Effects of Anti-Corruption Audits on Early-Life Mortality

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Abstract

Despite substantial evidence of corruption in public health systems, it remains unknown whether anti-corruption interventions can improve health outcomes. We show that an important intervention — which randomly audited 1,949 municipalities for corruption — reduced early-life mortality in Brazil. An audited municipality is estimated to experience 0.54 fewer child deaths (95% CI: -0.85, -0.23; $P < 0.001$) and 0.44 fewer infant deaths (95% CI: -0.71, -0.17; $P = 0.001$) per year, relative to remaining unaudited. We estimate that 8,465 (95% CI: 3,600, 13,331) fewer children perished due to the intervention, including 6,916 (95% CI: 2,715, 11,117) infants. Early-life mortality fell especially sharply for nonwhite Brazilians, who face significant health disparities. Effects show temporal persistence, with large effects even a decade after audits. Moreover, the anti-corruption program increased prenatal care, particularly for nonwhite Brazilians. These results suggest governments may be able to improve health outcomes and reduce health disparities through anti-corruption interventions.

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

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 expanding coverage, research suggests that increased healthcare expenditures could prevent millions of deaths across all ages through improvements in the quality of care[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 leads to the deaths of approximately 140,000 children across the world each year[6].

Researchers and practitioners largely ignored the pernicious effects of corruption on health for decades. By contrast, recent publications in leading medical journals have called for increased attention to the "hidden pandemic" of corruption[6,7,8,9]. 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. However 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 these audits' causal impact on infant and child mortality. A randomly audited municipality is estimated to experience 0.54 fewer child deaths (95% CI: -0.85, -0.23; $P < 0.001$) and 0.44 fewer infant deaths (95% CI: -0.71, -0.17; $P = 0.001$) per year, relative to never experiencing an audit. 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. Moreover, we show that the intervention led to a substantial increase in women receiving recommended levels of prenatal care, which can reduce early-life mortality[11]. This increase in prenatal care is similarly concentrated among nonwhite Brazilians.

Before turning to these results, we first provide context about public healthcare in Brazil, especially about the role of municipalities and how audits can affect health outcomes. 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 increased markedly in recent decades, *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 53 to 13 deaths per thousand births between 1990 and 2019, 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 the nation.

As one of the most decentralized countries in the world, Brazil places healthcare responsibilities on all levels of government, including its 5,570 municipalities 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[12,20]. 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]. During recent presidential campaigns in Brazil, Jair Bolsonaro and Luiz Inacio Lula da Silva frequently railed against corruption[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 COVID-19 crisis, which caused over 700,000 Brazilian deaths, corruption probes 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].

In order to investigate whether anti-corruption audits reduce early-life mortality, we examine an important intervention in Brazil. To combat corruption in 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. Over 99 percent of Brazilian municipalities were below this population threshold and thus eligible for audits.

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].

Our estimation strategy exploits the *CGU*'s randomization of anti-corruption audits. Random variation in the selection and timing of audits across municipalities enables us to identify the causal impact of this intervention on early-life mortality. We use data from the *CGU* about the date of each lottery and which municipalities were randomly drawn. We consider municipalities to be treated if they were audited in any prior year; the Supplemental Materials show robustness to an alternative coding. Given the staggered nature of the treatment — municipalities were selected for audits during 40 lotteries spanning 13 years — we employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference[38]. The Supplementary Materials show that all findings are robust to traditional two-way fixed effects estimation as well as alternative methods proposed by Sun & Abraham (2021) and Athey et al. (2021)[39,40]. No significant pre-intervention differences are observed between treatment and control municipalities (Tables A1-A2).

We use individual-level vital statistics collected by Brazil's Ministry of Health to examine health outcomes. The primary outcomes analyzed are counts of deaths, stratified by age group (infant and child), mother's race (white and nonwhite), and cause of death (preventable and non-preventable). Infant mortality refers to deaths before one year of age, and child mortality refers to deaths before five years of age. We excluded municipalities with populations above 500,000 as they were ineligible for randomized audits. Our database includes information on 545,157 infant deaths and 640,713 child deaths between 2000 and 2015. Neonatal mortality is also examined in the Supplementary Materials and discussed below. 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. To investigate the effects of prenatal visits, we use data from Brazil's Ministry of Health that indicates whether each mother

had at least seven visits with healthcare professionals during her pregnancy (stratified by race). We also employ data on births from the same source.

Results

Figure 1 shows the effects of Brazil's anti-corruption audits on early-life mortality over time. In the first year after treatment, an audited municipality is estimated to experience 0.25 fewer child deaths (95% CI: -0.49, -0.01; $P = 0.045$) relative to never experiencing an audit, with no significant effect in infant deaths. In the fifth year after treatment, effects intensified to 0.64 fewer child deaths (95% CI: -1.06, -0.23; $P = 0.002$) and 0.53 fewer infant deaths (95% CI: -0.88, -0.17; $P = 0.003$). In the tenth year after treatment, effects intensified further to 0.74 fewer child deaths (95% CI: -1.30, -0.18; $P = 0.009$) and 0.62 fewer infant deaths (95% CI: -1.11, -0.14; $P = 0.012$).

Figure 2 reports the average treatment effect on the treated (ATT), which aggregates effects by treatment duration presented in Figure 1. An audited municipality is estimated to experience 0.54 fewer child deaths (95% CI: -0.85, -0.23; $P < 0.001$) and 0.44 fewer infant deaths (95% CI: -0.71, -0.17; $P = 0.001$) per year, relative to never experiencing an audit. As shown in Table 1, the audit program is estimated to have prevented the deaths of 8,465 children (95% CI: 3,600, 13,331) under five years old, including 6,916 infants (95% CI: 2,715, 11,117) under one year old (see columns 1 and 2). The observed mortality in audited municipalities is approximately 93.3 percent of the child deaths, and 93.5 percent of the infant deaths that would have occurred in the absence of the intervention.

Figure 3 shows the effects of audits on early-life mortality over time, for nonwhite versus white mothers. The top panel shows that audits significantly reduce nonwhite child deaths between six and nine years after treatment. In the eighth year, an audited municipality is estimated to experience 0.48 fewer deaths (95% CI: -0.74, -0.22; $P < 0.001$) of children with nonwhite mothers, relative to never experiencing an audit. The bottom panel shows that audits significantly reduce white child deaths two and five years after treatment. In the fifth year, an audited municipality is estimated to experience 0.20 fewer deaths (95% CI: -0.40, -0.01; $P = 0.042$) of children with white mothers. Supplementary Figure C1 shows that results are similar when examining infant deaths by mother's race. As with overall effects in Figure 1, point estimates are always negative for all years for both white and nonwhite children and infants.

Aggregating effects by treatment duration, Table 1 shows the ATT by mother's race. An audited municipality is estimated to experience 0.22 fewer child deaths (95% CI: -0.38, -0.05; $P = 0.010$) and 0.16 fewer infant deaths (95% CI: -0.29, -0.02; $P = 0.026$) among nonwhites per year, compared to never experiencing an audit. The anti-corruption intervention is estimated to have averted the loss of 3,370 nonwhite children (95% CI: 793, 5,947), including 2,436 infants (95% CI: 288, 4,583). The observed mortality in audited municipalities is approximately 94.8 and 95.5 percent of the nonwhite child and infant deaths, respectively, that would have occurred in the absence of the program. With regards to the ATT for white mothers, audits' effects are statistically indistinguishable from 0 for both children and infants.

