Effects of Anti-Corruption Audits on Early-Life Mortality: Evidence from Brazil

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Abstract

Although various studies suggest that corruption affects public health systems, the literature lacks causal evidence about whether anti-corruption interventions can improve health outcomes. The present article provides novel evidence that one such intervention — anti-corruption audits — improved early-life mortality in Brazil. The Brazilian government conducted audits in 1,949 randomly selected municipalities between 2003 and 2015. To identify the causal effect of anti-corruption audits on early-life mortality, we analyze official data on health outcomes from individual-level vital statistics before and after the intervention. A randomly audited municipality is estimated to experience 0.48 fewer child deaths (95% CI: -0.81, -0.15) and 0.34 fewer infant deaths (-0.61, -0.07) per year, relative to never experiencing an audit. The audit program is estimated to have prevented the deaths of 7,014 (2,216, 11,813) children, including 5,028 (891, 9,165) infants. The observed mortality in audited municipalities is approximately 94 percent of the child deaths, and 95 percent of the infant deaths, that would have occurred in the absence of the intervention. Early-life mortality fell especially sharply for nonwhite Brazilians, who face significant health disparities. Effects are greater when examining deaths from preventable causes, and show temporal persistence with large effects even a decade after audits. In addition, the intervention led to a substantial increase in women receiving recommended levels of prenatal care; this effect is likewise concentrated among nonwhite Brazilians. This causal evidence suggests that government anti-corruption interventions have the potential to improve health outcomes, a finding that deserves investigation in other countries.

Keywords: Corruption; Program Evaluation; Child Health; Infant Mortality; Child Mortality.

Significance Statement

Many researchers and practitioners believe that corruption affects public health systems, but it is unknown whether programs that combat corruption can improve health. We show that an important program in Brazil — which audited municipalities for corruption — reduced the deaths of children and infants. In addition, the anti-corruption program increased prenatal care. Just as important, these improvements were especially strong for nonwhite Brazilians, who often experience health disparities. Effects were large even a decade after the audits. The credibility of our findings is enhanced by the fact that municipalities were randomly selected for audits through televised lotteries. The results of this study suggest that governments may be able to improve health outcomes and reduce health disparities through anti-corruption interventions.

Introduction

Although early-life mortality has fallen substantially across the world in recent decades, it remains disturbingly high in many countries. Moreover, significant disparities in child survival rates persist across ethnicity and socioeconomic characteristics in much of the world (1). In part to reduce early-life mortality, the WHO and other international organizations have embraced Universal Health Coverage (UHC), which was adopted as a Sustainable Development Goal by all UN member states. Achieving UHC will require substantial investments, estimated to be an additional $370 billion per year across low and middle-income countries (2). In addition to this expanded coverage, research suggests that millions of deaths across all ages could be prevented if additional resources improved low-quality healthcare (3).

Despite these potential benefits, increased healthcare expenditures do not always improve health outcomes, especially in countries with poor governance (4). Among various reasons is corruption — the abuse of public office for private gain — a phenomenon that siphons off significant healthcare investments in much of the world. According to Transparency International (2019), corruption in the healthcare sector leads to worldwide losses of over $500 billion per year, more than the amount necessary to provide Universal Health Coverage across the globe (5). For example, public officials in many contexts pilfer funds earmarked for medical equipment and accept bribes for suboptimal healthcare contracts. Evidence suggests that corruption has substantial effects not only on healthcare expenditures, but also on health outcomes. For example, Hanf et al. (2006) estimate that corruption in the healthcare sector kills approximately 140,000 children across the world each year (6).

For decades, researchers and practitioners largely ignored the pernicious effects of corruption on health. By contrast, recent publications in leading medical journals have called for increased attention to the "hidden pandemic" of corruption (7; 6; 8; 9). Yet many unanswered questions remain about how to address this pressing problem. Various interventions have been proposed to combat corruption in the healthcare sector, such as improving top-down controls, heightening bottom-up accountability, and raising salaries of civil servants (5). And numerous case studies and analyses of observational data investigate the effectiveness of specific measures, usually with mixed findings. But a systematic investigation by the Cochran Review (2016) found no published studies providing causal evidence about whether anti-corruption interventions can affect health outcomes (10).

To address this lacuna, the present article provides novel evidence that one such intervention — randomized anti-corruption audits — improved early-life mortality in Brazil. We identify the causal impact of these audits on both infant and child mortality. Moreover, we show that the intervention increased prenatal visits, which can reduce early-life mortality (11). Given substantial disparities in healthcare, we also examine effects by race. Information on race is included in vital statistics, and is highly associated with socioeconomic disparities in Brazil. Before turning to statistical analyses, we first provide context about public healthcare in Brazil, especially about the role of municipalities and how audits can affect health outcomes.

Background

Healthcare and Corruption in Brazil

Brazil's 1988 Constitution declares the universal right to comprehensive healthcare. The public healthcare system, known as *SUS* (*Sistema Único de Saúde*, or Unified Health System), serves the substantial majority of Brazilians, as only a quarter of the population enrolls in private health plans (12). Although Brazil's expenditures on healthcare grew nearly fivefold between 2000 and 2012, *SUS* faces numerous challenges including understaffing of doctors, lengthy delays for specialized services, and limited medical supplies (13). Indeed, healthcare is frequently identified as the most pressing problem in Brazilian public opinion surveys, with 87% of respondents in a recent survey rating the quality of health clinics and hospitals as "low" or "very low" (14). While various health indicators have improved markedly in recent years, substantial regional and socioeconomic inequities in health outcomes persist (15; 16; 17). For example, infant mortality fell from 77 to 14 deaths per thousand births between 1980 and 2016, but infants born in the relatively poor Northeast states of Bahia and Piauí have double the mortality rates as those born in wealthier southern states of Rio Grande do Sul and Santa Catarina (18; 19). Given that *SUS* is the primary provider of healthcare in Brazil, most infant and child deaths occur within the public healthcare system — motivating our investigation of whether political corruption is a cause of early-life mortality in Brazil.

As one of the most decentralized countries in the world, Brazil places healthcare responsibilities on all levels of government, including its 5,570 municipalities that are governed by elected mayors who wield considerable power. These municipalities implement primary care and many other aspects of healthcare, using funds primarily from the federal government. Federal transfers to municipalities account for nearly 48% of all public health expenditures, and municipal officials enjoy substantial autonomy and minimal oversight when expending much of these funds (20; 12). For example, municipalities receive significant resources to distribute free medicine through Brazil's Popular Pharmacy Program (*Programa Farmácia Popular do Brasil*), but municipalities often do not follow top-down procurement guidelines, and federal audits reveal poor monitoring of the inflow and outflow of medications from municipal clinics and hospitals (20). While civil society may provide oversight through municipal health councils, these councils often have limited autonomy, particularly in smaller municipalities where they depend on funds from local governments to operate (21).

Corruption is a pressing concern in contemporary Brazil (22; 23; 24). In recent years, scores of politicians and bureaucrats were prosecuted as part of the nation's largest-ever corruption investigation (*Operação Lava Jato*, or Operation Car Wash) (25; 26), and President Jair Bolsonaro railed against corruption as a pillar of his campaign platform (27; 28). Corruption often involves healthcare, the sector with the most local public procurement in Brazil (29). For example, the "Bloodsuckers" (*Sanguessugas*) corruption scandal — involving extraordinarily overpriced ambulances — implicated nearly a hundred politicians who reportedly received kickbacks and enabled these wasteful purchases (30; 31). During the current COVID-19 crisis, which has caused over 680,000 Brazilian deaths, corruption probes have implicated several state governors and dozens of public officials for similar infractions involving the procurement of medical supplies (32). Moreover, in a major cross-national survey, 10.9% of all Brazilian respondents reported paying a bribe to access healthcare during the past year (33).

Many policymakers suggest a link between corruption and poor health outcomes. For instance, a justice on Brazil's Supreme Court recently emphasized: "Corruption kills. It kills in the waiting line of *SUS* [the public healthcare system], in the lack of hospital beds, in the lack of medicine" (34).

Anti-Corruption Audits in Brazil

To combat corruption in Brazil's federal expenditures, the *Controladoria-Geral da União* (*CGU*, or Office of the Comptroller General) was formed in 2003. That year, the *CGU* initiated an impressive audit program to root out the corrupt use of federal funds by municipalities. This program, entitled *Programa de Fiscalização por Sorteios Públicos* (Monitoring Program with Public Lotteries), randomly selected municipalities in televised lotteries every few months, and sent teams of auditors to visit those municipalities and scrutinize their expenditures of federal funds. The *CGU* conducted these randomized anti-corruption audits between 2003 and 2015. By early 2015, the government agency had conducted 40 lotteries and performed audits in 1,949 municipalities, involving a careful investigation of over R$22 billion of federal funds (35). The lotteries employed stratification at the state level, and excluded municipalities with populations above 500,000, which were ineligible for the intervention. Less than one percent of Brazilian municipalities exceed this population threshold.

The *CGU* audits reveal substantial corruption in the sphere of healthcare. In fact, Colonnelli & Prem's (2022) analysis of the *CGU*'s audit reports show the healthcare sector has the highest share of private firms involved in municipal corruption (29). For example, the audit of Capelinha in Minas Gerais discovered that the municipality's financial records included many false receipts for medicine that was in fact never purchased for its public health clinics (36). Avis et al. (2018) show that the *CGU*'s audits substantially reduce the level of corruption in treated municipalities, for years after being subjected to the random audits (35). One important reason for this subsequent decline in corruption is that when these audits uncover corruption, politicians face electoral as well as legal punishments (35; 37). We leverage random assignment of these anti-corruption audits to identify their causal effect on early-life mortality.

Data and Estimation Strategy

Data Sources and Definitions

We use health outcomes from individual-level vital statistics collected by the Brazilian government. Data on all deaths between 2001 and 2015 are from the *Sistema de Informação sobre Mortalidade* (*SIM*) database, maintained by Ministry of Health's Information Technology division (*DATASUS*). Given the intervention's population threshold, we exclude data on municipalities with over 500,000 inhabitants.

