit penalty. One minus this number is that attributable to selection. We ran every combination of controls with and without worker fixed effects to calculate this share. The estimates range is stable at 74–83 percent of the initial penalty explained by worker fixed effects. The average of these calculations across specifications is 79.3 percent.

reports differences between 6 and 15 percent. The most similar analysis to ours, Ruhm and Borkoski (2003), finds an 11.7 percent gap. The fact that we uncover smaller cross-sectional differences could be a by-product of measurement error in nonprofit identification in past studies if, for instance, workers in low-earning jobs were more likely to believe they worked in a nonprofit either because they worked in charitable (low-paying) settings or if low-paying employers lead their staff to believe the operation is nonprofit. Leete (2001) finds no nonprofit penalty when controlling for observable characteristics (nonprofit workers earned 0.1 percent less, and the confidence intervals ruled out penalties larger than 0.3 percent). When using within-worker transitions, Ruhm and Borkowski (2003) find a larger penalty of 1.0 percent where the standard errors are nearly as large, creating a wide range of plausible penalties and premia. We find a similar point estimate to Ruhm and Borkowski, and—thanks to the large tax data available to us—the standard errors are tight, providing quite a precise estimate. Ruhm and Borkowski's confidence intervals spanned from −3.1 to 1.1, while our estimates rule out more than 90 percent of that interval, providing significant statistical clarity.

C. The Influence of Nonprofits on the Income Distribution

In addition to seeing how nonprofit employment affects earnings on average, we explore how nonprofits shape the distribution of earnings by studying heterogeneity in nonprofit penalties in various quantiles of the income distribution. When benefits or penalties are associated with firm characteristics, there is often a question of which workers are receiving such benefits (Card, Cardoso, and Kline 2016). We implement Specification 2 for 20 pre-event income ventiles to study how the nonprofit penalty varies along the income distribution.[15] We visualize the results in Figure 2 by plotting the estimated wage penalties at each percentile of the pre-event income distribution τ. Workers in the lowest pre-event earnings ventiles receive a 3 percent earnings premium in nonprofits. The small positive premium declines along the pre-event (that is, before the job change) income distribution, becoming negligible at the 30th percentile. A nonprofit penalty emerges at the 40th percentile and hovers near 4 percent through the 90th percentile. At the upper reaches of the income distribution, workers pay a significantly larger penalty when working in nonprofits. At the 95th percentile, for instance, the typical earnings penalty is 10 percent, an order of magnitude more than the average penalty. At the 99th percentile, the nonprofit penalty is large at 7.5 percent. In the for-profit distribution, the top 1 percent of earners earn 10.4 percent of the income. When we apply the distribution of the nonprofit penalty to the earnings distribution of for-profit workers, we find it shrinks the Gini coefficient by 60 percent. This suggests nonprofits have an egalitarian influence on the income distribution by compressing wages, especially at the high end.

Why does the nonprofit penalty take this shape along the income distribution? Especially, why do high earners face such significant penalties? One possibility is that nonprofit managers have significant discretion over the focus and direction of their organizations, which may be a valuable form of nonmonetary compensation, consistent with Glaeser (2002). Related is a second explanation in which the IRS's oversight of

15. To determine pre-treatment wage groups, we residualize log yearly wage from the pre-treatment year on year and industry fixed effects. We use this residualized log yearly wage to partition workers into pre-treatment earning bins.

