# The Cost of Financial Misconduct in Nonprofits

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**ABSTRACT**

How severely are nonprofits affected by disclosure of financial fraud? Existing literature focuses on declines in charitable giving, though donations account for merely 20% of nonprofit revenues. Using a large dataset extracted from IRS-filings, we find that post-fraud, there is sharp, long-term decline in service revenues and employee compensation. There is, however, surprisingly little change in volunteerism or employment. While charitable revenues decline, they rebound fairly quickly. We uncover suggestive evidence that fraud benefits *other* nonprofits in the same zip code. This is consistent with long-term revenue drop post-fraud and highlights the role of competing nonprofits as a governance mechanism.

*JEL Classification:* J3, G3, E3
*Keywords:* Disclosure; Nonprofits; Financial crime; Compensation; Revenue

Nonprofit organizations employ tens of millions of Americans and have more than 100 million volunteers annually (Philipson and Posner 2006). Nonprofits also make up 20% of all United States corporations, hold approximately $5 trillion in assets and have about $500 billion outstanding in tax-exempt bond liabilities. Despite their prominence in the US economy, studies on fraud and financial misconduct have focused primarily on publicly traded firms (e.g., Karpoff, Lee, and Martin 2008a, 2008b; Karpoff and Dupont 2020).[1] We seek to advance our collective understanding of the impact of financial fraud in the nonprofit sector by testing the impact of fraud on a variety of important fundamental financial measures. In doing so, we extend prior literature on the cost of financial crime and address the extent to which fundamentals rebound post fraud. To our knowledge, this paper presents the first large-sample examination of financial misconduct in the nonprofit sector that goes beyond donor response and studies the impact of fraud over an extended time-period.

Thus far, nonprofit academic studies have focused on short-term declines in donor contributions following disclosures of financial misconduct (Harris, Petrovits, and Yetman 2023; Lauck and Brozovsky 2018).[2] The bright side of such a decline in donor contributions is that it can serve a governance function. This is since nonprofits anticipating a punishing cut back in funds in the event of financial malfeasance will have the incentive to maintain adequate financial controls. However, contributions account for less than 20% of revenue for the typical nonprofit and

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[1] Many other scholars examine financial crime, its disclosure, and its cost to publicly traded, for-profit firms in finance, economics, and accounting (Bowen, Call, and Rajgopal 2007; Bushman, Williams, and Wittenberg-Moerman 2016; Call, Martin, Sharp, and Wilde 2018; Dyck, Morse, and Zingales 2010; Fich and Shivdasani 2007; Miller 2006; Yu and Yu 2011).

[2] Prior literature (Harris et al. 2018; Lauck and Brozovsky 2018) and our study use IRS information tax Form 990 disclosures to identify organizations that have reported financial misconduct or fraud within the organization during the past fiscal year. Specifically, organizations are required to respond to the following question from Part VI, Line 5 of Form 990: "Did the organization become aware during the year of a significant diversion of the organization's assets?". We use the terms financial misconduct, misconduct, fraud, financial crime, and crime interchangeably to identify organization's responding "yes" to this Form 990, Part VI question.

observing the impact of financial crime disclosure on service revenue, employee compensation, volunteerism and other outcomes can provide a more complete picture of the impact of fraud in the sector. We also examine how (and whether) these financial and non-financial measures change in the years following financial crime to understand the longer-term effects of financial frauds in US nonprofit organizations.[3]

For our empirical analysis, we use data with almost 2 million nonprofit observations and 22 million executive, board member, trustee, and key-employee compensation observations over a decade, starting in 2010. Employing this data, we address the following research questions related to nonprofit fundamentals in response to financial crime: (1) whether contributions and sources of revenue outside of contributions decline, (2) whether nonprofits engage in belt-tightening with respect to total employee compensation and total expenses, (3) whether there are changes in the number of employees and executives as well as the amount of compensation per employee or executive, and (4) whether organizations experience a decline in volunteerism.

Our finding is that the average nonprofit organization experiences approximately a 12% decline in service revenues and a 6% decline in total revenues in the year following the disclosure of crime. It is plausible that fraud disrupts the functioning of nonprofits, hurting employee morale and increasing turnover. This could affect the quality of the services provided and, in turn, the revenues generated. Nonprofits that obtain much of their revenue from contributions (donative nonprofits) suffer an average decline of 9% in contributions in the post-disclosure year, while the

[3] It is not immediately obvious whether nonprofit fundamentals should be expected to recover or decline after the discovery and elimination of fraud. One might suspect that fundamentals improve after nonprofits publicly signal that they have unearthed and eliminated fraud, since this might help explain why the nonprofits' projects may have previously underperformed. With the source of fraud gone, stakeholders may become more optimistic about the same nonprofit projects going forward in the absence of harmful misconduct. (Dewatripont and Maskin 1995; Bergemann and Hege 2005). On the other hand, the disclosure of nonprofit fraud may cause significant reputational harm to the organization and cause declines in future performance due to operational disruptions (Harris, Petrovits, and Yetman 2022).

impact is negative but insignificant across nonprofits as a whole. The decline in charitable contributions has been attributed to nonprofits suffering a loss of reputation when fraud is detected. Ex-ante, nonprofits' concern about the loss of reputation and donations should provide incentives for them to establish controls and reduce the likelihood of financial fraud.

We also find that total employee compensation declines almost 8% in the average post-disclosure year. This decline persists over time despite the lack of any statistically or economically significant decline in the number of employees, volunteers, or officers within the organization. This might be the result of resource constraints faced by nonprofits as well as factors such as labor market frictions that keep employees from leaving. Additionally, observing compensation for executives apart from all other employees demonstrates that non-executive employees suffer more in terms of compensation declines. The average employee suffers a 7% decrease in compensation, which generally does not recover in the subsequent five or more years. In comparison, the average executive experiences a decline of about 5%, with compensation becoming generally indistinguishable from that of unaffected peers three or four years after the crime. Total expenses also decline by approximately 6% for criminally involved nonprofits, suggesting overall belt-tightening.[4]

In terms of the longer-term impact of financial crime on these metrics, we find that while program revenues continue a downward trajectory for affected organizations, contributions appear to rebound three years following fraud. In terms of total compensation expense as well as compensation per employee, we find these expenditures continue to decline for several years after the discovery and disclosure of a crime. This is in contrast to officer pay which recovers just one

[4] We also find, in untabulated results, statistically significant increases in unsecured loans (as much as 30%), which nonprofits may be using to offset the declines in revenue to avoid belt-tightening as much as they otherwise might. Bond liabilities also experience a 13.5% increase in affected organizations (also untabulated).

year following fraud disclosure. This could reflect the necessity of retaining higher quality officers and an active labor market in which these officers are able to relocate with relative ease.

To expand our understanding of financial crime in the nonprofit sector, we conduct several additional analyses. First, we consider employee and officer retention post-fraud disclosure. Here we find that organizations reporting a financial crime have lower overall employee retention as well as lower retention of directors, officers, and key employees compared to unaffected organizations. We interpret this to mean that fraud affected firms experience personnel changes either due to voluntary or involuntary turnover following fraud disclosure. Next, we consider the potential spillover effect of fraud to peer nonprofits in the same zip code. Interestingly we find that peer nonprofit organizations report increases in program revenues, total compensation, as well as total employees, officers, and wages per employee following financial crime occurring in the same zip code. This suggests the possible movement of customers and employees from the fraud to non-fraud organizations in the area. Such substitution effects are consistent with the longer-term decline in revenues we document at affected organizations. These competing nonprofits, by attracting donations and service revenues increase the cost of financial malfeasance and, thereby, strengthen nonprofits' incentive to avert fraud.

Finally, we conduct our main analyses across subsamples of organizations by size and revenue concentration, finding that large, as well as commercial nonprofits are driving our service revenues response variable. We also find that smaller, as well as more donative organizations report declines in contributions post-fraud but do not see reductions in program revenues. In terms of compensation, we find that organizations across both size and revenue concentration subsamples experience declines in total compensation and wages per employee following financial crime.

We test our models using staggered differences-in-differences. In any differences-in-differences design, lack of parallel trends between the treated and control observations could drive the results and bias estimates. To address this potential weakness and recent literature on the proper weighting of the differences-in-differences estimators, we follow Sun and Abraham (2020) and Callaway and Sant'Anna (2020) and apply the required interaction weighting estimator for staggered event studies. We also supplement this approach by conducting the Goodman-Bacon decomposition (Goodman-Bacon 2021). The results are robust to these methods, and in some instances, become more pronounced. Furthermore, we believe these econometric techniques help to alleviate a large variety of concerns. Specifically, we confirm that pre-trends do not drive our findings, that our methodologies recalculate both pre-treatment and post-treatment coefficients using the interaction weighted estimator approach, and we demonstrate that pre-treatment coefficients are insignificantly different from zero. We also conduct a variety of additional robustness tests including propensity score nearest neighbor matching, entropy balancing, as well as placebos to address pre-trend concerns.

## I. Background Literature and Hypotheses

Prior research has documented the characteristics of fraud perpetrators (Greenlee, Fischer, Gordon, and Keating 2007; Holtfreter 2008), considered the impact of press reports of misconduct on nonprofit survival (Archambeault, Webber, and Greenlee 2015; Archambeault and Webber 2018), and evaluated the impact of crime disclosures on future contributions (Harris, Petrovits, and Yetman 2022; Bottman and Perez-Truglia 2015; Dupont 2021). Other papers have studied remediation measures in a laboratory setting (Lauck and Brozovsky 2018) and considered the mitigating effect of good governance on fraud (Harris, Petrovits, and Yetman 2015).

The first objective of our paper is to examine the impact of fraud on the largest source of nonprofit income: service revenue. This is unlike the existing literature on fraud in nonprofits that has focused largely on donations. The literature on misconduct in for-profit firms suggests that corporate misconduct allegations raise risk and reduce firm profitability (Murphy, Shrieves, and Tibbs 2009). Reputational effects post-fraud can lead to consumers, not just regulators and investors, to sanction for-profit firms (Johnson, Xie, and Yi 2014), which may also be the case for nonprofits which rely mostly on service revenue to function. The negative effect of fraud on revenues has been documented in for-profit firms outside the United States as well, (Xin, Zhou, and Hu 2018). Therefore, based on for-profit research which has documented declines in revenue post-misconduct, we likewise expect affected nonprofits to report declines in program revenues.