Data on the dates of audits are from the *CGU*, the government agency responsible for the intervention. For each year from 2003 to 2015, these data indicate whether a municipality was selected by lottery for a corruption audit. Municipal population was obtained from the Brazilian Institute of Geography and Statistics (*IBGE*).

The primary outcomes are counts of deaths, stratified by age group (infant and child), cause of death (preventable and non-preventable) and mother's race (white and nonwhite). Infant mortality refers to deaths before one year of age, and child mortality refers to deaths before five years of age. Our database includes information on 394,860 infant deaths and 463,611 child deaths. As discussed below, supplementary analyses in the Online Appendix show results for neonatal mortality, using information on 270,087 deaths before 28 days of age from the same database.[1]

Preventable causes follow standard definitions from the International Classification of Diseases (ICD-10). The two racial groups are based on the five categories provided in the *SIM* database. In addition to using the "white" (*branca*) category, we aggregate the other four categories as "nonwhite": "black" (*preta*), "brown" (*parda*), "Asian" (*amarela*), and "indigenous" (*indígena*).

We obtain data on prenatal visits from the *Sistema de Informações sobre Nascidos Vivos* (*SINASC*) database, also maintained by *DATASUS*. It codes whether a mother had received three categories of prenatal visits: 0, 1 to 6, or 7+ visits with healthcare professionals. Given that the WHO recommendation for routine antenatal care is at least eight prenatal visits (38), we focus on the latter category. As for other outcomes, we also stratify prenatal visits by race (white and nonwhite). Additional analyses employ individual-level information on births from the same dataset.

Estimation Strategy

Our estimation strategy exploits random variation in the timing of audits across municipalities. To do so, we estimate the following model:

$$D_{mst} = \theta \text{Audit}_{ms,t-1} + \alpha_{ms} + \delta_t + \varepsilon_{mst} \tag{1}$$

where D_{mst} represents the mortality outcome, such as the number of infant or child deaths, in municipality m within state s at time t. $\text{Audit}_{ms,t-1}$ is coded as 1 if the municipality m had ever been audited up to time $t-1$; 0 otherwise. In addition, α_{ms} and δ_t are municipality and year fixed effects, respectively. Of primary interest is θ, the estimated Average Treatment Effect on the Treated (ATT). We allow for the correlation of error terms ϵ_{mst} within municipalities over time by clustering standard errors at the municipality level.

For each outcome of interest, we also estimate the fully dynamic event study:

$$D_{mst} = \sum_{k=-10}^{-2} \gamma_k \text{Audit}_{ms,t-1+k} + \sum_{k=0}^{10} \theta_k \text{Audit}_{ms,t-1+k} + \alpha_{ms} + \delta_t + \varepsilon_{mst} \tag{2}$$

where estimates of γ_k capture differences in municipal mortality outcomes in the period up to ten years preceding the audit and estimates of θ_k capture audits' effects in each year k up to ten years following the audit. This specification enables direct testing for the presence of pre-trends in mortality outcomes, as well as examination of audits' dynamic effects.

Randomization of the audits ensures that counterfactual outcomes are mean independent of the timing of audits. As such, our estimation strategy not only meets the key identifying assumption of parallel trends across treatment status, but also exhibits

[1]Given the overlap in age ranges, neonatal deaths are included in data on infant mortality, and both neonatal and infant deaths are included in data on child mortality.

equivalent levels across audited versus unaudited municipalities.[2]

The context we study differs from a traditional differences-in-differences design, in which units experience their first treatment at time t or are never treated. In Brazil, municipalities were randomly selected for audits at different times, with some municipalities never audited at all. Given this staggered treatment adoption, the empirical models in Equations 1 and 2 involve a second implicit assumption: treatment effects are homogeneous across municipalities over time. We follow two strategies to address the possibility of treatment effect heterogeneity, which may cause bias in two-way fixed effects models.[3] First, we estimate these models focusing on the treatment of ever having an audit — which is a staggered treatment by construction — and include as pure controls municipalities that were never audited during the sample period. Second, we employ Sun & Abraham's (2021) method to estimate dynamic treatment effects (40), and show robustness to traditional two-way fixed effects estimation. Following this approach, which is described more extensively in Online Appendix A, we estimate models for the overall population of infants and children, as well as by subgroups defined above.

Furthermore, we use the dynamic treatment effects estimates described above to simulate the aggregate number of deaths prevented by the audit program, in overall terms as well as by race and cause of death. More specifically, we combine the average treatment effects estimates above, along with their 95 percent confidence intervals, to simulate the number of prevented deaths in treated municipalities up to 10 years following an audit. Online Appendix B provides a detailed description of this procedure.

Results

Table 1 provides summary statistics about key variables, for each municipality-year. During 2001-15, each Brazilian municipality in our sample averaged 7.5 child deaths per year, including 6.4 infant deaths (column 1). Approximately half of these deaths were of nonwhite Brazilians. Overall, municipalities averaged 4.9 child deaths, including 4.3 infant deaths, from preventable causes each year.[4] Mortality was greater in 2001-02, before the audit program commenced (column 2). No significant differences are observed between treatment and control municipalities before the intervention began, taking into account the audit program's stratified randomization by state (column 3).

Figure 1 shows the effects of Brazil's anti-corruption audits on early-life mortality over time. In the first year after treatment, audits had no statistically significant effect on child or infant deaths. In the fifth year after treatment, an audited municipality is estimated to experience 0.56 fewer child deaths (95% CI: -0.95, -0.17) and 0.43 fewer

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[2]Moreover, municipalities are selected by lottery, so local officials do not have private information about when or if they will be audited. Thus, they cannot change actions with regards to corruption, public goods provision, or healthcare in anticipation of audits.

[3]We do not employ two-way fixed effects models as our primary specifications, as recent studies in the applied econometrics literature show that violating this implicit assumption may lead to biased estimates as well as misleading tests for baseline balance and parallel trends (39; 40; 41).

[4]Counts by subgroup do not sum to overall counts, due to missing data (e.g., some deaths do not have information about race or cause of death).

infant deaths (-0.12, -0.75), relative to never experiencing an audit. And in the tenth year after treatment, this effect intensified to 0.84 fewer child deaths (-1.43, -0.26) and 0.66 fewer infant deaths (-1.15, -0.17).

Table 2 shows the average treatment effect on the treated (ATT), which aggregates effects by treatment duration shown in Figure 1. An audited municipality is estimated to experience 0.48 fewer child deaths (-0.81, -0.15) and 0.34 fewer infant deaths (-0.61, -0.07) per year, relative to never experiencing an audit (see columns 1 and 2). The audit program is estimated to have prevented the deaths of 7,014 children under five years old, including 5,028 infants under one year old.[5] The observed mortality in audited municipalities is approximately 94 percent of the child deaths, and 95 percent of the infant deaths, that would have occurred in the absence of the intervention.

Figure 2 shows the effects of audits on child mortality over time, for white versus nonwhite mothers. The top panel demonstrates that at no time after audits did the intervention have a statistically significant effect on the deaths of children with white mothers. By contrast, the bottom panel shows that in the fifth year after treatment, an audited municipality is estimated to experience -0.29 fewer deaths $(-0.03, -0.55)$ of children with nonwhite mothers, relative to never experiencing an audit. Similarly, in the tenth year after treatment, this effect was -0.44 fewer nonwhite child deaths $(-0.80, -0.08)$. Online Appendix Figure C1 shows that results are comparable when examining infant deaths.

Aggregating these effects by treatment duration, Table 2 shows the ATT by mother's race. Audits' effects on early-life mortality were concentrated among children and infants with nonwhite mothers. Both effects are statistically indistinguishable from zero for whites (see columns 1 and 2). By contrast, an audited municipality is estimated to experience -0.28 fewer child deaths (-0.08, -0.48) and 0.18 fewer infant deaths (-0.36, -0.004) among nonwhites per year, compared to never experiencing an audit. The anti-corruption intervention is estimated to have prevented the loss of 4,153 nonwhite children, including 2,656 infants.[6] The observed mortality in audited municipalities is approximately 93 and 95 percent of the nonwhite deaths, respectively, that would have occurred in the absence of the program.

Audits' effects on early-mortality are only observed for deaths from preventable causes. In Table 2, columns 1 and 2 demonstrate insignificant effects on child and infant deaths from non-preventable causes. In contrast, an audited municipality is estimated to experience 0.30 fewer child deaths (-0.54, -0.06) and 0.24 fewer infant deaths (-0.46, -0.02) from preventable diseases per year, relative to the counterfactual of never being audited. The audit program is estimated to have averted the deaths of 4,398 children from preventable causes, including 3,498 infants.[7] The observed mortality in audited municipalities is approximately 94 and 95 percent of the preventable deaths, respectively, that would have occurred in the absence of the intervention. Online Appendix Figures C2 and C3 confirm that when examining audits' effects on infant and child mortality over time, findings are significant only for deaths from preventable causes.

In addition, column 3 of Table 2 presents the effects of anti-corruption audits on

[5]The 95% confidence intervals are 2,216 to 11,813 children and 891 to 9,165 infants.

[6]The 95% confidence intervals are 1,185 to 7,121 children and 110 to 5,201 infants.

[7]The 95% confidence intervals are 859 to 7,937 children and 312 to 6,684 infants.

the number of women with at least seven prenatal visits. An audited municipality is estimated to have 7.18 (1.22, 13.14) more women per year obtaining this level of prenatal care, compared to never experiencing an audit. Again, these effects are concentrated among nonwhite mothers. Whereas effects are statistically insignificant for white mothers, an audited municipality is estimated to have 8.21 (2.68, 13.74) more nonwhite mothers per year with at least seven prenatal visits.