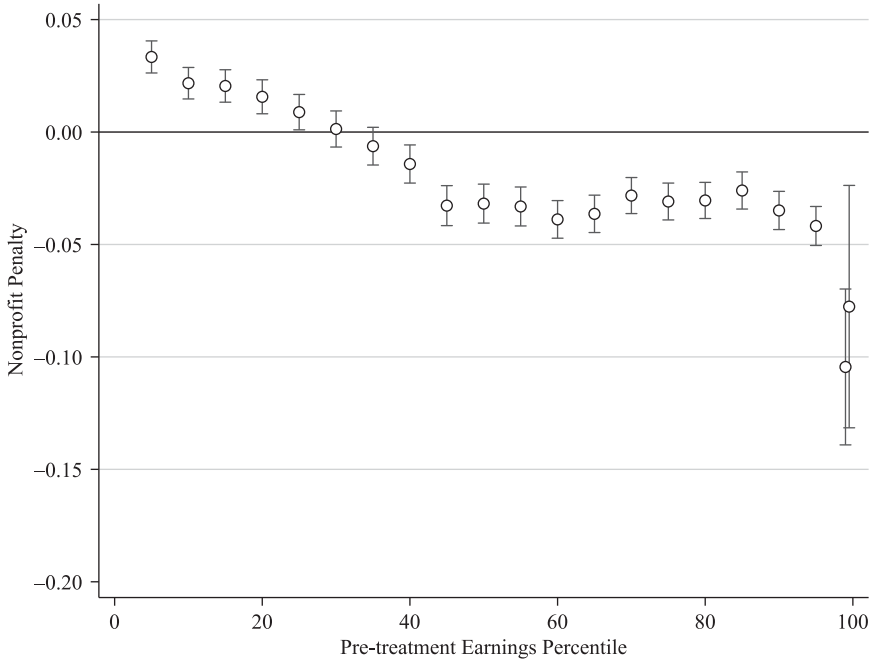

Figure 2

Nonprofit Influence on the Income Distribution

Notes: Figure shows the coefficients on the nonprofit indicator from Equation 2 for several pre-treatment income groups. To determine pre-treatment wage groups, we residualize log quarterly earnings from the pre-treatment year on event and industry fixed effects. We use this residualized log quarterly earnings to partition workers into pre-treatment wage quantiles. The data are from administrative unemployment insurance records for the universe of Florida workers, 2003–2012.

management compensation in nonprofits may discourage nonprofits from making generous offers to managers. If so, the market could plausibly clear when taking into account other dimensions, including discretion in hiring or new initiatives. Because nonprofits cannot reward owners or managers with net earnings, the presence of lower compensation at the top of the distribution is consistent with the more-than-binding influence of the nondistribution constraint.

D. Industry-Specific Event Studies

Nonprofits encompass both traditional charities (for example, churches and civic organizations) as well as commercial enterprises (for example, insurance companies, health providers, and broadcasting networks). These diverse types of employers may pay differently by nonprofit status. Several prior studies estimate a nonprofit penalty for a particular industry. We contribute to these industry-specific studies by plotting the earnings evolution of workers originating in the same sector of the same industry who

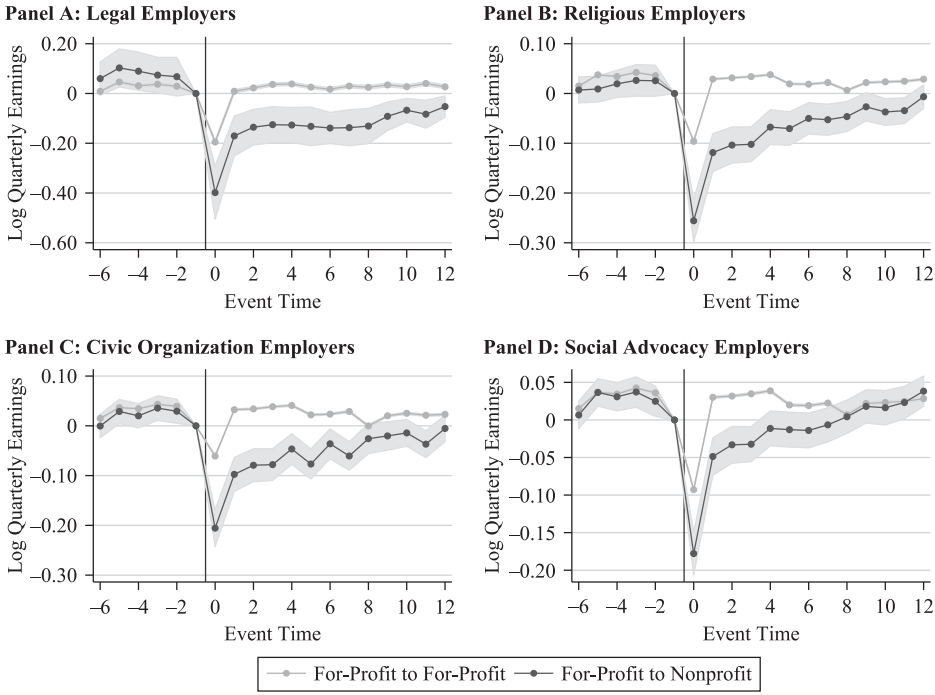

Figure 3

Classic Nonprofit Event-Study Figures

Notes: Figure shows the coefficients α_j^q in Equation 1 for two event types: moves from a for-profit to a nonprofit (which we refer to as the treatment group) and moves from a for-profit to another for-profit firm (the control group) for various three-digit NAICS industries. The dependent variable is log quarterly earnings. The event–time dummy at $t=-1$ is omitted. To generate the control event for religious, civic organization, and social advocacy industries, we identify the three three-digit NAICS codes that most commonly transition to that particular nonprofit type and identify workers transitioning between for-profit jobs in those industries. The gray shaded areas bounding each line represent the 95 percent confidence interval.