As indicated earlier, a decline in program service revenues could also be driven by a decline in the quality and level of services provided by the troubled nonprofit. Service provision could, for instance, be affected by a drop in employee morale and disruption to the nonprofit's functioning along with the possible departure of higher quality employees. As noted, a decline in revenues could be exacerbated by the presence of other nonprofits in the vicinity that attract the service revenue of customers away from affected organizations. This leads to our first testable hypothesis:

*H1: All else equal, nonprofits that report a material diversion of assets will be associated with a decrease in program service revenues in subsequent years.*

We would like to acknowledge that while a decline in revenue post-misconduct might be anticipated from prior for-profit studies and arguments above, are reasons to believe this may not be the case. That is, at least in principle, discovery of the crime, the apprehension of the perpetrator(s), and adjustments within the nonprofit could lead to better and more plentiful provision of services.

Our next research question relates to nonprofit expenditures, with a specific focus on the largest single-line-item expense: employee compensation. This is particularly important if we assume that employee compensation has a positive relationship with nonprofit output as the organization strives to achieve its mission. If financial fraud brings about lower future contributions, as established in prior literature, as well as reductions in program revenues, as predicted in H1, we might expect organizations to consider belt-tightening of expenses, especially variable costs such as employee compensation. A loss of donations and program revenues may also reduce programs which may further bring about reductions in compensation. Taken together we posit that organizations with declines in revenues will decrease aggregate employee pay following fraud. This culminates in our second hypothesis:

*H2: All else equal, nonprofits that have reported a material diversion of assets will be associated with lower total employee compensation in subsequent years,*

Our final research question considers whether the number of employees and executives falls (accounting for the decrease in total compensation) or whether compensation per employee and per executive falls while the number of individuals remains relatively constant. If workers receive no premium (and perhaps even a discount) for working in a criminally affected organization, despite a competitive labor market, one might expect a decline in employment. Yet, if the number of employees remains roughly the same, it may suggest that most workers are not dissuaded from working for a criminally affected nonprofit and accept lower pay. This would be consistent, for instance, with employees receiving more than market wages prior to fraud discovery. It could also result from higher quality employees leaving and being replaced by a similar number of lower quality employees receiving an appropriate market wage. The willingness of employees to accept lower wages could also reflect the existence of labor market frictions in the nonprofit space and/or altruistic loyalty of the workers toward the organization. Some or all of the above could lead to a decline in average compensation per employee. This results in our third and final study hypothesis:

> *H3: All else equal, nonprofits that have reported a material diversion of assets are associated with lower compensation per employee.*

## II. Data Description

The sample period for our analyses runs from 2010 to 2019. The data come from Form 990 electronic filings by nonprofit organizations made available to researchers from the IRS via Amazon Web Services.[5] The available datasets contain information about revenues, such as contributions, service revenue, investment earnings and other revenue, information about expenses including employee compensation, other expenses, as well as officer compensation. The Form 990 data also requires nonprofits to report the number of employees, officers, volunteers, and board members on an annual basis.

With respect to financial misconduct, the data includes an indicator for whether a nonprofit experienced a "significant diversion of assets" in a particular year. The IRS defines a significant diversion of assets as any diversion of funds from their intended purpose that exceeds the lesser of 5% of the organization's gross receipts in the year of the diversion, 5% of the organization's assets in the year of the diversion, or $250,000.[6] These diversions include frauds such as theft, embezzlement, and similar financial misconduct and are disclosed in Part IV, line 5 of the IRS Form 990. We use this check box to create test key variable, Crime. Crime is 1 in the year of diversion disclosure and in each year after diversion disclosure for criminally affected

[5] IRS Makes Electronically Filed Form 990 Data Available in New Format, *Internal Revenue Service* (June 16, 2016), https://www.irs.gov/newsroom/irs-makes-electronically-filed-form-990-data-available-in-new-format.
[6] Instructions for Form 990 Return of Organization Exempt From Income Tax, *Internal Revenue Service* (2019), https://www.irs.gov/pub/irs-pdf/i990.pdf.

organizations. Crime takes the value of 0 for all untreated nonprofits across time and all treated nonprofits prior to treatment.

The panel data containing nonprofit fundamentals and significant diversion indicators has almost 2 million observations concerning more than 325,000 distinct nonprofit organizations over the scope of ten years.[7] The data contains all nonprofits that file their IRS Form 990 electronically.[8] Some organizations remain within the panel data for all ten years, others appear for just a few years, and some appear only once. There are several reasons why nonprofits may exit or enter the sample of organizations filing their IRS Form 990 electronically. First, Form 990 tax filings come in three formats: the standard Form 990 filings (the ones used in this study), Form 990 EZ tax filings, and Form 990 Postcard filings. Because monetary thresholds determine which form the nonprofit files, some organizations drop in and out of the standard Form 990 panel because they fall below the threshold that makes a Form 990 filing required in some years.[9] Additionally, sometimes nonprofits cease operations altogether. The closing of a nonprofit organization between 2010 and 2019 will also result in its removal from the sample. Nonprofits beginning operations

[7] There are some instances where the nonprofits appear twice in the same year, usually due to filing two returns where the second served as a final return (there are approximately 1,000 such observations in the sample of almost 2 million). To avoid problems implementing organization fixed effects and potential inconsistencies from the same nonprofit filing two tax filings in one year, we eliminate double filings from the panel data, retaining only the final filing.

[8] President Trump recently signed legislation requiring all nonprofit organizations to begin filing returns electronically, starting in 2021. Taxpayer First Act, Pub. L. No. 116-25, § 3101, 133 Stat. 981, 1015 (2019). In the most recent year of the data, approximately two thirds of the 1.5 million nonprofit organizations filed their tax forms online. Lynch, Jim and Chris Worman, Electronic Filing of Nonprofit 990s Will Be Required in 2021 (Sept. 11, 2019), https://blog.techsoup.org/posts/electronic-filing-of-nonprofit-990s-will-be-required-in-2021.

[9] Any nonprofit with gross receipts greater than $200,000 or total assets of $500,000 must file a full Form 990, containing a large amount of information about the nonprofit. If an organization does not meet either threshold, but has gross receipts of more than $50,000, it may file the full Form 990 or may elect to file a Form 990-EZ, which contains less information. Nonprofits that are not required to file a Form 990 or a Form 990-EZ may still file these forms at their discretion, or they may elect to file a Form 990-N. The latter form contains even less information than the EZ. Neither the EZ Form nor the N Form contain information about significant diversions. The IRS requires that organizations with assets in excess of $10 million or filing at least 250 returns during a calendar year (filing a certain number of returns is usually associated with having a certain number of employees) file electronically. Private foundations and charitable trusts with at least 250 annual returns must file electronically, regardless of their assets. Larson, Donna, Form 990 electronic filing requirements expanded, BKD CPAs & Advisors (Oct. 7, 2019), https://www.bkd.com/article/2019/10/form-990-electronic-filing-requirements-expanded.

between 2010 and 2019 or beginning to file electronically with the IRS within that time frame would also cause new organizations to appear within the data, possibly midway through the panel. Finally, there may be years when nonprofit organizations begin filing their tax forms by mail rather than electronically. These physical forms and their information are not included in the dataset.

Table 1 presents descriptive statistics for the key variables used in our analyses. The average nonprofit between 2010 and 2019 received a little over $10 million in total revenue, spent a little less than $10 million in total expenses, and had almost $12 million in net assets. Nonprofit organizations also incurred a non-trivial amount of liabilities, with the average amount of outstanding debt approaching $11 million dollars per organization. Contributions make up less than $2 million in revenue for the average nonprofit. Service revenue, on the other hand, is $8 million for the average nonprofit and constitutes almost 80% of total revenues for the average organization. Investment income and other revenue appear to be relatively small components of total revenue. However, it is notable that 68% of nonprofits report service revenue above zero, while almost 78% report positive contributions, suggesting that there is a more concentrated number of high service revenue organizations driving sample averages.

Because we use natural logarithms to scale the variables of interest, which will exclude zeros and negative numbers automatically, we also include a second table of summary statistics in Panel B, but only for observations reporting values greater than zero. The exclusion of negative numbers is appropriate because the IRS specifically requires that all field entries are either zero or positive, meaning that negative entries are, generally speaking, accounting errors. We also exclude organizations from our regressions which report precisely zero in terms of service revenue, employee compensation, and other variables of interest. Including organizations that, for instance, never receive any service revenue in a regression seeking to find the impact of crime disclosure on service revenue may obscure the true impact on the nonprofits that collect service revenues regularly.

Panel C of Table 1 presents a breakdown of sample nonprofits by industry, also showing the number of financial crime disclosures in each industry group. Here we find that Human Services organizations make up the largest portion of our sample at 34%, followed by Public and Societal Benefit with 18%. Panel D of Table 1 provides fraud details for the significant diversions identified in the sample. While the total number of diversions disclosed in our sample is 1,897, only 744 nonprofits provide Schedule Os, indicating either non-compliance with IRS guidelines by many nonprofit organizations or the unavailability of these schedules from the IRS. Out of the 744 Schedule Os, sample nonprofits disclose the involvement of an employee 526 times, and out of that number, 265 instances involve either board members or officers. Nonprofits report seeking recovery well over 50% of the time and recover more than 50% of the amount lost in restitution, insurance payments, civil judgment, or a combination thereof. Only a single individual performs the typical crime, and the most common offense, by far, is embezzlement. Although there are few reports of a criminal punishment, the average sentence length for perpetrators that are sentenced approaches 5 years, with the average probationary period lasting 30 months. Almost one third of criminally afflicted nonprofits report extensive remedial measures in their Schedule O filings, which may impact future operations.

## III. Research Design

To isolate the cost of crime within our data, we employ a staggered differences-in-differences approach to observe how nonprofits fare in the years after the disclosure of financial crime compared to organizations that do not report a financial crime at all. A staggered differences-in-differences approach is appropriate when the treatment does not occur in a single year for all treated units but is staggered across the years of a panel dataset (Bertrand and Mullainathan 2003; Meer and West 2015; Borusyak and Jaravel 2017; Abraham and Sun 2019). Because crime occurs (and nonprofits discover and report it) in different years for different organizations, every year within the panel data includes dozens, if not hundreds, of financial crimes.

Specifically, the regression model used to test our hypotheses is estimated as:

$$Y_{it} = \alpha + \beta_1 Crime_{it} + \beta_2 Controls_{it} + \delta_i + \eta_t + \varepsilon_{it} \qquad (1)$$

where $Y_{\mathrm{it}}$ represents the dependent variable of interest, *Crime* represents the differences-in-differences dummy variable equaling 0 prior to disclosure of financial crime, 1 in the year of first disclosure, and 1 in every year after, $\delta_i$ represents organization fixed effects, $\eta_t$ represents year fixed effects, and $\varepsilon_{it}$ is an error term.[10] Continuous numerical variables appear in natural logarithm form and are Winsorized at the 1st and 99th percentile to eliminate the effect of outliers. The estimated impact of a significant diversion on the dependent variable is $\beta_1$, making it the coefficient of interest.[11]

While the sample includes 1,897 instances of nonprofits disclosing significant diversions between 2010 and 2019, the number of control organizations dwarfs this number of treated nonprofits. That is, with each year in the panel including between 97,000 and 250,000 organizations, the vast majority of nonprofits do not disclose major financial misconduct at any point between 2010 and 2019. This leaves a much larger number of organizations to serve as the control group. Based on this, we are careful to ensure that the differences between the control and

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[10] In our robustness tests we also specify models which include control variables following prior nonprofit and financial crime literatures, finding consistent results throughout.

