The Economic Consequences of Financial Audit Regulation in the Charitable Sector[*]

Raphael Duguay
Booth School of Business, University of Chicago

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Abstract

I investigate how financial audit regulation in the charitable sector affects donor behavior. I propose that audit mandates alleviate moral-hazard concerns by (1) reinforcing donors' belief that charities are monitored and (2) committing charities to obtain an audit ex post. My empirical strategy relies on variation in size-based exemption thresholds across states and differences in size driven by the nature of charities' activities. Consistent with audit mandates reducing donors' reliance on charity reputation, I find that donations are less concentrated on large, high-reputation charities. I show this reallocation of resources allows the charitable sector to serve more diverse geographic areas and social needs. In terms of the effect on willingness to give, I document that audit mandates are associated with a higher proportion of taxpayers who donate, especially among people with a high opportunity cost of time. However, I only observe a sizable impact on total contributions in dollars for charities that conduct activities that are particularly opaque to outside donors. Collectively, these results suggest audit regulation reduces information frictions and thereby affects resource allocation in the market for charitable giving.

Keywords: Financial-reporting regulation, auditing, resource allocation, nonprofit organizations

JEL Codes: M42, M48, M49, L31, L38

1. Introduction

Financial audit regulations lie at the heart of major policy debates. Concerns range from the scope of mandatory audit engagements (e.g., imposing internal-control attestations on public companies) to the requirement to obtain an audit in the first place (e.g., the audit mandate for European limited-liability companies). Regulation mandating that *charities* have their financial statements audited is also contentious. On one hand, such audit mandates could solve potential market failures or externality issues. For example, they could help regulators identify illegitimate charities that prey on naïve or apathetic donors and squander taxpayers' money by taking advantage of tax subsidies. On the other hand, mandatory audits can represent a financial and administrative burden for charities. In the mid-2000s, the Senate proposed a reform that would have required charities throughout the U.S. to obtain an audit (U.S. Senate 2005). Various parties, including representatives of the nonprofit sector, were divided in their view of the proposal. In the end, the federal government did not pass the nationwide audit mandate and instead continued to allow the states to decide whether to impose such regulation.[1]

In this paper, I evaluate the economic consequences of audit regulation in the charitable sector by exploiting variation in size-based exemption thresholds across states and differences in size driven by the nature of charities' activities. The entities I study are public charities, which are tax exempt under section 501(c)3 of the Internal Revenue Code. These organizations rely on financial support from the public to fulfill their mission.[2] Charities commonly draw their funding from a dispersed pool of donors dealing at arm's length. The separation between the charity

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[1] The U.S. is not an isolated case. Australia, Canada, and the U.K. also had recent debates over nonprofit audit regulation.

[2] Examples of missions pursued by public charities include providing relief to the poor, the distressed, or the underprivileged, preventing cruelty to children or animals, lessening the burdens of government, etc. (IRS 2018a). Organizations in the healthcare and education sectors are exempted from the state-level regulations studied in this paper, because they are subject to industry-specific audit mandates (Neely 2011, Desai and Yetman 2015).

managers and the donors gives rise to an agency tension, which is especially important for smaller, lesser-known charities.

I propose that audit mandates alleviate donors' moral-hazard concerns for two reasons. First, they engender the belief among donors that charities are better monitored by outside parties such as regulators, watchdogs, and the press.[3] Audited financial statements facilitate oversight by providing reliable information on whether donated funds are spent toward charitable programs (AICPA 2017, FASB 2018). Many donors rely on third-party oversight in part because performing their own due diligence would be too time consuming. Second, audit mandates *commit* charities to provide audited financial statements ex post. Charities can promise to undergo an audit at the end of the period, but renege ex post by blaming unforeseen circumstances (e.g., unmet fundraising targets). Knowing the true reason for the forgone audit then becomes difficult for donors. Audit mandates guarantee that charities will obtain an audit and thereby provide a reliable account of how they spent donated funds.

Through these mechanisms, I predict that audit mandates substitute for charities' own reputations, which should affect how donors allocate their contributions among charities, and raise individuals' willingness to give. I test this prediction empirically by analyzing a comprehensive sample of public charities in the U.S. This sample offers variation in the existence and scope of audit regulations across states.

Consistent with donors relying less on charities' reputation, I find that audit mandates are associated with a lower concentration of donations on large, high-reputation charities. The effect is muted (more pronounced) for types of charitable activity in which information asymmetry is

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[3] This explanation is consistent with motives cited by policymakers. For example, Hawaiian legislators state that requiring charities to file audited financial statements would "help enforcement officials spot red flags" and would help donors "find out if an organization is a legitimate charity" (Hawaii Senate 2008).

low (high), suggesting audit mandates alleviate moral-hazard concerns.[4] I show this reallocation of donations allows the charitable sector to serve more diverse geographic areas and social needs. In terms of individuals' willingness to give, I document that audit mandates are associated with a higher proportion of taxpayers who give. The effect is more pronounced among individuals who face a high opportunity cost of time. I only find that total donations in dollars increase for types of charitable activity in which information asymmetry is high (i.e., I find no effect for other types of charities).

Investigating the consequences of nonprofit audit regulation is important for several reasons. First, the nonprofit sector is economically significant. The U.S. counts approximately 300,000 public charities that collectively receive over $1.7 trillion in annual income (McKeever 2015). Charitable donations alone amount to 2% of the gross domestic product (List 2011). Furthermore, approximately 10% of employed Americans work for nonprofit organizations (Blackwood et al. 2008). In addition to the sheer size of aggregate donations, the practice of giving to charities is prevalent among households: between 67% and 89% of U.S. households do so in given year (Sullivan 2002, List 2011). Moreover, traditional monetary measures understate the economic importance of the charitable sector, because they fail to capture the economic growth and productivity improvement that stem from addressing social issues.

In addition, the policy debate over nonprofit audit regulation is relevant to researchers because it aims to address a plausible externality problem. That is, a negative externality arises when misbehavior by unscrupulous charity managers shatters the population's trust in the charitable sector. Fraud scandals can lead people to give less or alter how they allocate their

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[4] A social club is an example of a low information asymmetry charity because the donors tend to be the beneficiaries. On the other hand, a charity that conducts its programs abroad would involve high information asymmetry. Refer to section 4.2 for a more detailed discussion.

donations (Giannetti and Wang 2016, Gurun et al. 2017). Regulatory action may help contain this externality issue if it mitigates agency frictions that would remain unresolved in an unregulated regime. The agency problem is indeed especially difficult to address in the charitable sector (Core et al. 2006). For instance, private litigation is not a prevalent disciplining mechanism, because entering into formal private contracts with small, dispersed donors is generally unfeasible for charities. Similarly, class-action lawsuits against charities are nearly inexistent because donors rarely incur the damage from misappropriation of donated funds (the beneficiaries do). In addition, voluntary audited reporting may have weaker signaling power because many individuals may not spend the time to consult charities' financial filings to determine (or verify) whether they are audited. Given these frictions, positive research can shed light on whether audit mandates effectively mitigate the agency problem and address the potential externality issue.

However, investigating the effect of audit regulation on donor behavior is empirically challenging. First, the timing of the enactment of audit mandates is likely endogenous. Audit regulation often follows fraud scandals (Leuz and Wysocki 2016, Hail et al. 2018). Such scandals would likely trigger a response on behalf of charities and enforcement officials even if no regulation were passed (e.g., charities may provide more voluntarily disclosures) (Ball 1980). Second, the point in time at which the effects of audit mandates should manifest is uncertain. Charities report their financial information yearly and often take months to do so. Nonprofit regulators may also take time to enforce compliance of a newly passed audit mandate. Third, the majority of the states that passed audit mandates did so before the beginning of my sample period.

Given the importance of these concerns in my setting, I turn to a research design that exploits cross-sectional variation, as opposed to a traditional difference-in-differences approach (Currie and Gruber 1996, Rajan and Zingales 1998, Mahoney 2015, Breuer 2018). I exploit the

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fact that states with audit mandates have different size-based exemption thresholds and, in the nonprofit setting, certain states do not impose any audit requirement. In addition, I rely on variation in charity type: charitable activities that, for operational reasons, require a larger scale are more likely to trigger the audit requirement.[5]

Another consideration is that the economic consequences studied in this paper involve distinguishing between the effect on the allocation of donations and the effect on total donations. I therefore use an aggregate approach to perform the empirical analyses. Specifically, I aggregate the observations by state and by type of charitable activity for each year. I measure the treatment as the percentage of charities that are subject to a mandatory audit for a given state-activity-year.

I mitigate the endogeneity concern at the state or activity level by including state-year and activity-year fixed effects. For example, the within-state design controls for differences in wealth, culture, social needs, and operating costs across states that may simultaneously influence donor behavior and regulators' decision to pass audit mandates (and set the exemption threshold at a certain level). Similarly, the activity-year fixed effects control for donors' general preferences for various types of charities. For instance, charities that operate in certain domains may receive more donations on average and thus be more likely to exceed the exemption threshold (i.e., a case of reverse causality). Finally, I use a simulated instrumental variable to alleviate concerns over confounders at the state-activity level (Currie and Gruber 1996, Mahoney 2015, and Breuer 2018) (refer to section 3 for a detailed explanation of the instrument).