Audits' effects on early-life mortality are only observed for deaths from preventable causes. In Table 1, 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 0.33 fewer child deaths (95% CI: -0.57, -0.09; $P = 0.007$) and 0.28 fewer

infant deaths (95% CI: -0.49, -0.07; $P = 0.009$) from preventable causes per year, relative to the counterfactual of never being audited. The audit program is estimated to have averted the deaths of 5,168 children (95% CI: 1,440, 8,896) from preventable causes, including 4,381 infants (95% CI: 1,092, 7,669). The observed mortality in audited municipalities is approximately 93.9 and 94.1 percent of the preventable child and infant deaths, respectively, that would have occurred without the intervention. Supplementary 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.

Anti-corruption audits also increased the number of Brazilian women with at least seven prenatal visits. As shown in Figure 2, an audited municipality is estimated to have 8.13 (95% CI: 1.46, 14.80; $P = 0.017$) more women per year obtaining this level of prenatal care, compared to never experiencing an audit. These effects are concentrated among nonwhite mothers. Whereas effects are statistically insignificant for white mothers, an audited municipality is estimated to have 10.27 (95% CI: 4.08, 16.46; $P < 0.001$) more nonwhite mothers per year with at least seven prenatal visits (column 3 of Table 1).

The Supplementary Materials provide numerous additional analyses. Diagnostic tests corroborate the validity of identifying assumptions (Figures B1-B4 and F4-F7; Tables B1 and F2). For both child and infant mortality, audit effects are observed for both female and male youth (Figures C4-C5). Results are robust when employing the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022), which allows event study models under staggered treatment assignment to adjust flexibly for potential pre-trends (Figures F1-F7 and Tables F1-F4)[38,40]. Findings are robust when controlling for births (Tables D3-D5); audits reduce white but not nonwhite births (Table D6). Audits also reduce infant and child mortality rates when employing the MC estimator (Table F3), as well as with both estimators when examining municipalities with populations above 10,000 (Tables E2 and F4). The study's results are also robust to traditional two-way fixed effects estimation, as well as to Sun & Abraham's (2021) alternative approach for staggered treatment adoption (Tables D1-D2)[39]. Furthermore, we replicate analyses for neonatal mortality and observe significant aggregate effects but not by race (Figures G1-G9 and Tables G1-G3); by contrast, audits do not affect fetal mortality (Table D6).

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, and the magnitude of lives saved was substantial. We estimate that the audit program prevented the deaths of 8,465 children under five years old, including 6,916 infants under one-year-old. The observed mortality in audited municipalities is approximately 93.3 percent of the child deaths and 93.5 percent of the infant deaths, that would have occurred in the absence of the 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, 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, reducing the diversion of funds allocated to public healthcare. Examining the same intervention, Avis et al. (2018) show that corruption is 7.9% lower in municipalities that previously experienced random anti-corruption audits than in 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 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[41,42], increasing such expenditures may be especially likely to improve the 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[43]. Although democratic elections enable voters to punish corrupt politicians, citizens often lack information about how their elected representatives abuse public office for private gain[37,44]. 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 have 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 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 explain our study's results.

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)[45] finds that audits decreased spending of federal transfers earmarked for health. Another fruitful direction is to elaborate and test political mechanisms by which audits might reduce early-life mortality. Such work should carefully consider 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[44]. 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[46]. Overall, elaborating and unpacking such theoretical mechanisms can explain why anti-corruption audits reduce early-life mortality in the Brazilian context.

Although the CGU's randomization of anti-corruption audits enables us to identify the causal impact of this intervention on early-life mortality, it deserves mention that several aspects of the program are suboptimal for analysis. First, the CGU switched to non-random audits of municipalities after 2015; CGU officials we interviewed in Brasília explained this policy shift focused auditing resources on municipalities where corruption is deemed most likely. To avoid contamination from this non-random treatment assignment, we do not examine effects on mortality after 2015. Second, if all randomized audits had been executed simultaneously, it would be unnecessary to use relatively complicated techniques for staggered treatment. And third, analysis would be further simplified if the probability of being audited had been constant across states and time; we address such deviations from simple random assignment by including state and year fixed effects.

Notwithstanding these points, given that our results provide causal evidence that anti-corruption interventions can reduce early-life mortality, another key direction for future research is examining the broader implications. 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 various anti-corruption interventions, including other ways of heightening top-down controls and bottom-up accountability. Despite growing attention to the effects of corruption on health, our findings suggest that increased attention to such questions can save many lives, especially among vulnerable populations.

Methods

Data

Data on early-life mortality are from individual-level vital statistics collected by Brazil's Ministry of Health. More specifically, data on all deaths between 2000 and 2015 are from the *Sistema de Informação sobre Mortalidade* (*SIM*) database. Given the intervention's population threshold, we exclude municipalities with over 500,000

inhabitants, using census data from the Brazilian Institute of Geography and Statistics (*IBGE*).

The primary outcomes are counts of deaths, stratified by age group (infant and child), mother's race (white and nonwhite), and cause of death (preventable and non-preventable). Infant mortality refers to deaths before one year of age, and child mortality refers to deaths before five years of age. Neonatal and fetal mortality is examined in the Supplementary Materials, using information from the *SIM* database. In addition, mortality and fertility rates are analyzed in the Supplementary Materials, employing data from *DATASUS*.

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[47], we focus on the latter category. As for other outcomes, we also stratify prenatal visits by race (white and nonwhite). Additional analyses in the Supplementary Materials employ individual-level information on births from the *SINASC* dataset.

With regards to the treatment, we employ data from the *Controladoria-Geral da União* (*CGU*) on its randomized audits conducted through the *Programa de Fiscalização por Sorteios Públicos* (Monitoring Program with Public Lotteries) program. This information provides the dates of audits and which municipalities were randomly drawn.

Estimation Strategy

Our estimation strategy exploits the *CGU*'s randomization of anti-corruption audits. Random variation in the timing of audits across municipalities enables us to identify the causal impact of this intervention on early-life mortality. Our study context diverges 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, and other municipalities were never audited.

Given the staggered nature of the treatment, we employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference to estimate average and dynamic treatment effects (described below)[38]. This method addresses issues with conventional two-way fixed effects (TWFE) models commonly used in differences-in-differences designs. As investigated by numerous studies, TWFE models can suffer from a negative weights problem if treatment effects are heterogeneous[39,48,49]. More specifically, ATT estimates in TWFE models are weighted averages of treatment effects on each group under the treatment condition, and weights can be negative if the treatment is staggered across time and treatment effects are heterogeneous. As a result, ATT estimates from TWFE models may be biased and can provide misleading tests for baseline balance and parallel trends.

The fixed effects counterfactual estimator developed by Liu et al. (2022) takes observations under the treatment condition as missing, and uses untreated observations

to specify models — and then applies those models to impute counterfactuals of treated observations. This approach circumvents the negative weights problem because it does not use treated observations when specifying models and it applies uniform weights on each group's treatment effects. This approach has also been developed by Borusyak et al. (forthcoming), who show it is most efficient among the class of linear unbiased estimators of the ATT[50]. The Supplementary Information shows that our findings are robust to traditional TWFE estimation, as well as to Sun & Abraham's (2021) alternative approach for staggered treatment adoption (which aggregates cohort-specific average treatment effects on the treated)[39]. Furthermore, it shows robustness when employing the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022)[38,40]. The MC estimator allows event study models under staggered treatment assignment to adjust flexibly for potential pre-trends, which we conduct because visual inspection raises the possibility of pre-trends even though they are never statistically significant.