migrated into different sectors, for example, comparing how the earnings of for-profit workers changed when transitioning jobs to another for-profit employer as compared to those transitioning to a nonprofit employer in the same industry.

First, we walk through a representative figure visualizing the nonprofit earnings penalty in the legal industry (Figure 3). Those transitioning to another for-profit legal employer earn slightly more relative to the last quarter of employment in their former job, capturing the dynamics typical of changing jobs. In contrast, workers transitioning from a for-profit legal employer to a nonprofit one experience a significant drop in earnings that persists over the observation period. Before the event, earnings trends between the two groups are parallel and essentially identical, suggesting similar underlying dynamics in the two groups of workers. The relative fall of those transitioning to nonprofit work reflects the fact that among those transitioning from for-profit legal work to other legal firms, nonprofit workers bear a 17.1 percent penalty ($p<0.001$)

compared to their for-profit alternative, similar to that found by Weisbrod (1983), who estimated a 20 percent nonprofit penalty in law.[16] Religious employers and other classic charities do not have a significant for-profit share. To generate the control event for each of the other classic charities, we identify the three three-digit NAICS codes of workers that most commonly transition to that particular nonprofit type and identify workers transitioning between for-profit jobs in those industries. We find significant, visible declines in earnings for those transitioning to religious employers, civic organizations, and social advocacy groups compared to those transitioning to other for-profit employers (Figure 3). We observe convergence over time between the nonprofit earnings profile and that in for-profits. We do not observe this convergence in commercial nonprofits. It may be that classic charities have additional flexibility with workers to pay them less than market rates while vetting, training, or acculturing them. It may also be that new workers in charities pay a penalty, but established workers receive market rates for their service as the worker becomes core to the function of the organization. For workers originating from for-profit employers, the earnings paths of those moving to for-profits and nonprofits are predicted to converge midway through the first quarter of the fourth year after the transition.

The nonprofit penalty is not as large in several other industries. In Figure 4, we present a parallel figure for workers transitioning from for-profit education firms to either another for-profit educator or a nonprofit educator. In this setting, pre-event earnings trend in parallel and are overlapping. After the job change, workers migrating to nonprofits appear to have no systematic wage disadvantage when compared to peers moving to for-profits. In some industries, like outpatient healthcare (also in Figure 4), we observe that workers transitioning to nonprofit employers enjoy a significant wage advantage over their for-profit counterparts. Is this apparent nonprofit premium driven by workers shifting toward subindustries that are higher paid? To explore this, we show the event study for job changes from for-profit doctor's offices to nonprofit doctor's offices, a subset of outpatient healthcare. Here, we find similar nonprofit premia, suggesting a nonprofit advantage not driven by subindustry sorting. We include corollary figures for several other industries in the Online Appendix, including for hospitals, utilities, insurers, banking and credit, family services, and investment firms (Online Appendix Figures 3–8).

E. Nonprofit Penalties over the Business Cycle

Though nonprofits cannot distribute net earnings, they need not spend down their surplus each year, which may help them weather downturns with a cushion of savings stored during expansionary years. We leverage within-worker variation to study how the nonprofit penalty varies over the business cycle. To estimate nonprofit penalties over the business cycle, we implement the following specification.

$$W_{ste} = \sum_{j=2003}^{2012} \theta_j \times NP_{ste} \times 1[\text{year} = \text{j}] + \sum_y \beta_y \times 1[y = s] + \Gamma \mathbf{X} + \gamma_e + \varepsilon_{ste}$$

16. Our estimate adapts the event-study model presented above by regressing log wages on an indicator for nonprofit worker status, year–quarter dummies, event–time dummies, and event fixed effects among those who worked in the legal firms before and after a job-change event among those originating from for-profit firms in their event.