Admittedly, my research design cannot rule out all potential violations of the identifying assumption. For instance, such violation would arise if, upon exceeding the exemption threshold,

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[5] I use the terms "type of charitable activity," "charity type," and "activity" interchangeably to refer to industries in the nonprofit sector. Each charity is assigned to a type of charitable activity based on the National Taxonomy of Exempt Entities.

charities were required not only to obtain an audit but also to implement other practices (e.g., an independent board of directors). I carefully read the law and find that the cutoffs do *not* trigger regulatory requirements other than a mandatory audit. In section 3, I discuss other scenarios that could lead to a violation of the identifying assumption and I explain, based on institutional details, why I believe they are unlikely.

Finally, I provide additional discussions and analyses to support the credibility of the proposed mechanism. First, I discuss various ways in which individuals may become aware of the audit regulation. For example, the press sporadically mentions the audit mandates. Similarly, state regulators disseminate information about the mandates via their website. I also explain how individuals may be "indirectly" aware of the audit mandate but still respond to it. Second, I show that mandatory audits can enable outside parties such as regulators and intermediaries to more easily separate charities based on their financial reports. The idea is that audit mandates should prevent charities from pooling together and uniformly reporting that a high (and potentially inflated) percentage of their spending is directed toward the charitable programs. Specifically, I show that under broader audit mandates, the program ratio (excluding audit fees) is more dispersed and lower on average.[6] Third, I provide evidence consistent with the *belief* that audit mandates can detect fraud among charities. Drawing on institutional features, I show that some nonprofit organizations, potentially those more likely to engage in illegitimate spending, make legal-form choices consistent with reducing the risk of being detected by audit regulation, at the expense of tax privileges.

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[6] The program ratio captures the share of the spending allocated toward charitable programs as opposed to administration, overhead, fundraising, perquisite consumption, etc. (Core et al. 2006, Hofmann and McSwain 2013, Arya and Mittendorf 2016).

My paper makes several contributions to the literature. I add to the literatures on auditing (Defond and Zhang 2014) and financial-reporting regulation (Leuz and Wysocki 2016). This study is the first to evaluate the economic consequences of nonprofit audit regulation specifically. Such mandates are common in the U.S. and in other countries, but little is known about their effects and the mechanisms by which they operate. On a more general level, I show that audit regulation can substitute for organizations' own reputation, which is a novel result.

In addition, my findings contrast with those of Breuer (2018), who documents that reporting regulation, but not audit regulation, affects resource allocation for limited-liability firms in Europe. He finds that mandatory audits have little effect beyond representing a fixed cost for these private firms. However, in the nonprofit setting, I show that audit mandates can affect the allocation of resources. Our findings can be reconciled by considering how the economic forces and institutions differ between the two sectors of the economy we study. For example, shareholders of private firms (but generally not small dispersed donors dealing at arm's length) can have recourse to litigation or insider access. An audit mandate is therefore less likely to affect them. Hence, our papers shed light on the institutional characteristics that influence the effectiveness of financial-reporting regulation not only across jurisdictions (Leuz 2010), but also across sectors of the economy (e.g., public-equity market vs. private-equity market vs. charitable sector).

I also contribute to recent work that evaluates the determinants of household participation in various markets. Giannetti and Wang (2016) and Gurun et al. (2017) find that households decrease their participation in securities market in response to fraud. Christensen et al. (2017) find that securities regulation increases household equity ownership and potentially stock market participation when trust in others is low. I show that *audit regulation* can induce participation in the market for charitable giving.

Finally, I add to the nonprofit literature by extending our knowledge of the determinants of the allocation of donations among charities.[7] Most prior studies focus on the relation between donations and *voluntary* decisions made by individual charities (e.g., spending on fundraising, implementing governance mechanisms, offering high compensation to the CEO, etc.). My paper shows that *mandatory* audited reporting plays a role in the allocation of donations. I also contribute to the stream of literature on nonprofit regulation (Neely 2011, Desai and Yetman 2015). Whereas prior studies evaluate broad sets of regulations (e.g., the Californian Nonprofit Integrity Act), my paper isolates the economic consequences of audit mandates from other regulatory provisions.

The scope of this paper does not extend to the costs of the regulation. I focus on the effects on donor behavior because they are a priori ambiguous. In fact, policymakers are rarely explicit about the outcomes and the underlying economic mechanisms they have in mind when passing audit regulation in the charitable sector (AASB 2017). The main costs, on the other hand, are more straightforward and little disagreement exists over their nature.[8] Nevertheless, given that I find no increase in total donations on average, the audit mandates likely result in a net outflow of financial resources from the charitable sector (i.e., after considering audit fees). I do not evaluate whether the potential benefits of the regulation justify the costs.

2. Conceptual Underpinnings, Underlying Assumptions, Audit Requirements for Charities

2.1 Conceptual Underpinnings

A key reason people give to charity is to contribute to a public good (Hochman and Rodgers 1969, Kolm 1969, Andreoni 2006). Through economies of scale, charitable organizations can be

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[7] Among others, see Weisbrod and Dominguez (1986), Posnett and Sandler (1989), Tinkelman (1999), Kitching (2009), Thornton and Belski (2010), Petrovits et al. (2011), Balsam and Harris (2014), and Harris et al. (2015).

[8] The National Council of Nonprofits estimates that a financial audit costs between $10,000 and $20,000 for organizations with revenue of $1 million (NCN 2017). I arrive at a similar estimate: charities of that size in my sample report average fees for accounting and audit services of $14,100. There is also broad acceptance of the idea that audits can be costly in terms of time from managers and directors (McWhirter 2014).

more efficient than individuals acting independently to address social needs (Coase 1937).

However, delegating the production of public goods to organizations gives rise to agency problems

(Jensen and Meckling 1976, Fama and Jensen 1983, Core et al. 2006). Donors are concerned that

charity managers will extract private benefits and not use the contributed funds toward the intended

purpose. Small and dispersed donors dealing at arm's length usually cannot solve information

asymmetry through insider access or private contracting.[9] Furthermore, alternative means of

addressing the information problem (e.g., voluntarily providing audited financial reports or other

types of disclosures) may have limited effectiveness because the opportunity cost of time is a key

friction in the context of charitable giving (Andreoni 2006, DellaVigna et al. 2012, Karlan and List

2018). In other words, for many individuals, it is prohibitively costly to consult charities' financial

filings (or other types of voluntary disclosures) to determine which organizations report

information that reflects efficient and legitimate operations (Zingales 2012).

These information frictions are especially important for smaller lesser-known charities.

Organizations that lack a longstanding reputation are subject to less scrutiny and cannot

demonstrate their legitimacy through years of existence. At the same time, many individuals likely

believe that smaller specialized charities play a distinct role in generating social good and thus

deserve some of their donations (Borgloh et al. 2013). Unresolved moral-hazard concerns may

therefore cause donors to concentrate their donations on large high-reputation charities.

However, an audit mandate can reduce the moral-hazard problem because it facilitates

monitoring by the regulators, the press, the watchdogs, and the board of directors. The belief that

charities are better monitored can induce individuals to give to lesser-known charities or even to

start donating at all. For example, mandatory audits may help the government identify and

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[9] Major contributors, on the other hand, can obtain insider access and enter into private contracts. I do not expect audit mandates to have a sizeable impact on large donors.

terminate illegitimate charities. Audit regulation can also help nonprofit watchdogs or the press identify the worst-managed charities and disseminate that information through donor advisory lists or press articles, allowing donors to find out, at nearly no cost, which charities to be wary of.

Audit mandates facilitate monitoring because they curtail management's ability to misreport in order to conceal private-benefit extraction (Sunder 2016). For instance, misreporting may consist of inflating the reported program ratio, which is commonly measured as the ratio of expenses allocated toward charitable programs relative to administration, overhead, fundraising, perquisite consumption, etc. (Core et al. 2006, Hofmann and McSwain 2013, Arya and Mittendorf 2016). Audit mandates should prevent such behavior if auditors verify that the classification of expenses conforms to GAAP's guidance on nonprofit accounting (Parsons 2003, AICPA 2017, Crosson and Thompson 2017, FASB 2018).

Mandatory audits also convey information about whether *restricted* donations were spent as promised. Examples of restricted donations include child sponsorship and gifts directed toward specific projects. GAAP requires that the constraint involved with accepting such donations be reflected in the financial statements by classifying a portion of the net assets as restricted (FASB 2018). As long as a charity has not spent funds toward the intended purpose of the donation, it must continue to report restricted net assets year after year. To ensure compliance with GAAP, auditors search for the existence of restricted donations and track their fulfillment (AICPA 2017). Donors may then rely on third parties (e.g., regulators, watchdogs, the press) to use the audited information to carry out ex-post monitoring on their behalf. Furthermore, certain donors (i.e., those who do not face a high opportunity cost of time) may use the audited information to directly monitor charities and signal irregularities to the authorities.