Our analysis estimates 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. More specifically, we define treatment Audit_{mst-1} in a given municipality-year as taking value 1 if municipality m has ever been audited up to time $t-1$ and 0 otherwise. We code municipalities as treated if they were audited up to time $t-1$, and not up to time t, because of gestation periods and the timing of audits throughout the year. The Supplemental Materials show all results are robust if municipalities are coded as treated if they were audited up to time t (Appendix H). Following Liu et al. (2022), we make standard assumptions for differences-in-differences estimation: (i) additive separability or functional form assumptions of the structure of group and time fixed effects, and (ii) strict exogeneity. We follow their four-step procedure to execute the fixed effects counterfactual estimator, as summarized below.

Procedure

Step 1: On the subset of untreated observations, we fit a TWFE model of the relationship between the mortality outcome D_{mst} and municipality-by-state and year fixed effects:

$$D_{mst} = \alpha_{ms} + \delta_t + \varepsilon_{mst} \tag{1}$$

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, α_{ms} and δ_t are municipality and year fixed effects, respectively, obtaining estimates for these.

Step 2: We predict the counterfactual outcome $D_{mst}(0)$ for each treated observation using the estimated municipality-by-state and year fixed effects in step 1. That is, $\hat{D}_{mst}(0) = \hat{\alpha}_{ms} + \hat{\delta}_t$ for all m, s, t.

Step 3: We estimate the unit-specific treatment effects (θ_{mst}) for each treated observation using $\hat{\theta}_{mst} = D_{mst} - \hat{D}_{mst}(0)$. That is, we estimate the difference between the actual outcome and the counterfactual generated for each observation in step 2.

Step 4: We take averages of $\hat{\theta}_{mst}$ to generate estimates for the ATT. To determine each element of average dynamic effects, we also estimate the ATT s years since the treatment occurred. We do the latter by plotting the averages of the differences between D_{mst} and $\hat{D}_{mst}(0)$ for units in the treatment group ($\text{Audit}_{mst-1} = 1$), re-indexed based on the time $t+s$ relative to the onset of the treatment.

To generate overall estimates of the ATT, we perform the above procedure with data from the overall population. To generate subgroup-specific estimates of the ATT, we use data stratified by the subgroups of interest. We also report plots for estimates of dynamic treatment effects for the overall population and for subgroups. For inference, we implement a non-parametric block bootstrap (with blocking at the municipality level and 200 replications) to estimate standard errors for treatment effect estimates.

Diagnostics

Liu et al. (2022) provide excellent diagnostic tools, which we use to examine the validity of identifying assumptions. We describe these diagnostics and then summarize the findings shown in the Supplementary Materials:

1. No Pre-Trend Test: To examine whether our data are consistent with the staggered random assignment of municipalities to the audit treatment, we examine whether the estimated differences in the pre-treatment periods (when $s \leq 0$) in the dynamic treatment effect plots exhibit an upward or a downward trend and whether these are statistically significant from zero. If a key identifying assumption — strict exogeneity — is valid, the average pre-treatment residuals should converge to zero.

2. Equivalence Test: Given the possible lack of statistical power to detect pre-trends in the test just described, another diagnostic tool is to investigate if pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. We follow Liu et al.'s (2022) default procedure and specify a null hypothesis that pre-treatment differences are greater than 0.36σ.

3. Placebo Test: Applying this diagnostic tool, we assume each municipality in the treated group is audited s years earlier than the actual onset of the audit. We then apply the same counterfactual estimator to obtain estimates of ATTs for $s = -(S-1), \ldots,$-1, 0. Under the identifying assumptions, the magnitude of this "placebo ATT" estimate is statistically indistinguishable from zero. We report the p-value of such tests. We also perform analogous equivalence tests as in (2) for these placebo tests and report p-values.

Findings for these three diagnostics, provided in the Supplementary Materials (Figures B1-B4 and Table B1), are consistent with the strict exogeneity assumption for the causal inference analysis. To summarize, the joint hypotheses tests for the lack of pre-trends for overall child and infant deaths fail to reject the null hypothesis of no pre-trends (p-values = 0.42 and 0.40, respectively). Similarly, we fail to reject the null hypotheses of no pre-trends for child and infant deaths for both white and nonwhite mothers. More generally, we fail to reject null hypotheses at conventional confidence levels for 12 out of 13 such joint tests (see Table B1). Analogous positive results hold when examining the equivalence tests: we systematically reject the null hypothesis that pre-treatment differences are greater than 0.36σ for overall child and infant deaths, as well as for such deaths by mother's race. Finally, we also fail to reject the null hypotheses that there are no placebo effects five years before the random assignment of municipalities for overall child and infant deaths, as well as by mother's race. More specifically, we fail to reject the null hypothesis at conventional confidence levels for 12 out of 13 such joint tests (see Table B1).

Aggregate Estimates of Reduced Mortality

To illustrate the impact of the program, we also estimate the number of deaths prevented among treated municipalities. We do so for the overall population and by race and cause of death, using the ATT estimates described above and their 95% confidence intervals. Specifically, we estimate the average number of children per year who would have died in treated municipalities following their respective audits until 2015, had these municipalities remained unaudited. We employ the following algorithm to estimate the aggregate number of prevented deaths:

1) We first calculate $\bar{D}_{mst}(1)$, the average number of deaths per municipality-year in treated municipalities after their audits until 2015.

2) We estimate the proportional decrease in average deaths among treated municipalities by dividing the ATT described above by $\bar{D}_{mst}(1)$.

3) To estimate the prevented child deaths, we multiply this result by the total actual number of child deaths in treated municipalities after their respective audits until 2015.

4) We construct 95% confidence intervals of this estimate of prevented child deaths using the analogous confidence intervals of the ATT.

In addition to conducting this analysis for children, we also do so for infants and each relevant subgroup.

Data Availability

All data employed in this study are available in the following OSF replication archive: https://tinyurl.com/3b342c84. The Data section above describes the original sources of these data.

Replication Materials

All code to conduct the analyses are available at the following OSF replication archive: https://tinyurl.com/3b342c84. The R files in this archive reproduce all figures and tables in this article, as well as in its appendices.

Acknowledgments:

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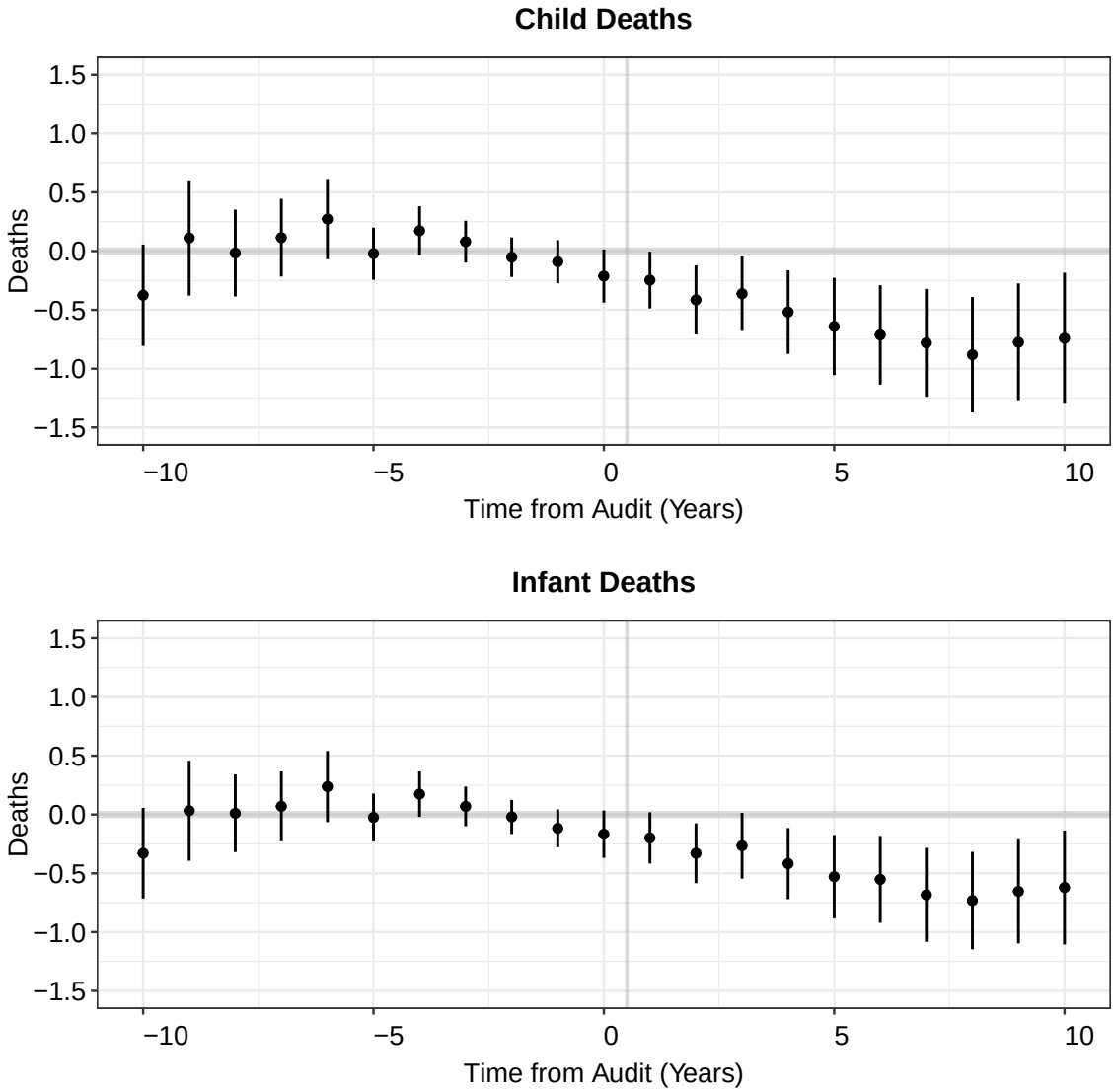
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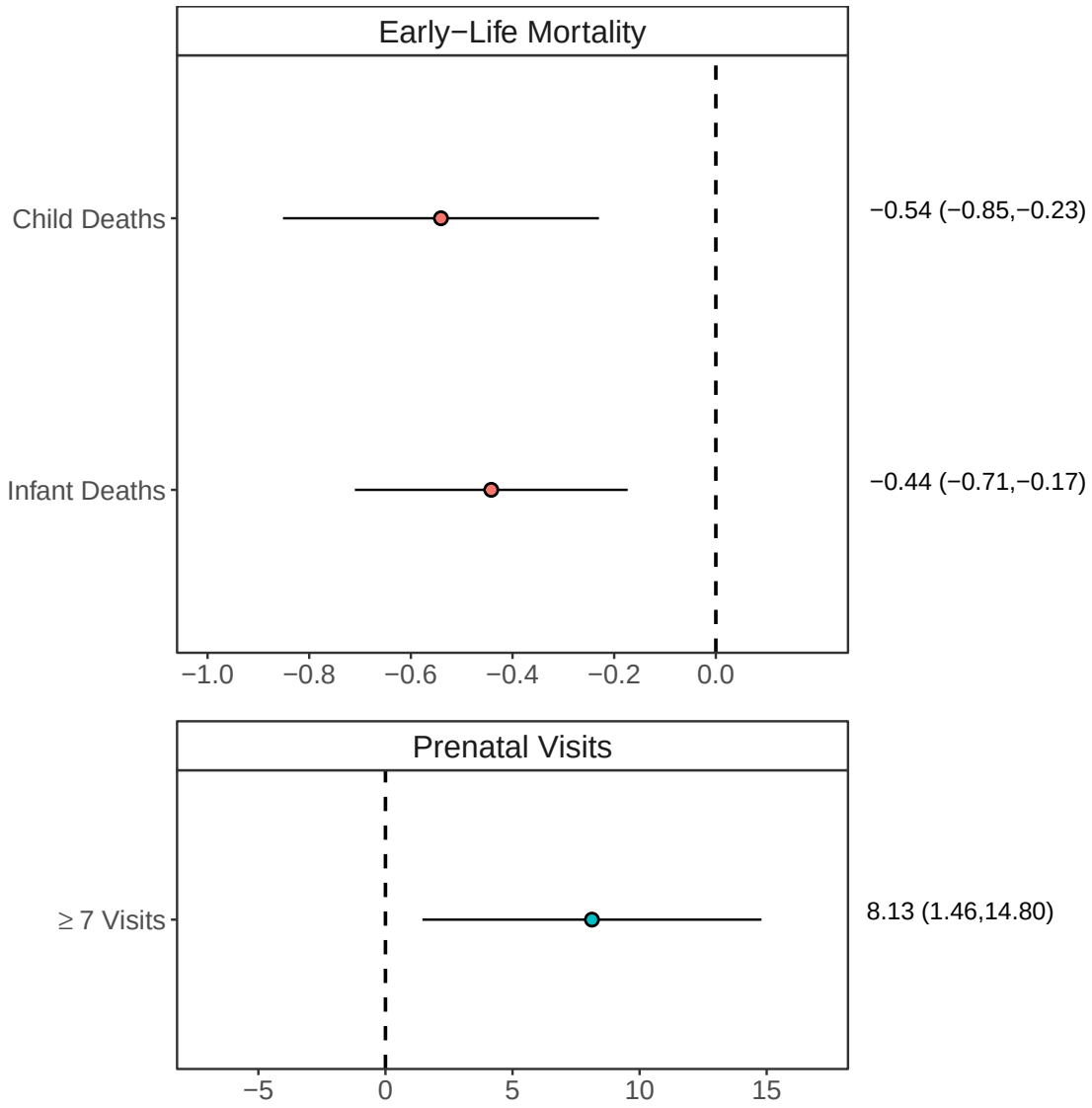
Figures and Tables