[11] Results throughout are robust to alternatively measuring our response variables, $Y_{it}$, at time t+1 to allow additional time for the financial crime to impact the organization, which may be relevant if the crime was not uncovered until later in the organization's fiscal period. Despite this, we note that the fundaments we focus on relate to organizational outcomes unaffiliated with outside stakeholder response, therefore, we are unconstrained by, for example, donors requiring that the Form 990 be available for inspection of fraud disclosures.

treatment groups are not so great as to obscure inferences about the effects of financial crime. Moreover, since financial crime is frequently (if not always) endogenous, it is crucial to address the challenges associated with such a treatment. The following section details how we address these concerns in our analyses.

A. *Fixed Effects*

We begin by using year fixed effects to address the potential differences in our response variables that could be ascribed to the year being observed. To account for persistent differences, we also absorb organization fixed effects within our regression models. This allows for the separation of the effect of a financial crime from the organization-specific effects on the variables of interest. The inclusion of both organization fixed effects and time fixed effects is also sufficient to make the regression a staggered differences-in-differences regression (Bertrand and Mullainathan 2003; Meer and West 2015; Borusyak and Jaravel 2017; Abraham and Sun 2019).

Organization fixed effects also serve another important purpose. Part of the challenge in finding proper control observations for nonprofits affected by crime is that the treated organizations may be different from non-treated nonprofits in ways that cause their treatment in the first place. Some characteristics of this sort may not appear in the data available to us, making it difficult to control for them. The inclusion of organization fixed effects partly corrects for this problem. That is, if treated organizations have constant characteristics that lead to financial fraud, then organization fixed effects should absorb these characteristics and allow the control nonprofits to simulate how the treated nonprofits would have performed but-for financial misconduct. This can reduce, but does not eliminate, the endogeneity problem. It is still possible that some non-constant variable may cause the financial misconduct in the treated nonprofits, which would not be captured by organization fixed effects.

Another way to reduce the bias introduced by endogeneity is by exploiting the staggered occurrence of financial misconduct. Specifically, the control group need not be restricted to nonprofits where no fraud ever occurred. Model (1) can be estimated even if all nonprofits experience a financial crime such that the control group includes all nonprofits where no significant diversion occurred prior to time *t*, even if one might occur later. Hence, a nonprofit can be a control for a treatment organization so long as no significant diversion has occurred and remain as a control until a diversion takes place within the organization. For example, if a diversion takes place in the control nonprofit in 2016, it could still serve as a control during the 2013–2015 period, allowing the post-crime performance of an organization treated in 2013 to be compared to a later-affected nonprofit for three years.

Running a regression that includes only the "eventually treated" organizations as controls may help alleviate concerns about the endogeneity of the treatment to the treated, since the treated are part of the control group for each other over some time periods. If our results are driven by endogeneity - that there is something unique about criminally involved nonprofits that causes both the crime and the effect on fundamentals, one would expect to see statistically insignificant coefficients in this type of limited regression. Instead, we re-estimate all models using only the treated nonprofits and exploit the time variation in the detection and reporting of crime to isolate the effect of crime only among treated organizations, finding consistent results.

B. *Differences-in-Differences Design*

In order to apply a differences-in-differences design, we must confirm that the standard differences-in-differences assumptions hold, and if they do not hold, how that would impact the interpretation of the results (Glaeser 2018). Because we can never observe what would have happened to an organization but-for financial crime and its discovery, we cannot directly test the parallel trends assumption. Moreover, due to the endogeneity of financial misconduct, there may be concerns about whether the parallel trends assumption holds. To address these challenges, we employ placebo tests to see if there was some statistically significant difference between the treated and untreated nonprofits *prior* to financial misconduct (Meer and West 2015). The lack of such trends generally raises confidence that the parallel trends assumption holds, though once again, researchers can never be completely certain. Another way to address this is to graph the trends of the differences-in-differences coefficients prior to and after treatment to visually justify the parallel trends assumption (Beck, Levine, and Levkov 2010). We employ both methodologies to confirm the robustness of our results.

## IV. Results

Table 2 presents results of running model (1) using the DID design described above. Panel A includes four variations of nonprofit cash inflows as dependent variables in columns 1–4. For the full sample, there is no significant decline in contributions. As we discuss later, however, there is a significant drop in contributions for nonprofits that are donation dependent. Next, we find that service revenue declines significantly after disclosure of financial crime, consistent with our predictions and H1. We also find that overall total revenue declines, while other revenue increases in the period following financial crime. In economic terms, service revenue drops almost 12% on average, contributing to an overall loss in total revenue of about 6%. This occurs despite a 17% rise on other revenue.[12] Moreover, the loss in revenue for the average fraud affected firm exceeds the average loss amount reported in Schedule O filings. That is, while the average amount of loss

[12] It is possible that other revenue does not rise as a direct result of crime or its disclosure. Instead, other revenue may rise as a result of seeking recovery for the misconduct. That is, nonprofits may be recovering their losses from criminal restitution, civil litigation, insurance, or all of the above. The percent increase in other revenue may appear high because other revenue is usually the smallest portion of revenue for the average nonprofit ($0.5 million), and even a partial recovery of the amount lost to financial crime would greatly increase this metric for the nonprofit if the organization classifies the recovery as other revenue.

reported in the Schedule O filings for sample observations comes in at approximately $0.8 million, a 12% reduction to service revenue for the average criminally affected nonprofit in our sample would be $20.8 million using our sample statistics.[13]

Next, Panel A, columns 5 and 6 test hypothesis two and measure our dependent variables as total employee compensation and total expenses, respectively. Here we find employee compensation declining almost 8% post-disclosure, driving the 6% decline in total expenses. Once again, the average loss in compensation for nonprofit employees per year greatly exceeds the average loss amount reported in Schedule O filings. Specifically, while the average amount of fraud loss is approximately $0.8 million, an 8% reduction to compensation paid by the average criminally affected nonprofit would be approximately $4.7 million using our sample statistics.[14]

Next, to test whether nonprofits are forced to compensate some workers at a higher rate and therefore lay off other workers, we evaluate the impact of crime on the size of the workforce overall and at the executive level. Specifically, we run model (1) using three variations of compensation per employee and two variations of the number of employees as the dependent variables and present our results in Panel B. Here, the fall in employee compensation appears to be directly connected (and very close in magnitude) to a reduction in compensation per employee. That is, we find statistically significant declines in pay per officer (-5%) and pay per employee (-7%). Total officer compensation falls a little less than total employee compensation, showing a 6% fall instead of the overall decrease of 8% in total compensation (Panel A). However, we fail to find an impact of crime on the number of employees or officers the nonprofit retains, as presented

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[13] We arrive at this figure by calculating the average annual service revenue for all criminally affected nonprofits in the years before a crime, which is $173 million per year. 12% of this is $20.8 million, which would approximate the average amount of service revenue lost in the average year after disclosing financial misconduct.
[14] We arrive at this figure by calculating the average annual service revenue for all criminally affected nonprofits in the years before a crime, which is $57 million per year. 8% of this is $4.7 million, which would approximate the average reduction in employee compensation in the average year after disclosing financial misconduct.

in columns 4 and 5 of Panel B. These results are consistent with our H3 conjecture that nonprofits affected by financial crime might be associated with lower compensation per employee, rather than employee turnover. Finally, column 6 considers volunteerism: we fail to find any significant difference in the number of volunteers at treated nonprofits following financial crime, which is notable, since volunteerism is similar to monetary contributions, but instead, contributes labor to the nonprofit.

## V. Additional Analyses

### A. *The Dynamic Relation between Crime and Fundamentals*

In addition to our main models, which include contemporaneous measurement of our dependent variables, our panel data permits us to delve deeper into the dynamic relations between financial misconduct and the nonprofit fundamentals under review. The length of our panel allows for a novel review of how our dependent variables behave post-treatment and confirm that pre-trends do not drive the results. Moreover, we find heterogenous behavior over time across our various variables of interest, suggesting important nuance in how nonprofits experience financial misconduct and attempt to rebound therefrom. Specifically, Table 3 presents graphs of the fundamentals of interest in the seven years preceding and following reported financial crime.

Figure 1 presents contributions, which shows a statistically significant decline the first two years after crime but then exhibits a forgiveness effect, becoming insignificantly different from zero (and even slightly positive) in subsequent years. This figure helps explain why the contribution result presented in Table 2 is negative but not statistically significant, in contrast to Harris, Petrovits, and Yetman (2023) who find a negative association with fraud.[15] That is, we demonstrate that once the focus reaches beyond the second post-treatment year, the results become

[15] Harris, Petrovits, and Yetman (2023) focus on contributions just one year after the disclosure of financial misconduct, which are statistically significant in our findings as well.

insignificantly different from zero. In sum, it appears that when our sample is pooled over all post-treatment years, the overall effect on contributions is insignificant; however, this does not mean the disclosure of crime has no effect on contributions; on the contrary, the effect is quite sharp at first (nearly a 10% decline) and dissipates with time as donors have an opportunity to "forgive."

Figure 2 presents program revenues, where the impact of fraud is far more persistent. Here we find that while program revenues appeared to be fairly stable in the seven years preceding financial crime, we see a sharp decline in the years after. Additionally, we fail to observe a rebounding effect in any of the seven post-crime periods. Instead, the declines appear to worsen with time. This largely echoes empirical literature on for-profit organizations. Revenue begins to fall post- discovery and disclosure of financial misconduct and continues to fall compared to control nonprofit organizations over time. The joint effect of contribution declines in Figure 1 and service revenue declines in Figure 2 provide for a joint negative effect in Figure 3 for total revenue, which exhibits a negative and statistically significant effect at every post-treatment time period.

Figure 4 illustrates that while total expenses are also significantly reduced post-financial fraud, they continue to decline through year three, with a one-year uptick in year four, before additional decreases in overall spending in subsequent years. That is, there appears to be no general downward trend in total expenses prior to a diversion of assets, and pre-treatment coefficients are not statistically different from zero while, all post-diversion coefficients indicate a statistically and economically significant decline.

