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The Economic Consequences of Financial Audit Regulation in the Charitable Sector

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ABSTRACT

I provide evidence on the effects of financial audit mandates in the charitable sector, in particular their influence on donor behavior. 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 audit mandates are associated with a lower concentration of donations on the

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largest, most well-known 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. However, I only observe a sizable impact on total contributions in dollars for charities with high inherent information asymmetry. Collectively, these results suggest financial audit regulation reduces information frictions and thereby affects resource allocation in the market for charitable giving.

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

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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 United States 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 financial 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]

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[1] The United States is not an isolated case. For example, Australia, Canada, and the United Kingdom 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]).

However, the separation between the charity managers and the donors gives rise to an agency tension.

In the context of agency issues in the charitable-giving sector, investigating the consequences of nonprofit audit regulation is particularly important, given its sheer economic significance. Indeed, the United States 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, Poliak, and Wing [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, agency problems are especially difficult to address in the charitable sector (Core, Guay, and Verdi [2006]). For instance, private litigation is not a prevalent disciplining mechanism, because entering into formal private contracts with small and 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). Given these frictions, research shedding light on whether financial audit regulation can alleviate the agency problem becomes salient.

I propose that state-level audit mandates reduce donors' moral-hazard concerns for four reasons.[3] First, in the charitable sector, audited financial statements provide assurance over the extent to which donated funds are spent toward charitable programs (AICPA [2017], FASB [2018]). Second, an audit can prompt organizations to improve their governance practices (Duguay [2021]). Third, audit mandates facilitate oversight by third parties like state regulators, the press, and nonprofit watchdogs (e.g., Charity Navigator).[4] Many donors indeed implicitly rely on third-party oversight because performing their own due diligence would be too time consuming. Fourth, audit mandates guarantee that charities will obtain an audit ex post and thereby provide a reliable account of how they spent donated funds.

Through these mechanisms, I predict that audit mandates not only raise individuals' willingness to give, but also shape how donors allocate their contributions among charities. Specifically, I hypothesize that audit mandates lead donors to shift part of their donations toward smaller, lesser

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[3] This paper seeks to evaluate the *overall* effect of nonprofit audit mandates on resource allocation. In other words, I do not to aim to isolate the regulatory component of audit mandates from the generic effects of obtaining a financial audit. (See Kausar et al. [2016] for a paper whose objective is to separate the effects of an audit depending on whether it is obtained voluntarily or mandatorily.)

[4] This explanation is consistent with motives cited by policy makers. 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]).

known charities. Indeed, 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 solving social issues and thus deserve some of their donations (Borgloh, Dannenbergb, and Aretz [2013]). To the extent that audit mandates reduce moral-hazard concerns, donors may rely less on charities' reputation and thus be less likely to concentrate their donations on large, well-known charities.

To test these hypotheses, I conduct empirical analyses at the aggregate level as it allows me to distinguish between the effect on the allocation of donations and the effect on total donations. Specifically, I aggregate the observations by type-of-charity $\times$ state $\times$ year, and I measure the treatment as the percentage of charities that are subject to a mandatory audit in a given aggregation cluster.[5]

The aggregate approach also allows me to use a "differential exposure" research design to account for the endogenous nature of these audit mandates. Under this empirical approach, the intervention occurs at a given level of jurisdiction (at the state level in this case), but the exposure of organizations to the intervention varies along another dimension based on a predetermined characteristic (the type of charitable activity in this case; Goldsmith-Pinkham, Sorkin, and Swift [2020], Breuer [2021b]). Indeed, in the United States, states always enact a single size threshold to exempt smaller charities within their jurisdiction and, as a result, different types of charities have various exposure to audit mandates. For example, homeless shelters are more likely to be large and exceed the exemption threshold compared to after-school programs because operating a homeless shelter requires a building and full-time staff, whereas after-school programs may depend on volunteers. One advantage of this differential exposure design is that it mitigates concerns over the endogenous *timing* of the enactment of audit mandates because it relies on cross-sectional identification, as opposed to time-series identification as with a traditional difference-in-differences (Rajan and Zingales [1998], Mahoney [2015], Breuer, Leuz, and Vanhaverbeke [2021], Breuer [2021a]).[6]

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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. Charities are classified by type of charitable activity by the National Taxonomy of Exempt Entities (see section 4 of the online appendix for a detailed discussion).

[6] Relying on timeseries identification would not be ideal in this setting for three reasons. First, 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 data set.

In addition, this research design allows me to include state-year and type-year fixed effects in my regressions. The state-year fixed effects control 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 type-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., reverse causality). Given these fixed effects and the fact that each observation is a type × state × year, any confounder would have to be specific to a given charity type *and* a given state. One example of such a confounding factor would be a cultural preference, unique to a particular state, for donating to a specific type of charity. I use a simulated instrumental variable to alleviate this particular endogeneity concern. (Currie and Gruber [1996], Mahoney [2015], Breuer, Leuz and Vanhaverbeke [2021], Breuer [2021a]; refer to section 3 for a detailed explanation of the instrument).

I test my hypotheses by analyzing a comprehensive sample of public charities in the United States, covering the period from 1998 to 2015. I aggregate over 2 million charity-year observations representing approximately 380,000 individual charities to arrive at a sample of 119,445 observations at the type-of-charity × state × year level.

Consistent with my empirical predictions, I find that audit mandates are associated with a lower concentration of donations on the largest, most well-known charities. For instance, an audit mandate of moderate scope (i.e., where about 25% of charities are required to obtain an audit) is associated with a 1.8-percentage-point decline in the market share of the largest charity, within a given state and activity type. The effect is muted (more pronounced) for types of charitable activity in which information asymmetry is inherently low (high).[7] This cross-sectional result is consistent with audit mandates alleviating donors' moral-hazard concerns as opposed to charities changing their behavior for reasons unrelated to agency frictions.

I also show 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 can foster greater diversity in the provision of social good. I operationalize this analysis by

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[7] Inherent information asymmetry refers to the degree of separation between donors and recipients. For example, a social club, where donors also tend to be the beneficiaries, has low inherent information asymmetry. In contrast, inherent information asymmetry is high for a charity that conducts its programs abroad. Please refer to subsection 4.2 for a more detailed discussion.

measuring the concentration of donations among (1) geographic areas and (2) granular groups of charities that address specific social needs. For instance, I find that an audit mandate of moderate scope is associated with a 1.4-percentage-point decline in the share of donations that go to charities located in the largest city of a given state.

In terms of individuals' willingness to give, I document that audit mandates are associated with a higher proportion of taxpayers who give. However, I do not find an increase in total donations in dollars (except for types of charitable activity in which information asymmetry is inherently high). An additional analysis suggests the increase in the number of donors comes with a decrease in large donations, explaining why total donations in dollars do not rise (i.e., a crowding-out effect).