Figure 1: Audit Effects on Early-Life Mortality over Time



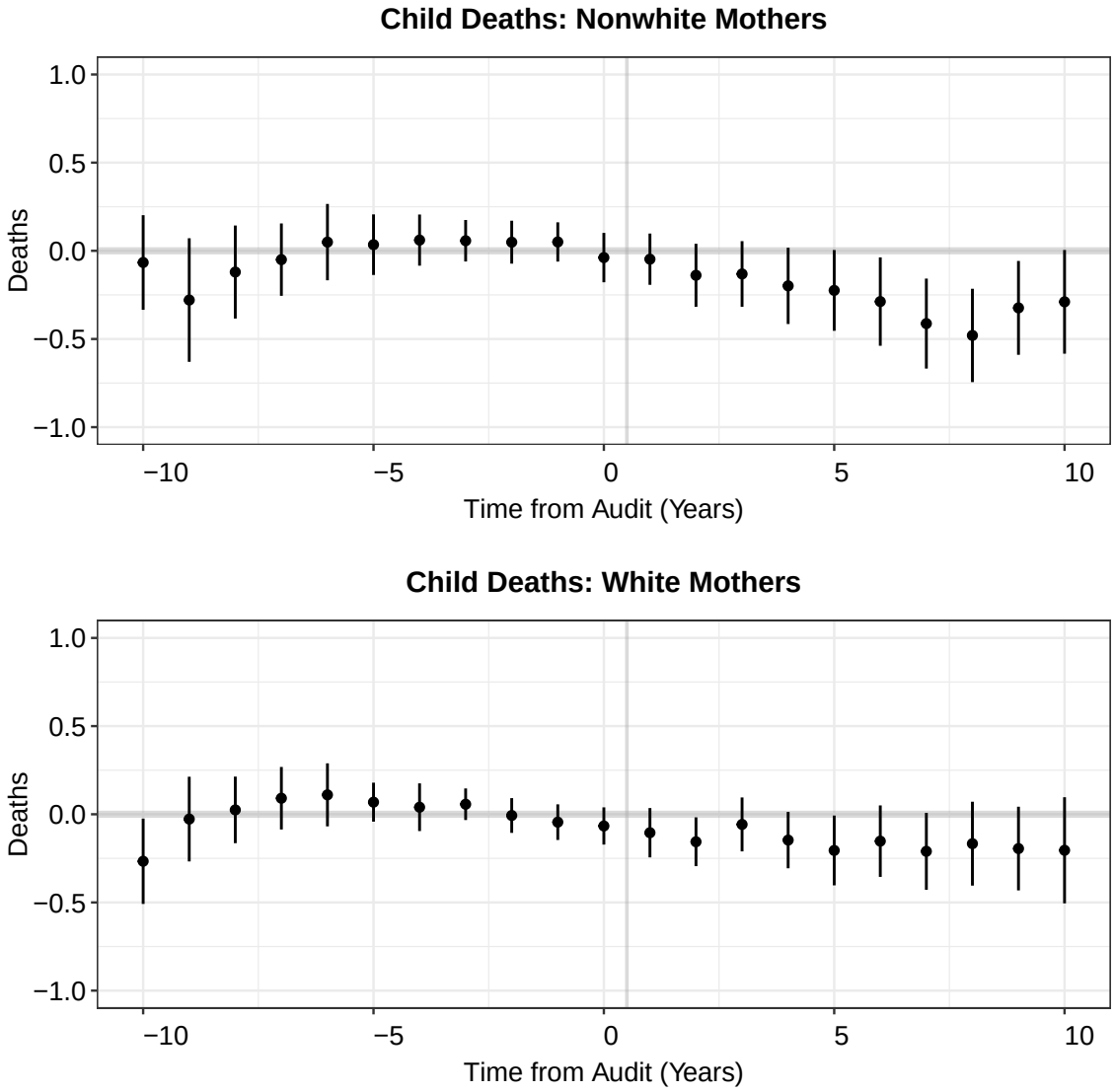
Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Figure 2: Audit Effects on Early-Life Mortality and Prenatal Care Coverage



Notes: This figure reports plots of the overall Average Treatment Effect on the Treated, employing Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Figure 3: Audit Effects on Child Mortality over Time, by Race of Mother



Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Table 1: Audit Effects on Early-Life Mortality and Prenatal Care Coverage

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.54***	-0.44**	8.13*
	(0.16)	(0.14)	(3.40)
	[0.001]	[0.001]	[0.017]
Prevented Deaths	8,465	6,916	
	(3,600 –13,331)	(2,715 – 11,117)	
Observed/Expected Mortality Ratio	0.933	0.935	
	(0.898 – 0.970)	(0.900 – 0.974)	
Nonwhite Mothers			
ATT	-0.22*	-0.16*	10.27**
	(0.08)	(0.07)	(3.16)
	[0.010]	[0.026]	[0.001]
Prevented Deaths	3,370	2,436	
	(793 – 5,947)	(288 – 4,583)	
Observed/Expected Mortality Ratio	0.948	0.955	
	(0.912 – 0.987)	(0.918 – 0.994)	
White Mothers			
ATT	-0.13	-0.11	-3.16
	(0.07)	(0.06)	(1.75)
	[0.070]	[0.097]	[0.070]
Prevented Deaths	2,070	1,650	
	(-169 – 4,309)	(-301 – 3,600)	
Observed/Expected Mortality Ratio	0.954	0.957	
	(0.909 – 1.004)	(0.910 – 1.008)	
Preventable Causes			
ATT	-0.33**	-0.28**	
	(0.12)	(0.11)	
	[0.007]	[0.009]	
Prevented Deaths	5,168	4,381	
	(1,440 – 8,896)	(1,092 – 7,669)	
Observed/Expected Mortality Ratio	0.939	0.941	
	(0.899 – 0.982)	(0.901 – 0.984)	
Non-Preventable Causes			
ATT	-0.01	-0.01	
	(0.04)	(0.03)	
	[0.702]	[0.720]	
Prevented Deaths	212	165	
	(-873 – 1,298)	(-737 – 1,067)	
Observed/Expected Mortality Ratio	0.994	0.994	
	(0.963 – 1.026)	(0.962 – 1.028)	
Num. of municipalities	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370

Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference (as presented in the Methods section). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Supplementary Materials for
"Effects of Anti-Corruption Audits on Early-Life Mortality"

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Appendix A: Descriptive Statistics

Table A1: Descriptive Statistics

	All Municipalities, Annual 2000-2015	All Municipalities, Annual 2000-2002	Pre-Intervention Difference, Annual 2000-2002
	(1)	(2)	(3)
Child Deaths			
Overall	7.7	10.1	0.5
	[14.6]	[18.7]	(0.5)
			[0.338]
White	3.1	4.1	0.3
	[7.0]	[8.9]	(0.2)
			[0.239]
Nonwhite	3.3	3.3	0.2
	[7.8]	[8.2]	(0.2)
			[0.464]
Preventable	5.1	6.6	0.3
	[10.2]	[13.2]	(0.4)
			[0.379]
Non-Preventable	2.1	2.3	0.1
	[4.2]	[4.8]	(0.1)
			[0.673]
Infant Deaths			
Overall	6.5	8.6	0.4
	[12.6]	[16.2]	(0.5)
			[0.405]
White	2.6	3.5	0.2
	[6.1]	[7.8]	(0.2)
			[0.310]
Nonwhite	2.7	2.7	0.1
	[6.6]	[6.8]	(0.2)
			[0.528]
Preventable	4.5	5.8	0.3
	[9.1]	[11.8]	(0.3)
			[0.434]
Non-Preventable	1.6	1.8	0.0
	[3.3]	[3.8]	(0.1)
			[0.732]
Prenatal Care Coverage (≥ 7)			
Overall	208.2	173.1	7.1
	[445.2]	[407.5]	(11.5)
			[0.536]
White	113.0	110.0	8.7
	[299.6]	[289.2]	(8.0)
			[0.278]
Non-White	85.8	50.4	-0.0
	[210.8]	[147.2]	(4.3)
			[0.992]
Birth			
Overall	393.0	410.7	20.2
	[727.8]	[752.6]	(21.8)
			[0.354]
White	173.3	208.9	16.2
	[415.2]	[458.8]	(13.0)
			[0.212]
Nonwhite	199.9	168.7	2.8
	[448.2]	[397.9]	(11.0)
			[0.802]
Num. of municipalities	5203	5203	5203
Num. of observations	83248	15609	15609