In addition to helping various parties perform their monitoring role, audit mandates commit charities to obtain an audit ex post. Entering into private contracts with a charity in order to ensure it will undergo an audit is generally unfeasible for small dispersed donors. A charity that claims it will obtain an audit at the end of the period might renege ex post, blaming an unforeseen circumstance. Knowing whether the charity managers are telling the truth or whether they are foregoing the audit to conceal private-benefit extraction then becomes difficult for donors. In addition, many donors may not spend the time ex post to verify whether a charity was audited as promised. This commitment problem is especially salient for younger, lower-reputation charities. These organizations have not been in existence for many years. They cannot point to the fact that they have been providing audited financial statements year after year, which would otherwise help them signal their intention to continue to do so. The audit mandate assures donors that the charity will undergo the audit. Donors may also believe that outside parties will take action against the charity on their behalf should the audit reveal flagrant mismanagement.

The commitment role of audit mandates also applies to the lifecycle of charities. A financial audit contains an important fixed-cost component. Both charity managers and donors may agree that an audit can be disproportionally costly for a young, growing organization. In fact, regulators recognize it as well because all state-level audit mandates provide exemptions for smaller charities. At the same time, donors may be reluctant to give to young growing charities, unless they know these organizations will be forced to obtain an audit once they become larger. A mandate can solve the commitment issue by exempting charities from the audit requirement until they reach a certain size.

Through these mechanisms, I predict that individuals respond to audit mandates by giving more and by reducing the extent to which they concentrate their donations on large high-reputation

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charities. I further predict that this reallocation of donations allows the charitable sector to serve more diverse geographic areas and social needs. The idea is that the most well-known charities tend to be located in large cities, which is also where they tend to run their charitable programs. Similarly, the largest and most mature charities tend to cater to the few types of social needs that most rely on economies of scale (e.g., disaster relief). By shifting donations toward lesser-known charities, mandatory audits may foster greater diversity in the provision of social good.

My empirical predictions are not obvious a priori. Donors might not be convinced that audit mandates are effective at ensuring their contributions are spent toward legitimate charitable purposes. Deviant charities may, for instance, hire incompetent or corrupt auditors. Even if donors believe audit mandates are effective, they may assess the risk of being expropriated by charity managers as negligible in the first place. Another reason is that individuals give in part out of warm-glow motives (e.g., they gain utility from feeling generous or from not feeling guilty for not giving upon being solicited). Prior evidence shows that warm-glow feelings generally dominate the desire to contribute to a public good (Ribar and Wilhelm 2002, Andreoni 2006). If donors care very little about contributing to a public good per se, an audit mandate is unlikely to affect their decisions to give (i.e., even if the audit regulation is effective at curtailing managers' ability to extract private benefits). Finally, when regulation forces all charities to obtain an audit, donors cannot rely on audit choice as a signal for quality (Kausar et al. 2016). Donors may then turn to reputation (in lieu of audit choice) to separate charities. To the extent that such a response is prevalent, we should observe an increase in the concentration of donations on high-reputation charities (i.e., the opposite of my empirical prediction). Such a force would work against me finding results.[10] In sum, the effect of audit mandates on donor behavior is an empirical question.

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[10] As previously noted, I expect voluntary audits to have weaker signaling power in the non-profit setting because many individuals likely find it too time consuming to perform their own due diligence.

2.2 Underlying Assumptions

My empirical prediction rests on three assumptions that warrant additional discussion. First, donors have some awareness of the audit regulation. Second, mandatory audits curtail management's ability to misreport (and thereby conceal private-benefit extraction). The third assumption builds on the other two: donors and charity managers believe fraudulent charities are more likely to be detected when an audit mandate is in place.

Individuals can become aware of the audit regulation in several ways. The press sporadically mentions the nonprofit audit mandates. Each state regulator also maintains a website dedicated to charitable giving and addressed to the constituents. These websites generally mention the audit mandate and contain a repository of the audited financial information filed by the charities. Furthermore, some donors may be *indirectly* aware of the regulation. For example, individuals can learn via the media and press releases that the nonprofit regulator in their state is actively identifying and terminating illegitimate charities. Although donors may not be aware that the audit mandate is facilitating the regulator's work, they may extend greater trust in response to the regulator's proactivity. Similarly, donors may observe that nonprofit watchdogs can maintain donor advisor lists, without necessarily knowing the watchdogs' work is made possible through the audit mandate. In the online appendix, I provide excerpts from press articles, regulators' websites, and enforcement action press releases to illustrate how donors may become aware (directly or indirectly) of the audit mandate.

Furthermore, for the audit mandate to affect donor behavior, individuals need not know the exact exemption threshold of their state, nor the exact revenue figure of the charities to which they consider giving. Rather, they need to have a general idea of the intensity or broadness of the audit mandate in their state. For example, the state of New York has a broad audit regulation, meaning

that a large fraction of charities are subject to a mandatory audit. New Yorkers can gain the impression that the audit regulation in their state is indeed broad, because they hear from the media that the Attorney General of New York regularly takes action against suspicious charities. Consequently, New Yorkers may infer that a medium-size charity would likely be subject to some monitoring. For instance, a homeless shelter located in New York City most likely has to obtain a mandatory audit because the approximate costs involved in running such an establishment suggests it ought to be relatively important in size.[11]

The second assumption underlying my prediction about donors' response to audit mandates is that mandatory audits curtail management's ability to misreport (and thereby conceal private-benefit extraction). That is, audit mandates should prevent charities from pooling together and uniformly reporting that a high (and potentially inflated) proportion of their spending is directed toward the charitable programs. Such improvement in the reliability of financial reporting can facilitate monitoring by various parties including regulators. In other words, when an audit mandate is in place, separating charities based on the reported program ratio should be easier. Specifically, I expect the reported program ratio to be more dispersed and, on average, lower. Reported numbers are evidently a function of both the reporting process and the actual economic activity. Charity managers likely improve the *real* ratio in response to the audit requirement (i.e., they extract fewer private benefits, because they can no longer conceal such behavior by misreporting). The *reported* program ratio, which is empirically observable, therefore captures a

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[11] Similarly, donors need not know the activity type in which a given charity is classified according to the classification system I use in my empirical analyses. The exemption thresholds of the state-level audit regulations do not vary across activities within a given state. Therefore, the only requirement is that some donors have a general idea of the size of a given charity so as to deduce (with a margin of error) whether it is subject to the regulation.

lower bound of the effect of audit mandates on the reporting process, because it is partly offset by changes in real economic behavior.[12]

My empirical predictions also suggest donors and charity managers believe that audit mandates increase the probability that fraudulent illegitimate organizations be detected and terminated. I use the expression "illegitimate organizations" to refer to entities that adopt the legal form of a public charity but direct their funds toward non-charitable purposes (e.g., private benefits). Assessing the validity of this assumption by designing an empirical test based on fraud patterns may not be sensible, because only *detected* fraud is observable. On the one hand, the increase in the probability of detection should raise the number of fraud cases brought to light. On the other hand, rational individuals should be less likely to commit fraud, knowing that an audit mandate puts them at greater risk of being uncovered, thus reducing the number of fraud cases detected. In sum, merely testing for a relation between audit mandates and detected fraud is not suitable to assess whether people believe audit mandates are effective at detecting fraud.

Instead, I empirically test for a displacement effect. That is, if people believe audit mandates are effective at detecting fraud, individuals who intend to commit fraud are likely to alter their scheme to avoid being caught. Building on this rationale, I develop an empirical prediction based on the institutional features of the nonprofit setting. Throughout this paper, I study public charities, but nonprofit organizations choose whether or not to take on the legal form known as the "public charity" status. Public charities are in fact a subset of 501(c)3 organizations. All 501(c)3 organizations are nonprofit organizations that operate for purposes that are "charitable, religious, educational, scientific, literary, [promoting] public safety, fostering national or international

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[12] The improvement in financial reporting is not the exclusive mechanism by which audits can constrain managers from extracting private benefits. For example, audits can cause organizations to strengthen their governance practices (Duguay 2018).

amateur sports competition, or preventing cruelty to children or animals" (IRS 2018a). Importantly, the audit mandates extend to all 501(c)3 organizations. The 501(c)3 entities that receive donations from the public predominantly elect to hold the public-charity status.[13] Such status confers additional tax benefits and privileges, but it comes with broader disclosure requirements. The supplemental disclosures include reporting the amount of total expenses classified as program service expenses. These disclosures in particular are likely to allow mandatory audits to reveal illegitimate organizations. Audit mandates should therefore be effective in helping monitor organizations that hold public-charity status, but not other 501(c)3 organizations. In other words, when an audit mandate is in place, fraudulent nonprofit organizations can reduce the probability of being found out, by not electing for the public-charity status. I therefore predict that fewer 501(c)3 organizations hold public-charity status when broader audit mandates are in place.

2.3 Audit Requirements for Charities

Public charities are organizations that receive financial support from the public and enjoy tax-exempt status under section 501(c)3 of the Internal Revenue Code. The IRS requires that all charities with gross receipts above $50,000 file a form 990 or form 990-EZ annually (IRS 2018b).[14] After receiving the forms, the IRS makes them publicly available. The filings contain income-statement and balance-sheet information. These financial reports include the portion of expenses

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[13] An organization like the Bill & Melinda Gates Foundation would not be eligible to elect for public-charity status, because it receives its financial support from a handful of individuals rather than the public. In this test, I am interested in organizations that rely on financial support from the public and can therefore choose whether to avail themselves of the public-charity status.