To support the credibility of the proposed mechanism, I provide additional discussions and analyses. First, I discuss how individuals may respond to the audit mandate without being explicitly aware of it. For instance, people may react to ratings by nonprofit watchdogs—indeed, charities often obtain higher ratings if they undergo an audit. Similarly, individuals may react to seals of transparency awarded to audited charities by Guidestar, a popular nonprofit directory featured on major giving platforms such as Facebook and Vanguard Charitable. People may also respond to the financial reports posted on charities' website.[8] Second, I show that separating charities based on their financial reports is easier when audit mandates are in place. The idea is that audit mandates should prevent charities from 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 is more dispersed and lower on average.[9] Third, I provide evidence consistent with audit mandates raising the cost of misconduct for charity managers. 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 (Erickson, Hanlon, and Maydew [2004]).

My paper contributes to two streams of research. First, it complements the literature on the consequences of mandatory audits. Within this literature, researchers turn to settings like private firms in Europe to exploit variation in audit mandate exposure (Kausar, Shroff, and White [2016], Minnis and Shroff [2017], Bernard, Burgstahler and Kaya [2018], Nakhmurina [2021], Breuer [2021a]). I add to this stream of research by studying

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[8] Some individuals may also be directly aware of the regulation because the press sporadically mentions the audit mandates and state regulators disseminate information about the mandates via their website.

[9] 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]).

the effect of audit mandates on a different type of organizations: public charities in the United States. These organizations matter economically and their audit mandates are at the heart of a policy debate. However, due the economic differences of the charitable sector, one cannot infer the consequences of nonprofit audit mandates based on research in settings like European private firms.[10] In addition, this paper documents novel insight on mandatory audits. For example, I show that audit regulation can substitute for organizations' own reputation. This finding complements recent work on the market-wide and long-run effects of audit mandates (Breuer [2021a]) and highlights that such effects vary across sectors of the economy (e.g., private-equity market vs. charitable sector) due to institutional differences. As another example, this study documents that audit mandates induce household participation in the market for charitable giving—that is, an empirical finding that connects the audit literature to the stream of research on the determinants of household participation in various markets (e.g., Giannetti and Wang [2016], Gurun, Stoffman, and Yonker [2017], Christensen, Maffett, and Vollon [2019]).

Second, I contribute to the nonprofit literature by demonstrating the real effect of audit mandates on the allocation of donations among charities. 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.).[11] My paper shows that *mandatory* audited reporting plays a role in the allocation of donations. I also contribute to the stream of research on nonprofit regulation (Neely [2011], Desai and Yetman [2015]). Although prior studies evaluate broad sets of regulations (e.g., the Californian Nonprofit Integrity Act), my paper seeks to isolate the economic consequences of audit mandates from other regulatory provisions.

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

2.1 CONCEPTUAL UNDERPINNINGS

2.1.1. Moral Hazard and Financial Audits in the Charitable Setting. 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 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

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[10] For instance, charities receive funding from dispersed donors who are not entitled to a financial return and have limited legal recourse.

[11] 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], Harris et al. [2015], and Harris et al. [2019].

problems (Jensen and Meckling [1976], Fama and Jensen [1983], Core, Guay, and Verdi [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.[12]

These concerns tend to be significant for smaller lesser known charities because such organizations have not yet established a longstanding reputation and cannot demonstrate their legitimacy through years of existence. At the same time, many individuals likely believe that smaller specialized charities play a unique role in generating social good and thus deserve some of their donations (Borgloh, Dannenbergb, and Aretz [2013]). Unresolved moral-hazard concerns may therefore cause donors to concentrate their donations on large, well-known charities.

Audited financial reports can alleviate this moral hazard concern by providing assurance over the extent to which donated funds are spent toward charitable programs. Accounting standards and audit guidance for non-profits emphasize the importance of the classification of expenses (Parsons [2003], AICPA [2017], Crosson and Thompson [2017], FASB [2018]). An audit can foster better stewardship by preventing managers from concealing private-benefit extraction by inflating the ratio of expenses allocated toward charitable programs relative to administration, overhead, fundraising, perquisite consumption, etc. (Core, Guay, and Verdi [2006], Hofmann and McSwain [2013], Arya and Mittendorf [2016]).

Audited financial reports also convey reliable 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]).

Charities may also strengthen their governance as a result of an audit. During the audit process, auditors can identify weaknesses in an organization's governance environment and recommend the implementation of specific governance mechanisms. Duguay [2021] finds that charities subject to an audit mandate are more likely to put in place conflict of interest policies, whistleblower policies, and board approval of the CEO's compensation.

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[12] Major contributors, on the other hand, can obtain insider access and enter into private contracts.

2.1.2. Audit Mandates. In the absence of an audit mandate, charities choose to obtain an audit if they believe it presents a net benefit. Charities frequently do so for contractual reasons (e.g., to abide by the terms of a grant agreement or a mortgage contract). However, in such regime, the potential of audits to address the agency concerns of small individual donors may be limited. Many donors are unlikely to spend the time to determine (or verify) whether charities are audited (Andreoni [2006], DellaVigna, List, and Malmendier [2012], Zingales [2012], Karlan and List [2020]). Others may lack the sophistication to understand what an audit entails.

Because individual donors may not take the time to perform their own due diligence, they may (implicitly) rely on third-party oversight, which can be facilitated by audit mandates. For instance, state-level audit regulations in the United States always require charities to submit their audited financial statement to the state regulator. The regulator can in turn single out charities to investigate based on irregularities (e.g., lack of spending toward charitable programs) and terminate illegitimate charities or take enforcement actions against managers engaged in misconduct. Audit regulation can also help nonprofit watchdogs and 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.

In addition, it is generally unfeasible for small and dispersed donors to enter into private contracts with a charity in order to ensure it will undergo an audit ex post. 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, less-known 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.

Finally, smaller charities may not always be aware of the full benefit of hiring an auditor and therefore, in the absence of an audit mandate, underinvest in audits (Bloom et al. [2013], Bruhn, Karlan, and Schoar [2018]).

2.1.3. Economic Predictions and Null Hypothesis. 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 the largest, most well-known charities. I further predict that this reallocation of donations allows the charitable sector to serve more diverse geographic areas and social needs. The rationale is that the largest

cities tend to host the most well-known charities and thus benefit the most from their local 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). Thus, to the extent that audit mandates shift donations toward lesser known charities, they can foster greater diversity in the provision of social good.

My empirical predictions are not obvious a priori. First, although audited financial reports should provide assurance over donated funds being spent toward charitable programs, they do not contain information on the effectiveness of the programs at solving social issues. In contrast, audited financial reports of for-profit entities provide direct information on financial return. If donors are primarily concerned with the effectiveness of charitable programs rather than moral hazard, they are unlikely to respond to audit mandates.

In addition, 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, Shroff, and White [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).[13] Such a force would work against me finding results. In sum, the effect of audit mandates on donor behavior is an empirical question.