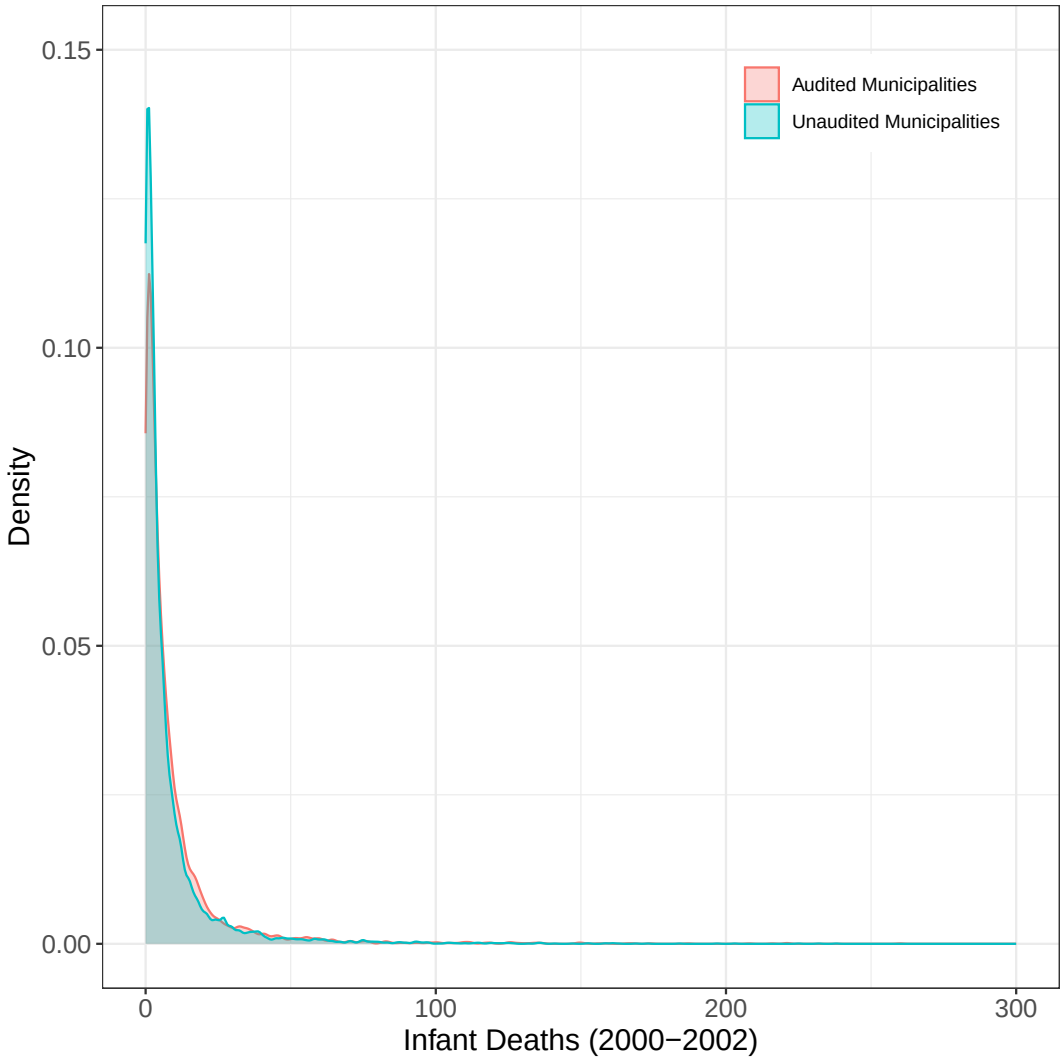
Notes: Columns 1 and 2 present the mean of each variable and its standard deviation in brackets, the first column for the overall period 2000-2015, and the second for the baseline period preceding the start of the audit program (2000-02). Due to missing data, figures by race and preventability do not sum to overall figures. 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%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. As with all analyses in this study, the sample excludes municipalities with populations above 500,000, which were ineligible for the CGU's randomized anti-corruption audits.

Table A2: Additional Balance Tests

	Unaudited Municipalities, 2000	Audited Municipalities, 2000	Pre-Intervention Difference, 2000
	(1)	(2)	(3)
Expected Years of Schooling	8.4	8.1	0.0
	[1.8]	[1.8]	(0.0)
			[0.574]
Share Illiterate	21.7	23.3	-0.0
	[12.4]	[12.5]	(0.2)
			[0.863]
Share Primary School	19.4	18.8	0.1
	[9.6]	[9.2]	(0.2)
			[0.616]
Share Secondary School	11.5	11.2	0.1
	[6.4]	[6.1]	(0.2)
			[0.612]
Share Higher Education	2.4	2.2	0.0
	[2.3]	[2.1]	(0.1)
			[0.749]
Income Inequality (Gini)	0.5	0.6	0.0
	[0.1]	[0.1]	(0.0)
			[0.618]
Share Extremely Poor	20.5	23.0	0.1
	[17.1]	[17.5]	(0.3)
			[0.828]
Share Poor	40.9	44.1	-0.0
	[22.7]	[22.7]	(0.4)
			[0.894]
Income per Capita	340.2	316.1	-1.8
	[192.3]	[180.3]	(3.6)
			[0.626]
Self-employed Workers (% of Total Employment)	27.8	28.1	-0.1
	[10.7]	[10.4]	(0.3)
			[0.689]
Employment in Agriculture (% of Total Employment)	42.3	41.9	-0.9
	[20.2]	[20.1]	(0.5)
			[0.097]
Unemployment Rate	9.5	10.2	0.5
	[5.3]	[5.9]	(0.1)
			[0.001]
Households with Access to Piped Water (%)	67.5	62.5	0.2
	[29.1]	[29.4]	(0.5)
			[0.681]
Households with Access to Garbage Collection (%)	79.8	77.9	0.4
	[25.1]	[25.3]	(0.5)
			[0.397]
Households with Access to Electricity (%)	86.8	84.8	0.1
	[16.9]	[17.8]	(0.3)
			[0.860]
Female Population	10,892.8	11,652.2	203.8
	[19,687.0]	[20,083.6]	(565.5)
			[0.720]
Economically Active Population	8,836.3	9,202.3	146.3
	[17,107.3]	[16,973.8]	(488.3)
			[0.764]
Rural Population	5,518.1	6,292.0	73.7
	[5,518.1]	[6,711.2]	(155.6)
			[0.636]
Population	21,722.2	23,282.2	424.2
	[38,371.9]	[39,284.8]	(1,109.0)
			[0.702]
Human Development Index	0.5	0.5	0.0
	[0.1]	[0.1]	(0.0)
			[0.892]
Num. of municipalities	5203	5203	5203
Num. of observations	5203	5203	5203