[14] Form 990-EZ is the abbreviated version of form 990. It involves more aggregated financial disclosures and fewer non-financial disclosures, such as those pertaining to the adoption of various governance policies. Form 990-EZ still requires that charities report the amount of expenses spent on program services. Charities can choose to file either form, unless they have total assets above $500,000 *or* gross receipts above $200,000, in which case they must file a regular form 990.

classified as program service expenditures, as well as the share of net assets that have donor restrictions.

In the U.S., the states decide whether to require charities to file audited financial statements. Currently, 23 states have audit mandates. These audit requirements always exempt smaller charities that fall under a revenue threshold. As of the end of my sample period, the exemption cutoffs range from $300,000 to $3,000,000 in revenue. The specific way to calculate revenue also varies across states (e.g., certain states prescribe that government grants be excluded). Charities are subject to the regulation of the state in which they are located. If a charity solicits donations in another state, it must also comply with the regulation of that state. In other words, individuals in a given state can only be solicited by charities that abide by the local audit regulation. Therefore, donors need only be aware of their local audit mandate (and such awareness may be indirect, as long as perceived local regulatory enforcement correlates with the intensity of local audit mandates).

3. Research Design, Residual Empirical Concern, Data, and Sample Selection

3.1 Research Design

I seek to evaluate the effect of audit mandates on how donors allocate their contributions among charities and on individuals' willingness to give. I exploit the fact that states implement different exemption cutoffs and certain states choose not to impose any audit requirement. At the same time, the type of charitable activity a charity runs affects its size, which in turn determines whether it exceeds the audit-exemption cutoff.

I rely on cross-sectional variation (as opposed to time-series variation) for three reasons. First, the timing of the enactment of audit regulations is likely endogenous. For example, scandals often give rise to simultaneous responses from policymakers and market players (Ball 1980, Leuz

and Wysocki 2016, Hail et al. 2018). Second, a cross-sectional approach can better capture the long-run effects of audit mandates. Uncertainty exists over the timing of the initial manifestation of the effects of audit regulation. Whether donors will begin responding to audit mandates at the time the law is proposed, is passed, or comes into effect is unclear (Mulherin 2007). It may also take time for the population to become aware of the audit mandates. Furthermore, charities report their financial information only once a year and often take several months to do so. A time-series approach designed to capture sharp short-run effects would likely be unsuitable. Third, my sample offers limited time-series variation. Only five states enact their audit mandate during my sample period; the other audit regulations were passed before the beginning of my sample period.[15]

I perform the empirical analyses using an aggregate approach. Specifically, I construct observations at the state-activity-year level. Doing so allows me to distinguish between the effect on the allocation of donations and the effect on total donations (i.e., within aggregation cluster). The aggregate approach is also suitable to implement the simulated instrument strategy. Finally, it allows me to use data that are only available at the aggregate level (e.g., the IRS data on taxpayers).

Following Breuer (2018), I construct a treatment variable that captures the broadness of audit mandates. Specifically, I measure the treatment as the proportion of charities i that are subject to a mandatory audit for a given state s, type of charitable activity a, year t:

$$\%MandatoryAudit_{sat} = \frac{1}{\mathbb{N}_{sat}}\sum_{it\in\mathbb{N}_{sat}}\mathbf{1}(Revenue_{it} \geq ExemptionCutoff_{st}). \qquad (1)$$

$\mathbb{N}_{sat}$ is the entire set of state $\times$ activity $\times$ year in the data.

My specification includes state-year and activity-year fixed effects. The state-year fixed effects control for potential confounders such as differences in GDP, productivity, population size, tax incentives to give, regulatory enforcement, costs of operating a charity, and individuals'

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[15] Certain states also adjust their cutoffs for inflation over time, but these changes are minor.

generosity across states. Similarly, the activity-year fixed effects allow me to control for variation in reliance on donations, operating costs, and individuals' tastes across types of charitable activity. Finally, the interaction of the year dummies with the state and activity fixed effects controls for time-series variation within each state and activity.

Any remaining endogeneity concern must therefore arise at the state-activity level. Consider, for instance, the state of Michigan, which offers a special tax credit to taxpayers who donate to foodbanks specifically.[16] In response to this incentive, Michiganders likely donate more to foodbanks. Michigander foodbanks therefore receive more donations collectively and potentially have higher average revenue (relative to foodbanks in other states and relative to Michigander charities that conduct other types of charitable activity). Higher average revenue implies that a higher percentage of Michigander foodbanks are subject to a mandatory audit because more of them exceed the size-based exemption threshold. Such an omitted variable (i.e., the special tax credit) thus gives rise to a spurious positive relation between one of the outcomes (i.e., total donations) and the treatment (i.e., *%MandatoryAudit*). To address this concern, I use a simulated instrumental variable (Currie and Gruber 1996, Mahoney 2015, and Breuer 2018).

I construct the simulated instrument for a given state-activity-year observation following Mahoney (2015) and Breuer (2018). Specifically, I determine the percentage of charities that would be subject to a mandatory audit if the exemption cutoff of the given state were applicable to the entire population of charities that belong to the given activity (i.e., as opposed to only the charities *in the given state* that belong to the given activity). In the example of the Michigan foodbanks, the instrument captures the fraction of foodbanks throughout the U.S. that would have to obtain a mandatory audit based on the exemption threshold of the state of Michigan. In other

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[16] This tax provision is no longer in place (NCN 2015). Nevertheless, I use it for illustrative purposes.

words, for a given type of charitable activity, I instrument the treatment using the same distribution of charities for all states. Because this distribution represents charities from all states rather than one state in particular, the instrumentation approach removes endogenous variation specific to state-activity pairs. In the case of the Michigan tax credit, the simulated instrument would purge the endogenous inflation in the treatment, because foodbanks outside of Michigan are not "abnormally" large, because their donors do not have access to a special tax credit. Formally, I construct the instrument, *Instrument%MandatoryAudit*, as follows:

$$Instrument\%MandatoryAudit_{sat} = \frac{1}{\mathbb{N}_{at}} \sum_{it \in \mathbb{N}_{at}} \mathbf{1}(Revenue_{it} \geq ExemptionCutoff_{st}). \quad (2)$$

$\mathbb{N}_{at}$ is the entire set of activity × year in the data. In the online appendix, I perform an analysis that illustrates the ability of my identification strategy to purge confounding variables.

I estimate the following reduced-form regression using ordinary least squares (OLS)[17]:

$$Y_{sat} = \alpha_{st} + \alpha_{at} + Instrument\%MandatoryAudit_{sat-1} + \epsilon_{sat}. \quad (3)$$

Y_{sat} is the outcome variable and varies across analyses. *α_{st}* and *α_{at}* are state-year and activity-year fixed effects, respectively. *Instrument%MandatoryAudit* is the instrumental variable as defined in equation (2).

I cluster the standard errors by state. I evaluate the sensitivity of my results to two alternative approaches to clustering the standard errors. The first is to use two-way clustering by state-year and state-broad category of activity (Breuer 2018). The broad categories aggregate the activities at a high level and therefore allow for greater dependence based on the nature of the charitable activities. This approach also alleviates concerns over mechanical correlation across clusters when the fixed effects are not nested within the clustering structure (Conley et al. 2018).

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[17] Following Breuer (2018), I report coefficient estimates for the reduced-form regressions of the outcome variable on the instrument. The advantage of the reduced-form specification is that it produces unbiased OLS estimates (Angrist and Pischke 2009). Nevertheless, I report the coefficient estimates of the second-stage regressions in the online appendix.

The second alternative method is to cluster the standard errors by state-activity, simply allowing for infinite serial correlation across state-activity-year observations. None of my inferences are sensitive to these two alternative methods. In several cases, the level of statistical significance increases, suggesting clustering by state is conservative.

3.2 Residual Empirical Concern

Next, I discuss potential threats to my research design. A violation of the identifying assumption requires the subsistence of confounders despite the fixed-effects structure and the simulated instrument strategy. An example of such a scenario would be the existence of other regulatory provisions triggered by exceeding the audit-exemption threshold. For instance, the results could be confounded if the state regulations dictated that, upon exceeding the size-based exemption cutoff, charities put in place an independent board of directors (i.e., in addition to obtaining a financial audit). To address this concern, I read the state laws to ensure that exceeding the size-based cutoff does not trigger regulatory requirements other than obtaining an audit. I find no such case.

A similar concern would arise if state regulators relied on the exemption threshold to determine when to enforce the law. Consider a regulator who, from time to time, receives alerts from the public about doubtful charities but chooses not to investigate when an organization falls under the audit exemption threshold. My inference would be confounded because, in this case, the exemption cutoff captures not only the requirement to obtain an audit, but also the regulator's responsiveness to alerts from the public.

Another form of violation would require that state regulators choose exemption thresholds such that the percentage of organizations subject to a mandatory audit coincides with the population's preferences across types of charities, in a way that is state specific. I believe such a

scenario is unlikely. A single state-level threshold targeting specific types of activity, which are uniquely important to the constituents of that state, would incidentally extend to several other types of activity the population does not necessarily prefer. For example, in a state with particularly cold weather, donors may have a preference for homeless shelters (i.e., a preference at the state-activity level). The regulator may therefore choose a size-based exemption cutoff that ensures a high proportion of homeless shelters are subject to a mandatory audit. However, such a cutoff would have a similar (but incidental) impact on shelters for women victims of domestic violence. The two types of shelters have similar size distributions because they have comparable requirements in terms of amenities and staffing, but state-specific weather is unlikely to influence the population's preferences for the second type of shelter (i.e., cold weather should be unrelated to domestic violence).