2.2 UNDERLYING ASSUMPTION: AUDIT REGULATIONS CAN ELICIT A RESPONSE FROM DONORS

Having outlined my economic prediction, I discuss the assumption that audit regulations can elicit a response from donors. In other words, even if donors may not explicitly know about the existence of a nonprofit audit

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[13] I use the expression "high-reputation charities" to refer to charities that are large and well known to the public, not the perception of being of good or bad character.

regulation in their state, they may still respond to such a regulation. For instance, nonprofit watchdogs such as Charity Navigator and Charity Watch rate charities based on several criteria including whether they obtain a financial audit. More than 11 million people visit Charity Navigator's website every year and many charities post their Charity Navigator rating on their own website (Charity Navigator [2020a]). These ratings can influence how individuals give, although people may not be explicitly aware that a regulation induces charities to obtain an audit, which in turn affects ratings (Gordon, Knock, and Neely [2009], Harris and Neely [2016], Yörük [2016]). In subsection 4.3, I perform a cross-sectional test that shows the main result is more pronounced when coverage by Charity Navigator is higher.

Similarly, millions of individuals consult Guidestar, a nonprofit directory that covers all charities in the United States (Guidestar [2020a]). These donors may be influenced by the seals of transparency that Guidestar affixes next to certain charities—organizations that obtain a financial audit receive higher seals (Guidestar [2020b]). Furthermore, Facebook's giving platform includes hyperlinks to charities' profile on Guidestar. Similarly, large donor-advised funds such as Vanguard Charitable direct donors to Guidestar's website.

However, ratings and seals by information intermediaries are not the sole mechanism by which an audit regulation can elicit a response from donors. For example, individuals may also respond to financial disclosures on charities' website. Charity Navigator documents that 49% of audited charities publish their audited financial statements online (Charity Navigator [2020c]). In addition, charities subject to an audit mandate are 88% more likely to post their Form 990 financial report on their website.[14] Donors may be more inclined to trust charities that publish their financial reports online.

2.3 AUDIT REQUIREMENTS FOR CHARITIES

Next, I discuss institutional details about charities and the state-level audit mandates. Indeed, 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]).[15] After receiving the forms, the IRS makes them publicly

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[14] I derive this statistic by estimating a parsimonious model using the subset of observations in my sample that file the new long form 990 (i.e., the form that reports whether the Form 990 was posted online). Specifically, I regress an indicator variable for posting the Form 990 online on an indicator variable for the requirement to obtain an audit, the log of total assets, and type-state-year fixed effects.

[15] Form 990-EZ is the abbreviated version of Form 990. It involves more aggregated financial disclosures and fewer nonfinancial 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

available. The filings contain income-statement and balance-sheet information. These financial reports include the portion of expenses classified as program service expenditures, as well as the share of net assets that have donor restrictions.

In the United States, 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 (see table 1). 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.[16]

3. Research Design, Data, and Sample Selection

3.1 RESEARCH DESIGN

3.1.1. Differential Exposure Design. I seek to evaluate the effect of audit mandates on how donors allocate their contributions among charities and on individuals' willingness to give. The treatment in this paper is the scope of financial audit regulation, which I operationalize as the fraction of charities that must obtain an audit to comply with state-level mandates (i.e., as determined by whether a charity's total revenue or contributions exceeds a regulatory threshold).

My empirical analyses are based on a differential exposure design. Under this empirical approach, the intervention occurs at a given level of jurisdiction (e.g., state-level regulations), but the exposure of organizations to the intervention varies along another dimension such as the industry, which is a predetermined characteristic (Goldsmith-Pinkham, Sorkin, and Swift [2020], Breuer [2021b]).

In my setting, the intervention consists of state-level audit mandates in the United States. These mandates always provide a size-based exemption threshold for smaller charities—the thresholds vary across states. In other words, a food pantry in Tennessee (where the cutoff is $500,000 in revenue) is more likely to be subject to a mandatory audit than a food pantry in Georgia (where the cutoff is $1,000,000 in revenue).

In line with prior work, I use industry affiliation, which I refer to as "charity type" given the nonprofit setting, as the source of differential exposure

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total assets above $500,000 or gross receipts above $200,000, in which case they must file a regular Form 990.

[16] See section 4 of the online appendix for a detailed discussion of potential measurement error related to interstate donations.

TABLE 1
State-Level Financial Audit Regulations

State	Median Revenue (in Thousands of $)	Audit Regulation	Exemption Threshold (in Thousands of $)	Measure for Threshold Application	Charities Subject to Mandatory Audit (%)
Alabama	221	N			
Alaska	274	N			
Arizona	264	N			
Arkansas	219	Y	500	Total revenue	29
California	236	Y	2,000	Total revenue	13
Colorado	254	N			
Connecticut	275	Y	500	Total revenue	33
District of Columbia	304	N			
Delaware	827	N			
Florida	248	Y	1,000	Revenue from contributions	9
Georgia	226	Y	1,000	Total revenue	22
Hawaii	269	Y	500	Total revenue	30
Idaho	209	N			
Illinois	260	Y	300	Revenue from contributions	23
Indiana	228	N			
Iowa	223	N			
Kansas	252	Y	500	Revenue from contributions	13
Kentucky	228	N			
Louisiana	266	N			
Maine	272	N			
Maryland	284	Y	500	Revenue from contributions	15
Massachusetts	293	Y	500	Total revenue	39
Michigan	251	Y	525	Revenue from contributions	14
Minnesota	242	Y	750	Total revenue	29
Mississippi	253	Y	500	Revenue from contributions	13
Missouri	263	N			

(Continued)

TABLE 1—*(Continued)*

State	Median Revenue (in Thousands of $)	Audit Regulation	Exemption Threshold (in Thousands of $)	Measure for Threshold Application	Charities Subject to Mandatory Audit (%)
Montana	243	N			
Nebraska	248	N			
Nevada	217	N			
New Hampshire	245	Y	1,000	Total revenue	23
New Jersey	195	Y	500	Total revenue	33
New Mexico	253	Y	500	Total revenue	35
New York	342	Y	500	Total revenue	42
North Carolina	251	N			
North Dakota	290	N			
Ohio	254	N			
Oklahoma	233	N			
Oregon	228	N			
Pennsylvania	277	Y	300	Revenue from contributions	19
Rhode Island	313	Y	500	Total revenue	41
South Carolina	216	N			
South Dakota	246	N			
Tennessee	236	Y	500	Total revenue	30
Texas	212	N			
Utah	291	N			
Vermont	290	N			
Virginia	247	N			
Washington	224	Y	3,000	Total revenue	10
West Virginia	251	Y	500	Revenue from contributions	11
Wisconsin	233	Y	400	Revenue from contributions	15
Wyoming	240	N			

This table presents states that have a financial audit mandate, along with the size-based exemption thresholds, as of the year 2015 (i.e., the end of my sample period). The table also reports the median charity revenue and the fraction of charities subject to a mandatory financial by state. Note that my analyses use the thresholds and mandates in place in a given year, taking into account inflation adjustments to the cutoffs and enactments of new audit mandates during the sample period.

to audit mandates (Bartik [1991], Currie and Gruber [1996], Rajan and Zingales [1998], Autor, Dorn, and Hanson [2013], Bourveau, She, and Žaldokas [2020], Breuer, Leuz, and Vanhaverbeke [2021], Breuer [2021a]).[17] Indeed, the nature of a charity's operations, which varies across types of charities, influences the charity's size and thus whether it exceeds the exemption cutoff. Because states always enact a single exemption threshold for all charities within their jurisdiction, different types of charities have various exposure to audit mandates. Consider homeless shelters and after-school programs. The former are more likely be large and exceed the exemption threshold because their operations require specialized facilities and full-time staff, whereas the latter might get by with volunteers.