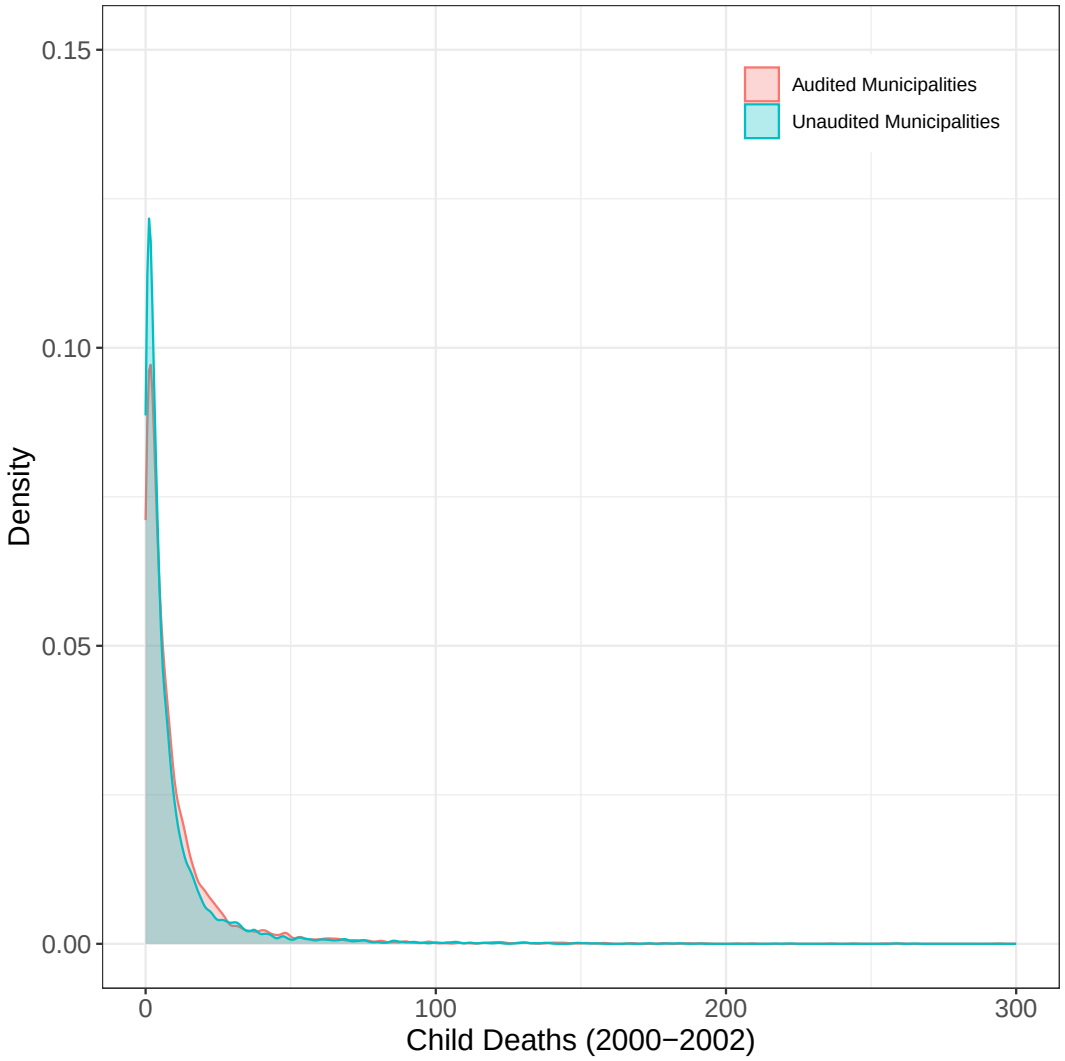
Notes: Columns 1 and 2 present the mean of each variable and its standard deviation in brackets, the first column for unaudited municipalities, and the second for the audited municipalities. All data are for 2000, which precedes the start of the audit program. Column 3 reports the pre-intervention difference between audited and unaudited municipalities based on a regression that adjusts for state effects to take into account the stratification of random assignment by state. Standard errors of adjusted difference are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. As with all analyses in this study, the sample excludes municipalities with populations above 500,000, which were ineligible for the CGU's randomized anti-corruption audits. Data in this table are from 2000 IBGE census data reported in the UNDP's Atlas do Desenvolvimento Humano 2013.

Figure A1: Infant Deaths in Pre-Intervention Period (Kernel Density Plot)



Notes: This figure overlays two kernel density plots of the number of infant deaths per municipality in 2000-02, comparing audited and unaudited municipalities. As with all analyses in this study, the sample excludes municipalities with populations above 500,000, which were ineligible for the CGU's randomized anti-corruption audits.

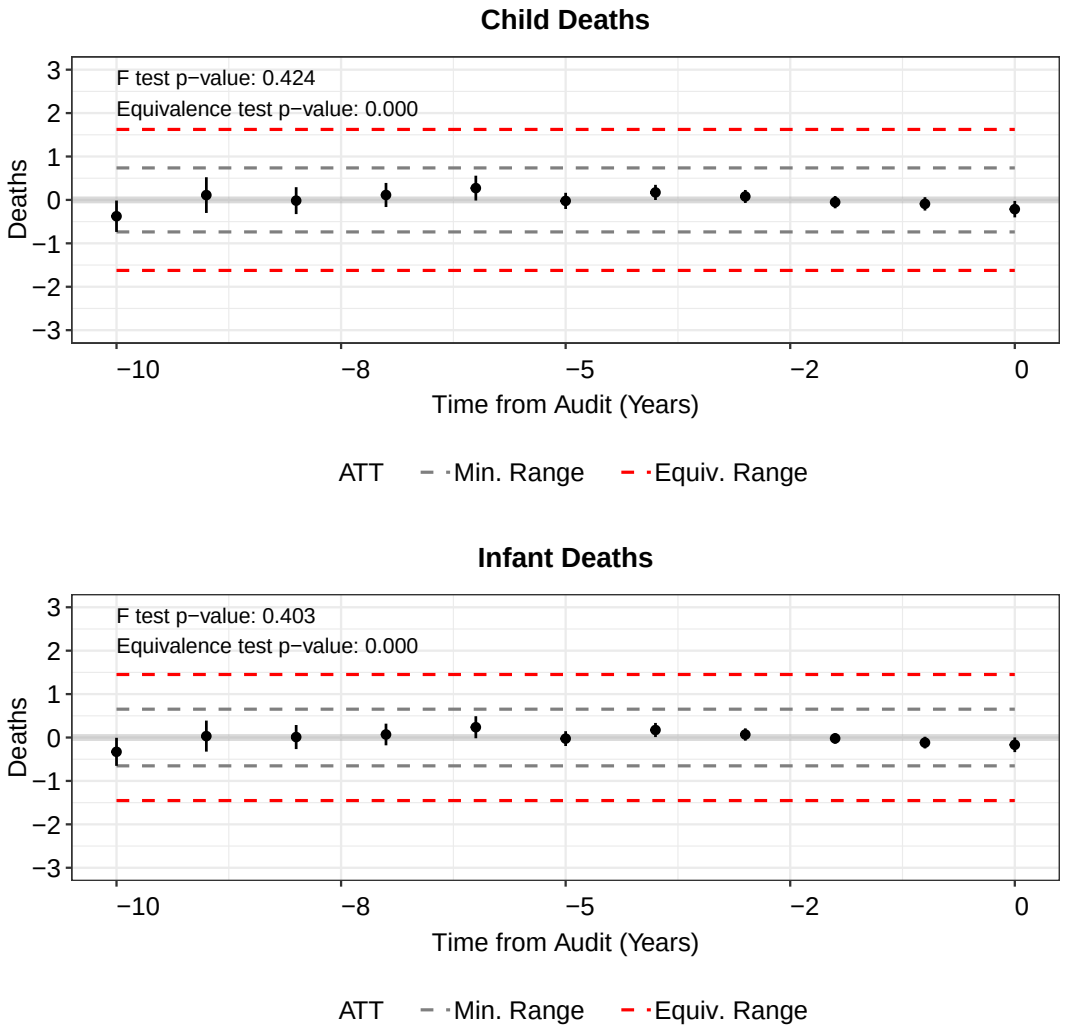
Figure A2: Child Deaths in Pre-Intervention Period (Kernel Density Plot)



Notes: This figure overlays two kernel density plots of the number of child deaths per municipality in 2000-02, comparing audited and unaudited municipalities. As with all analyses in this study, the sample excludes municipalities with populations above 500,000, which were ineligible for the CGU's randomized anti-corruption audits.