Our next set of figures compares compensation per employee and per officer. Specifically, Figure 5 presents compensation per employee. We note that significant declines in non-officer compensation per employee did not seriously precede the discovery and reporting of financial misconduct (though the misconduct could have been responsible for the slight, statistically insignificant declining trend prior to its disclosure). Second, the negative impact after financial misconduct is apparent immediately following financial fraud and the decline continues with time. These results contrast with compensation per officer results presented in Figure 6, which indicate that while pre-fraud trends are relatively stable, compensation per director steadily rebounds in the periods following the financial misconduct. As noted, this may be in an effort to retain higher quality officers, along with an active labor market in which these officers face low costs to relocate.

Overall, Table 3 presents the dynamic relationships between nonprofit fundamentals and financial misconduct finding that organizations accrue lower levels of service revenue and as a result cut employee compensation expenses. Interestingly these post-fraud reductions continue across all revenue and expenses specifications with the exception of contributions and compensation per officer which appear to rebound following financial fraud.

B. *The Effect of Financial Fraud on Retention Rate*

To better understand our compensation results and explore a plausible channel by which compensation declines take place, we additionally study the impact of nonprofit financial misconduct on employee turnover. The data presents a unique opportunity to do this by using the 22 million names of directors, officers, key employees, and highly compensated employees within sample organizations.[16] Using this data, we construct a retention rate by comparing the year-to-year change in the names appearing within Form 990s as directors, officers, etc. for each nonprofit organization. A retention rate of 1 would indicate that every director, officer, key employee, and highly compensated employee within the organization in the prior year remained employed at the

[16] This does not represent all employees, as the majority of employees do not appear by name on most organizations' tax forms. Nevertheless, this allows our study to focus on workers the organization considers and designates as most critical.

organization. A rate of 0 indicates that all of the previous directors, officers, key employees, and highly compensated employees have been dismissed and/or replaced.

Table 4 presents the impact of disclosing financial misconduct on the retention rate within the population of electronically filing organizations. The results vary somewhat by position, though the overall effect of discovering and disclosing crime has a clear negative effect on the retention of critical employees within the organization. As Panel A shows, the retention of key employees, directors, and officers drops between 3 and 4 percent, while retention of the highest compensated employees on the nonprofit payroll does not have a statistically significant relationship with disclosure.[17]

C. *Spillover to Other Nonprofit Organizations*

As an extension to our main analyses, we are also interested in how financial crime at a focal nonprofit organization impacts organizations unaffected by financial crime in the same zip code. To do so we offer Table 5, which provides results for the twelve fundamentals we study for unaffected organizations in the same zip code as organizations with documented financial crime. Here we find that unaffected organizations have increased program revenues (perhaps capturing revenues from criminally involved nonprofits in the area) as well as increased total compensation and total expenses. We interpret this to mean that in areas where financial fraud has been sustained, organizations unaffected by this disruption attract more in program revenues and are in turn able to spend more to retain employees and directors. We also find an increase in total employees and officers at unaffected organizations indicating that nonprofits avoiding financial fraud are able to retain and even hire additional employees and officers. Overall, the revenue increases experienced by nonprofits in the vicinity of a crime-affected nonprofit are consistent with the finding that

[17] It appears that highly compensated employees are, indeed, so critical to the operations of the organization that they generally avoid replacement even after a significant diversion.

affected nonprofits experience a long-term drop in revenues and the associated cutback in employee compensation.

The spillover results are particularly important because they complement our primary findings while avoiding many of the endogeneity concerns of our main analysis. That is, if nonprofits that suffer from crime have some other factor (which controls, fixed effects, and robustness checks fail to neutralize) driving the result then we would not expect to find a positive result for unaffected organizations in the same vicinity. We have no reason to expect any difference between unaffected nonprofits compared to controls with the exception of their vicinity to a major financial crime in a separate organization. As such, the treatment becomes quasi-exogenous for these nonprofit organizations, allowing us to capture effects entirely consistent with a random shock. Moreover, as suggested, the positive spillover effects we observe in nearby uninvolved nonprofits complement the negative effects exhibited by criminally involved nonprofits.

### D. *Organization Type Analyses*

In addition to our main analyses, we are also interested in understanding the impact of financial fraud on the fundamentals of different types of nonprofit organizations. To do so we undertake two separate analyses categorizing organizations by size (large/small) and revenue concentration (commercial/donative).[18] Here we find important results that help explain which type of organizations are driving our overall results. First, presented in Table 6, Panel A, we note that larger nonprofits suffer steeper declines in service revenue (as high as 14.5%), while smaller nonprofits, in Panel C, show no statistically significant effect. On the other hand, smaller nonprofits suffer a significant decline of more than 10% in contributions, while larger nonprofits

[18] We define large as organizations in the top decile of total assets and small as all others. We define commercial following Aggarwal et al. (2012) as organizations with program revenues to total revenues in excess of 90%. We define donative as nonprofits with donations to total revenues greater than 50%.

show no statistically significant impact. The belt-tightening that occurs post- significant diversion also differs based on size: larger nonprofits experience declines in wages as a result of both a reduction in the number of employees (nearly an 8% cut) as well as a reduction in wages per-employee (a 4.5% loss). Compensation for officers does not appear to decrease and neither does the number of officers. For smaller nonprofits, however, the number of overall employees remains the same while wages per employee absorb the entirety of the decrease in compensation. Here both total officer compensation and compensation per officer fall significantly (8.1% and 9.5%, respectively) and at a higher rate than wages for the average employee (-7.8%).

We observe similar differences between commercial (Panels E and F) and donative (Panels G & H) nonprofits. That is, total officer compensation and compensation per officer fall significantly (10% and 9.2%, respectively), exceeding the change in the wages of an average employee (-8.1%). However, it appears that neither group loses employees at a statistically significant rate, so these nonprofits achieve all of their compensation reductions by lowering employee pay rather than eliminating positions. Furthermore, we find that commercial nonprofits, such as hospitals and universities, tend to lose quite a bit of their contributions (-18.5%) while retaining most of their service revenue post- financial misconduct. Because most of their revenue does not derive from contributions, these commercial entities experience a below-average loss of -3.8% in total revenue, while donative nonprofits (which experience a proportionally lower decrease in contributions of 9%) report total revenue losses above the average decline of 6% across our sample.

We also observe similar patterns for employee retention rates at larger, and commercial nonprofits (Panel I) when compared to smaller, and donative organizations (Panel J). Specifically, the pooled retention rates of board members, executives, key employees, and highly compensated employees do not show statistically significant declines for larger and commercial nonprofits. The same is true for retention rates for directors and officers. On the other hand, smaller and donative organizations suffer statistically significant declines in the pooled retention rate and the retention rates of board members and executives. Interestingly, donative organizations (but no others) also suffer statistically significant declines of nearly 11% in key employees, statistically significant at the 5% level (untabulated result). This shows that at the upper levels of the organization, executives and board members for larger nonprofits that base their revenues in services enjoy significantly more job security post-financial diversions than their smaller, donative counterparts.

## VI. Robustness Tests

In addition to our main and supplemental analyses, we also test the robustness of our results to several alternative specifications. First, we confirm the robustness of our results to the temporal placebo test in an effort to rule out the possibility of pre-trends driving our results. Next, we employ propensity score nearest neighbor matching (Table 7) as well as entropy balancing to help identify ideal controls or balance the existing controls for an improved comparison to treated organizations. Additionally, we limit our sample to treated organizations to mitigate omitted variable concerns related to unobservable characteristics unique to criminally effected firms. We also test the robustness of our results to control organizations in the same zip code and counties. Following Sun and Abraham (2020) we additionally test the robustness of our results to implementing the interaction weighted estimator for an event study. Finally, we confirm the consistency of our model results to including a comprehensive set of control variables beyond the year, industry, and organizational fixed effects already included in our main models.

*A. Temporal Placebo Test*

Table 8 presents regressions used to determine whether the results are robust to the temporal placebo test (Meer and West 2015; Manchiraju, Pandey, and Subramanyam 2017). Here we test model (1) with additional regressors: placebo treatment dummy variables at $t - 1$ and $t - 2$. This should simulate the disclosure of major financial misconduct one year or two years prior to its actual occurrence, respectively. If pre-trends drive the results, we would expect to see statistically significant coefficients on one or both placebos.

On the revenue side, in Panel A, there appear to be no notable pre-trend concerns for service revenue, total revenue, or total employee compensation. The coefficients on the placebo regressors lack statistical significance at the 1%, 5%, and even the 10% level. While a lack of pre-trends cannot completely be ruled out, this finding provides confidence in the outcome. In Panel B, we apply the same placebo test to compensation per employee, compensation per officer, and total expenses. All the results pass the placebo test, as the placebo coefficients indicate no statistical significance at any level. In both panels, the sign and magnitude of the treatment coefficient remains approximately the same as in Table 2. There is some variation in the statistical significance of the treatment variables, but this is not unexpected when fitting a model that includes placebo treatment dummy variables for treatments that did not actually take place. Even with the addition of placebos, all dependent variables of interest remain statistically significant with the exception of compensation per officer.

*B. Propensity Score Matching*

Next, because prior regressions used almost the entire population of US electronically filing nonprofits as a control group, it is important to show that the results are persistent when we use only a single nearest neighbor propensity score match as a control for each nonprofit.

Moreover, because the IRS defines significant diversions both proportionally to size (5% of assets or gross receipts) and in absolute terms ($250,000), larger organizations will represent a disproportionate amount of the treated nonprofits. It may also be possible that larger organizations are larger targets for financial crime and engage in more transactions that expose them to internal and external fraud. Nearest neighbor propensity score matching can help ensure that each treated nonprofit is assigned a control nonprofit that resembles it based on a propensity score (Michels 2017).

To address these concerns, we employ a one-to-one propensity score, nearest neighbor matched subsample following Michels (2017). This sample includes all treated nonprofit organizations for which a match could be assigned as well as the nonprofits designated as controls. We drop all treated nonprofits from the sample which did not have a pre-treatment period (137 in total) and then matched the remaining nonprofits to their nearest neighbor by propensity score across all years that the treated organization appeared in the sample. Table 8 contains the results of these regressions, demonstrating consistency with the main results.

Despite decreasing the sample size by a factor of more than 100, the results still indicate statistically significant decreases in nonprofit service revenue, total employee compensation, compensation per employee, total officer compensation, and compensation per officer. The negative relation of crime to contributions observed by prior scholarship now becomes statistically significant (Harris, Petrovits, and Yetman 2023). All coefficients of interest remain approximately the same in magnitude and statistical significance, suggesting that the results are robust to nearest neighbor propensity score matching within the staggered differences-in-differences design.