Differential exposure designs are geared toward conducting analyses at an aggregate level. Given the setting, I construct observations at the type-of-charity × state × year level. In line with the intent of the paper, this aggregate approach allows me to distinguish between the effect on the allocation of donations and the effect on total donations (i.e., within aggregation cluster). In addition, this research design is suitable to conducting analyses that focus on cross-sectional variation.[18]

Following Breuer [2021a] and Breuer, Leuz, and Vanhaverbeke [2021], 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 type of charity a, state s, year t. To do so, I first determine whether each charity, in a given year, is required to obtain an audit by comparing the charity's revenue to the revenue threshold in effect in the state[19]:

$$MandatoryAudit_{it} = \mathbf{1}\left(RevenueMeasure_{it} \geq ExemptionThreshold_{st}\right). \quad (1)$$

Then, for each type-of-charity × state × year, I calculate the number of charities required to be audited and divide it by the total number of charities:

$$\%MandatoryAudit_{ast} = \frac{1}{N_{ast}} \sum_{it \in N_{ast}} MandatoryAudit_{it}. \quad (2)$$

N_{ast} is the entire set of type-of-charity × state × year in the data.

My specification includes state-year and type-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' generosity across states. Moreover, these fixed effects allow for state-level endogeneity in the

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[17] I define types of charities based on two-digit NTEE codes. See section 4 of the online appendix for a detailed discussion of this choice.

[18] In section 1 of the online appendix, I provide additional reasons, given the institutional setting, for relying on a cross-sectional approach as opposed to a traditional difference-in-differences design based on a time-series variation.

[19] For charities in states without an audit mandate, $MandatoryAudit_{it}$ is naturally equal to 0.

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

3.1.2. Simulated Instrument. Despite these fixed effects, the differential exposure design described so far remains subject to endogeneity at the type-state level (refer to section 1 of the online appendix for examples of this endogeneity concern). To address this concern, I use a simulated instrumental variable (Currie and Gruber [1996], Mahoney [2015], Breuer, Leuz, and Vanhaverbeke [2021], Breuer [2021a]).[20] I construct the instrument in two steps. Consider a given type-state-year observation (e.g., animal protection organizations, New York, 2015). First, I pool all the charities for the given type across all states (e.g., animal protection, across all states, 2015). Second, within this set of charities, I determine the fraction of charities that would be required to obtain an audit if the given state's threshold applied (e.g., the fraction of animal protection charities across all states that have revenue above \$500,000 in 2015, where \$500,000 is New York's threshold).[21]

Therefore, for a given charity type, the instrument always relies on the same national distribution of charity size. This distribution can be viewed as a typical distribution for that charity type (Breuer [2021b]). In other words, the instrument measures the fraction of charities that would typically be required to obtain an audit if the state's threshold were applied to the typical size distribution (i.e., for each charity type). Any variation in the instrument must come from the differential effect of the state-level thresholds on the different charity types (driven, for instance, by operational factors such as homeless shelters being usually larger than after-school programs). Therefore, because these national size distributions represent charities from all states rather than one state in particular, the instrumentation approach removes endogenous variation specific to type-state pairs (e.g., a donation tax credit that applies to a specific type of charity in a specific state).

Formally, I construct the instrument, *Instrument%MandatoryAudit*, as follows:

$$Instrument\%MandatoryAudit_{ast} = \frac{1}{N_{at}} \sum_{it \in N_{at}} MandatoryAudit_{it}. \quad (3)$$

N_{at} is the entire set of type-of-charity $\times$ year in the data. Refer to section 6 of the online appendix for an empirical analysis that illustrates the ability of my identification strategy to purge confounding variables.

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[20] The simulated instrument is a special of the Bartik instrument commonly used in economics (e.g., Bartik [1991], Autor et al. [2013], Greenstone et al. [2020]). Breuer [2021b] provides an applied introduction to Bartik instruments for accounting researchers.

[21] Refer to section 2 of the online appendix for a real numerical example.

3.1.3. Regression Model. Estimate the following regressions using ordinary least squares (OLS)[22]

$$Y_{ast} = \alpha_{st} + \alpha_{at} + \beta Instrument\%MandatoryAudit_{ast-1} + \in_{ast}. \qquad (4)$$

Y_{ast} is the outcome variable and varies across analyses. α_{st} and α_{at} are state-year and type-year fixed effects, respectively. *Instrument%MandatoryAudit* is the instrumental variable as defined in equation (3).

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 broad category of activity-state (Breuer [2021a]). The broad categories aggregate the types of charity 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, Gonçalves, and Hansen [2018]). The second alternative method is to cluster the standard errors by type-state, simply allowing for infinite serial correlation across type-state-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.

Finally, I also acknowledge that this research design cannot completely rule out endogeneity. In section 1 of the online appendix, I describe the residual endogeneity concern and explain, based on institutional details, why I believe a violation of the identifying assumption is unlikely.

3.2 DATA

I use data from the Forms 990 and 990-EZ that public charities file annually. The IRS in turn makes the forms publicly available. 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.[23]

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

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[22] Following Breuer [2021a], I report the coefficient estimates of OLS regressions of the outcome variable on the instrument variable. Angrist and Pischke [2009, 2015] refer to this specification as the reduced form, in contrast to the two-stage least squares (2SLS) specification (which first regresses the treatment on the instrument and then regresses the outcome variable on the predicted values of the first stage). The advantage of the reduced-form specification is that, unlike 2SLS, it produces unbiased estimates (Angrist and Pischke [2009]). Nevertheless, I report the coefficient estimates of the second stage of 2SLS regressions in the online appendix (table A5).

[23] See Feng et al. [2014] for a detailed discussion of the availability of Form 990 data.

not available in the NCCS core files. Some of my additional tests require this particular data point to measure the effect on financial reporting. Therefore, for the tests 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 2015. Guidestar covers essentially all the data points on the Form 990 but has less complete historical coverage of smaller charities.

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, and the Bureau of Economic Analysis. 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 (see table 1).

3.3 SAMPLE SELECTION

I retain all unique charity-year observations with at least $50,000 in gross receipts during the period from 1998 to 2015.[24] I then construct observations at the type-state-year level from over two million charity-year observations representing approximately 380,000 individual charities. I eliminate observations without prior-year data as well as observations for which the type is assigned to the unknown category. I also drop observations from sectors exempted from the audit requirements studied in this paper such as health, education, mutual organizations (i.e., these sectors fall within the purview of distinct industry-specific audit regulations; Neely [2011], Desai and Yetman [2015], Duguay [2021]).[25] I winsorize continuous variables at the 1st and 99th percentile prior to aggregation to mitigate concerns over extreme values.[26] The final sample comprises 119,445 type-state-year observations, each containing on average 23 charities, and representing 182 types of charities. I provide summary statistics in table 2, panel A. The other panels of table 2 contain statistics on the samples I use in additional analyses. Refer to section 4 of the online appendix for additional details about the data and sample.