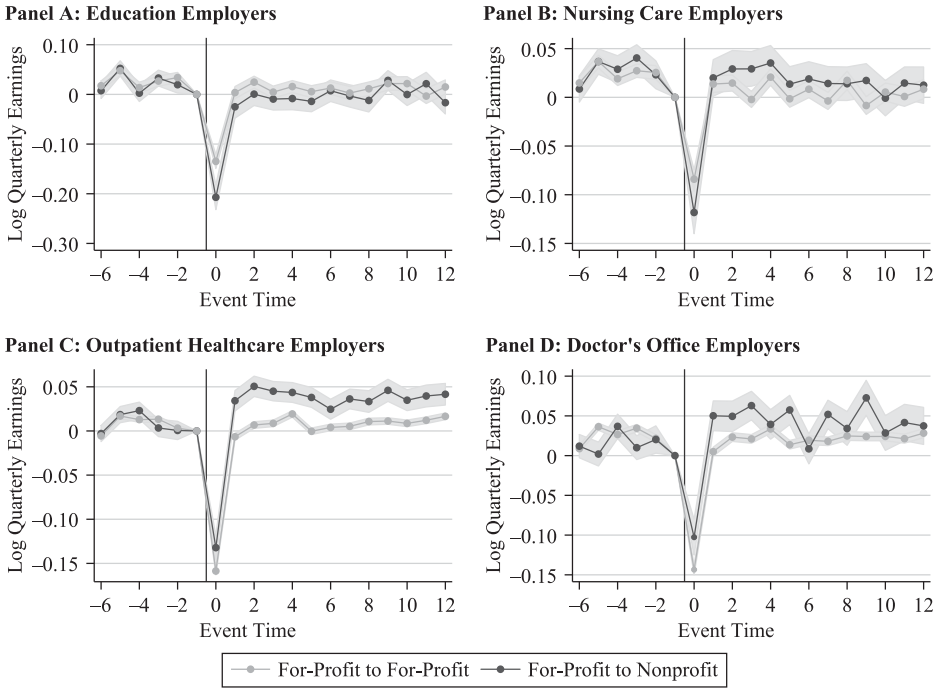

Figure 4

Commercial Nonprofit Event-Study Figures

Notes: Figure shows the coefficients α_j^g in Equation 1 for two event types: moves from a for-profit to a nonprofit (which we refer to as the treatment group) and moves from a for-profit to another for-profit firm (the control group) for various industries. The dependent variable is log quarterly earnings. The event–time dummy at $t = -1$ is omitted. All industries are determined by three-digit NAICS codes, except for doctors' offices, which corresponds to a six-digit NAICS code. The gray shaded areas bounding each line represent the 95 percent confidence interval.

The θ values are the coefficients of interest on the interaction of the nonprofit indicator with a year indicator. We include year–quarter dummies (β), individual–event dummies (γ), and county controls. Including industry fixed effects yields similar results. The nonprofit gap is estimated similarly but lacks individual or event fix effects, and the coefficients for both are plotted in Figure 5. Before the recession, the nonprofit penalty was similar to the total differential suggesting little difference in worker fixed effects. In 2005, for instance, the cross-sectional wage difference was 8 percent, and the nonprofit penalty was 6.5 percent. During the recession, the nonprofit gap fell slightly, while the nonprofit penalty fell to zero by 2008 and became a 2–3 percent wage premium for 2009–2012, potentially the result of nonprofit cash stores.

The fact that the nonprofit differential remained negative, while the nonprofit penalty shrank and became a premium during the recession suggests that nonprofits increased wages relative to for-profit firms, while the composition of nonprofit workers