*C. Entropy Balancing*

Additionally, we employ entropy balancing on factors describing nonprofit size to provide an additional robustness check following Hainmueller (2012) and Hainmueller and Xu (2013). Entropy balancing will help scale up the entire population of control organizations to be approximately similar to the treated organizations across the relevant variables of interest (Hainmueller 2012; Hainmueller and Xu 2013). The factors used to entropy balance include prior year assets and gross receipts, both used by the IRS as measures of size to determine if nonprofits are sufficiently large for a Form 990 filing to be required. We also entropy balance on the number of employees across sample organizations as an additional measure of size. Across all variables, we balance for mean, variance, and skewness and apply the balance weights in our regressions. All the balanced variables are in natural logarithm form and Winsorized at the $1^{st}$ and $99^{th}$ percentile.

For brevity, we do not tabulate these results, however, our findings are consistent with our Table 2 results indicating that financial crime is negatively associated with nonprofit service revenue and employee compensation. Dividing compensation into total compensation, total officer compensation, compensation per employee, and compensation per officer shows that the effect is of the same approximate magnitude and statistical significance as the main results. We believe this provides additional confidence that the effect of financial misconduct is not driven by nonprofit size.

*D. Staggered Differences-in-Differences among the Treated*

Despite the usefulness of nearest neighbor matching and entropy balancing in ensuring similarity between the treated and control groups, these methods can only match on observable characteristics. It is possible that criminally affected nonprofits have unobservable characteristics that lead to significant diversions. These may be characteristics that are not captured by the data, but which are nevertheless related to both crime and the statistically significant changes in nonprofit fundamentals. Matching and entropy balancing cannot account for unobservable variables that might be driving the result precisely because these variables are unavailable to entropy balance or match on. Therefore, it may be helpful to run our model using only nonprofit organizations that become criminally affected at some point.

In untabulated analyses, we re-run our models only among criminally affected nonprofits. Using this subsample, our results for service revenue, employee compensation, officer compensation, compensation per employee, and compensation per officer are of the same approximate magnitude and statistical significance as in prior tables. This provides additional confidence that the results are not driven by some unobserved attributes of criminally affected nonprofits.

*E. Staggered Differences-in-Differences within Zip Codes and Counties*

Based on the distribution of nonprofits across the United States, it may also be useful to run model (1) among only the organizations in the same zip code as the criminally affected organization. Zip code level is more useful than state and county level, since every state and almost every major county containing nonprofit organizations has a criminally affected nonprofit, rendering the regression almost identical to that in Table 2. However, at the zip code level, we can eliminate many dissimilar control nonprofits (cutting the number of observations five-fold) and test the robustness of our results.

Using this truncated sample, we once again find that our variables of interest are of the same sign, approximate magnitude, and statistical significance as in prior tables. That is, our dependent variables have the same relationships to financial crime as in prior regressions: service revenue, total revenue, total compensation, and total expenses remain negative and statistically significant. We find similar results for our panel B response variables, finding negative relationships between compensation per employee, officer, and total officer compensation and financial crime, while failing to find any significant reductions in the number of employees, officers, or volunteers.

We find similar relationships when we include observations in the same county, rather than zip code (though this does not exclude as many observations as the in-same-zip-code sample). This robustness check helps alleviate concerns about geographic dissimilarity between the treated and the untreated firms that are not captured by limiting the regression to the same zip code. The number of observations naturally rises in this approach, but the regression results remain quite similar in statistical and economic significance to the regressions previously presented. This also helps bolster the conclusion that geographic subsets of control firms are sufficient to demonstrate the effect presented in the full sample as well as the matched and entropy-balanced sub-samples.

### *F. Implementing the Interaction Weighted Estimator for an Event Study*

Next, following Sun and Abraham (2020) we implement the interaction weighted estimator for our event study, splitting the coefficients into pre- and post- treatment years to confirm that the results are robust to potential weaknesses in the classic staggered differences-in-differences design. These tests also help identify pre-trends, if any, by expressing the pre-treatment coefficients as far back as seven years prior to a significant diversion. In untabulated analyses we are able to confirm, and even bolster, the results presented in Table 2. We find no statistically significant pre-treatment effect on criminally affected nonprofits with respect to revenues, expenses, and employment, suggesting that pre-trends do not drive the results.

In terms of revenues, we find that in the year when financial crime is discovered and reported, the losses in contribution amounts can be as high as 20%. The subsequent two years manifest similar levels of decline, though the statistical significance varies between 5 and 10 percent, respectively. Subsequent years exhibit mixed negative results with most results being statistically insignificant three or more years after the discovery of crime, which maps almost indistinguishably to the contribution results observed in Table 3. Related to program revenues we find consistent significant post-treatment declines. Specifically, the effect appears to be immediately negative starting with the first year of treatment and becomes statistically significant at traditional levels starting at $t + 3$. The results become statistically significant despite the sample size and show high economic significance. That is, the results become even stronger than in prior tables after implementing the interaction weighted estimator.

On the expenses side, total compensation exhibits post-disclosure effects beginning in the year of disclosure and continuing through the end of the panel. The effects are statistically significant at the 1% level with a magnitude of roughly 10%. Slight pre-trends appear in the data, but these are statistically significant at the ten percent level only and are positive rather than negative. Total expenses also decline at a similar level and show statistical significance in all but one post-treatment year. These results likewise show no pre-trend concerns. When we inspect results concerning wages per employee, we also find significant declines post-treatment, just as in earlier results. Overall, the implementation of the interaction weighted estimator not only addresses potential robustness concerns identified by recent scholarship in the staggered differences-in-differences model but also indicates that the results become more pronounced when the potential weighting issues are alleviated.

### G. *Additional Regressions with an Extensive List of Control Variables*

To test the robustness of our results to including a more comprehensive set of control variables, in untabulated analyses we re-run the regressions presented above employing sixteen different control variables[19] that speak to firms' liquidity, their exposure to certain investment assets, their exposure to market risk, and a host of other variables that may correlate to crime and the dependent variable.[20] We test these regressions in the full sample, in the entropy-balanced sample, and among the zip codes and counties of the criminally involved nonprofit organizations. All of the coefficients remain statistically significant and economically significant at approximately the same levels as in previous models. Finally, following Boland, Harris, Petrovits, and Yetman (2020), we also control for nonprofit governance. Our untabulated results show that the coefficients of interest remain of the same statistical and economic significance.

## VII. Conclusion

Prior studies on nonprofits and crime focus on the relationship between financial misconduct and short-term future contributions to the affected organization. This study examines the effects of crime on an expanded set of fundamentals while also considering the long-term effect of crime. Specifically, we focus on service revenue, compensation, and employment to demonstrate that a variety of previously unexplored fundamentals change within a nonprofit post-disclosure of crime. We find that service revenue of affected nonprofits (which accounts for almost

[19] Specifically, we include the following control variables: assets, the number of employees, liabilities, non-interest bearing cash, savings and temporary cash, pledges receivable, accounts receivable, loans receivable, securities, accounts payable, deferred revenue, bond liabilities, secured mortgages, unsecured loans, and the number of board members.
[20] To ensure that the maximum number of observations remains within these regressions, we add one to the zero observations prior to taking the natural logarithm to ensure that these organization-year observations are not automatically removed from the sample. This allows the regression to encompass the greatest number of firms. The control variables are: assets, the number of employees, liabilities, non-interest bearing cash, savings and temporary cash, pledges receivable, accounts receivable, loans receivable, securities, accounts payable, deferred revenue, bond liabilities, secured mortgages, unsecured loans, and the number of board members.

80% of the average nonprofit's revenue) declines by approximately 12% after a significant diversion compared to the control group.

Along with a decline in service revenue, the average employees and officers of the affected nonprofit experience a decline of approximately 5 to 8% in their compensation. Some of these declines persist across time, even though nonprofits demonstrate some ability to partially recover the cash value of their losses, suggesting that financial crime harms a nonprofit organization well beyond the mere dollar value stolen by the criminal. Interestingly, the leadership of the organization (which may be involved in fraud against the organization) initially suffer significant declines in their compensation post-treatment, just like ordinary employees, but over time, recover from the loss while the ordinary employee continues to suffer from a downward wage spiral. This shows the disparate impact that fraud can have on different agents within the organization while also demonstrating the importance of a longitudinal perspective to better understand the impact of fraud. A variety of robustness tests bolster these findings and add to our collective understanding of the effects of financial crime in the nonprofit sector.

We believe that understanding the cost of nonprofit financial crime is important for future policy on laws regarding civil and criminal recovery. Knowing the true amount of the loss post-crime, and how that loss may manifest over time, may also help nonprofit organizations better appraise the potential effects of financial crime and whether additional internal policies to prevent misconduct are a worthy investment. Given the contribution to local employment and economics nonprofits provide, estimating the focused and spillover effects of crime paints a critical picture of internal and external repercussions in the US nonprofit sector.

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## Table 1: Summary statistics

**Panel A**

| VARIABLES | Mean ($m) | sd | p1 | p25 | p50 | p75 | p99 |
|---|---|---|---|---|---|---|---|
| Contributions | 1.8 | 26.1 | 0 | 0 | 0.1 | 0.5 | 23.4 |
| Service Revenue | 8.0 | 143 | 0 | 0 | 0.1 | 0.7 | 129 |
| Investment Income | 0.5 | 15.5 | 0 | 0 | 0 | 0 | 5.3 |
| Other Revenue | 0.2 | 5.2 | -0.1 | 0 | 0 | 0 | 3.5 |
| Total Revenue | 10.5 | 156 | 0 | 0.2 | 0.5 | 1.9 | 163 |
| Grants Paid | 0.7 | 13.5 | 0 | 0 | 0 | 0 | 9.7 |
| Member Benefits | 0.8 | 34.7 | 0 | 0 | 0 | 0 | 3.9 |
| Total Compensation | 3.8 | 49.1 | 0 | 0 | 0.1 | 0.6 | 56.8 |
| Other Expenses | 4.6 | 103 | 0 | 0.1 | 0.2 | 0.8 | 67.8 |
| Total Expenses | 9.9 | 144 | 0 | 0.2 | 0.5 | 1.8 | 155 |
| Net Revenue | 0.6 | 41.2 | -2.9 | 0 | 0 | 0.1 | 11.7 |
| Assets | 22.2 | 391 | 0 | 0.2 | 0.8 | 3.3 | 337 |
| Liabilities | 10.4 | 238 | 0 | 0 | 0.1 | 0.6 | 154 |
| Net Assets | 11.8 | 238 | -2.3 | 0.1 | 0.5 | 2.1 | 173 |
| Num. of Employees | 91 | 1,765 | 0 | 0 | 3 | 22 | 1,428 |
| Num. of Volunteers | 920 | 144,269 | 0 | 0 | 17 | 80 | 3,600 |
| Num. of Officers | 4 | 8 | 0 | 3 | 4 | 5 | 14 |
| Comp. per Employee | $40,610 | $507,647 | $4,066 | $14,998 | $28,471 | $49,186 | $180,616 |
| Comp. per Officer | $43,770 | $144,407 | $0 | $0 | $2,040 | $36,566 | $544,956 |
| Tot. Officer Comp. | $213,521 | $853,927 | $0 | $0 | $0 | $142,688 | $2,905,097 |

This table presents the raw summary statistics for the model variables. Observations span the entirety of the panel data between the beginning of 2010 and part of 2019, encompassing all online-filed Form 990 tax returns of nonprofit organizations within the United States. Panel A describes the mean, standard deviation, median (p50), and other relevant percentile distributions of the financials within the sample. All values are expressed in millions of dollars, rounded to the nearest $0.1 million, using a maximum of three digits. There are a total of 1,783,822 observations.