One may argue that regulators may still choose the state-level threshold that best aligns with state-specific donor preferences across types of charities. I believe such a scenario is also unlikely. If the regulators intended the intensity of the audit mandate to vary across types of activity (in a way that is state specific), they would likely establish cutoffs at the state-activity level. Furthermore, in cases where documentation exists over the process of setting the threshold, the deliberations (1) involve the regulators, the proponents, and the opponents of the audit mandates, and (2) revolve around the burden the audit fees may represent for the average charity, not an attempt to align the cutoff with donors' preferences across activity types.[18]

The three examples above would constitute violations of the exclusion restriction. In other words, the causal explanation for the observed results is not the audit regulation, despite the

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[18] For example, in California, the regulator initially proposed a revenue cutoff of $250,000, but representatives of the nonprofit sector who opposed the audit mandate requested that the threshold be set at $5,000,000. The regulator responded by proposing a revised cutoff of $500,000. The opponents continued to pressure the legislators and, in the end, the audit mandate was passed with an exemption threshold of $2,000,000 (Manzo 2004, Takagi 2005).

instrument being statistically associated with the outcome. One key limitation of my inferences is that I cannot *prove* the exclusion restriction is satisfied because (1) I may not conceive all the potential scenarios that give rise to a violation, and (2) certain scenarios, albeit unlikely, cannot be entirely ruled out (e.g., the aforementioned example in which regulators' responsiveness coincides with the threshold).

3.3 Data

I use data from the forms 990 and 990-EZ that public charities file annually. The IRS makes the forms publicly available in tagged image file (TIF) format and extensible markup language (XML) format.[19] Information intermediaries maintain search engines that allow the public to consult charities' filings. Data providers extract information from the IRS's raw files and sell readily useable structured data.[20]

To perform my main analyses, I obtain data from the National Center for Charitable Statistics (NCCS) core files from 1998 to 2015. The NCCS core files contain all the forms 990 and 990-EZ that charities have filed. However, the core files do not cover all the data points on the form 990. For example, the proportion of expenses allocated toward program services is not available in the NCCS core files. One of my additional tests requires this particular data point to measure the effect on financial reporting. Therefore, for the test in question, I use data from Guidestar. I obtain all the forms 990 and 990-EZ that have been digitized by Guidestar from 1998 to 2013. Guidestar covers essentially all the data points on the form 990 but has less complete historical coverage of smaller charities. For recent years, I supplement the Guidestar data using the XML files made available by the IRS.

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[19] The XML files are only available for recent periods and for forms that were filed electronically (IRS 2016).
[20] See Feng et al. (2014) for a detailed discussion of the availability of form 990 data.

For the additional analyses, I collect data from the IRS (i.e., the statistics of income on individuals' tax returns), the Bureau of Labor Statistics, the Bureau of Economic Analysis, and the Census Bureau. I also obtain data from the IRS business master files to carry out the test by which I determine the share of 501(c)3 organizations that hold public-charity status.

I hand collect data on state-level audit mandates by reading the law using LexisNexis. I manually identify the enactment dates, amendment dates, and exemption thresholds for all audit regulations imposed by state regulators during my sample period.

One data limitation is that I cannot distinguish the portion of donations that charities receive from out-of-state donors. I view this issue as a minor source of measurement error for three reasons. First, national networks such as United Way and the YMCA usually consist of separate charities in each city or metropolitan area. To the extent that individuals give to their local United Way or YMCA, measurement error should be minimal. Second, certain states such as New York are more likely to attract large charities that draw funding from out-of-state donors. As long as this variation is state specific, it should be purged by the state-year fixed effects. Third, my results hold when I replicate the analyses excluding charities that solicit donations in other states.[21]

3.4 Sample Selection

I retain all unique charity-year observations with at least $50,000 in gross receipts during the period from 1998 to 2015.[22] I construct observations at the state-activity-year level from over 2 million charity-year observations representing approximately 380,000 individual charities. The types of charitable activity are based on the two-digit codes of the National Taxonomy of Exempt Entities (NTEE). I eliminate observations without prior-year data as well as observations for which

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[21] Charities report on their form 990 whether they are required to file with other states. This requirement is triggered when a charity solicits donations in other states.

[22] Charities with less than $50,000 in gross receipts file form 990-N, which contains no financial information.

the activity is assigned to the unknown category. I also drop observations in the health and education sectors, because these sectors are exempted from the audit requirements studied in this paper (i.e., they fall within the purview of distinct industry-specific audit regulations) (Neeley 2011, Desai and Yetman 2015, Duguay 2018). The final sample therefore comprises 119,379 state-activity-year observations. I provide summary statistics in Table 1.

4. Results

4.1 Instrument Relevance

I first evaluate whether the instrument satisfies the relevance criterion. I expect a positive and significant association in the first-stage regression. The first stage consists of estimating equation (3) with *%MandatoryAudit* as the left-hand-side variable. In column 1 of Table 2, I report the coefficient estimates of the first-stage regression. As expected, I find a strong positive relation between the instrument and the treatment variable. The F-statistic for the first stage based on Sanderson and Windmeijer (2016) is above 50, allowing me to conclude the instrument is not weak.[23] The large magnitude of the first stage is consistent with prior studies that use a similar simulated instrument approach (Mahoney 2015, Breuer 2018). Despite the coefficient of the first stage being relatively close to 1, the instrument brings an important contribution to the identification strategy. In the online appendix, I show that variables that are potentially correlated with the treatment and that may confound the results are properly purged only when I employ the instrument.

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[23] The first stage is common among the main analyses. However, some of the additional tests involve modifying the sample or the empirical specification, thereby changing the first stage. I report the first-stage regressions for these additional tests in the online appendix and find similar positive and strongly significant relations.

In column 2 of Table 2, I present the coefficient estimate of the reduced-form regression of the proportion of charities that obtain an audit on the instrument.[24] I find a positive and significant relation. The coefficient is naturally smaller because charities can voluntarily obtain an audit regardless of the regulation. Recall that the treatment in this study is the audit regulation, *not* the audit per se. Nevertheless, finding a positive association between the share of charities that obtain an audit and the instrument is comforting because it suggests donors can rationally expect audit regulations to result in more charities being audited. One would not observe a significantly positive relation if, for example, almost all charities voluntarily chose to obtain an audit in the absence of a mandate.

4.2 Audit Mandates and the Allocation of Donations among Charities

I investigate the effect of audit mandates on how donors allocate their contributions among charities. Specifically, I test whether donors are less likely to concentrate their donations on large high-reputation charities. The proxy I use is the sum of the squared shares of donations within a given state-activity-year observation. This measure is a version of the Herfindahl-Hirschman index where the market share is based on donations. By squaring each charity's share of the total donations, the measure captures the extent to which donations are concentrated among the larger charities. The measure is higher when contributions are concentrated among a few large charities. Conversely, the measure decreases when donations move from large to smaller lower-reputation charities. In the online appendix, I replicate my analyses using alternative proxies and find similar results.

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[24] The number of observations decreases for this regression because I only observe audit choice for form-990 data that I obtain from Guidestar for the period 2008-2013. However, the public can always observe charities' audit choices (e.g., through the public repositories maintained by the state regulators).

Table 3, column 1, reports the coefficient estimates of the reduced-form regression of the concentration of donations among charities on the instrument. Consistent with my prediction, I find a negative and statistically significant relation. This finding suggests donors are less likely to concentrate their contributions on large high-reputation charities when an audit mandate is in place. The coefficient estimates reported in the tables represent the effect of a one-unit change in the instrument on the outcome variable (i.e., going from 0% to 100% of charities being subject to a mandatory audit). However, in reality, the state-level audit regulations never extend to 100% of charities, because smaller organizations are exempted based on state-specific thresholds. When I discuss the economic magnitudes, I divide the coefficients by 4 to provide estimates that are more applicable (i.e., a 25-percentage-point increase in the proportion of charities subject to a mandatory audit). Such a change is similar to the difference between a state with no audit regulation, such as Ohio, and a state with an audit mandate of moderate scope, such as Georgia.[25] I find that a 25-percentage-point increase in the instrumented percentage of charities subject to a mandatory audit is associated with a 0.05-standard-deviation decrease in the concentration of donations among charities.

To evaluate the credibility of the proposed mechanism, I perform a cross-sectional test in which I exploit variation in information asymmetry across types of charitable activity. I predict that the effect of audit mandates on donor behavior is stronger (muted) when information asymmetry between donors and charity managers is particularly high (low). This test sheds light on whether the observed effects are attributable to donor behavior and information frictions (i.e., the proposed mechanism) as opposed to other potential effects of audit mandates. For example, charities could respond to audit mandates by organizing as small entities that fall under the

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[25] Dividing the coefficient estimates by 2 rather than by 4 would be representative of the difference between a state with no audit regulation and a state with a fairly broad audit mandate, such as New York.

exemption cutoff or as large organizations that can better absorb the fixed costs of a mandatory audit. Such an organizational change could impact the allocation of donations but would constitute a different mechanism than a change in donor behavior. This type of cross-sectional test is obviously subject to the standard endogeneity concern (Christensen et al. 2016). Although intended to support the credibility of the proposed channel, it cannot prove it.