Table 1 reports, for each state, the fractions of charities required to obtain an audit by an audit mandate (i.e., the treatment). The table reports sizeable variation in the scope of audit mandates across states, consistent with this setting being suitable to exploit cross-sectional variation in audit

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

[25] In addition, veteran organization are exempted from the audit requirement in Florida, Illinois, Massachusetts, New Jersey, Rhode Island, and Wisconsin, while fraternal societies are exempted in Illinois and Kansas. My sample therefore exclude these charities.

[26] None of the statistical and economic inferences in the paper are sensitive to the decision to winsorize.

TABLE 2
Summary Statistics

Panel A: Summary statistics for variables used in the tests reported in tables 3–6

Variable	Mean	SD	P25	Median	P75	N	Unit of Observation
Instrument%MandatoryAudit	0.124	0.183	0.000	0.000	0.236	119,445	Type-state-year
%MandatoryAudit	0.120	0.227	0.000	0.000	0.167	119,445	Type-state-year
ConcentrationDonation	0.492	0.324	0.208	0.426	0.787	119,445	Type-state-year
LnTotalDonations	14.639	2.257	13.138	14.783	16.295	119,445	Type-state-year
GeographicConcentration	0.572	0.314	0.295	0.520	0.926	119,445	Type-state-year
ShareLargestCity	0.667	0.271	0.438	0.668	0.962	119,445	Type-state-year
SocialNeedConcentration	0.770	0.251	0.533	0.877	1.000	119,445	Type-state-year

Panel B: Summary statistics for variables used in the test reported in table 4, panel B

Variable	Mean	SD	P25	Median	P75	N	Unit of Observation
Instrument%MandatoryAud	0.121	0.172	0.000	0.000	0.231	91,537	Type-state-year
ConcentrationDonation	0.490	0.323	0.209	0.425	0.781	91,537	Type-state-year
LnTotalDonations	14.703	2.255	13.209	14.859	16.370	91,537	Type-state-year
RatingsCoverage	0.000	1.000	−0.551	−0.247	0.187	91,537	Type-state-year

Panel C: Summary statistics for variables used in the test reported in table 6, panel A

Variable	Mean	SD	P25	Median	P75	N	Unit of Observation
Instrument%MandatoryAudit	0.116	0.162	0.000	0.000	0.252	864	State-year
%TaxpayersWhoGive	0.831	0.048	0.803	0.833	0.864	864	State-year
GDPPerCapita (in thousands)	47.661	17.214	39.347	44.520	51.213	864	State-year
UnemploymentRate	0.058	0.020	0.043	0.054	0.068	864	State-year
AdjGrossIncome (in thousands)	171.707	44.838	140.075	162.901	193.836	864	State-year

Panel D: Summary statistics for variables used in the test reported in table 6, panel A

Variable	Mean	SD	P25	Median	P75	N	Unit of Observation
%MandatoryAudit	0.069	0.129	0.000	0.000	0.100	14,930	County-year
%TaxpayersWhoGive	0.747	0.123	0.712	0.770	0.818	14,930	County-year
GDPPerCapita (in thousands)	44.032	48.061	27.115	36.419	49.296	14,930	County-year
UnemploymentRate	0.072	0.028	0.051	0.069	0.088	14,930	County-year
AdjGrossIncome (in thousands)	237.317	80.229	183.065	221.909	272.573	14,930	County-year

Panel E: Summary statistics for variables used in the test reported in table 3 and table 6, panel D

Variable	Mean	SD	P25	Median	P75	N	Unit of Observation
Instrument%MandatoryAudit	0.215	0.276	0.000	0.000	0.441	44,561	Type-state-year
%MandatoryAudit	0.206	0.319	0.000	0.000	0.375	44,561	Type-state-year
%Audit	0.596	0.318	0.385	0.636	0.857	44,561	Type-state-year
Donation≥5K	1.721	1.092	0.693	1.609	2.398	44,561	Type-state-year

(*Continued*)

T A B L E 2—*(Continued)*

This table reports the summary statistics for variables used in the tests reported in tables 3–6. See the appendix for variables definitions. In panel A, the sample is constructed using the Form 990 data in the NCCS data set (complemented by Guidestar, if applicable) for the period from 1998 to 2015. The macroeconomic variables are retrieved from the Bureau of Labor Statistics and the Bureau of Economic Analysis. In panel B, the sample is constructed using the Form 990 data in the NCCS data set (complemented by Guidestar, if applicable) for the period from 2003 to 2015. The macroeconomic variables are retrieved from the Bureau of Labor Statistics and the Bureau of Economic Analysis. In panel C, the sample is constructed using the IRS statistics on individual tax returns at the state-year level for the period from 1998 to 2015. The macroeconomic variables are retrieved from the Bureau of Labor Statistics and the Bureau of Economic Analysis. In panel D, the sample is constructed using the IRS statistics on individual tax returns at the county-year level for the period from 2010 to 2015 (the county-year data are not available prior to 2010). The macroeconomic variables are retrieved from the Bureau of Labor Statistics and the Bureau of Economic Analysis. In panel E, the sample is constructed using the post-2008 Form 990 data in the Guidestar data set for the period 2008–2015.

T A B L E 3
Instrument Relevance

	%MandatoryAudit
Instrument %MandatoryAudit	0.978***
	[34.29]
Fixed effects:	
State × year	Yes
Activity × year	Yes
Adjusted R^2	0.642
N	119,445
Unit of observation	Type-state-year
Number of clusters for standard errors	51
Clustering level for standard errors	State

This table shows the association between the instrument and the actual share of charities subject to a mandatory—that is, the first-stage regression. This test allows to assess whether the instrument satisfies the relevance criterion. Observations are at the type-state-year level. Types of charity are based on the National Taxonomy of Exempt Entities. The sample aggregates all public charities and covers the period 1998–2015. Reported below the coefficients are *t*-statistics calculated with standard errors clustered at the state level. *** indicates significance at the two-tailed 1% level. See the appendix for variables definitions.

mandates. In section 4 of the online appendix, I perform a test to alleviate the concern that audit mandates have little impact on the propensity to obtain audits (e.g., if charities were predominantly large and audited regardless of audit mandates). Comfortingly, the results are consistent with the scope of audit mandates driving the prevalence of audits.

4. Main 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 (4) with *%MandatoryAudit* as the left-hand-side variable. In table 3, 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.[27] By construction, the coefficient estimate is close to 1, which is consistent with prior studies that use a similar simulated instrument approach (Mahoney [2015], Breuer, Leuz, and Vanhaverbeke [2021], Breuer [2021a]). Nevertheless, the instrument has a distribution across observations and contributes to the identification strategy. In section 6 of the online appendix, I perform an analysis to illustrate the role of the simulated instrument, using an approach similar to that of Breuer [2021a].

4.2 AUDIT MANDATES AND THE ALLOCATION OF DONATIONS AMONG CHARITIES

Having shown the relevance of the simulated instrument, I now 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 well-known charities. The proxy I use is the Herfindahl–Hirschman index, which is the sum of the squared shares of donations within a given type-state-year observation. 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 charities. In the online appendix (table A6), I find similar results as I replicate my analyses using alternative proxies (i.e., market share of largest donation charity, market share of largest revenue charity, and market share of largest asset charity).