**PANEL B**

| VARIABLES | obs. | mean | Sd | p1 | p25 | p50 | p75 | p99 |
|---|---|---|---|---|---|---|---|---|
| Contributions | 1.4 | $2.3 | $29.6 | $0 | $0.1 | $0.2 | $0.8 | $29.4 |
| Service Revenue | 1.2 | $11.7 | $173 | $0 | $0.1 | $0.3 | $1.5 | $205 |
| Investment Income | 1.3 | $0.6 | $17.9 | $0 | $0 | $0 | $0 | $7.6 |
| Other Revenue | 1.0 | $0.5 | $6.9 | $0 | $0 | $0 | $0.1 | $6.6 |
| Total Revenue | 1.8 | $10.6 | $157 | $0 | $0.2 | $0.5 | $2.0 | $165 |
| Grants Paid | 0.5 | $2.3 | $25 | $0 | $0 | $0.1 | $0.4 | $40 |
| Member Benefits | 0.1 | $17.6 | $163 | $0 | $0 | $0.2 | $3 | $270 |
| Total Compensation | 1.3 | $5.4 | $58.5 | $0 | $0.1 | $0.3 | $1.2 | $83.8 |
| Other Expenses | 1.8 | $4.6 | $103 | $0 | $0.1 | $0.2 | $0.8 | $68.8 |
| Total Expenses | 1.8 | $9.9 | $145 | $0 | $0.2 | $0.5 | $1.8 | $156 |
| Net Revenue | 1.1 | $1.4 | $53 | $0 | $0 | $0.1 | $0.3 | $21.2 |
| Assets | 1.8 | $22.4 | $393 | $0 | $0.2 | $0.8 | $3.5 | $342 |
| Liabilities | 1.4 | $13.3 | $270 | $0 | $0 | $0.2 | $1.1 | $204 |
| Net Assets | 1.6 | $13.2 | $248 | $0 | $0.2 | $0.6 | $2.5 | $189 |
| Num. of Employees | 1.1 | 148 | 2,248 | 1 | 5 | 14 | 56 | 2k |
| Num. of Volunteers | 1.0 | 1,245 | 0.2 mil. | 2 | 12 | 36 | 130 | 5k |
| Num. of Officers | 1.5 | 1 | 0.44 | 1 | 6 | 10 | 16 | 51 |
| Comp. per Emp. | 1.1 | $41k | $0.5 mil. | $1,502 | $15k | $29k | $50k | $0.2 mil. |
| Comp. per Officer | 0.8 | $83k | $0.2 mil. | $333 | $15k | $34k | $85k | $0.7 mil. |
| Tot. Off. Comp. | 0.8 | $0.4 mil. | $1.1 mil. | $1,498 | $65k | $0.1 mil. | $0.4 mil. | $4.2 mil. |

Panel B contains additional statistics, with the mean, standard deviation, and percentile distributions expressed as raw numbers rounded to the nearest integer for nonzero values of each variable (i.e. observations included in our models).

**PANEL C**

| Industries | N | % of sample | Number of observations with financial crime |
|---|---|---|---|
| Arts, Culture, and Humanities | 128,792 | 7.22% | 124 |
| Education | 228,345 | 12.80% | 193 |
| Environment and Animals | 64,940 | 3.64% | 60 |
| Health | 230,540 | 12.92% | 245 |
| Human Services | 610,741 | 34.24% | 610 |
| International, Foreign Affairs | 28,272 | 1.58% | 54 |
| Public, Societal Benefit | 317,381 | 17.79% | 363 |
| Religion Related | 69,278 | 3.88% | 90 |
| Mutual/Membership Benefit | 50,725 | 2.84% | 47 |
| Unknown, Unclassified | 54,808 | 3.07% | 111 |
| Total | **1,783,822** | **100.00%** | **1,897** |

Panel C presents industry breakdown of the sample as well as observations reporting a financial crime in the sample.

**PANEL D**

| VARIABLES | Observations | VARIABLES | Observations | VARIABLES | Observations | Mean |
|---|---|---|---|---|---|---|
| Schedules/Disclosures | 744/1,897 | Terminated | 403 | Prison Sentence | 24 | 55 mon. |
| Sought Recovery | 428 | Indictments | 192 | Term of Probation | 10 | 30 mon. |
| Civil Cases | 27 | Convictions | 105 | Amount Lost | 506 | $0.8 mil. |
| Remedial Measures | 221 | **Character of Crime:** | | Average Recovery | 163 | $0.5 mil. |
| Male/Female | 38/56 | Embezzlement | 358 | Restitution Ordered | 19 | $0.2 mil. |
| Board Members | 63 | Theft | 170 | Number of Suspects | 598 | 1.2 |
| Officers | 202 | Fraud | 142 | Crime characteristics are based on information disclosed in Schedule O of the Form 990. | | |
| Employees Involved | 526 | Cyber Fraud | 14 | | | |
| Contractors | 52 | Larceny by Trick | 13 | | | |
| Volunteers | 13 | Burglaries | 6 | | | |

Panel D represents the characteristics of crimes and the offenders as reported on the Schedule O forms filed with the IRS by the affected nonprofits. These characteristics include hand-collected data for a total of 744 Schedules O (out of a possible 1,897 disclosed significant diversions).

## Table 2: Regression results: Nonprofit fundamentals after financial misconduct

This table demonstrates the impact of financial misconduct between 2010 and 2019 on a variety of nonprofit fundamentals by displaying the results of Model (1) with different fundamentals appearing as the dependent variable in each column. We use a staggered differences-in-differences approach, designating an organization as treated if the organization disclosed financial misconduct in the current year or in prior years. We designate the treatment variable as "Crime," equal to 1 in the year of treatment and in each year after treatment for the criminally affected organizations. The variable reads 0 for all untreated nonprofits across time and all treated nonprofits prior to treatment. The coefficient associated with the "Crime" variable designates the staggered differences-in-differences effect of misconduct on the fundamentals of affected nonprofits. All variables except for dummy variables and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. The model absorbs year and organization fixed effects and clusters standard errors at the organization level. We include prior year assets and the number of employees within the nonprofit as controls for size. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level. Standard errors appear in parentheses.

**PANEL A**

| VARIABLES | (1) Total Contributions | (2) Service Revenue | (3) Other Revenue | (4) Total Revenue | (5) Total Compensation | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | -0.04 | -0.12*** | 0.17*** | -0.06*** | -0.08*** | -0.06*** |
| | (0.04) | (0.03) | (0.05) | (0.01) | (0.01) | (0.01) |
| Assets | 0.03*** | 0.15*** | 0.13*** | 0.09*** | 0.16*** | 0.21*** |
| | (0.00) | (0.00) | (0.01) | (0.00) | (0.00) | (0.00) |
| Number of Employees | 0.25*** | 0.31*** | 0.17*** | 0.28*** | 0.43*** | 0.27*** |
| | (0.00) | (0.01) | (0.01) | (0.00) | (0.00) | (0.00) |
| Observations | 873,473 | 833,675 | 673,992 | 1,065,419 | 1,054,367 | 1,066,776 |

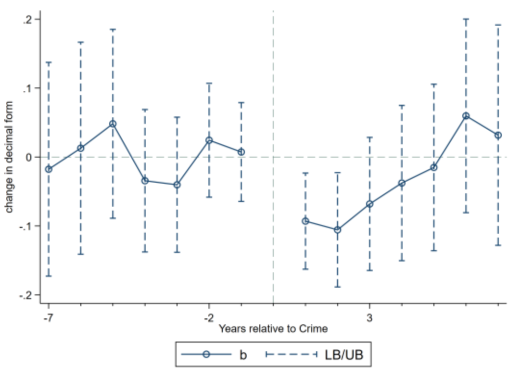
**PANEL B**

| VARIABLES | (1) Wages per Employee | (2) Comp. per Officer | (3) Total Officer Comp. | (4) Number of Employees | (5) Number of Officers | (6) Number of Volunteers |
|---|---|---|---|---|---|---|
| Crime | -0.07*** | -0.05** | -0.06** | -0.02 | 0.00 | 0.02 |
| | (0.01) | (0.03) | (0.02) | (0.01) | (0.02) | (0.04) |
| Assets | 0.15*** | 0.09*** | 0.10*** | 0.18*** | 0.02*** | 0.07*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Number of Employees | -0.53*** | 0.06*** | 0.10*** | | 0.03*** | 0.12*** |
| | (0.00) | (0.00) | (0.00) | | (0.00) | (0.00) |
| Observations | 1,054,367 | 650,539 | 650,539 | 1,067,651 | 1,005,229 | 644,449 |

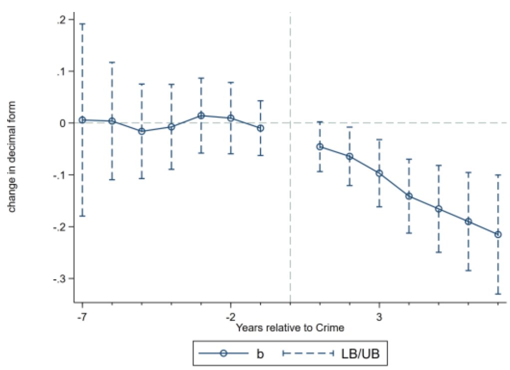
## Table 3: The dynamic relationship of crime to nonprofit fundamentals

This figure plots the relationship of financial crime to nonprofit fundamentals, highlighting the differences between executives and ordinary employees. We consider a 15-year window, spanning from 7 years before a financial crime until seven years after. We include controls for assets, the number of employees, liabilities, non-interest bearing cash, savings and temporary cash, pledges receivable, accounts receivable, loans receivable, securities, accounts payable, deferred revenue, bond liabilities, secured mortgages, unsecured loans, and the number of board members as controls in the regression. We absorb year and organization fixed effects and cluster standard errors at the nonprofit level. All variables except for dummy variables and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. The dashed lines represent 95% confidence intervals, adjusted for organization-level clustering. Estimated coefficients from Regression (2) appear in each figure with its corresponding year relative to disclosure of financial misconduct.