I single out the types of charitable activities in which information asymmetry is likely to be particularly high or low. The types of activity I identify as having low information asymmetry include community clubs, recreational or social clubs, amateur sport clubs, fraternal societies, and youth programs like Boy and Girl Scouts. For these types of charities, the donors (or their immediate family) also tend to be beneficiaries. Therefore, donors can more concretely observe how the funds are spent. On the other hand, the charities I identify as having high information asymmetry include services related to addictive disorders, substance abuse, services to immigrants, and international programs. Programs that seek to treat substance-abuse disorders generally ensure the anonymity of the beneficiaries. The same is true of charities that serve undocumented immigrants. International programs operate abroad. Therefore, individuals who give to these types of charities are limited in their ability to learn and assess how the funds are spent (i.e., through means other than the financial reports). A homeless shelter is an example of charity for which information asymmetry is neither particularly high nor particularly low. The donors may not be the beneficiaries, but they may, for example, be able observe individuals lining up to receive shelter every day.

I create the cross-sectional variables *LowInfoAsymmetry* and *HighInfoAsymmetry* to single out the types of charities that have particularly low or high information asymmetry. The other types

of activity fall in the *MedInfoAsymmetry* category.[26] One drawback with the approach is the subjectivity involved in assigning the types of activity to each category. In the online appendix, I find similar results if I use an alternative cross-sectional variable based on government scrutiny, which is slightly more objective. The rationale of the alternative test is that information asymmetry is less of a concern for types of activities that depend more on government funding and are therefore subject to greater government monitoring.

I report the coefficient estimates of the cross-sectional test in column 2 of Table 3. As predicted, the effect on the allocation of donations is nonexistent (more pronounced) when information asymmetry is particularly low (high) and mandatory audits are expected to be the least (most) useful.[27] For the types of charitable activities with high information asymmetry, a 25-percentage-point increase in *Instrument%MandatoryAudit* is associated with a 0.10-standard-deviation decrease in the concentration of donations among charities. These results are consistent with donors allocating less of their donations to large high-reputation charities when broader audit mandates are in place.

4.3 Audit Mandates and Individuals' Willingness to Give

I evaluate the effect of audit mandates on individuals' willingness to give. I study two outcomes: (1) the effect on total donations in dollars and (2) the effect on the number of individuals who make charitable donations. I first estimate a version of equation (3) in which the outcome variable is the natural logarithm of the sum of the donations received by charities in a given state-activity-year. I report the coefficient estimates in Table 4, Panel A, column 1. The estimate for

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[26] The specification does not include separate main-effect variables because each type of charitable activity is assigned to one of the three levels of information asymmetry. In other words, the fixed effects subsume the main effects.

[27] I perform t-tests of the statistical difference in coefficients and conclude that the low and high terms as well as the medium and high terms are statistically different (p-values of 0.031 and 0.028, respectively), but I cannot reject the null for the low and medium terms.

LnTotalDonations is close to zero and statistically insignificant. I then perform a cross-sectional test similar to the one in Table 3. Column 2 of Panel A in Table 4 reveals a positive and significant relation between the breadth of audit mandates and total donations, but only when information asymmetry is high.[28] For the types of charitable activity with high information asymmetry, a 25-percentage-point increase in *Instrument%MandatoryAudit* is associated with a 0.11-standard-deviation increase in total donations. Although this increase in donations is economically significant for the charities with high information asymmetry, these charities constitute a small subset of the entire charitable sector in the U.S. As such, the aggregate result in column 1 is dominated by the other types of charities, which experience no significant effect on donations in dollars.

Next, I investigate the effect of audit mandates on the number of individuals who make charitable donations (i.e., donors on the extensive margin). The form 990 data allow researchers to observe the total donations charities receive but not the identity of the donors. I therefore turn to IRS data on individuals' tax returns. Following the economics literature, I proxy for the proportion of individuals who give, using the fraction of itemized tax filers who claim a deduction for charitable donations (Bakija and Heim 2011, Weber 2014, Duquette 2016). This proxy, although broadly relied upon in the literature, likely introduces some measurement error. Specifically, it does not capture donors who do not file an itemized tax return or who omit to claim a deduction for charitable contributions on their itemized tax return (Andreoni 2006).

The IRS only provides the data on individuals at the state-year or county-year level. I modify the specification in equation (3) to accommodate the data. I use observations at the state-

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[28] I perform t-tests of the statistical difference in coefficients and conclude that the low and high terms as well as the medium and high terms are statistically different (p-values of 0.023 and 0.005, respectively), but I cannot reject the null for the low and medium terms.

year level (rather than state-activity-year level) and include year fixed effects. This specification allows me to implement a version of the simulated instrument at the state-year level similar to some of the specifications in Mahoney (2015). However, because I exploit variation across states, I cannot include state-year fixed effects. I therefore add the following controls at the state-year level: the natural log of GDP per capita, the unemployment rate, the natural log of population size, and a polynomial term for population size. I report the coefficient estimates in column 1 of Panel B of Table 4. I find a positive and statistically significant association between the instrument and the proportion of taxpayers who give.

Because I cannot include state-year fixed effects in the specification reported in column 1, there is a concern that I fail to control for all state-level confounders. I exploit the fact that the data are also available at the county-year level, and I perform a within-state analysis that relies on variation across counties. [29] This approach allows me to include state-year fixed effects but prevents me from using the simulated-instrument approach (the state-year fixed effects would subsume any variation in the instrument). The two specifications are therefore complementary. The first specification allows me to implement the simulated instrument but not a stringent fixed-effects structure, whereas the converse is true of the second specification. I report the results of the regression in column 2 of Panel B. Again, I find a positive and significant association. Specifically, a 25-percentage-point increase in *%MandatoryAudit* is associated with a 1.55-percentage-point increase in the share of tax payers who donate to charities (i.e., a 2% increase relative to a mean of 0.757).

The county-year data also allow me to perform a cross-sectional test based on variation in individuals' opportunity cost of time across counties. The opportunity cost of time is a key friction

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[29] The IRS data at the county-year level are only available beginning in 2010.

for people who consider giving (Andreoni 2006, DellaVigna et al. 2012, Karlan and List 2018). For certain individuals, the cost of performing their own due diligence using charities' disclosures (or alternative sources) may be high enough to keep them from giving at all. An audit mandate can mitigate this issue if individuals believe the regulation allows various parties such as the government and intermediaries to better perform their monitoring role (e.g., by weeding out illegitimate charities). I therefore predict that audit regulation is especially likely to induce participation in charitable giving among individuals who face a high opportunity cost of time. I proxy for the opportunity cost of time using earnings. I construct the cross-sectional variable *HighOppCostTime*, which is equal to 1 for counties where the average individual earnings are in the top quintile in a given year. In column 3, I present the coefficient estimates of the cross-sectional test. I find a positive and statistically significant coefficient on the interaction term. This result is consistent with audit mandates' ability to induce people to give being stronger for individuals who face a high opportunity cost of time.

Again, such a cross-sectional test should be interpreted with caution. Although earnings are commonly used to proxy for the opportunity cost of time, they may also capture omitted correlated variables. For example, high earners may be more responsive to the audit regulation because they are more aware of it or because they have a better understanding of what a financial audit entails. It is however comforting that constructs like awareness or sophistication, which may be captured by my cross-sectional proxy, also make for valid cross-sectional variables to test whether the effect of audit mandates is more pronounced among certain groups of individuals.

Next, I seek to reconcile the absence of an effect on total donations and the increase in the number of donors. One potential explanation is a crowding-out effect. That is, when the regulation induces new people to give, donations from those who would contribute regardless of the mandate

(i.e., the intensive-margin donors) may decline, offsetting the increase in total donations. Such an explanation is consistent with the literature on giving. Prior studies find that when a charity receives more funding for a given source, it tends to receive less contributions from other sources. (Roberts 1984, Bergstrom et al. 1986, Andreoni and Payne 2013, Correa and Yildirim 2013). For example, government grants to a given charity have been shown to crowd out contributions by private donors. This crowding-out phenomenon occurs because charities solicit less money from each individual donor when securing funding form a larger pool of people becomes easier. (Andreoni and Payne 2003, Andreoni and Payne 2011).

Testing for such a crowding-out effect is empirically challenging. Charities' public filings include the total amount of donations received in a given year, but not how many people contributed nor how much each donor gave. However, a charity must report whether it received donations of $5,000 or more from any one contributor during the year. I use this data point to proxy for infra-marginal donations. I predict that as new people give to a given charity, other donors may decrease the amount of money they contribute (i.e., a crowding-out effect). To the extent that, in some cases, this crowding-out effect causes intensive-margin contributors to move from giving more than $5,000 to giving less than $5,000, the proxy captures movement in infra-marginal donations.