The Online Appendix provides numerous additional analyses. Figures C4 and C5 show that audits' effects on infant and child mortality (respectively) are observed for both females and males. We also replicate all analyses above for neonatal mortality, and observe similar effects with less precision; see Figures C6-C8 and Tables D1-D2. Table D3 shows the robustness of findings to traditional two-way fixed effects estimation without Sun and Abraham's (2021) correction for treatment effect heterogeneity (40).[8]

Discussion

The findings of this study provide causal evidence that anti-corruption interventions can improve health outcomes. Between 2003 and 2015, the Brazilian government randomly selected 1,949 municipalities for extensive audits of federal transfers. Analyses suggest that these randomized anti-corruption audits decreased early-life mortality in Brazil, and the magnitude of lives saved was substantial. We estimate that the audit program prevented the deaths of 7,014 children under five years old, including 5,028 infants under one year old. The observed mortality in audited municipalities is approximately 94 percent of the child deaths, and 95 percent of the infant deaths, that would have occurred in the absence of the anti-corruption intervention. The decrease in mortality was concentrated among nonwhite Brazilians, who face significant health disparities. Audits significantly reduced deaths from preventable causes, and as expected had no effect on deaths from non-preventable causes. Audits had a relatively small immediate effect, which magnified substantially over time and demonstrated temporal persistence. The intervention also improved prenatal care, which is commonly understood to be a determinant of early-life mortality (11). This finding is likewise concentrated among nonwhite Brazilians. Overall, these results suggest that anti-corruption audits improved health outcomes.

Several mechanisms may explain why randomized audits in Brazil reduced early-life mortality. One potential mechanism is that audits caused local public officials to pilfer less of the municipal budget, thereby reducing diversion of funds allocated to public healthcare. Examining the same intervention, Avis et al. (2018) shows that corruption is 7.9% lower in municipalities that previously experienced random anti-corruption audits, compared to unaudited municipalities. Less corruption would be expected to reduce diversion in health budgets, because many corrupt acts discovered by these audits involve officials misappropriating federal transfers earmarked for the public health system. For example, *CGU* audits have revealed municipal officials

[8] In addition, Tables D4 and D5 show that findings are robust with the inclusion of a control for births in models using two-way fixed effects as well as Sun & Abraham's (2021) approach (respectively). Audits do not have similar effects on births; ATT estimates are small, and statistically insignificant. Our preferred specifications exclude controls for births due to the potential endogeneity bias that may result from including this control variable in the regression models.

purchasing apartments with funds allocated to health projects, submitting fake receipts for medicine that was never purchased, over-invoicing medical purchases in exchange for kickbacks, and awarding bids to shill companies that never delivered public services (36). Reducing such diversion in health budgets increases funds available for various expenditures, some of which may reduce early-life mortality. For example, WHO (2018) identifies prematurity as the leading cause of death worldwide for children under five — and emphasizes that improved care can avert over three-fourths of fatalities of premature newborns (11). In addition to improved healthcare during and after childbirth, preterm births (and early-life mortality) can be reduced through prenatal visits with health professionals who play key roles in screening for risk factors, conducting ultrasounds, and providing information about dietary requirements and substance use (11). Given frequent disparities in access to these services in Brazil and beyond (42; 43), increasing such expenditures may be especially likely to improve health outcomes of underrepresented groups.

Another potential mechanism is that anti-corruption audits improve the targeting of public resources to needy populations through improvements in public accountability (44). Although democratic elections enable voters to punish corrupt politicians, citizens often lack information about the extent to which their elected representatives abuse public office for private gain (37; 45). Whereas the mechanism discussed above reduces corruption by existing officials, the *CGU*'s widely disseminated audit results may also have alleviated this information asymmetry and allowed voters to punish local corrupt politicians at the ballot box. Consistent with this possibility, Ferraz & Finan (2008) show that Brazil's randomized audits decreased the reelection of mayors — the chief executives of municipal governments — in municipalities with above-average levels of corruption (37). In contexts where corrupt mayors had diverted resources from the health sector, electing challengers may lead to reduced diversion from health budgets. Furthermore, challengers may also adopt policies that reduce early-life mortality, either by directly channeling resources to the public health system or through indirect channels such as infrastructure investments in water and sanitation. Beyond ousting corrupt incumbents, audits may also increase the quality of municipal politicians by improving the pool of challenger candidates. Many local politicians have been prosecuted using information gleaned from audits, which may in turn dissuade corrupt individuals from entering politics if they update their perceptions of how risky it is to obtain corrupt rents as an elected official (35). More broadly, such political effects, which we do not examine, may help to explain the results of our study.

An important avenue for future research is thoroughly investigating potential mechanisms underlying the findings of this study. With regards to the first mechanism discussed above, one next step would be to examine whether the anti-corruption audits increased expenditures on public goods — both in the public health system and more broadly — that are generally understood to improve early-life mortality. Existing studies do not definitively address this question: Avis et al. (2018) indicates that audits did not affect overall health spending (35), while an unpublished study by Lichand et al. (2017) finds that audits decreased spending of federal transfers earmarked for health (46).[9] Another fruitful direction is to elaborate and test political mechanisms by which audits might reduce early-life mortality. Such work should carefully consider reasons

[9]Moreover, Zamboni & Litschig (2018) find that temporarily increasing a municipality's audit risk does not affect health worker absenteeism (according to user satisfaction surveys) (47).

why voters may have supported corrupt politicians in the past: whereas many scholars posit the informational argument presented above, others suggest that voters may also deliberately choose corrupt politicians who effectively provide public goods or other benefits (45). Furthermore, this line of investigation should examine whether audits affect the creation of municipal councils, which promote civil society participation in health policy and have been shown to reduce infant mortality in Brazil (48). Overall, elaborating and unpacking such theoretical mechanisms can shed light on why anti-corruption audits reduce early-life mortality in the Brazilian context.

While our results provide causal evidence that anti-corruption interventions can reduce early-life mortality, another key direction for future research is examining broader implications of the present study. With respect to generalizability, it is important to clarify whether government audits reduce deaths in many countries, and to understand scope conditions if the relationship is rarely or never observed elsewhere. Our findings also underscore the importance of examining the potential health impacts of a wider array of anti-corruption interventions, including other ways of heightening top-down controls as well as bottom-up accountability.[10] Despite growing attention to effects of corruption on health, our findings suggest that increased attention to such questions can potentially save many lives, especially among vulnerable populations.

[10]For surveys of the literature on the effects of broader governance institutions in the US and the role of accountability reforms in both developed and developing countries, see Besley & Case (2003), and Finan, Olken & Pande (2017) (49; 50).

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Figures and Tables

Table 1: Descriptive Statistics

	All Municipalities, Annual 2001-2015	All Municipalities, Annual 2001-2002	Pre-Intervention Difference, Annual 2001-2002
	[1]	[2]	(3)
Child Deaths			
Overall	7.5	9.8	1.7
	[13.4]	[17.7]	(25.5)
White	3.0	4.0	0.2
	[6.4]	[8.4]	(11.9)
Nonwhite	3.3	3.4	1.1
	[7.4]	[8.1]	(12.1)
Preventable	4.9	6.4	1.1
	[9.1]	[12.3]	(17.7)
Non-Preventable	2.1	2.2	0.2
	[3.8]	[4.3]	(6.2)
Infant Deaths			
Overall	6.4	8.3	1.4
	[11.6]	[15.3]	(22.0)
White	2.5	3.4	0.1
	[5.5]	[7.3]	(10.3)
Nonwhite	2.8	2.7	0.9
	[6.2]	[6.7]	(10.0)
Preventable	4.3	5.7	1.0
	[8.1]	[11.0]	(15.8)
Non-Preventable	1.7	1.8	0.2
	[3.1]	[3.5]	(5.1)
Prenatal Visits ($\geq$7)			
Overall	210.9	175.6	5.3
	[439.6]	[403.3]	(566.9)
White	113.2	110.2	-3.3
	[294.5]	[285.9]	(401.2)
Non-White	88.4	52.2	10.9
	[198.1]	[147.2]	(210.0)
Birth			
Overall	391.5	408.0	51.1
	[720.8]	[737.3]	(1057.1)
White	170.3	204.6	3.8
	[401.2]	[444.4]	(629.4)
Nonwhite	202.4	171.9	47.8
	[439.6]	[398.8]	(581.7)
Num. of municipalities	5203	5203	5203
Num. of observations	78045	10406	10406