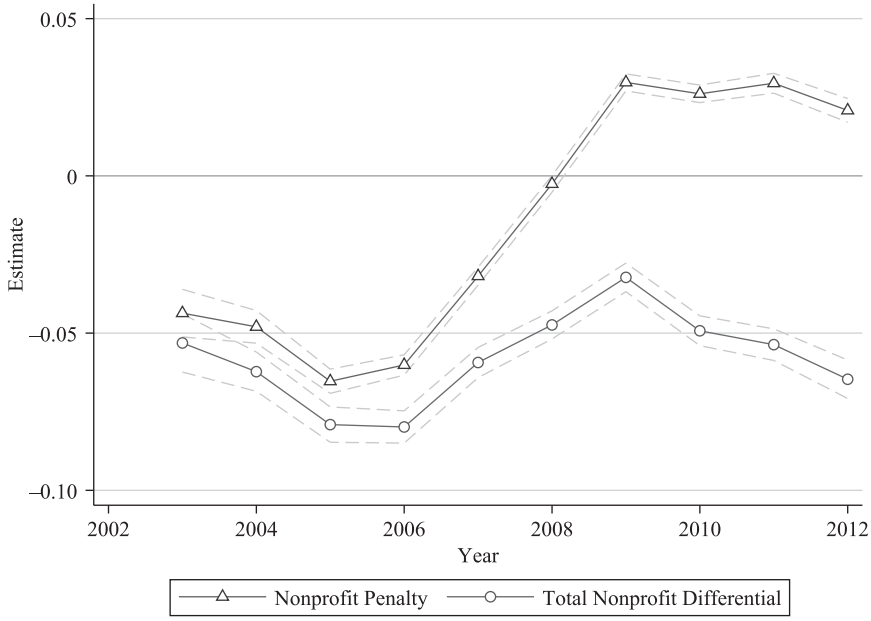

Figure 5

Nonprofit Differential and Nonprofit Penalty over Time

Notes: The circle-dotted line represents the cross-sectional nonprofit differential in each year. The triangle-dotted line presents the nonprofit penalty in each year, which accounts for worker-specific differences using individual fixed effects.

became less skilled. The new lower-skilled workers at nonprofit jobs earned more than they would at a for-profit job, either because for-profit firms in general pay lower-skilled workers less, or because for-profit firms cut worker payments across the board during the recession. This would account for the reducing nonprofit penalty and emergence of a modest nonprofit premium.

To test directly whether nonprofit firms substituted toward lower-skilled workers during the recession, we estimate Abowd–Kramarz–Margolis models to recover worker fixed effects (Abowd, Kramarz, and Margolis 1999). We model log quarterly wages w_{it} of individual i in year t as a worker component α_i, a firm premium $\varphi_{J(i,t)}$, and controls contained in $x'_{it}\beta$ (including year, county, and imputed experience), and an error term, ε_{it}.

$$w_{it} = \alpha_i + \phi_{J(i,\,t)} + x'_{it}\beta + \varepsilon_{it}$$

Following Abowd, Kramarz, and Margolis (1999), we interpret the worker effect α_i as human capital factors (such as skills, education, and ability) that are rewarded equally by employers. We interpret the establishment effect φ_j as a proportional pay premium or

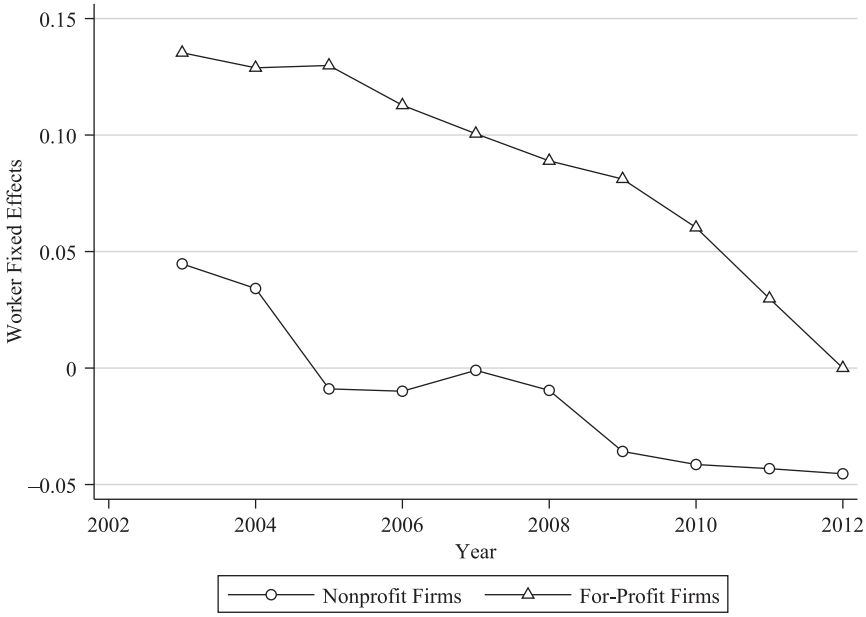

Figure 6

Worker Type over the Business Cycle

Notes: Figure shows the worker fixed effects plotted over time. Worker fixed effects are estimated from an Abowd–Kramarz–Margolis worker–firm fixed-effects model with year and quarter controls. All sample restrictions described in Section III are imposed.

penalty that is paid by establishment j to all its employees. Using our full sample (data from 2003–2012), we recover worker and firm fixed effects. We then plot the average worker fixed effects levels for nonprofit and for-profit firms over time. Figure 6 demonstrates that during the recession, nonprofits substituted toward workers with lower fix effects at the onset of the recession, while for-profits followed their trend line. This and the evidence above are consistent with nonprofits substituting toward workers with lower worker fix effects relative to for-profit employers.