In Panel C of Table 4, I find a negative association between the instrument and infra-marginal donations. The relation is weakly significant, which is not surprising given the coarseness of the proxy I use. Note that the number of observations is lower than in Table 3 and Table 4 Panel A. The sample shrinks because (1) charities did not report information on large donors on their form 990 prior to 2008, and (2) I am constrained to rely on the Guidestar dataset (as opposed to the NCCS core files), which covers all data points on the form 990, but has imperfect coverage of

smaller charities. In the online appendix, I replicate the main results using this smaller sample to alleviate the concern that a change in sample composition plays into my findings. The results in Panel C are therefore consistent with audit mandates being associated with more people who give, which in turn crowds out part of the infra-marginal donations. Other factors may also be responsible for the absence of a sizeable effect on total donations, despite an increase in the number of people who give. For example, the new donors might be making small contributions in dollars, causing little variation in total donations (i.e., donors along the extensive margin likely go from giving zero dollars to giving a small amount money).

4.4 Audit Mandates and Diversity in the Provision of Social Good by the Charitable Sector

In the prior sections, I show that audit mandates affect the allocation of donations in a way consistent with donors relying less on charities' reputation. I now test whether this reallocation translates into contributions being less concentrated by geographic area and social needs. Such a finding would be in line with mandatory audits allowing the charitable sector to address more diverse social issues.

In Table 5, column 1, I estimate equation (3) with the outcome variable being a version of the Herfindahl-Hirschman index that captures the concentration of donations among cities or towns (i.e., for a given state-activity-year observation). I find a significant and negative association between audit mandates and the concentration of donations among geographic areas. In column 2, the left-hand-side variable measures the concentration of donations among charitable activities of specific natures. I use three-digit NTEE codes to proxy for the specific nature of activities. For example, charities whose activities relate to food-service distribution include food pantries, soup kitchens, and meals on wheels organizations, all of which address distinct and specific social needs. My test investigates the effect of audit mandates on the concentration of donations across

these very specific types of organizations. I find the coefficient estimate to be negative and statistically significant. Together, these results are consistent with audit mandates allowing the charitable sector to serve more diverse geographic areas and social needs.

4.5 Audit Mandates and the Ability to Separate Charities Based on their Financial Reports

To further evaluate the credibility of the proposed mechanism, I test whether audit mandates make it easier to use financial reports to separate charities. Such an outcome would suggest audit mandates can facilitate monitoring. If mandatory audits are effective, they should prevent charities from pooling together and uniformly reporting a high program ratio. Specifically, I expect the reported program ratio to be more dispersed and, on average, lower. I define the reported program ratio as reported program service expenses over total expenses, but I exclude fees for accounting and audit services, because these fees are mechanically affected by the treatment (Hofmann and McSwain 2013, Arya and Mittendorf 2016). I measure dispersion in the reported program ratio using the standard deviation and the interquartile range. In columns 1 and 2 of Table 6, I report the coefficient estimates for equation (3), with the outcome variable being the standard deviation and the interquartile range of the reported program ratio, respectively.[30] As predicted, I find a positive and statistically significant relation between the instrument and the dispersion of the reported program ratio. I then estimate equation (3) using average reported program ratio as the dependent variable. In column 3, I find a negative and statistically significant association, which is in line with audit mandates forcing charity managers to be more conservative in reporting their efficiency. Together, these results confirm that separating charities based on their financial reports is easier when audit mandates are in place.

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[30] As pointed out in section 3, the number of observations decreases in this specification because I require data points that are not available in the NCCS core files. I therefore use the Guidestar database, which covers all the data points in the form 990 but has less complete historical coverage of smaller charities. Note that data on program expenses are available throughout the sample period (in contrast to data on large donors, which are only available as of 2008).

4.6 Audit Mandates and the Propensity to Hold Public-Charity Status

I perform a test to evaluate whether nonprofit managers appear to believe audit mandates can detect fraudulent charities. Specifically, I test for a displacement effect by evaluating whether broader audit mandates are associated with fewer 501(c)3 nonprofit organizations holding public-charity status. Electing to hold public-charity status confers more tax privileges but involves additional disclosure requirements. These supplemental disclosures increase the likelihood that an illegitimate charity will be found out (i.e., conditional on an audit being in place).[31]

I estimate a version of equation (3) in which the outcome variable is the share of 501(c)3 nonprofit organizations that hold public-charity status. I make one modification to the specification: I use *broad categories* of charitable activity instead of *types* of charitable activity. The broad categories of charitable activity are aggregated at a much higher level. The reason for using the broad-category classification is that electing to hold public-charity status can affect the *type* of charitable activity in which the NTEE classifies an organization, but not the *broad category*. I report the coefficient estimates in Table 7. As predicted, I find a negative and statistically significant relation between *%CharityStatus* and the instrument. This result is consistent with the belief that fraudulent charities are more likely to be detected and weeded out when audit mandates are in place.

5. Conclusion

In this paper, I study the economic consequences of financial audit regulation in the charitable sector. I design an identification strategy that relies on variation in size-based exemption thresholds across states and differences in size across types of charitable activity. I find that when audit mandates are in place, donations are less concentrated on high-reputation charities and more

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[31] Electing to hold public-charity status does not affect whether an organization is subject to a mandatory audit.

people give. Finally, this reallocation of resources allows the charitable sector to serve more diverse geographic areas and social needs.

This study has at least three important limitations. My identification strategy allows me to gather evidence in support of a causal relation between audit mandates and the allocation of donations. However, the validity of my causal inferences rests on empirical assumptions. One of the key assumptions is that the instrument satisfies the exclusion restriction, but I cannot prove that it does. In addition, I do not provide causal evidence with respect to the proposed mechanisms. In other words, my paper does not prove that audit mandates affect donor behavior *because* they alleviate moral-hazard concerns. Although I present economic arguments and perform additional tests that yield results consistent with such an explanation, I cannot rule out every alternative mechanism.

Second, measuring the production of social good in the charitable sector is very challenging. This study shows that audit mandates affect the allocation of donations, but not the production of social good per se. Similarly, I cannot conclude whether the reallocation of donations away from high-reputation charities enhances welfare.

Finally, the scope of this paper does not extend to the costs of audit mandates. There is general agreement that audit regulation imposes costs on charities in the form of audit fees and time spent managing the audit process (Duguay et al. 2018). Because I do not find that total donations increase on average, audit mandates likely cause a net outflow of monetary resources from the charitable sector. I cannot conclude whether the reallocation of donations justifies this aggregate cost. An interesting avenue for future research is to study the political economy of these audit mandates. For example, audit firms may seek to influence the legislative process in favor of mandatory audits because they financially benefit from such regulation.

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Appendix: Variable Definitions

Variable	Definition
ConcentrationDonations	The sum of the squared shares of donations among charities within a given state-activity-year.
GeographicConcentration	The sum of the squared shares of donations among cities within a given state-activity-year.
HighInfoAsymmetry	An indicator variable equal to 1 for types of charitable activity that include services related to addictive disorders, substance abuse, services to immigrants, and international programs.
HighOppCostTime	An indicator variable equal to 1 for counties where the average individual earnings are in the top quintile in a given year.
Instrument%MandatoryAudit	The fraction of charities that would be required to obtain an audit if the audit exemption cutoff for the given state were applied to all charities in the given activity-year.
IntraMarginalDonations	The natural log of 1 plus the number of charities that receive, from any one donor, contributions of $5,000 or greater, within a given state-activity-year.
LowInfoAsymmetry	An indicator variable equal to 1 for types of charitable activity that include community clubs, recreational or social clubs, amateur sport clubs, fraternal societies, and youth programs like Boy and Girl Scouts.
ReportedProgRatioStdev	The standard deviation of the reported program ratio across charities within a given state-activity-year. The reported program ratio is calculated as total program expenses over total expenses (excluding fees for accounting and audit services).
ReportedProgRatioIQR	The difference between the third and first quartiles of the reported program ratio across charities within a given state-activity-year. The reported program ratio is calculated as total program expenses over total expenses (excluding fees for accounting and audit services).
ReportedProgRatioAvg	The average reported program ratio for charities within a given state-activity-year. The reported program ratio is calculated as total program expenses over total expenses (excluding fees for accounting and audit services).
SocialNeedConcentration	The sum of the squared shares of donations among social needs within a given state-activity-year. (Three-digit NTEE codes proxy for social needs within an activity type.)
LnTotalDonations	The natural log of the sum of the donations of the charities in a given state-activity-year.
%Audit	The fraction of charities that obtain a financial audit within a given state-activity-year.
%CharityStatus	The fraction of 501(c)3 nonprofit organizations that hold public-charity status within a given state-broad category-year.

%MandatoryAudit	The fraction of charities subject to a mandatory financial audit within a given state-activity-year.
%TaxpayersWhoGive	The fraction of itemizing taxpayers who claim a deduction for charitable contributions within a given state-year or county-year.

Table 1: Summary Statistics

This table reports the summary statistics for variables used in the tests reported in Tables 2-7. See appendix for variables definitions.