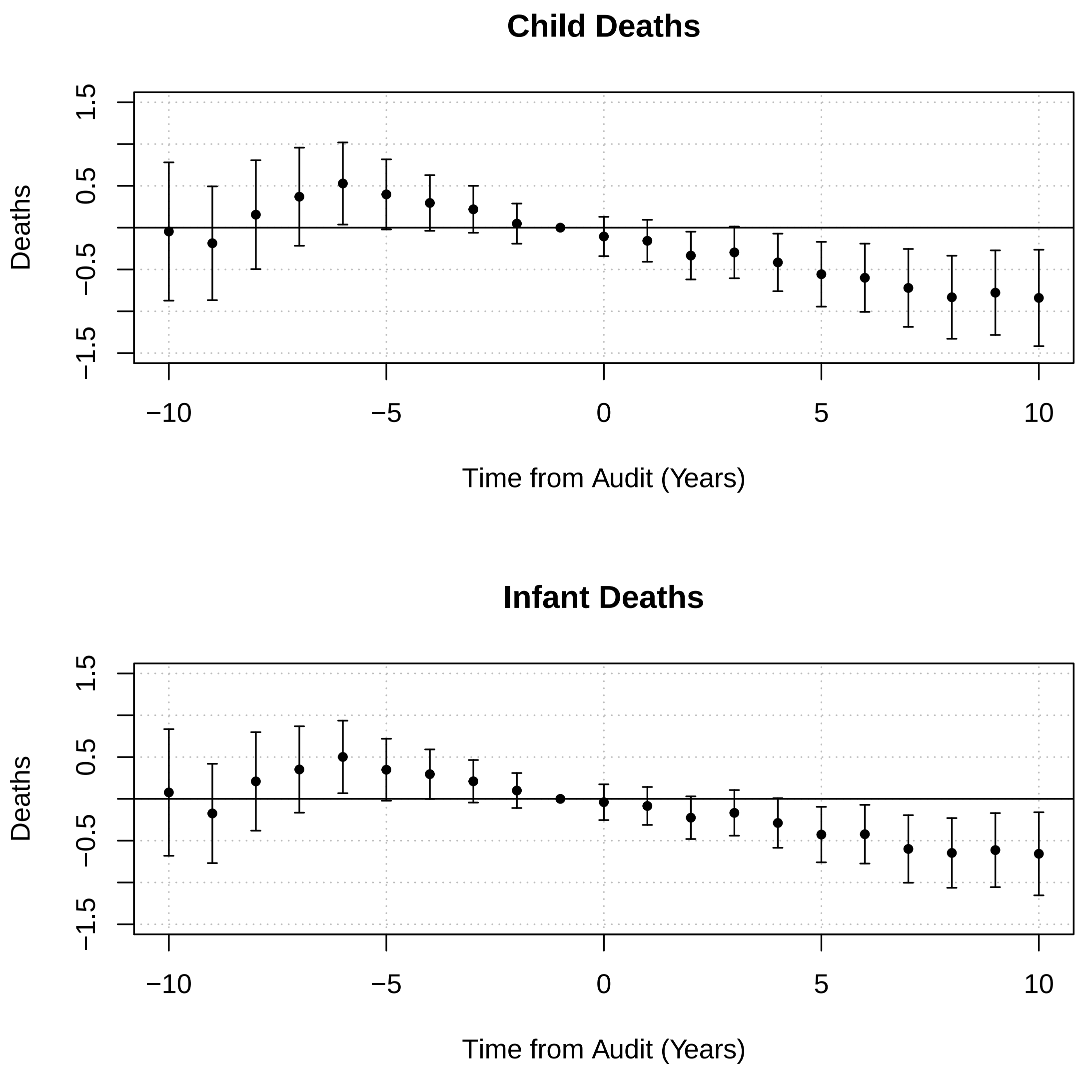
Notes: Columns 1 and 2 present the mean of each variable and its standard deviation in brackets, the first column for the overall period 2001-2015, and the second for the baseline period preceding the start of the audit program (2001-02). Figures by race and preventability do not sum to overall figures due to missing data. Column 3 reports the pre-intervention difference between audited and unaudited municipalities based on a regression that adjusts for state and year fixed effects to take into account the stratification of random assignment by state and the repeated observations across years by municipality. Standard errors of adjusted difference are clustered at the municipality level and reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels.

Table 2: Audits' Effects on Early-Life Mortality and Prenatal Visits

	Dependent variables:		
	Child Deaths	Infant Deaths	Prenatal Visits
	(1)	(2)	(3)
Overall Population			
ATT	-0.48**	-0.34*	7.18*
	(0.17)	(0.14)	(3.04)
Prevented Deaths	7,014	5,028	–
	(2,216 – 11,813)	(891 – 9,165)	
Observed/Expected Mortality Ratio	0.941	0.950	
	(0.904 – 0.980)	(0.912 – 0.991)	
Child Deaths: Nonwhite Mothers			
ATT	-0.28**	-0.18*	8.21**
	(0.10)	(0.09)	(2.82)
Prevented Deaths	4,153	2,656	–
	(1,185 – 7,121)	(110 – 5,201)	
Observed/Expected Mortality Ratio	0.933	0.948	
	(0.891 – 0.980)	(0.903 – 0.998)	
Child Deaths: White Mothers			
ATT	-0.09	-0.05	-2.09
	(0.09)	(0.08)	(1.90)
Prevented Deaths	1,296	803	–
	(-1,166 – 3,758)	(-1,372 – 2,978)	
Observed/Expected Mortality Ratio	0.969	0.977	
	(0.915 – 1.030)	(0.921 – 1.041)	
Preventable Causes			
ATT	-0.30*	-0.24*	–
	(0.12)	(0.11)	
Prevented Deaths	4,398	3,498	
	(859 – 7,937)	(312 – 6,684)	
Observed/Expected Mortality Ratio	0.944	0.949	
	(0.903 – 0.988)	(0.907 – 0.995)	
Non-Preventable Causes			
ATT	-0.01	0.02	–
	(0.05)	(0.04)	
Prevented Deaths	213	-325	
	(-1,106 – 1,532)	(-1,473 – 822)	
Observed/Expected Mortality Ratio	0.993	1.013	
	(0.955 – 1.035)	(0.970 – 1.059)	

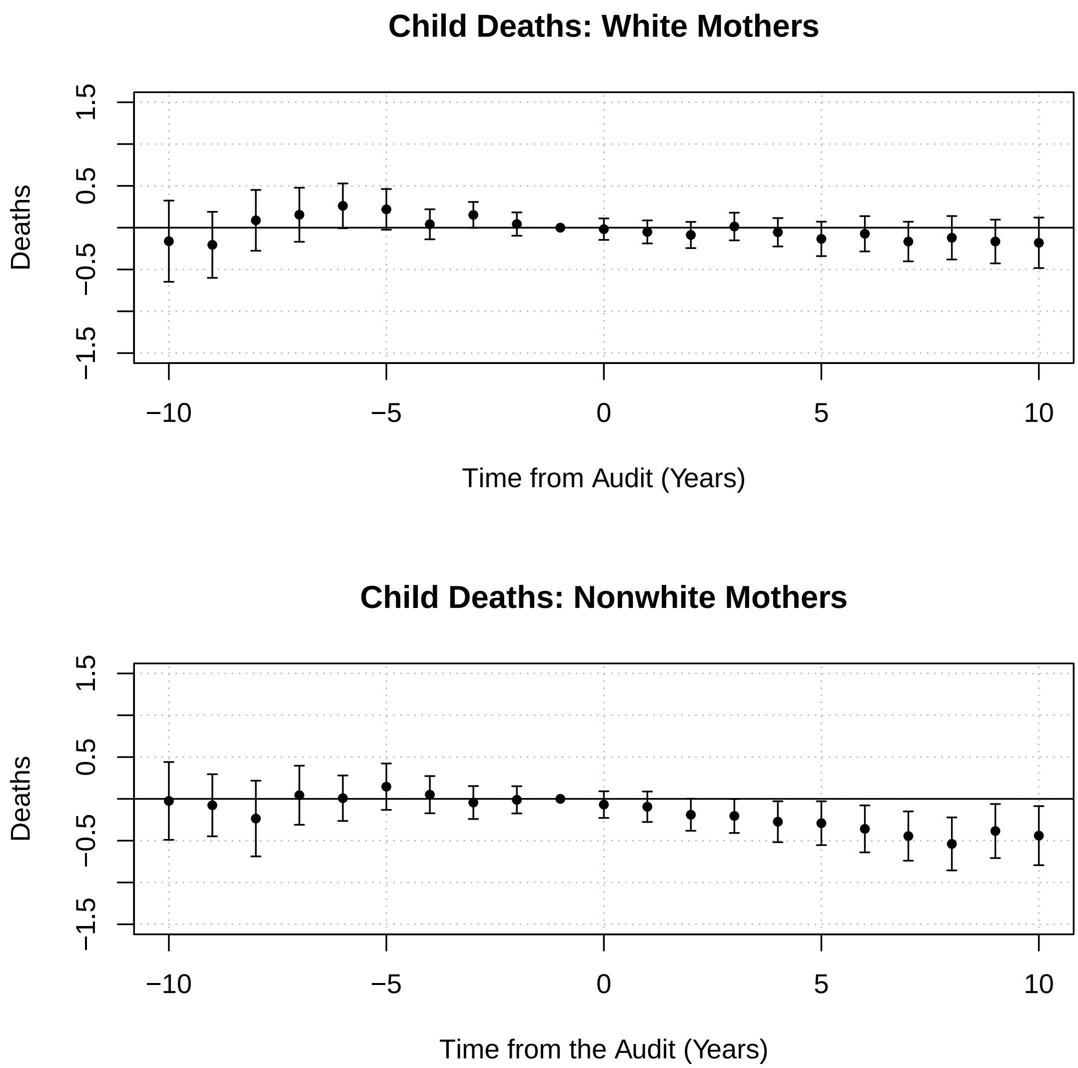
Notes: The first row in each panel reports the ATT estimates of the effect of the audits on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels. Rows 2 and 3 of each panel report the estimates of prevented deaths and the ratio of observed to expected deaths calculated from observed deaths and estimates of changes in mortality for each group (row 1); 95 percent confidence intervals of the level of prevented deaths and of the ratio of observed to expected deaths are reported in parentheses.

Figure 1: Audits' Effects on Early-Life Mortality over Time



Notes: Coefficients and 95% confidence intervals are estimated using Equation (2) at the municipality level. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Figure 2: Effects of the Audits on Child Mortality over Time, by Race of Mother



Notes: Coefficients and 95% confidence intervals are estimated using Equation (2) at the municipality level. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Supporting Information for

"Effects of Anti-Corruption Audits on Early-Life Mortality: Evidence from Brazil"

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Appendix A: Supporting text
Appendix B: Supporting text
Appendix C: Figures C1-C8
Appendix D: Tables D1-C5

Appendix A: Procedure for ATT Estimation Under Staggered Treatment

The estimation strategy exploits the experimental variation in the timing of audits across municipalities. As discussed above, the context we study differs from a traditional differences-in-differences design, in which units experience their first treatment in time t or are never treated. In Brazil, municipalities were randomly selected for audits at different times, with some municipalities never audited at all.

Given this staggered treatment adoption, the empirical models in Equations 1 and 2 in the *Estimation Strategy* section involve an implicit assumption: treatment effects are homogeneous across municipalities over time. We follow two strategies to address the possibility of treatment effect heterogeneity, which may cause bias in two-way fixed effects models. We do not employ two-way fixed effects models as our primary specifications, as recent studies in the applied econometrics literature show that violating this implicit assumption may lead to biased estimates as well as misleading tests for baseline balance and parallel trends (39; 40; 41).

First, we estimate these models focusing on the treatment of ever having an audit — which is a staggered treatment by construction — and include as pure controls municipalities that were never audited during the sample period. Second, we employ Sun & Abraham's (2021) method to estimate dynamic treatment effects (40), and show robustness to traditional two-way fixed effects estimation.