F. Industry-Specific Estimates of Nonprofit Penalties and Premia

The event studies show visually that nonprofit penalties vary significantly by industry. We use Equation 2 to estimate the nonprofit penalty in each industry (Table 4). The preferred model estimates the within-worker nonprofit wage differential ρ while accounting for the wage dynamics of workers changing jobs within sector in each industry, seen in Column 3.

In Table 4, we compare the wages of workers as they transition between nonprofit and for-profit work in various industries. For instance, some insurance carriers are nonprofit,

Table 4
Nonprofit Differential Estimates by Industry

	Nonprofit Differential Estimates			Difference Decomposition	
Industry	Cross-Sectional Difference (1)	Within-Worker Estimate (2)	Gen-Diff-in-Diff Estimate (3)	Percent Demand (5)	Percent Supply (6)
All industries	–0.053***	–0.012***	–0.010***	19	81
	(0.001)	(0.001)	(0.001)		
	26,919,859	26,919,859	26,919,859		
Health and human services					
Outpatient healthcare (621)	0.068***	0.035***	0.035***	51	49
	(0.010)	(0.004)	(0.004)		
	905,031	1,030,102	905,031		
Doctor's offices (621,111)	0.227***	0.025**	0.026**	11	89
	(0.026)	(0.010)	(0.010)		
	360,097	383,801	360,097		
Hospitals (622)	0.008	0.000	–0.002	–26	126
	(0.005)	(0.003)	(0.003)		
	381,971	757,031	381,971		
Nursing facilities (623)	0.005	–0.002	–0.001	–16	116
	(0.011)	(0.006)	(0.006)		
	110,191	183,575	110,191		

(*continued*)

Table 4 (*continued*)

Industry	Nonprofit Differential Estimates			Difference Decomposition	
	Cross-Sectional Difference (1)	Within-Worker Estimate (2)	Gen-Diff-in-Diff Estimate (3)	Percent Demand (5)	Percent Supply (6)
Family services (624)	0.128***	0.033***	0.034***	26	74
	(0.013)	(0.007)	(0.008)		
	65,926	152,957	65,926		
Childcare (62,441)	0.049***	0.054***	0.050***	102	–2
	(0.016)	(0.011)	(0.011)		
	42,360	54,401	42,360		
Education (611)	–0.049***	–0.023***	–0.025***	50	50
	(0.011)	(0.008)	(0.008)		
	104,528	165,407	104,528		
Finance and management					
Banking & credit (522)	–0.270***	–0.027***	–0.026***	10	90
	(0.010)	(0.007)	(0.007)		
	976,814	988,174	976,814		
Investments (523)	0.345***	–0.026	–0.023	–7	107
	(0.037)	(0.018)	(0.018)		
	80,185	80,349	80,185		

(*continued*)

Table 4 (*continued*)

Industry	Nonprofit Differential Estimates			Difference Decomposition	
	Cross-Sectional Difference (1)	Within-Worker Estimate (2)	Gen-Diff-in-Diff Estimate (3)	Percent Demand (5)	Percent Supply (6)
Insurance (524)	–0.068	–0.038	–0.038	55	45
	(0.107)	(0.085)	(0.082)		
	567,213	567,270	567,213		
Administration (561)	0.096***	0.039**	0.039**	40	60
	(0.024)	(0.014)	(0.013)		
	2,279,915	2,280,313	2,279,915		
Utilities (221)	0.118	0.012	0.101	86	14
	(0.251)	(0.188)	(0.199)		
	161,157	161,853	161,157		
Classic charities					
Legal aid (54,111)	–0.256***	–0.131***	–0.129***	51	49
	(0.036)	(0.036)	(0.036)		
	270,984	271,996	270,984		
Religious organizations (8131)	–0.264***	–0.099***	–0.098***	37	63
	(0.014)	(0.013)	(0.013)		
	4,003,999	4,012,994	4,003,999		