Variable	Mean	SD	P25	Median	P75	N	Unit of observation
Instrument%MandatoryAudit	0.120	0.184	0.000	0.000	0.228	119,379	state-activity-year
%MandatoryAudit	0.124	0.232	0.000	0.000	0.167	119,379	state-activity-year
%Audit	0.622	0.316	0.429	0.667	0.900	29,494	state-activity-year
ConcentrationDonation	0.504	0.321	0.222	0.443	0.805	119,379	state-activity-year
LnTotalDonations	14.70	2.33	13.15	14.79	16.30	119,379	state-activity-year
%TaxpayersWhoGive (state)	0.832	0.048	0.803	0.833	0.864	867	state-year
%TaxpayersWhoGive (county)	0.757	0.107	0.719	0.774	0.821	15,159	county-year
IntensiveMarginDonors	1.698	1.091	0.693	1.386	2.398	29,494	state-activity-year
GeographicConcentration	0.572	0.314	0.296	0.520	0.926	119,379	state-activity-year
SocialCauseConcentration	0.768	0.248	0.536	0.862	1.000	119,379	state-activity-year
ReportedProgRatioStdev	0.189	0.145	0.097	0.157	0.240	70,264	state-activity-year
ReportedProgRatioIQR	0.252	0.234	0.119	0.189	0.298	70,264	state-activity-year
ReportedProgRatioAvg	0.802	0.086	0.762	0.815	0.857	70,264	state-activity-year
%CharityStatus	0.904	0.151	0.914	0.956	0.980	8,235	state-broad category-year

Table 2: Relation between the Instrument and the Proportion of Charities Subject to a Mandatory Audit

This table shows the association between the instrument and (1) the actual share of charities subject to a mandatory audit or (2) the share of charities audited. Column 1 is the first-stage regression for the main analyses. It helps assess whether the instrument satisfies the relevance criterion. Column 2 shows the association between the simulated instrument and the proportion of charities that obtain an audit either voluntarily or mandatorily. Observations are at the state-activity-year level. Types of charitable activity are based on the National Taxonomy of Exempt Entities. In column 1, the sample aggregates all public charities and covers the period 1998 to 2015. In column 2, the sample is restricted to the Guidestar data for the period 2008 to 2013 (i.e., the subsample for which I have data on audit choice). Reported below the coefficients are t-statistics calculated with standard errors clustered at the state-type of charitable activity level. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See appendix for variables definitions.

	(1) %MandatoryAudit	(2) %Audit
Instrument%MandatoryAudit	0.979***	0.100***
	[34.93]	[2.75]
Fixed effects:		
State × year	Yes	Yes
Activity × year	Yes	Yes
Adj R-Sq.	0.643	0.286
N	119,379	29,494
Unit of observation	State-activity-year	State-activity-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State
Sample restriction	-	Audit choice observable in researcher's dataset

Table 3: The Effect of Mandatory Audits on Donors' Reliance on Charities' Reputation

This table presents estimates of reduced-form regressions of the concentration of donations on the simulated instrument. The sample aggregates all public charities and covers the period from 1998 to 2015. The observations are at the state-activity-year level. Types of charitable activity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are t-statistics calculated with clustered standard errors. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See appendix for variables definitions.

	(1)	(2)
	ConcentrationDonations	ConcentrationDonations
Instrument%MandatoryAudit	-0.060**	
	[-2.42]	
Instrument%MandatoryAudit*LowInfoAsymmetry		-0.009
		[-0.20]
Instrument%MandatoryAudit*MedInfoAsymmetry		-0.055**
		[-2.20]
Instrument%MandatoryAudit*HighInfoAsymmetry		-0.132***
		[-3.09]
Fixed effects:		
State × year	Yes	Yes
Activity × year	Yes	Yes
Adj R-Sq.	0.560	0.560
N	119,379	119,379
Unit of observation	State-activity-year	State-activity-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State

Table 4: The Effect of Mandatory Audits on Individuals' Willingness to Give

This table shows estimates of regressions of variables that capture individuals' willingness to give on the simulated instrument (or *%MandatoryAudit*).

In Panel A, the sample is constructed using form-990 data filed by all charities for the period 1998 to 2015. Observations are at the state-activity-year level.

In Panel B, the sample is constructed using data from the IRS aggregate statistics on individual tax returns. Observations in column 1 (2-3) of Panel B are at the state-year level (county-year level) and cover the period from 1998 to 2015 (2010 to 2015). The specification in column 1 of Panel B includes the following controls at the state-year level: (log) GDP per capita, unemployment, (log) population, and a polynomial term for population. *HighOppCostTime* is an indicator variable equal to 1 for counties where the average individual earnings are in the top quintile in a given year.

In Panel C, the sample is constructed using the form-990 data in the Guidestar dataset for the period 2008 to 2013. Observations are at the state-activity-year level. *InfraMarginalDonations* is the natural log of 1 plus the number of charities that receive, from any one donor, contributions of $5,000 or greater, within a given state-activity-year.

Types of charitable activity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are t-statistics calculated with clustered standard errors. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See appendix for variables definitions.

Panel A: The effect of mandatory audits on total donations in dollars

	(1) LnTotalDonations	(2) LnTotalDonations
Instrument%MandatoryAudit	0.014	
	[0.06]	
Instrument%MandatoryAudit*LowInfoAsymmetry		-0.214
		[-0.62]
Instrument%MandatoryAudit*MedInfoAsymmetry		-0.070
		[-0.29]
Instrument%MandatoryAudit*HighInfoAsymmetry		1.011**
		[2.13]
Fixed effects:		
State × year	Yes	Yes
Activity × year	Yes	Yes
Adj R-Sq.	0.615	0.616
N	119,379	119,379
Unit of observation	State-activity-year	State-activity-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State

Panel B: The effect of mandatory audits on the proportion of taxpayers who give

	(1) %TaxpayersWhoGive (State-year)	(2) %TaxpayersWhoGive (County-year)	(3) %TaxpayersWhoGive (County-year)
Instrument%MandatoryAudit	0.052** [2.03]		
%MandatoryAudit		0.062** [2.45]	0.040** [2.20]
%MandatoryAudit*HighOppCostTime			0.060** [2.36]
HighOppCostTime			0.030*** [4.48]
Controls at state-year level	Yes	Implied	Implied
Year FEs	Yes	Implied	Implied
State-year FEs	No	Yes	Yes
Adj R-Sq.	0.636	0.300	0.315
N	867	15,159	15,159
Unit of observation	State-year	County-year	County-year
Number of clusters for standard errors	51	51	51
Clustering level for standard errors	State	State	State

Panel C: The crowding-out effect of the increase in the number of donors

	InframarginalDonations
Instrument%MandatoryAudit	-0.226* [-1.77]
Fixed effects:	
State × year	Yes
Activity × year	Yes
Adj R-Sq.	0.772
N	29,494
Unit of observation	State-activity-year
Number of clusters for standard errors	51
Clustering level for standard errors	State

Table 5: The Effect of Mandatory Audits on the Diversity in Geographic Areas and Social Needs Served by the Charitable Sector

This table presents estimates of reduced-form regressions of the concentration of donations among cities and among social needs on the simulated instrument. The sample aggregates all public charities and covers the period from 1998 to 2015. The observations are at the state-activity-year level. Types of charitable activity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are t-statistics calculated with clustered standard errors. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See appendix for variables definitions.

	(1) GeographicConcentration	(2) SocialNeedConcentration
Instrument%MandatoryAudit	-0.058**	-0.047***
	[-2.32]	[-2.77]
Fixed effects:		
State × year	Yes	Yes
Activity × year	Yes	Yes
Adj R-Sq.	0.573	0.601
N	119,379	119,379
Unit of observation	State-activity-year	State-activity-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State

Table 6: The Effect of Mandatory Audits on Charities' Financial Reports
This table shows estimates of reduced-form regressions of the dispersion and average of the reported program ratio on the simulated instrument. The reported program ratio is calculated as total program expenses over total expenses (excluding fees for accounting and audit services). The sample aggregates public charities in Guidestar (i.e., the dataset that contains data on the classification of expenses) and covers the period from 1998 to 2015. Types of charitable activity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are t-statistics calculated with clustered standard errors. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See appendix for variables definitions.

	Reported Program Ratio		
	Dispersion		Average
	(1)	(2)	(3)
	ReportedProgRatioStdev	ReportedProgRatioIQR	ReportedProgRatioAvg
Instrument%MandatoryAudit	0.026**	0.043**	-0.023***
	[2.65]	[2.41]	[-2.90]
Fixed effects:			
State × year	Yes	Yes	Yes
Activity × year	Yes	Yes	Yes
Adj R-Sq.	0.138	0.134	0.175
N	70,264	70,264	70,264
Unit of observation	State-activity-year	State-activity-year	State-activity-year
Number of clusters for SEs	51	51	51
Clustering level for SEs	State	State	State

Table 7: The Effect of Mandatory Audits on the Propensity to Hold Public-Charity Status
The table presents estimates of the reduced-form regression of the proportion of 501(c)3 nonprofits that hold public-charity status on the simulated instrument. Observations are at the state-broad category-year level. Broad categories of charitable activity are based on the National Taxonomy of Exempt Entities and are highly aggregated. The broad-category classification is not affected by whether organizations hold public-charity status. The sample aggregates all 501(c)3 organizations and covers the period from 1998 to 2015. Reported below the coefficients are t-statistics calculated with standard errors clustered at the state-broad category level. *, **, and *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively. See appendix for variables definitions.

	%CharityStatus
Instrument%MandatoryAudit	-0.055**
	[-2.51]
Fixed effects:	
State × year	Yes
Broad catogary × year	Yes
Adj R-Sq.	0.880
N	8,235
Unit of observation	State-broad category-year
Number of clusters for standard errors	51
Clustering level for standard errors	State