More specifically, we define treatment Audit_{mst} as taking value 1 if municipality m has ever been audited up to time t and 0 otherwise. Municipalities are categorized into cohorts based on E_i, which is defined as the year in which they first experienced an audit. There are thus 13 cohorts E_i in 2003, 2004, ... 2005. Following Sun and Abraham (2021), we estimate the cohort-specific average treatment effects on the treated ($CATT_{e,l}$), which reflect the average treatment effect l years from an audit for the cohort of municipalities first audited in year e. The method first estimates each $CATT_{e,l}$ using a regression saturated with indicators for cohorts and relative periods, and then averages $CATT_{e,l}$ estimates across cohorts (e) for a given period of time since treatment (l). These $CATT_{e,l}$ estimates provide the path of treatment effects for cohort e following an anti-corruption audit, relative to never experiencing an audit. We estimate such models for the overall population of infants and children, as well as by the subgroups defined in the text.

Appendix B: Aggregate Estimates of Reduced Mortality, Overall and by Subgroup

We use the dynamic treatment effects estimates described in Appendix A to simulate the aggregate number of deaths prevented by the audit program, in overall terms as well as by race and cause of death. More specifically, we combine the average treatment effects estimates above, along with their 95 percent confidence intervals, to simulate the number of prevented deaths in treated municipalities up to 10 years following an audit.

We use the following algorithm to estimate the aggregate number of prevented deaths:

1) We estimate the ATT across all years, employing Sun and Abraham's (2021) procedure for generating unbiased ATT estimates (40). For more details, see the *Estimation Strategy* section in the text, as well as Appendix A.

2) We calculate the total number of deaths in treated municipalities for ten years after the audit, or up to year 2015 (whichever is earlier).

3) Employing these data, we simulate counterfactual scenarios of what would have transpired if treated municipalities had not been audited. We estimate the proportional decrease in average deaths per municipality using the ratio of the ATT estimate of number of deaths to the mean number of deaths in treated municipalities in the post-audit period E_{n_d} (from the sample in step 2 above).

4) We estimate the aggregate number of prevented deaths comparing the counterfactual scenario of expected deaths to actual deaths, which results in:

$$\text{Prevented deaths} = \sum_{n=1}^{10} \times [\text{ATT} \ / \ E[n_d | T = 1, \text{Post}]$$

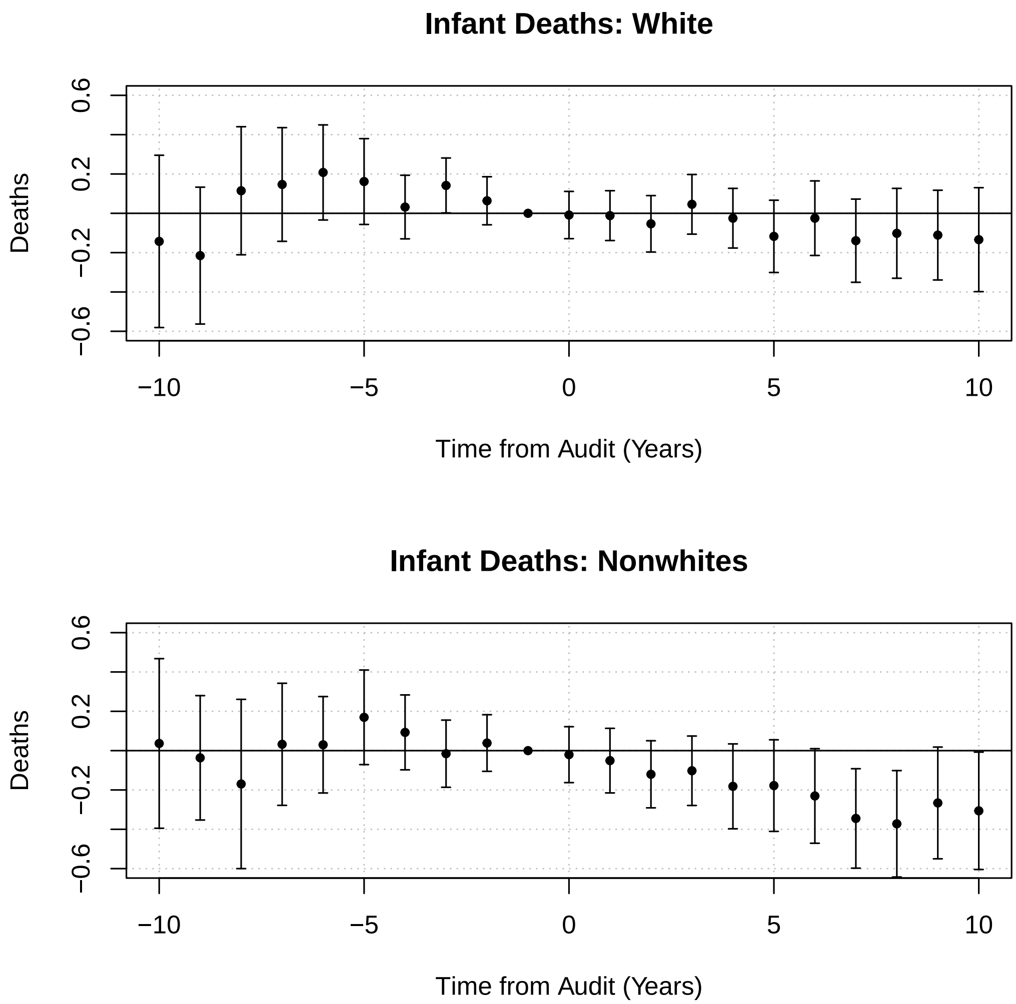
Where n is the number of years after the treatment and $E[n_d|T = 1]$ is the expected number of deaths in the treated municipalities, starting from the year following the treatment.

5) We construct 95% confidence intervals (CI) of the estimates of saved lives using the analogous CI's of the ATT estimate.

We also replicate this analysis, which is performed for the overall sample, using information for each relevant population subgroup.

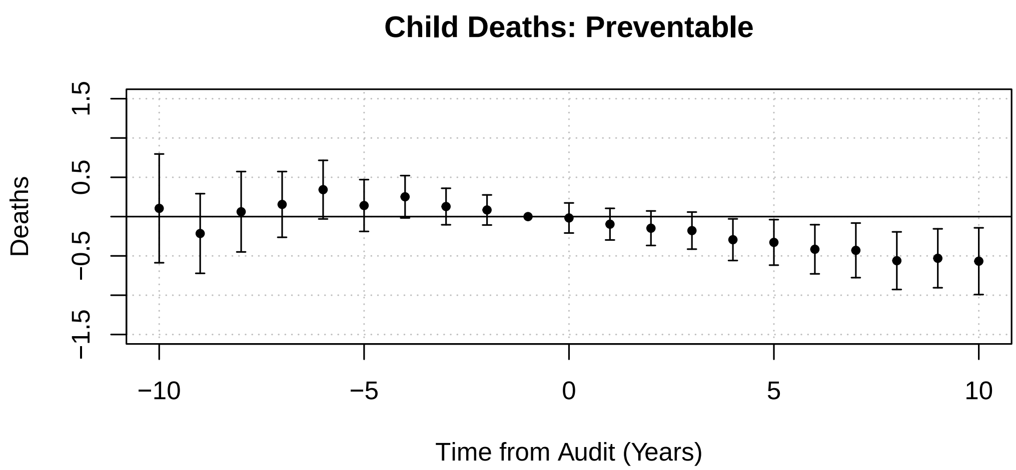
Appendix C

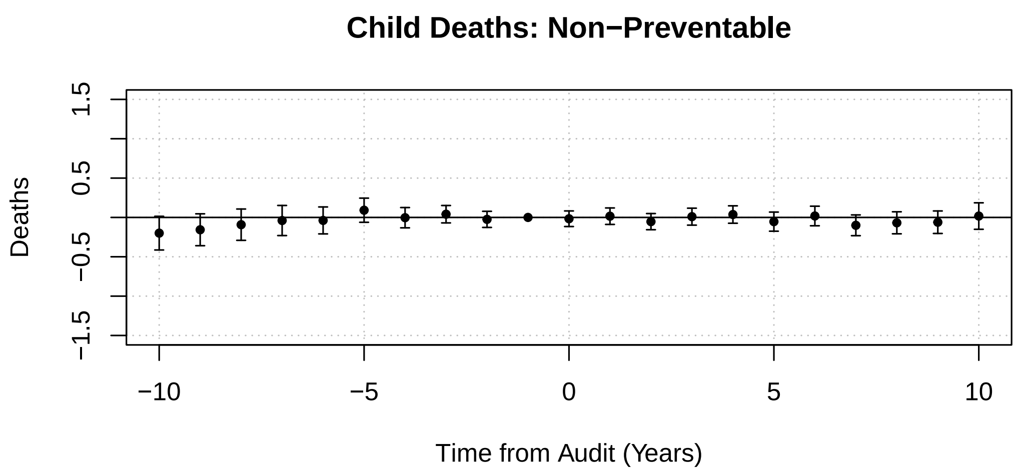
Figure C1: Audits' Effects on Infant Mortality over Time, by Mother's Race



Notes: Coefficients and 95% confidence intervals are estimated using equation (2) at the municipality level. Changes are measured relative to the death count in the year before the Audit. ($t = -1$). Standard errors are clustered at the municipality level.