(*continued*)

Table 4 (*continued*)

Industry	Nonprofit Differential Estimates			Difference Decomposition	
	Cross-Sectional Difference (1)	Within-Worker Estimate (2)	Gen-Diff-in-Diff Estimate (3)	Percent Demand (5)	Percent Supply (6)
Grant-making foundations (8,132)	–0.123***	–0.020*	–0.017	14	86
	(0.012)	(0.009)	(0.009)		
	3,943,822	3,971,955	3,943,822		
Social advocacy (8,133)	–0.243***	–0.024***	–0.021*	9	91
	(0.012)	(0.009)	(0.009)		
	3,949,345	3,974,332	3,949,345		
Civic organizations (8,134)	–0.081***	–0.051***	–0.050***	62	38
	(0.015)	(0.011)	(0.011)		
	2,581,211	2,593,224	2,581,211		

Notes: Table is based on the estimation of Equation 2 estimated for various industries. Column 1 reflects the cross-sectional earning difference between nonprofit and for-profit workers in each category. Column 2 estimates the nonprofit penalty by adding worker fixed effects to the estimation of Equation 1. Column 3 implements a generalized difference-in-difference that adds to the specification in Column 2 event–time fixed effects to account for general dynamics surrounding job changes. Column 4 reflects an estimate of how much of the nonprofit differential arises from demand-side forces, calculated by dividing the value in Column 3 with the value in Column 1. Column 5 reflects the share of the nonprofit differential arising from supply differences, which is the remaining nonprofit differential unexplained by demand. The third row for each estimate provides the relevant *N*. Significance: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

while others are for-profit. When we look at worker transitions within this class, we find that a given worker is paid 4 percent less when working for the nonprofit insurance provider.[17] Similarly, commercial banks can be registered as corporations or as nonprofits (a nonprofit commercial bank is sometimes known as a credit union). When we examine workers transitioning to and from nonprofit banks to and from for-profit banks, we learn that those workers earn 3 percent less in the nonprofit setting.[18] In contrast, we find that outpatient healthcare workers earn 4 percent more in nonprofits. To make sure we are comparing like settings, we condition on those nonprofit and for-profit employers that work in "physician offices" and find that a given worker similarly earns 3 percent *more* in the nonprofit setting, suggesting the nonprofit premium is spuriously formed by subgroup heterogeneity.[19] We estimate comparable models for traditional nonprofit charities. As in the event study, our sample of for-profit workers come from industries that have a high probability of receiving or giving workers from charity-type firms. We find significant penalties associated with working for these traditional charities on the order of 13 percent for law firms.[20] Religious bodies pay a given worker 10 percent less, civic organizations pay 5 percent less, and social advocacy groups pay 2 percent less.

V. Discussion

In classic charities (for example, churches, philanthropies, and civic organizations), nonprofit workers tend to take a pay cut, evidence of a labor donation. In "commercial" nonprofits (for example, insurance providers, commercial banks, healthcare providers), however, firms pay an attenuated penalty, and, in some industries, nonprofits pay as much or more than their for-profit peers—a striking feature.

Firms can only rely on a labor donation from workers if the marginal worker is willing to accept lower wages for the "warm glow" of an employer (Rose-Ackerman 1996). Even if some workers would be willing to accept lower wages for employment at nonprofits in a given industry, labor markets with lots of nonprofit employment may have to raise wages to attract the marginal worker who is unaffected by warm glow (Jones 2015)—this provides a plausible explanation for the divergence in the penalties across industry, but we find no evidence, for instance, that larger nonprofit sectors in an industry exhibit smaller penalties. Given that many workers misclassify the nonprofit status of their employer in commercial industries, labor donation, δ, may not be a significant factor in those settings. We find, however, that the nonprofit penalty in each industry is not significantly correlated with the misidentification rate in that industry ($p = 0.974$).