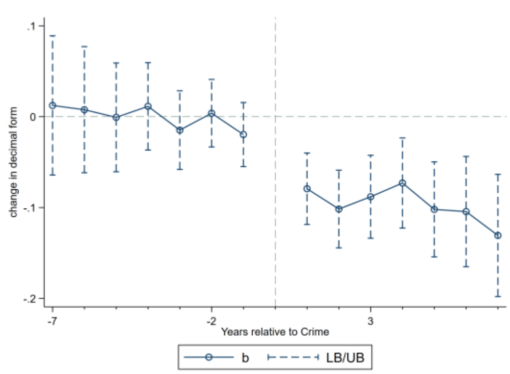
**1. Contributions**



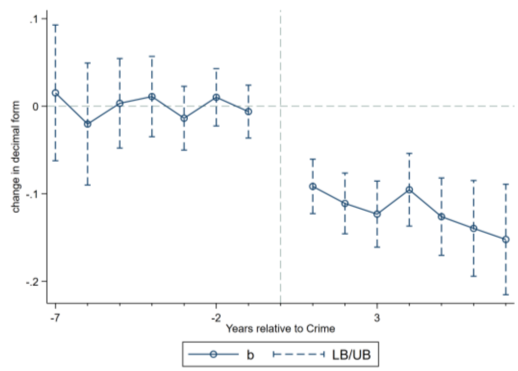

**2. Service Revenue**



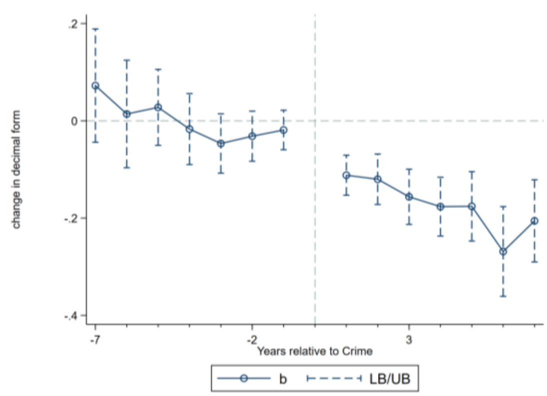

**3. Total Revenue**



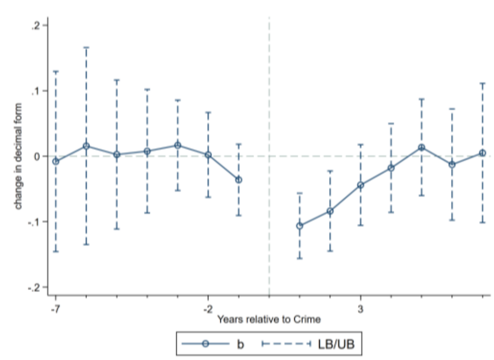

**4. Total Expenses**




**5. Compensation per Employee**




**6. Compensation per Officer**



## Table 4: Nonprofit Retention Rate After Financial Misconduct

This table demonstrates the impact of financial misconduct between 2010 and 2019 on the retention rate within nonprofit organizations by displaying the results of Regression (1) with the different retention rates associated with a variety of key positions within these organizations. We use a staggered differences-in-differences approach, designating an organization as treated if the organization disclosed financial misconduct in the current year or in prior years. We designate the treatment variable as "Crime," which reads 1 in the year of treatment and in each year after treatment for the criminally affected organizations. The variable reads 0 for all untreated nonprofits across time and all treated nonprofits prior to treatment. The coefficient associated with the "Crime" variable designates the staggered differences-in-differences effect of misconduct on the retention rate of affected nonprofits. All variables except for the retention rate, the dummy variables, and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. We absorb year and organization fixed effects and cluster standard errors at the organization level. We include prior year assets and the number of employees within the nonprofit as controls for size. Following conventional practice, we use *, **, and *** to denote statistical significance at the 10%, 5%, and 1% level, in that order. Standard errors appear in parentheses.

| VARIABLES | (1) Retention Rate | (2) Retained Directors | (3) Retained Officers | (4) Key Employees | (5) Highly Compensated |
|---|---|---|---|---|---|
| Crime | -0.03** | -0.03*** | -0.04*** | -0.04* | 0.01 |
| | (0.01) | (0.01) | (0.01) | (0.03) | (0.02) |
| Assets | -0.00 | 0.00 | -0.00 | -0.00 | 0.01* |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Receipts | 0.03*** | 0.03*** | 0.03*** | 0.04*** | 0.04*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Employees | -0.00*** | -0.00 | -0.01*** | -0.01** | -0.02*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Observations | 1,042,942 | 925,897 | 971,987 | 113,064 | 209,022 |

## Table 5: Spillover for Unaffected Organizations in the same Zip Codes

This table displays the spillover effects of one nonprofit disclosing a significant diversion on other nonprofits in the same zip code. The table displays the results of Regression (1) while modifying the treatment variable from "Crime" to "Crime in Zip." The variable reads 1 if a nonprofit shares a zip code with an organization that disclosed a significant diversion in the year of disclosure and every year after. The variable reads zero for every prior year. The variable also reads zero at all times for every nonprofit in a zip code where no significant diversions were reported. We employ a staggered differences-in-differences approach, with the coefficient for "Crime in Zip" designating the effect of financial misconduct on the fundamentals of affected nonprofits. All variables except for dummy variables and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. We absorb year and organization fixed effects and cluster standard errors at the organization level. We include prior year assets and the number of employees within the nonprofit as controls for size. Following conventional practice, we use *, **, and *** to denote statistical significance at the 10%, 5%, and 1% level, in that order. Standard errors appear in parentheses.

**PANEL A**

| VARIABLES | (1) Contributions | (2) Service Revenue | (3) Other Revenue | (4) Total Revenue | (5) Total Comp. | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime in Zip | 0.00 | 0.01*** | 0.01 | 0.01*** | 0.01*** | 0.00* |
| | (0.00) | (0.00) | (0.01) | (0.00) | (0.00) | (0.00) |
| Assets | -0.01*** | 0.07*** | 0.05*** | 0.02*** | 0.07*** | 0.09*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Number of Employees | 0.15*** | 0.17*** | 0.10*** | 0.21*** | 0.25*** | 0.21*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Observations | 1,334,668 | 1,180,946 | 936,308 | 1,712,995 | 1,220,709 | 1,719,191 |

**PANEL B**

| VARIABLES | (1) Wages per Employee | (2) Comp. per Officer | (3) Total Officer Comp. | (4) Number of Employees | (5) Number of Officers | (6) Number of Volunteers |
|---|---|---|---|---|---|---|
| Crime in Zip | 0.01*** | -0.00 | 0.01*** | 0.01*** | 0.01*** | -0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Assets | 0.06*** | 0.03*** | 0.03*** | 0.07*** | 0.00*** | 0.02*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Number of Employees | -0.57*** | 0.03*** | 0.05*** | | 0.03*** | 0.09*** |
| | (0.00) | (0.00) | (0.00) | | (0.00) | (0.00) |
| Observations | 1,053,094 | 809,321 | 809,321 | 1,066,498 | 1,571,731 | 924,823 |

## Table 6: Nonprofit Fundamentals After Financial Misconduct by Size and Revenue Concentration

This table demonstrates the impact of financial misconduct between 2010 and 2019 on nonprofit fundamentals by displaying the results of Regression (1). We separate results for organizations in the top 10% of all observations by size (as indicated by the top 10% of all nonprofits when ranked by assets at the beginning of the sample period) and compare the results to the remaining nonprofit observations. We also divide the samples a different way: into commercial (service revenue makes up 90%+ of total revenue) and donative (contributions make up 50%+ of total revenue) organizations. We use a staggered differences-in-differences approach, designating an organization as treated if the organization disclosed financial misconduct in the current year or in prior years. We designate the treatment variable as "Crime," which reads 1 in the year of treatment and in each year after treatment for the criminally affected organizations. The variable reads 0 for all untreated nonprofits across time and all treated nonprofits prior to treatment. All variables except for the retention rate, the dummy variables, and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. We absorb year and organization fixed effects and cluster standard errors at the organization level. We include prior year assets and the number of employees within the nonprofit as controls for size. Following conventional practice, we use *, **, and *** to denote statistical significance at the 10%, 5%, and 1% level, in that order. Standard errors appear in parentheses.

**PANEL A: LARGE NONPROFITS**

| VARIABLES | (1) Total Contributions | (2) Service Revenue | (3) Total Revenue | (4) Total Comp. | (5) Other Expenses | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | 0.07 | -0.15*** | -0.13*** | -0.11*** | -0.13*** | -0.15*** |
| | (0.09) | (0.05) | (0.03) | (0.02) | (0.03) | (0.03) |
| Observations | 74,730 | 96,856 | 110,452 | 98,740 | 110,110 | 110,419 |

**PANEL B: LARGE NONPROFITS**

| VARIABLES | (1) Number of Employees | (2) Number of Volunteers | (3) Number of Officers | (4) Wages per Employee | (5) Comp. per Officer | (6) Tot. Off. Compensation |
|---|---|---|---|---|---|---|
| Crime | -0.08*** | -0.03 | 0.00 | -0.05** | -0.01 | -0.03 |
| | (0.02) | (0.06) | (0.03) | (0.02) | (0.04) | (0.03) |
| Observations | 92,772 | 76,748 | 104,692 | 91,961 | 92,856 | 92,856 |

**PANEL C: SMALLER NONPROFITS**

| VARIABLES | (1) Total Contributions | (2) Service Revenue | (3) Total Revenue | (4) Total Comp. | (5) Other Expenses | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | -0.11*** | -0.05 | -0.05*** | -0.07*** | -0.04* | -0.06*** |
| | (0.04) | (0.03) | (0.02) | (0.02) | (0.02) | (0.02) |
| Observations | 1,247,216 | 1,071,039 | 1,584,817 | 1,108,926 | 1,583,413 | 1,590,999 |

**PANEL D: SMALLER NONPROFITS**

| VARIABLES | (1) Number of Employees | (2) Number of Volunteers | (3) Number of Officers | (4) Wages per Employee | (5) Comp. per Officer | (6) Tot. Off. Compensation |
|---|---|---|---|---|---|---|
| Crime | 0.004 | 0.01 | 0.00 | -0.08*** | -0.10*** | -0.08*** |
| | (0.02) | (0.04) | (0.02) | (0.02) | (0.03) | (0.03) |
| Observations | 962,988 | 840,273 | 1,452,022 | 950,707 | 710,417 | 710,417 |