Table 4, panel A, column 1, reports the coefficient estimates of the 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.

I acknowledge that the economic magnitude is not straightforward to interpret. For one, the dependent variable can be difficult to gauge intuitively because it is a measure of the concentration of donations among charities. In addition, to calculate the average treatment effect of the audit mandate on the outcomes of interest, one must technically refer to the second-stage regressions, which I report in the online appendix (see table A5; Angrist and Pischke [2009, 2015]).[28] One should also bear in mind that a one-unit

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[27] 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 (table A3) and find similar positive and strongly significant relations.

[28] The coefficient estimates I report in the main tables are proportional, but not equal, to the causal effect of interest because they result from regressions of the outcome of the instrument (Angrist and Pischke 2009).

TABLE 4

The Effect of Mandatory Audits on Donors' Reliance on Charities' Reputation

Panel A: The effect of mandatory audits on the concentration of donations among charities

	(1) *Concentration-Donations*	(2) *Concentration-Donations*
Instrument%MandatoryAudit	−0.061**	−0.057**
	[−2.34]	[−2.16]
Instrument%MandatoryAudit × LowInfoAsymmetry		0.050
		[0.97]
Instrument%MandatoryAudit × HighInfoAsymmetry		−0.072**
		[−2.63]
Fixed effects:		
State × year	Yes	Yes
Type × year	Yes	Yes
Adjusted R^2	0.598	0.598
N	119,445	119,445
Unit of observation	Type-state-year	Type-state-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State
F-test for sum or difference of coefficients (*p*-value):		
Inst%MandatoryAudit + Inst%MandatoryAudit × LowIA = 0		0.889
Inst%MandatoryAudit + Inst%MandatoryAudit × HighIA = 0		0.003
Inst%MandatoryAudit × HighIA = Inst%MandatoryAudit × LowIA		0.059

Panel B: The role of charity evaluators

	Concentration-Donations
Instrument%MandatoryAudit × RatingCoverage	−0.035*
	[−1.70]
Instrument%MandatoryAudit	−0.060**
	[−2.17]
RatingCoverage	−0.007
	[−1.59]
Fixed effects:	
State × year	Yes
Type × year	Yes
Adjusted R^2	0.597
N	91,537
Unit of observation	Type-state-year
Number of clusters for standard errors	51
Clustering level for standard errors	State

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 (panel A) and 2003 to 2015 (panel B). The observations are at the type-state-year level. Types of charity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are *t*-statistics calculated with standard errors clustered at the state level. * and ** indicate significance at the two-tailed 10% and 5% levels, respectively. See the appendix for variables definitions.

change in these coefficient estimates is equivalent to going from no audit mandate 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. Dividing the coefficients by 4 provides estimates that reflect 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. Finally, I caveat that these coefficients represent average effects and do not speak to heterogeneity in the effect across states and activity types.

One may be concerned the observed effects are not attributable to donor behavior and moral hazard concerns (i.e., the proposed mechanism), but rather to changes in charity behavior unrelated to agency frictions. For example, charities could respond to audit mandates by organizing as small entities that fall under the exemption cutoff or as large organizations that can better absorb the fixed costs of a mandatory audit. Similarly, charities could increase fundraising to offset the cost of an audit. To address this concern, I perform a cross-sectional test in which I exploit variation in information asymmetry between donors and charity managers across types of charitable activity. I predict the effect on the allocation of donations to be stronger (muted) when information asymmetry is particularly high (low). On the other hand, I do not predict a positive correlation between the effect and information asymmetry if charities are merely responding to audit mandates for reasons unrelated to agency frictions. Naturally, the endogeneity concern in this cross-sectional test is that an omitted variable correlates with both my measure of information asymmetry and the intensity of the effect.

I single out the types of charitable activities in which information asymmetry is likely to be inherently high or low based upon the degree of separation between donors and the eventual beneficiaries of the charity. The types of charity I identify as having inherently 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 inherently 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 inherent 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.[29]

I create the cross-sectional variables *LowInfoAsymmetry* and *HighInfoAsymmetry* to single out the types of charities that have inherently low or high information asymmetry, and interact each variable with *Instrument%MandatoryAudit.* One drawback with the approach is the subjectivity involved in assigning the types of activity to each category. In the online appendix (table A8), I find similar results if I use an alternative cross-sectional variable based on scrutiny by government or federated funds (e.g., the United Way), which is more objective. The rationale of the alternative test is that moral hazard is less of a concern for types of charity that depend more on such funding and are therefore subject to greater monitoring by the government or federated funds.[30]

I report the coefficient estimates of the cross-sectional test in column 2 of panel A of table 4.[31] As predicted, the effect on the allocation of donations is more pronounced when information asymmetry is inherently high and mandatory audits are expected to be the most useful. Although the interaction term with *LowInfoAsymmetry* is not statistically significant, the sum of *Instrument%MandatoryAudit* and *Instrument%MandatoryAudit×LowInfoAsymmetry* is close to 0 and, according to the *F*-test, statistically indistinguishable from 0, consistent with the effect being nonexistent when information asymmetry is particularly low. Furthermore, an *F*-test reveals that the low and high interaction terms are statistically different.

4.3 THE ROLE OF CHARITY EVALUATORS

To further evaluate the channel by which audit mandates can shape the allocation of donations, I exploit variation in coverage by Charity Navigator. Charity Navigator, one of the largest charity evaluators in the United States, rates charities based on multiple factors, including obtaining an audit. Prior work documents that donors respond to ratings by Charity Navigator and other evaluators (Gordon, Knock, and Neely [2009], Harris and Neely [2016], Yörük [2016]).

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[29] My definition of inherent information asymmetry refers to the ex ante information asymmetry between donors and charity managers, based on the extent to which donors are able to observe the disbursement of funds to intended beneficiaries. This definition thus does not include the ex post effects of managerial actions to provide transparency to donors through better reporting, nor does it include the efforts of third-party agencies to mitigate the information asymmetry ex post through ratings. Refer to section 9 of the online appendix for more details about how I define and validate my information asymmetry proxies.

[30] Although more objective, this alternative cross-sectional test is subject to greater endogeneity concern. For example, types of charities that receive more donations for unrelated reasons may apply to fewer grants, because their financial needs are already met. Such explanation differs from the proposed interpretation that donors respond more to audit mandates for types of charities that are not already monitored by the government or federated funds.

[31] The main effects for *LowInfoAsymmetry* and *HighInfoAsymmetry* are subsumed by the activity type fixed effects.

In its methodology, Charity Navigator states that it rates charities that meet screening criteria with respect to length of operations, public support, fundraising revenue, and administrative expenses (Charity Navigator [2020b]). These screening criteria create cross-sectional variation in coverage by Charity Navigator.