An unobserved component of w includes nonwage benefits and amenities. If nonprofits differ in nonwage benefits or work requirements by industry, this variation could explain differing nonprofit penalties across industries. Bishow and Monaco (2016) present data suggesting that nonwage benefits are roughly proportional with wages in

17. These insurance carriers have three-digit NAICS code 524.
18. Credit unions have NAICS code 522130, and other commercial banks have code 522110.
19. Physician's offices operate under NAICS code 621111.
20. In the previous estimate, we only used observations used in the figure, so it excluded workers transitioning from nonprofits to for-profits. The estimates in Table 4 include workers making transitions in both directions.

nonprofits in various industries.[21] Hirsch, MacPherson, and Preston (2018) show evidence that nonprofits workers work 4 percent fewer hours than do for-profit employees, suggesting that nonprofit wage penalties could reflect a compensating differential for a less demanding work environment. To gauge unobserved nonprofit utility by industry, we calculate the length of the average employment spell for nonprofits and for-profits in each industry, which we use as a measure of how happy employees are at each type of employer. Though employment spells vary in length between nonprofits and for-profits in each industry, these differences do not predict industry-specific nonprofit penalties ($p = 0.833$), suggesting that nonwage utility does not explain the earnings penalties across industries.

Another plausible rationale for why nonprofit penalties vary across industries is that a higher level of labor-market competition in some industries drives nonprofits to pay a higher premium. We focus on the outpatient healthcare sector, where competition can vary significantly from county to county because some areas will have few of each outpatient firm type and others will have several (Bresnahan and Reiss 1991).[22] We find no evidence that nonprofit employer wages are higher (relative to for-profit employers) in areas with more labor-market competition on the employer side, suggesting that differences in competitive environments also fall short of explaining the variation in nonprofit penalties and premia across industries. Online Appendix C details this empirical exercise.

We find, however, that nonprofit penalties and premia are strongly related to cross-sectional differences between the earnings of nonprofits and for-profit employers. That is, when nonprofits in a given industry pay higher-income people relative to the for-profit sector, the nonprofit within-worker premium is also larger ($p < 0.01$), which is interesting but not highly indicative of any hypothesis we have considered.[23] To be precise about this statement, when nonprofits tend to employ lower-earning workers, other workers in that industry are paid less, conditional on their worker fix effects. This suggests the variance in nonprofit penalties and premia could result from productivity spillovers or egalitarian norms in nonprofit firms.

VI. Conclusion

The literature features a debate between analysts who argue nonprofit wage differences arise from demand-side factors (that is, that nonprofits pay differently) (Weisbrod 1983; Borjas, Frech, and Ginsburg 1983) and others who contend these differences arise from supply-side factors (namely, nonprofits employ a different *kind* of worker) (Goddeeris 1988; Holtmann and Idson 1993). For example, a nonprofit wage

21. The relationship between hourly wages and health benefits or retirement benefits across nonprofit industries is highly linear; wages, for instance, predict 98 percent of the variance in health benefits and 94 percent of the variance in retirement benefits (Bishow and Monaco 2016).

22. We construct a measure of how many like-industry firms exist in each county. The specific outpatient healthcare firm types include physical therapists, occupational therapists, podiatrists, dialysis centers, diagnostic labs, imaging centers, optometrists, etc., so that the competition measure captures how much local competition there is in a given firm's detailed industry.

23. This is a test of the correlation between an industry's nonprofit penalty and the average earnings gap of nonprofit workers in that industry.

gap could arise from disproportionate labor supply of less experienced or less educated workers who also would earn less in for-profit employment (Hirsch, Macpherson, and Preston 2018). The difficulty in this debate is that nonprofit workers may differ in a host of unobservable ways that are challenging to assess. The purpose of this work is to evaluate the nonprofit differential using administrative data on workers who changed jobs to account for differences in worker characteristics, which persuasively controls for unobserved personal factors that might otherwise bias measures of the nonprofit penalty. Nonprofit wage penalties from the demand side suggest labor donation in classic charities, a topic of sustained interest (Hansmann 1980; Hirsh, MacPherson, and Preston 2018).

We find that a large share of the nonprofit gap is a product of worker composition (supply-side factors) and that a small but distinct share is attributable to a nonprofit penalty (on the demand side). These penalties are large in classic charities and smaller in commercial nonprofits, where nonprofit premia appear in some industries. Though we explore several explanations for the varying nonprofit penalty/premium, we cannot find a convincing or intuitive explanation. Understanding why some nonprofits pay more may illuminate policies that promote greater wages among workers. These estimates build on prior literature by accounting for worker-specific unobservables in a large data set, which allows for unbiasedness, but also statistical precision, which rules out more than 90 percent of the confidence intervals provided by previous work. We also find that nonprofits compress the earnings distribution, especially at the high end, suggesting that the rapid growth of the nonprofit sector may have fostered greater income equality than would have otherwise existed.

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