**PANEL E: COMMERCIAL NONPROFITS**

| VARIABLES | (1) Total Contributions | (2) Service Revenue | (3) Total Revenue | (4) Total Comp. | (5) Other Expenses | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | -0.19** | -0.03* | -0.04** | -0.06** | -0.00 | -0.03 |
| | (0.07) | (0.02) | (0.02) | (0.03) | (0.03) | (0.02) |
| Observations | 158,222 | 406,898 | 406,898 | 306,139 | 404,902 | 406,447 |

**PANEL F: COMMERCIAL NONPROFITS**

| VARIABLES | (1) Number of Employees | (2) Number of Volunteers | (3) Number of Officers | (4) Wages per Employee | (5) Comp. per Officer | (6) Tot. Off. Compensation |
|---|---|---|---|---|---|---|
| Crime | 0.01 | 0.00 | -0.00 | -0.07*** | -0.01 | -0.02 |
| | (0.02) | (0.05) | (0.03) | (0.02) | (0.04) | (0.04) |
| Observations | 262,112 | 174,144 | 362,919 | 258,958 | 215,581 | 215,581 |

**PANEL G: DONATIVE NONPROFITS**

| VARIABLES | (1) Total Contributions | (2) Service Revenue | (3) Total Revenue | (4) Total Comp. | (5) Other Expenses | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | -0.09*** | -0.07 | -0.08*** | -0.07** | -0.06** | -0.09*** |
| | (0.03) | (0.07) | (0.03) | (0.03) | (0.03) | (0.03) |
| Observations | 732,845 | 341,212 | 732,845 | 522,480 | 728,611 | 731,471 |

**PANEL H: DONATIVE NONPROFITS**

| VARIABLES | (1) Number of Employees | (2) Number of Volunteers | (3) Number of Officers | (4) Wages per Employee | (5) Comp. per Officer | (6) Tot. Off. Compensation |
|---|---|---|---|---|---|---|
| Crime | -0.01 | 0.04 | 0.01 | -0.08*** | -0.09** | -0.10** |
| | (0.02) | (0.06) | (0.02) | (0.02) | (0.04) | (0.04) |
| Observations | 458,565 | 437,459 | 675,713 | 455,506 | 341,080 | 341,080 |

**PANEL I: RETENTION RATES AT LARGE VS SMALL NONPROFITS**

| VARIABLES | (1) Large Org. Ret. Rate | (2) Large Org. Dir. Ret. Rate | (3) Large Org. Off. Ret. Rate. | (4) Small Org. Ret. Rate | (5) Small Org. Dir. Ret. Rate | (6) Small Org. Off. Ret. Rate. |
|---|---|---|---|---|---|---|
| Crime | -0.02 | -0.02 | -0.02 | -0.03*** | -0.03** | -0.03*** |
| | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) |
| Observations | 107,247 | 103,271 | 100,489 | 1,556,365 | 1,281,881 | 1,407,158 |

**PANEL J: RETENTION RATES AT COMMERCIAL VS DONATIVE NONPROFITS**

| VARIABLES | (1) Comm. Ret. Rate | (2) Comm. Dir. Ret. Rate | (3) Comm. Off. Ret. Rate. | (4) Don. Ret. Rate | (5) Don. Dir. Ret. Rate | (6) Don. Off. Ret. Rate. |
|---|---|---|---|---|---|---|
| Crime | -0.02 | -0.02 | -0.01 | -0.04*** | -0.03* | -0.05*** |
| | (0.02) | (0.02) | (0.02) | (0.01) | (0.02) | (0.02) |
| Observations | 458,565 | 437,459 | 675,713 | 455,506 | 341,080 | 341,080 |

## Table 7: Nonprofit fundamentals after financial misconduct with placebos

This table contains the results of the same regression as in Table 2 (Regression (1)) but with one-year and two-year placebos of the treatment included in the regression at *t - 1* and *t - 2*, respectively. We use a staggered differences-in-differences approach, designating an organization as treated if the organization disclosed financial misconduct in the current year or in prior years. We designate the treatment variable as "Crime," which reads 1 in the year of treatment and in each year after treatment for the criminally affected organizations. The variable reads 0 for all untreated nonprofits across time and all treated nonprofits prior to treatment. The coefficient associated with the "Crime" variable designates the staggered differences-in-differences effect of misconduct on the fundamentals of affected nonprofits. All variables except for dummy variables and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. We absorb year and organization fixed effects and cluster standard errors at the organization level. We include prior year assets and the number of employees within the nonprofit as controls for size. Following conventional practice, we use *, **, and *** to denote statistical significance at the 10%, 5%, and 1% level, in that order. Standard errors appear in parentheses.

**PANEL A**

| VARIABLES | (1) Total Contributions | (2) Service Revenue | (3) Other Revenue | (4) Total Revenue | (5) Total Comp. | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | -0.00 | -0.12*** | 0.19** | -0.06*** | -0.06*** | -0.06*** |
| | (0.05) | (0.04) | (0.09) | (0.02) | (0.02) | (0.02) |
| Placebo at *t - 1* | 0.01 | -0.03 | -0.02 | -0.04** | 0.00 | -0.01 |
| | (0.04) | (0.03) | (0.07) | (0.02) | (0.02) | (0.01) |
| Placebo at *t - 2* | 0.06 | -0.00 | -0.09 | -0.01 | -0.02 | -0.00 |
| | (0.04) | (0.02) | (0.07) | (0.01) | (0.01) | (0.01) |
| Assets | -0.01*** | 0.13*** | 0.11*** | 0.05*** | 0.14*** | 0.19*** |
| | (0.00) | (0.00) | (0.01) | (0.00) | (0.00) | (0.00) |
| Number of Employees | 0.20*** | 0.25*** | 0.14*** | 0.22*** | 0.36*** | 0.23*** |
| | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) | (0.00) |
| Observations | 542,824 | 531,503 | 428,979 | 668,498 | 661,709 | 669,111 |

**PANEL B**

| VARIABLES | (1) Wages per Employee | (2) Comp. per Officer | (3) Total Officer Comp. | (4) Number of Employees | (5) Number of Officers | (6) Number of Volunteers |
|---|---|---|---|---|---|---|
| Crime | -0.05*** | -0.01 | -0.06* | -0.00 | -0.02 | 0.01 |
| | (0.02) | (0.04) | (0.03) | (0.02) | (0.02) | (0.06) |
| Placebo at *t - 1* | 0.00 | -0.04 | -0.05* | 0.00 | -0.01 | -0.01 |
| | (0.01) | (0.03) | (0.03) | (0.02) | (0.02) | (0.04) |
| Placebo at *t - 2* | -0.01 | -0.01 | 0.00 | 0.01 | 0.01 | -0.07* |
| | (0.01) | (0.03) | (0.03) | (0.02) | (0.02) | (0.04) |
| Assets | 0.14*** | 0.08*** | 0.09*** | 0.16*** | 0.01*** | 0.06*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Number of Employees | -0.59*** | 0.04*** | 0.08*** | | 0.03*** | 0.09*** |
| | (0.00) | (0.00) | (0.00) | | (0.00) | (0.01) |
| Observations | 661,709 | 415,114 | 415,114 | 669,603 | 629,728 | 401,703 |

## Table 8: Nonprofit fundamentals after financial misconduct with nearest neighbor matching

This table contains the results of the same regression as in Table 2 (Regression (1)) but including only the nearest neighbor matches to the treated nonprofits as the controls. I use a staggered differences-in-differences approach, designating an organization as treated if the organization disclosed financial misconduct in the current year or in prior years. I designate the treatment variable as "Crime," which reads 1 in the year of treatment and in each year after treatment for the criminally affected nonprofits. The variable reads 0 for all untreated nonprofits and all treated nonprofits prior to treatment. The coefficient associated with the "Crime" variable designates the staggered differences-in-differences effect of misconduct on the fundamentals of affected nonprofits. All variables except for dummy variables and fixed effects are in natural logarithm form and Winsorized at the 1st and 99th percentile. I absorb year and organization fixed effects and cluster standard errors at the organization level. I include prior year assets and the number of employees within the nonprofit as controls for size. Following conventional practice, I use *, **, and *** to denote statistical significance at the 10%, 5%, and 1% level, in that order. Standard errors appear in parentheses.

**PANEL A**

| VARIABLES | (1) Contributions | (2) Service Revenue | (3) Investment Income | (4) Other Revenue | (5) Total Revenue |
|---|---|---|---|---|---|
| Crime | -0.09** | -0.06** | -0.09 | 0.16** | -0.026* |
| | (0.04) | (0.03) | (0.07) | (0.06) | (0.015) |
| Assets | 0.05 | 0.14*** | 0.79*** | 0.18*** | 0.08*** |
| | (0.04) | (0.04) | (0.08) | (0.07) | (0.02) |
| Number of Employees | 0.31*** | 0.34*** | 0.04 | 0.17** | 0.31*** |
| | (0.05) | (0.05) | (0.08) | (0.07) | (0.03) |
| Observations | 8,015 | 8,085 | 8,053 | 6,932 | 10,039 |

**PANEL B**

| VARIABLES | (1) Grants Paid | (2) Member Benefits | (3) Total Comp. | (4) Fundraising Fees | (5) Other Expenses | (6) Total Expenses |
|---|---|---|---|---|---|---|
| Crime | 0.01 | 0.03 | -0.05*** | 0.01 | -0.01 | -0.03** |
| | (0.06) | (0.15) | (0.01) | (0.13) | (0.02) | (0.01) |
| Assets | 0.30*** | 0.25 | 0.14*** | 0.02 | 0.23*** | 0.20*** |
| | (0.08) | (0.20) | (0.02) | (0.14) | (0.02) | (0.02) |
| Number of Employees | 0.10 | -0.05 | 0.47*** | 0.08 | 0.23*** | 0.30*** |
| | (0.08) | (0.18) | (0.04) | (0.20) | (0.03) | (0.02) |
| Observations | 3,420 | 425 | 9,943 | 672 | 10,047 | 10,054 |

**PANEL C**

| VARIABLES | (1) Number of Employees | (2) Number of Volunteers | (3) Number of Officers | (4) Comp. per Employee | (5) Comp. per Officer | (6) Total Officer Comp. |
|---|---|---|---|---|---|---|
| Crime | 0.02 | 0.05 | -0.01 | -0.06*** | -0.06** | -0.06** |
| | (0.02) | (0.04) | (0.02) | (0.01) | (0.03) | (0.03) |
| Assets | 0.16*** | 0.05 | 0.01 | 0.13*** | 0.05** | 0.06** |
| | (0.02) | (0.04) | (0.02) | (0.02) | (0.03) | (0.03) |
| Number of Employees | | 0.18*** | 0.04** | -0.50*** | 0.08** | 0.15*** |
| | | (0.05) | (0.02) | (0.03) | (0.04) | (0.04) |
| Observations | 10,056 | 6,441 | 9,611 | 9,943 | 6,846 | 6,846 |