I obtain coverage data from Charity Navigator and construct the cross-sectional variable *RatingCoverage*. Specifically, I compute, within a given type-state-year, the share of charities rated by Charity Navigator, and standardize the variable for ease of interpretation. I estimate a version of equation (4) where I interact *Instrument%MandatoryAudit* with *RatingCoverage*, and report the results in panel B of table 4.[32] I find a weakly significant coefficient of -0.035, suggesting a one-standard-deviation increase in ratings coverage is associated with a 57% increase in the magnitude of the effect (i.e., $-0.035/-0.061$). This result is consistent with ratings being a channel by which audit mandates affect the allocation of donations.

4.4 AUDIT MANDATES AND DIVERSITY IN THE PROVISION OF SOCIAL GOOD BY THE CHARITABLE SECTOR

So far, 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 (4) with the outcome variable being the Herfindahl-Hirschman index that captures the concentration of donations among cities or towns (i.e., for a given type-state-year observation). Recall, the within-state design controls for differences between urban and rural population across states. I find a significant and negative association between audit mandates and the concentration of donations among geographic areas. Column 2 shows a similar result using, as an alternative proxy, the share of donations that flows to the largest city within a given type-state-year.[33] In column 3, 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

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[32] For this test, the sample period begins in 2003, which is the earliest year for which Charity Navigator data are available.

[33] The result in column 2 suggests that an audit mandate of moderate scope (i.e., where about 25% of charities are required to obtain an audit) is associated with a 1.4-percentage-point (i.e., -0.056×0.25) decrease in the share of donations that go to charities located in the largest city of a given a state.

TABLE 5

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

	(1) *Geographic-Concentration*	(2) *ShareLargest-City*	(3) *SocialNeed-Concentration*
Instrument %MandatoryAudit	−0.059**	−0.056**	−0.041**
	[−2.32]	[−2.46]	[−2.17]
Fixed effects:			
State × year	Yes	Yes	Yes
Type × year	Yes	Yes	Yes
Adjusted R^2	0.573	0.531	0.620
N	119,445	119,445	119,445
Unit of observation	Type-state-year	Type-state-year	Type-state-year
Number of clusters for SEs	51	51	51
Clustering level for SEs	State	State	State

This table presents estimates of reduced-form regressions of the concentration of donations among cities, the share of donations of the largest city, and the concentration of donations 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 type-state-year level. Types of charity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are *t*-statistics calculated with clustered standard errors. ** indicates significance at the two-tailed 5% level. See the appendix for variables definitions.

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 INDIVIDUALS' WILLINGNESS TO GIVE

Next, I evaluate the effect of audit mandates on individuals' willingness to give. I study two outcomes: (1) the number of individuals who make charitable donations and (2) total donations in dollars. To perform the first analysis, I turn to IRS data on individuals' tax returns because the Form 990 data only report information on the total donations charities receive. 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]).[34]

The IRS only provides the data on individuals at the state-year or county-year level. To accommodate the data, I modify the specification in equation (4) to use observations at the state-year level (rather than type-state-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

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[34] This proxy, although broadly relied upon in the literature, may introduce 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]).

some of the specifications in Mahoney [2015]. Because this particular analysis exploits cross-state variation, I cannot include state-year fixed effects. I therefore add macroeconomic controls at the state-year level that may influence the likelihood that taxpayers donate: the GDP per capita, the unemployment rate, and the average adjusted gross income for the taxpayers in question.[35] I report the coefficient estimates in column 1 of panel A of table 6. I find a positive and statistically significant association between the instrument and the proportion of taxpayers who give, consistent with audit mandates increasing the likelihood that people donate.

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.[36] This approach allows me to include state-year fixed effects and county-year controls, but prevents me from using the instrument (the state-year fixed effects would subsume any variation in the instrument). I report the results of the regression in column 2 of panel A. Again, I find a positive and significant association, suggesting an increase in the share of tax payers who donate to charities.

Next, I investigate the effect on total donations in dollars. I estimate equation (4) with the outcome variable as the natural logarithm of the sum of the donations received by charities in a given type-state-year. I report the coefficient estimates in table 6, panel B, column 1. The estimate for *LnTotalDonations* is statistically indistinguishable from 0. I then perform a cross-sectional test using the same information asymmetry proxies as in table 4. Column 2 reveals a positive and significant coefficient on the interaction term with *HighInfoAssymetry*. In addition, an *F*-test shows that sum of *Instrument%MandatoryAudit* and *Instrument%MandatoryAudit×HighInfoAssymetry* is positive and statistically significant. In other words, there is a positive relation between the scope of audit mandates and total donations, but only when information asymmetry is high.[37]

In panel C, I perform a cross-sectional test based on ratings, similar to table 4, panel B. Unsurprisingly, I find the coefficient of the triple interaction term to be statistically indistinguishable from 0. This pattern is consistent with ratings influencing how individuals allocate their donation

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[35] I do not have a specific ex ante prediction for the sign or significance of the control variables. For instance, in richer states, people tend to have more disposable income and may therefore be more likely to give. On the other hand, prior research documents that lower income groups are often more likely to donate than richer groups mainly because of religious motives (List [2011]).

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

[37] Although the increase in donations may be significant for the charities with high inherent information asymmetry, these charities constitute a small subset of the entire charitable sector in the United States, which is not economically significant. 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.

TABLE 6

The Effect of Mandatory Audits on Individuals' Willingness to Give

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

	(1) %TaxpayersWhoGive (State-Year)	(2) %TaxpayersWhoGive (County-Year)
Instrument%MandatoryAudit	0.060***	
	[2.97]	
%MandatoryAudit		0.059**
		[2.48]
GDPPerCapita	0.003**	−0.001
	[2.62]	[−0.75]
%Unemployment	0.097	−0.825***
	[0.39]	[−4.25]
AdjGrossIncome	−0.006***	−0.001
	[−4.51]	[−0.75]
Fixed effects:		
Year	Yes	Implied
State × year	No	Yes
Adjusted R^2	0.652	0.308
N	864	14,930
Unit of observation	State-year	County-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State

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

	(1) LnTotalDonations	(2) LnTotalDonations
Instrument%MandatoryAudit	0.052	−0.018
	[0.24]	[−0.08]
Instrument%MandatoryAudit × LowInfoAsymmetry		−0.157
		[−0.60]
Instrument%MandatoryAudit × HighInfoAsymmetry		0.923***
		[3.23]
Fixed effects:		
State × year	Yes	Yes
Type × year	Yes	Yes
Adjusted R^2	0.626	0.627
N	119,445	119,445
Unit of observation	Type-state-year	Type-state-year
Number of clusters for standard errors	51	51
Clustering level for standard errors	State	State
F-test for sum or difference of coefficients (*p*-value):		
Inst%MandatoryAudit + Inst%MandatoryAudit × LowIA = 0		0.566
Inst%MandatoryAudit + Inst%MandatoryAudit × HighIA = 0		0.023
Inst%MandatoryAudit × HighIA = Inst%MandatoryAudit × LowIA		0.017

(*Continued*)

T A B L E   6—*(Continued)*

Panel C: The role of charity evaluators

	LnTotalDonations
Instrument%MandatoryAudit × RatingCoverage	0.011
	[0.08]
Instrument%MandatoryAudit	0.092
	[0.43]
RatingCoverage	0.193***
	[6.75]
Fixed effects:	
State × year	Yes
Type × year	Yes
Adjusted R^2	0.628
N	91,537
Unit of observation	Type-state-year
Number of clusters for standard errors	51
Clustering level for standard errors	State