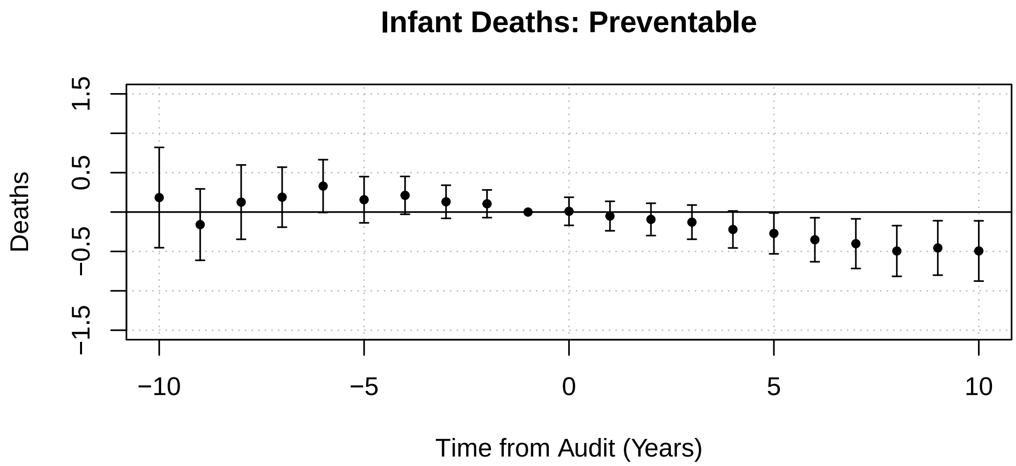
Figure C2: Audits' Effects on Child Deaths over Time, by Cause of Death

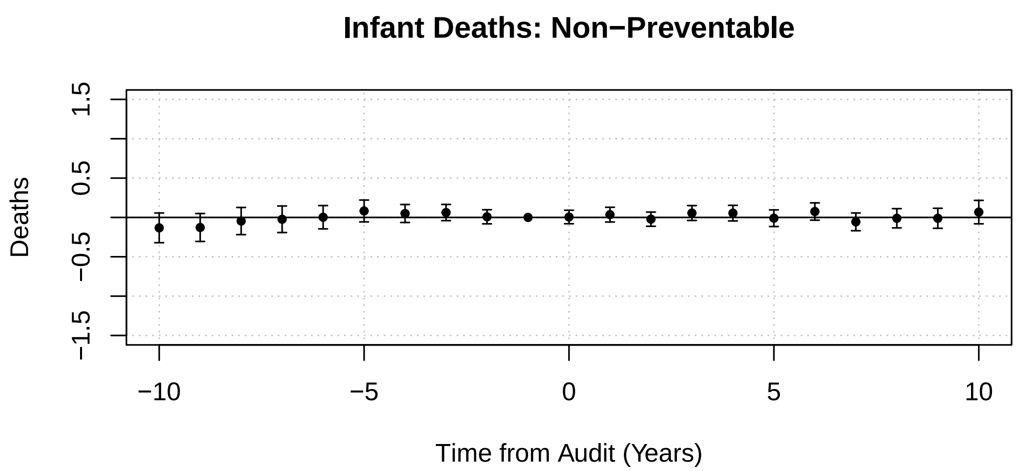




Notes: Coefficients and 95% confidence intervals are estimated using equation (2) at the municipality level. Changes are measured relative to the death count in the Audit ($t = -1$). Standard errors are clustered at the municipality level.

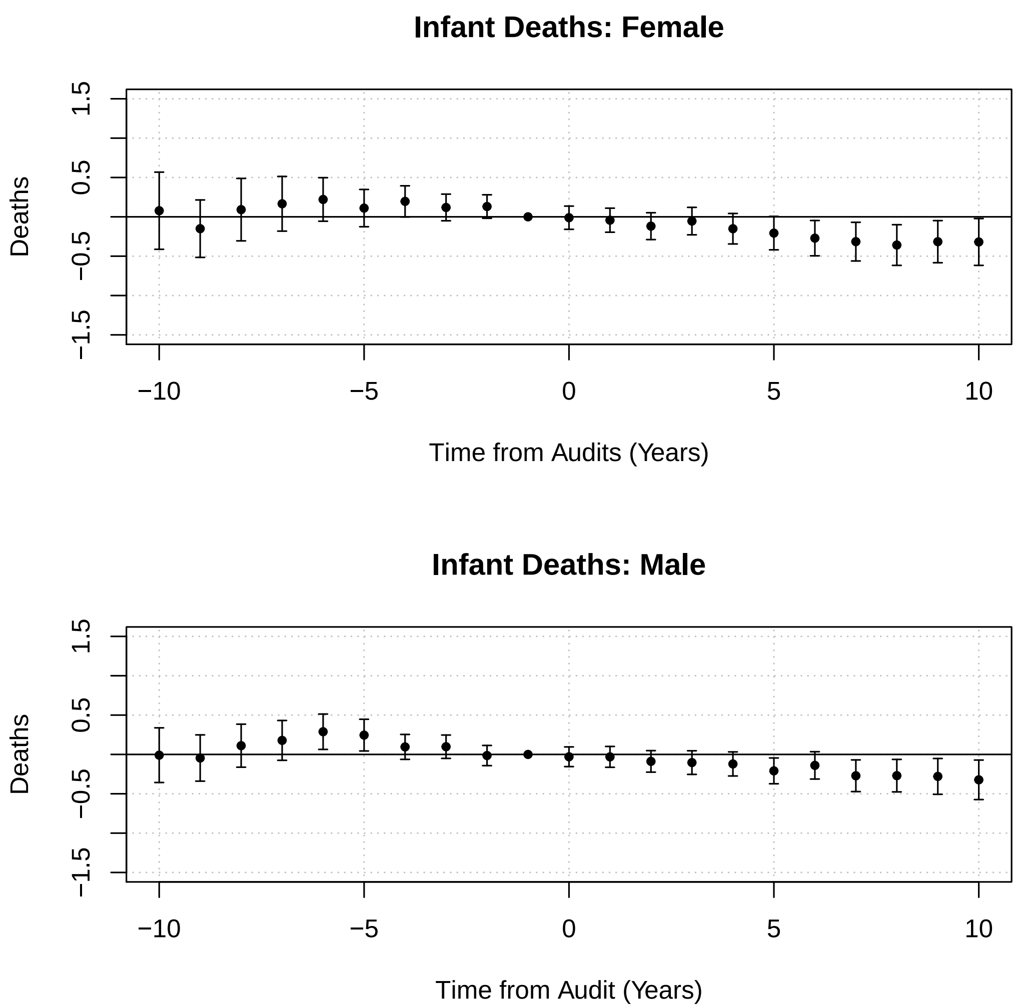
Figure C3: Audits' Effects on Infant Deaths over Time, by Cause of Death





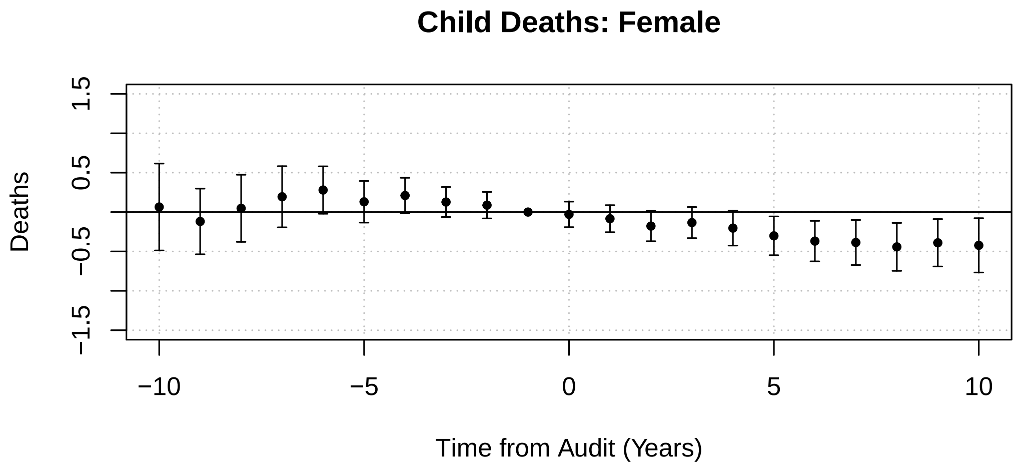
Notes: Coefficients and 95% confidence intervals are estimated using equation (2) at the municipality level. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

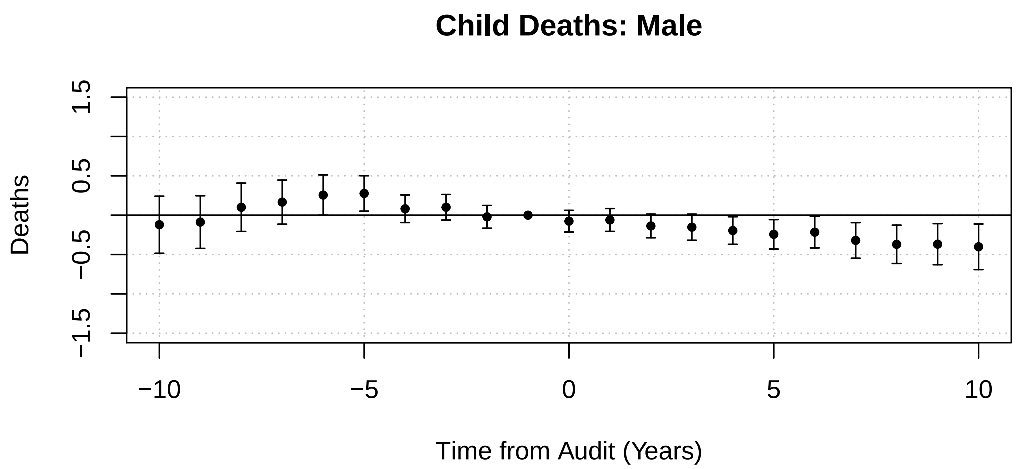
Figure C4: Audits' Effects on Infant Mortality over Time, Female vs. Male



Notes: Coefficients and 95% confidence intervals are estimated using equation (2 at the municipality level. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

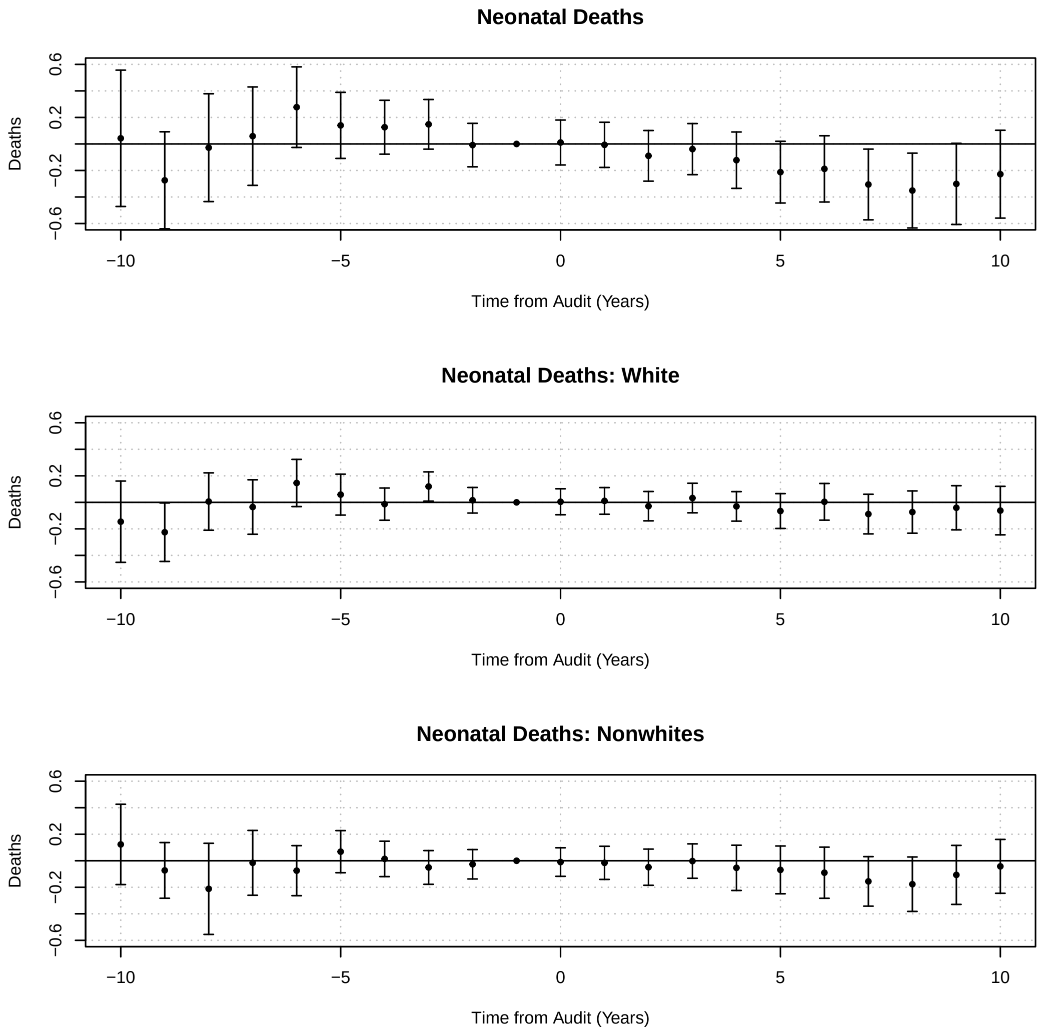
Figure C5: Audits' Effects on Child Mortality over Time, Female vs. Male





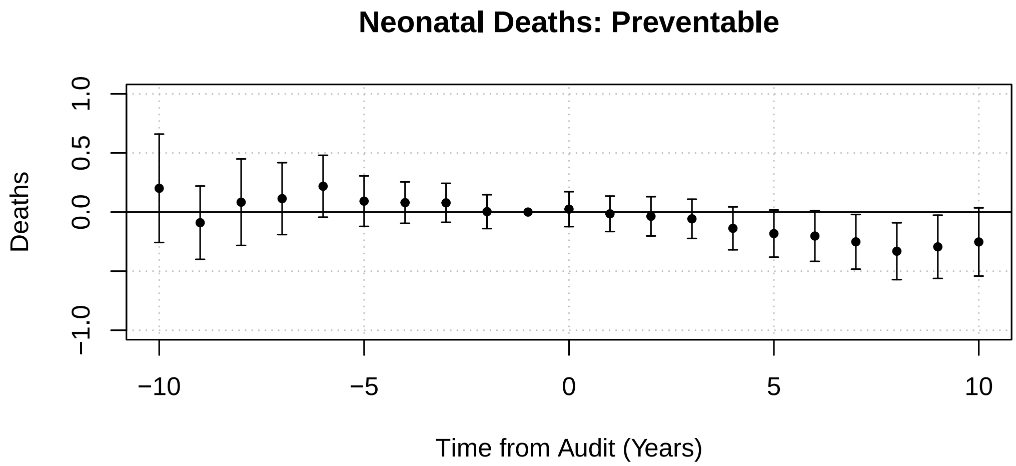
Notes: Coefficients and 95% confidence intervals are estimated using equation (2) at the municipality level. Changes are measured relative to the death count before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

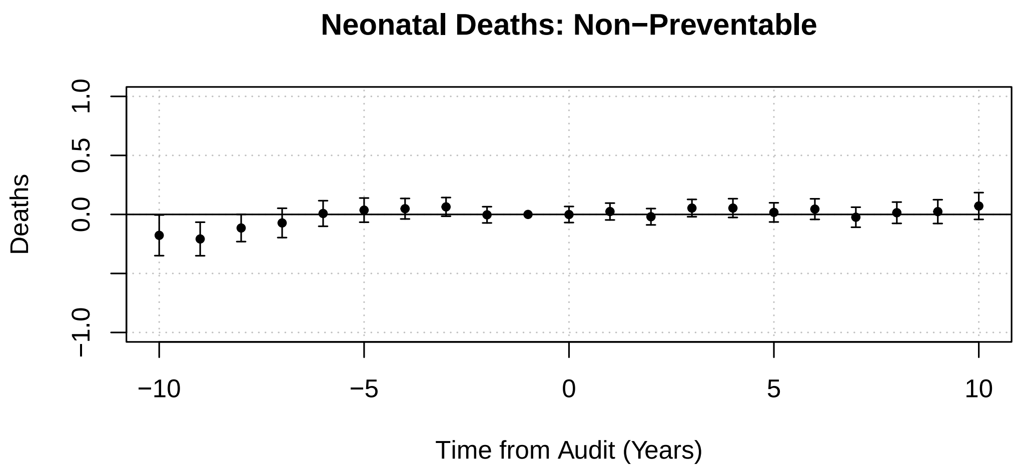
Figure C6: Audits' Effects on Neonatal Mortality over Time, Overall and by Mother's Race



Notes: Coefficients and 95% confidence intervals are estimated using equation (2) at the municipality level. Changes are measured relative to the death count in the year before the Audit. ($t = -1$). Standard errors are clustered at the municipality level.

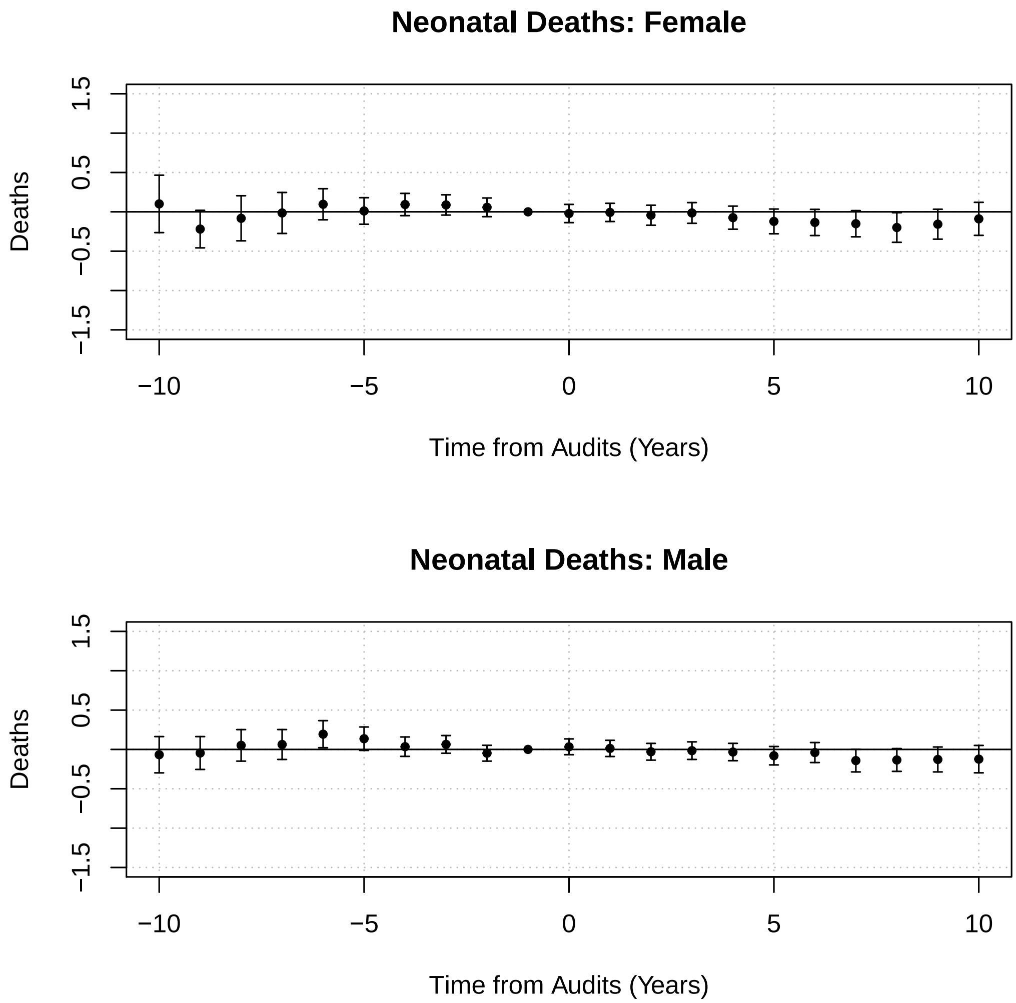
Figure C7: Audit's Effects on Neonatal Deaths over Time, by Cause of Death





Notes: Coefficients and 95% confidence intervals are estimated using equation (2), where the outcome captures child death counts at the municipality level. Changes are measured relative to the death count before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Figure C8: Audits' Effects on Neonatal Mortality over Time, Female vs. Male



Notes: Coefficients and 95% confidence intervals are estimated using equation (2) at the municipality level. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Appendix D

Table D1: Descriptive Statistics - Neonatal Deaths

	All Municipalities, Annual 2001-2015	All Municipalities, Annual 2001-2002	Pre-Intervention Difference, Annual 2001-2002
	[1]	[2]	(3)
Neonatal Deaths			
Overall	4.3	5.3	0.8
	[8.0]	[10.4]	(11.7)
White	1.7	2.2	0.0
	[3.8]	[5.0]	(5.4)
Nonwhite	1.8	1.6	0.5
	[4.4]	[4.4]	(5.8)
Preventable	3.2	4.0	0.6
	[6.1]	[8.2]	(9.3)
Non-Preventable	1.0	1.0	0.1
	[1.9]	[2.3]	(2.8)
Num. of municipalities	5203	5203	5203
Num. of observations	78045	10406	10406

Notes: Columns 1 and 2 present the mean of each variable and its standard deviation in brackets, the first column for the overall period 2001-2015, and the second for the baseline period preceding the start of the audit program (2001-02). Column 3 reports the pre-intervention difference between audited and unaudited municipalities based on a regression that adjusts for state and year fixed effects to take into account the stratification of random assignment by state and the repeated observations across years by municipality. Standard errors of adjusted difference are clustered at the municipality level and reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels.