Panel D: The effect of mandatory audits on large contributions from individual donors

	Donation≥ 5K
Instrument%MandatoryAudit	−0.235**
	[−2.28]
Fixed effects:	
State × year	Yes
Type × year	Yes
Adjusted R^2	0.776
N	44,561
Unit of observation	Type-state-year
Number of clusters for standard errors	51
Clustering level for standard errors	State

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 data from the IRS aggregate statistics on individual tax returns. Observations in column 1 (2) of panel A are at the state-year level (county-year level) and cover the period from 1998 to 2015 (2010 to 2015). The macroeconomic variables in columns 1 (2) are at the state-year level (county-year level) and are retrieved from the Bureau of Labor Statistics and the Bureau of Economic Analysis. *GDPPerCapita* and *AdjGrossIncome* are in tens of thousands. In panel B, the sample is constructed using Form 990 data filed by all charities for the period 1998–2015. Observations are at the type-state-year level. In panel C, the sample is constructed using Form 990 data filed by all charities and Charity Navigator's ratings for the period 2003–2015. Observations are at the type-state-year level. In panel D, the sample is constructed using the post-2008 Form 990 data in the Guidestar data set for the period 2008–2015. Observations are at the type-state-year level. *Donation≥ 5K* is the natural log of the number of charities that receive, from any one donor, contributions of $5,000 or greater, within a given type-state-year. Types of charity are based on the National Taxonomy of Exempt Entities. Reported below the coefficients are *t*-statistics calculated with standard errors clustered at the state level. **, and *** indicate significance at the two-tailed 5% and 1% levels, respectively. See the appendix for variables definitions.

among charities, but not the total amount they give to a given cause—that is, in line with Charity Navigator's reports on how people use its ratings (Charity Navigator [2021]).

Finally, I seek to reconcile the absence of an effect on total donations with the increase in the number of donors. One potential explanation is a crowding-out effect (Roberts [1984], Bergstrom, Blume, and

Varian [1986], Andreoni and Payne [2013], Correa and Yildirim [2013]). 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 crowding-out phenomenon can occur because charities solicit less from each individual donor when securing funding is easier (in this case because more people give; Andreoni and Payne [2011], Andreoni and Payne [2013]).

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 inframarginal 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 inframarginal donations.

Consistent with this prediction, in panel D of table 6, I find a negative and significant association between the instrument and *Donation≥ 5K* (i.e., the proxy for inframarginal donations).[38] This result is consistent with audit mandates being associated with more people who give, which in turn crowds out part of the inframarginal donations.

5. *Additional Analyses*

Having discussed the main results, I perform additional analyses to support the assumptions that audit mandates (1) curtail management's ability to misreport (and thereby conceal private-benefit extraction) and (2) raise the cost of misconduct for charity managers.

Specifically, in section 12 of the online appendix, I test whether audit mandates make it easier to use financial reports to separate charities. If mandatory audits are effective, they should prevent charities from

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[38] The number of observations in table 6, panel D is lower than in table 4, panel A and table 6, panel B, because (1) I use information that charities only began reporting on their Form 990 in 2008, (2) many charities file the Form 990-EZ, which does not contain the information in question, and (3) I rely on the Guidestar data set (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 (table A9), I replicate the main results using this smaller sample to alleviate the concern that a change in sample composition plays into my findings. Although I find similar results, the association between the instrument and *ConcentrationDonations* becomes weakly significant. This decline may be attributable to a loss in statistical power due to sample size, but also to unspecified economic factors associated with the change in sample composition. The observed decrease in inframarginal donations may therefore not completely generalize to the main sample.

uniformly reporting that a high (and potentially inflated) proportion of their spending is directed toward the charitable programs. I find that, when an audit mandate is in place, the reported program ratio net of audit fees is more dispersed and, on average, lower, suggesting that separating charities based on the reported program ratio is easier.

I provide support for the second assumption in section 13 of the online appendix. I explain that nonprofit managers choose whether to elect for the public charity stratus, which can raise the risk that illegitimate spending be detected when audits are mandatory. I then hypothesize and find that fewer organizations hold public charity status when an audit mandate is in place.[39]

6. Conclusion

In this paper, I study the economic consequences of financial audit regulation in the charitable sector. I use a differential exposure design that relies on variation in size-based exemption thresholds across states and differences in size across types of charity. I find that, when audit mandates are in place, donations are less concentrated on high-reputation charities and more people give. In addition, the reallocation of resources allows the charitable sector to serve more diverse geographic areas and social needs.

This study is not without limitations. For instance, my empirical analyses rests on empirical assumptions likes the exclusion restriction, which can be argued for but not proven. In addition, I show 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, I choose not to extend the scope of this paper to the costs of audit mandates because there is general agreement that audit regulation imposes costs on charities in the form of audit fees and time spent managing the audit process (Minnis [2011], McWhirter [2014], Duguay, Minnis, and Sutherland [2020]). Despite these limitations, this study offers novel insight on the role of mandatory audits and provide empirical findings that may prove informative to the policy debate over nonprofit audit mandates.

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[39] Note that the cross-sectional analyses based on ratings coverage and information asymmetry are useful to evaluate heterogeneity in the effect of audit mandates on *donors*, who are outsiders. In contrast, such cross-sectional tests would not be relevant for these additional analyses, which focus on *managers*' response to audit mandates.

APPENDIX

Variable Definitions

Variable	Definition
AdjGrossIncome	The average adjusted gross income of taxpayers in a given state-year or county-year.
ConcentrationDonations	The sum of the squared shares of donations among charities within a given type-state-year.
GeographicConcentration	The sum of the squared shares of donations among cities within a given type-state-year.
Donation≥ 5K	The natural log of the number of charities that receive, from any one donor, contributions of $5,000 or greater, within a given type-state-year.
GDPPerCapita	The gross domestic product per capita in a given state-year or county-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. Refer to section 9 of the online appendix for the detailed list of NTEE codes.
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 type-year.
LnTotalDonations	The natural log of the sum of the donations of the charities in a given type-state-year.
LowInfoAsymmetry	An indicator variable equal to 1 for types of charity that include community clubs, recreational or social clubs, amateur sport clubs, fraternal societies, and youth programs like Boy and Girl Scouts. Refer to section 9 of the online appendix for the detailed list of NTEE codes.
RatingCoverage	The fraction of charities for which a rating from Charity Navigator is available, standardized.
ShareLargestCity	The share of total donations received by charities in the largest city within a given type-state-year.
SocialNeedConcentration	The sum of the squared shares of donations among social needs within a given type-state-year. (Three-digit NTEE codes proxy for social needs within a charity type.)
UnemploymentRate	The unemployment rate in a given state-year or county-year.
%Audit	The fraction of charities that obtain a financial audit within a given type-state-year.
%MandatoryAudit	The fraction of charities subject to a mandatory financial audit within a given type-state-year.
%TaxpayersWhoGive	The fraction of itemizing taxpayers who claim a deduction for charitable contributions within a given state-year or county-year.

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