Table D2: Audit's Effects on Neonatal Mortality

	Dependent variable: Neonatal Deaths (1)
Overall Population	
ATT	-0.14
	(0.10)
Prevented Deaths	2,118
	(-633 – 4,869)
Observed/Expected Mortality Ratio	0.968
	(0.930 –1.010)
Nonwhite Mothers	
ATT	-0.06
	(0.06)
Prevented Deaths	879
	(-960 – 2,718)
Observed/Expected Mortality Ratio	0.974
	(0.925 –1.029)
White Mothers	
ATT	-0.03
	(0.05)
Prevented Deaths	410
	(-1,132 – 1,952)
Observed/Expected Mortality Ratio	0.982
	(0.921 – 1.052)
Preventable Deaths	
ATT	-0.14
	(0.08)
Prevented Deaths	1,993
	(-368 – 4,353)
Observed/Expected Mortality Ratio	0.961
	(0.918 –1.008)
Non-Preventable Deaths	
ATT	0.03
	(0.03)
Prevented Deaths	-379
	(-1,257 – 498)
Observed/Expected Mortality Ratio	1.025
	(0.969 – 1.088)

Notes: The first row in each panel reports the ATT estimates of the effect of the audits on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels. Rows 2 and 3 of each panel report the estimates of prevented deaths and the ratio of observed to expected deaths calculated from observed deaths and estimates of changes in mortality for each group (row 1); 95 percent confidence intervals of the level of prevented deaths and of the ratio of observed to expected deaths are reported in parentheses.

Table D3: Audits' Effects on Early-Life Mortality and Prenatal Visits (Two-Way Fixed Effects Estimates)

	Dependent variables:		
	Child Deaths	Infant Deaths	Prenatal Visits
	(1)	(2)	(3)
Overall Population			
ATT	-0.52**	-0.43***	7.66*
	(0.14)	(0.12)	(3.14)
Prevented Deaths	7,629	6,302	-
	(3,455 – 11,802)	(2,746 – 9,858)	
Observed/Expected Mortality Ratio	0.936	0.938	
	(0.904 – 0.970)	(0.906 – 0.972)	
Nonwhite Mothers			
ATT	-0.23**	-0.18*	8.89**
	(0.08)	(0.07)	(3.01)
Prevented Deaths	3,373	2,576	-
	(931 – 5,816)	(515 – 4,638)	
Observed/Expected Mortality Ratio	0.945	0.950	
	(0.909 – 0.984)	(0.913 – 0.990)	
White Mothers			
ATT	-0.04	-0.11	-2.11
	(0.07)	(0.06)	(1.74)
Prevented Deaths	2,010	1,605	-
	(-4 – 4,024)	(-133 – 3,344)	
Observed/Expected Mortality Ratio	0.953	0.956	
	(0.910 – 1.000)	(0.912 – 1.004)	
Preventable Deaths			
ATT	-0.35**	-0.31**	-
	(0.11)	(0.10)	
Prevented Deaths	5,181	4,568	
	(2,041 – 8,321)	(1,788 – 7,349)	
Observed/Expected Mortality Ratio	0.934	0.934	
	(0.899 – 0.973)	(0.898 – 0.973)	
Non-Preventable Deaths			
ATT	-0.02	-0.01	-
	(0.03)	(0.03)	
Prevented Deaths	239	112	
	(-691 – 1,169)	(-706 – 930)	
Observed/Expected Mortality Ratio	0.993	0.996	
	(0.965 – 1.022)	(0.966 – 1.028)	

Notes: Table shows results using two-way fixed effects estimations (with municipality and year fixed effects), instead of the Sun & Abraham (2021) method. The first row in each panel reports the ATT estimates of the effect of the audits on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels. Rows 2 and 3 of each panel report the estimates of prevented deaths and the ratio of observed to expected deaths calculated from observed deaths and estimates of changes in mortality for each group (row 1); 95 percent confidence intervals of the level of prevented deaths and of the ratio of observed to expected deaths are reported in parentheses.

Table D4: Audits' Effects on Early-Life Mortality and Prenatal Visits (Two-Way Fixed Effects Estimates, Controlling for Births)

	Dependent variables:		
	Child Deaths	Infant Deaths	Prenatal Visits
	(1)	(2)	(3)
Overall Population			
ATT	-0.44***	-0.36**	9.72***
	(0.13)	(0.11)	(2.90)
Prevented Deaths	6,518	5,321	-
	(2,651 – 10,385)	(2,038 – 8,605)	
Observed/Expected Mortality Ratio	0.945	0.947	
	(0.915 – 0.977)	(0.917 – 0.979)	
Nonwhite Mothers			
ATT	-0.20*	-0.15*	10.15***
	(0.09)	(0.07)	(2.94)
Prevented Deaths	2,987	2,259	-
	(525 – 5,449)	(170 – 4,348)	
Observed/Expected Mortality Ratio	0.951	0.956	
	(0.914 – 0.991)	(0.918 – 0.997)	
White Mothers			
ATT	-0.11	-0.09	-1.54
	(0.07)	(0.06)	(1.68)
Prevented Deaths	1,634	1,276	-
	(-310 – 3,577)	(-403 – 2,955)	
Observed/Expected Mortality Ratio	0.961	0.964	
	(0.919 – 1.008)	(0.921 – 1.012)	
Preventable Deaths			
ATT	-0.30**	-0.27**	-
	(0.10)	(0.09)	
Prevented Deaths	4,482	3,932	
	(1,497 – 7,466)	(1,291 – 6,573)	
Observed/Expected Mortality Ratio	0.943	0.943	
	(0.908 – 0.980)	(0.908 – 0.980)	
Non-Preventable Deaths			
ATT	0.00	0.01	-
	(0.03)	(0.03)	
Prevented Deaths	-67	-146	
	(-891 – 758)	(-881 – 589)	
Observed/Expected Mortality Ratio	1.002	1.006	
	(0.977 – 1.028)	(0.978 – 1.035)	

Notes: Specifications employ two-way fixed effects estimations (with municipality and year fixed effects), controlling for births. The first row in each panel reports the ATT estimates of the effect of the audits on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels. Rows 2 and 3 of each panel report the estimates of prevented deaths and the ratio of observed to expected deaths calculated from observed deaths and estimates of changes in mortality for each group (row 1); 95 percent confidence intervals of the level of prevented deaths and of the ratio of observed to expected deaths are reported in parentheses.

Table D5: Audits' Effects on Early-Life Mortality and Prenatal Visits (Sun & Abraham Estimates, Controlling for Births)

	Dependent variables:		
	Child Deaths	Infant Deaths	Prenatal Visits
	(1)	(2)	(3)
Overall Population			
ATT	-0.40**	-0.27*	9.34**
	(0.15)	(0.13)	(2.87)
Prevented Deaths	5,849	3,999	-
	(1,405 – 10,292)	(172 – 7,826)	
Observed/Expected Mortality Ratio	0.950	0.959	
	(0.915 – 0.998)	(0.924 – 0.998)	
Nonwhite Mothers			
ATT	-0.25*	-0.16	9.53***
	(0.10)	(0.09)	(2.81)
Prevented Deaths	3,745	2,321	-
	(808 – 6,683)	(-205 – 4,847)	
Observed/Expected Mortality Ratio	0.939	0.954	
	(0.897 – 0.986)	(0.909 –1.004)	
White Mothers			
ATT	-0.06	-0.03	-1.49
	(0.08)	(0.07)	(1.83)
Prevented Deaths	900	456	-
	(-1,452 – 3,251)	(-1,624 – 2,536)	
Observed/Expected Mortality Ratio	0.978	0.987	
	(0.926 – 1.037)	(0.932 – 1.049)	
Preventable Deaths			
ATT	-0.25*	-0.19	-
	(0.12)	(0.10)	
Prevented Deaths	3,664	2,830	
	(321 – 7,008)	(-172 – 5,833)	
Observed/Expected Mortality Ratio	0.953	0.958	
	(0.913 – 0.996)	(0.917 – 1.003)	
Non-Preventable Deaths			
ATT	0.01	0.04	-
	(0.04)	(0.04)	
Prevented Deaths	-3	-596	
	(-42 – 35)	(-1,701 – 510)	
Observed/Expected Mortality Ratio	1.003	1.023	
	(0.966 – 1.044)	(0.981 – 1.069)	

Notes: Table shows results using the Sun & Abraham (2021) method, controlling for births. The first row in each panel reports the ATT estimates of the effect of the audits on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5 percent, $**$ 1 percent, $***$ 0.1 percent significance levels. Rows 2 and 3 of each panel report the estimates of prevented deaths and the ratio of observed to expected deaths calculated from observed deaths and estimates of changes in mortality for each group (row 1); 95 percent confidence intervals of the level of prevented deaths and of the ratio of observed to expected deaths are reported in parentheses.