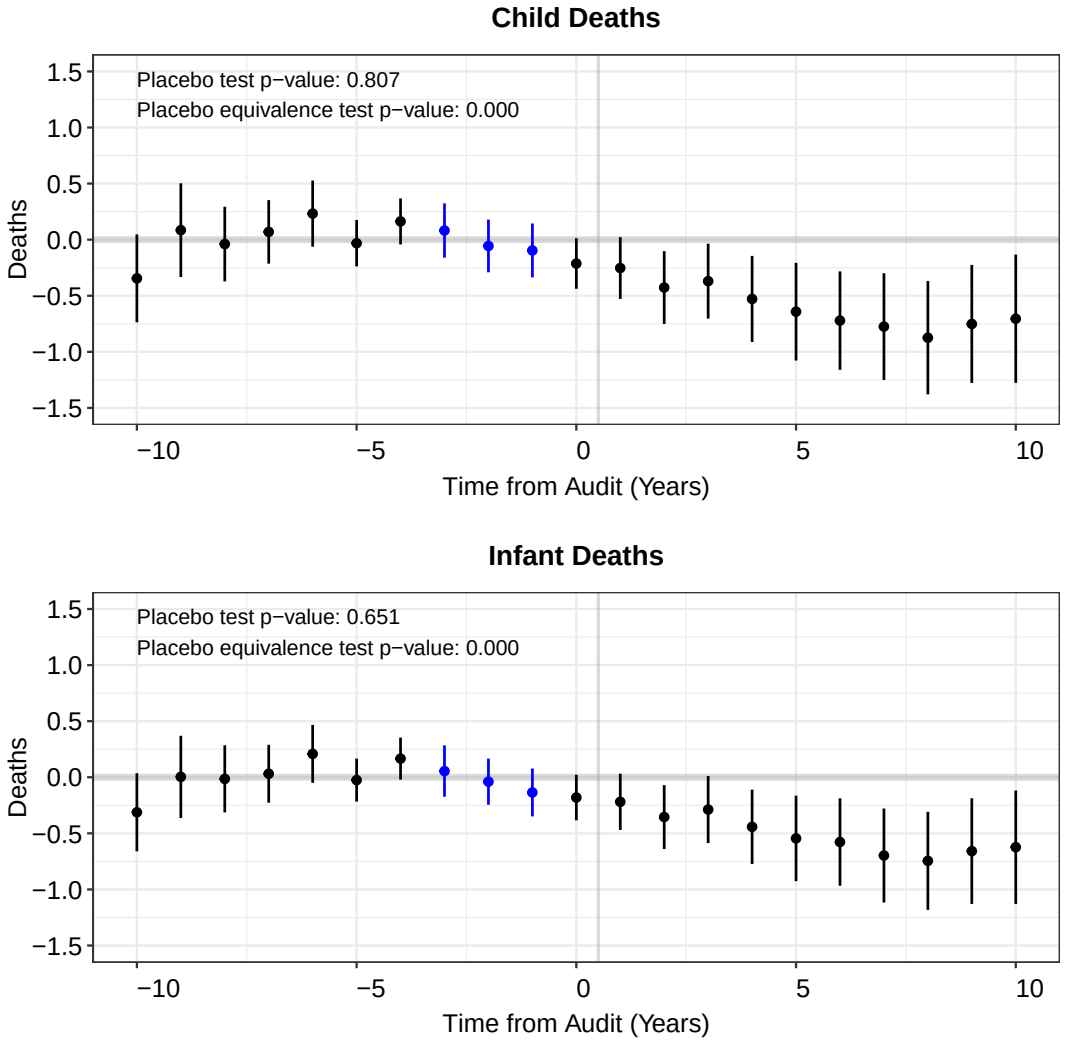
Appendix B: Diagnostic Tests - Primary Results for Fixed Effects Counterfactual Estimator

Figure B1: Pre-Trend Tests for Audit Effects on Early-Life Mortality over Time (Fig. H1)



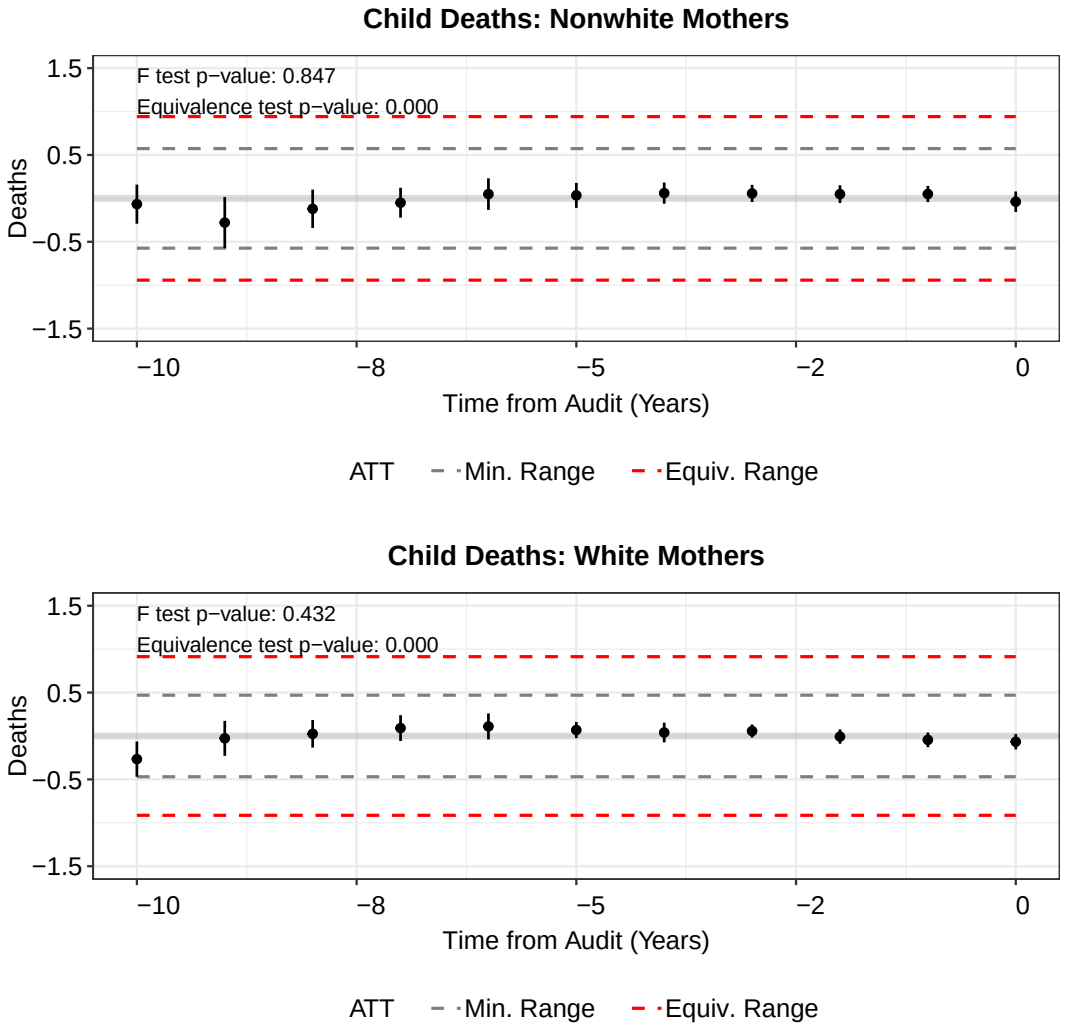
Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on the Liu et al.'s (2022) fixed effects counterfactual estimator for child deaths and infant deaths, respectively. In addition, we report the p-value of a joint test of the significance of pre-treatment differences and p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure B2: Placebo Tests for Audit Effects on Early-Life Mortality over Time (Fig. H1)



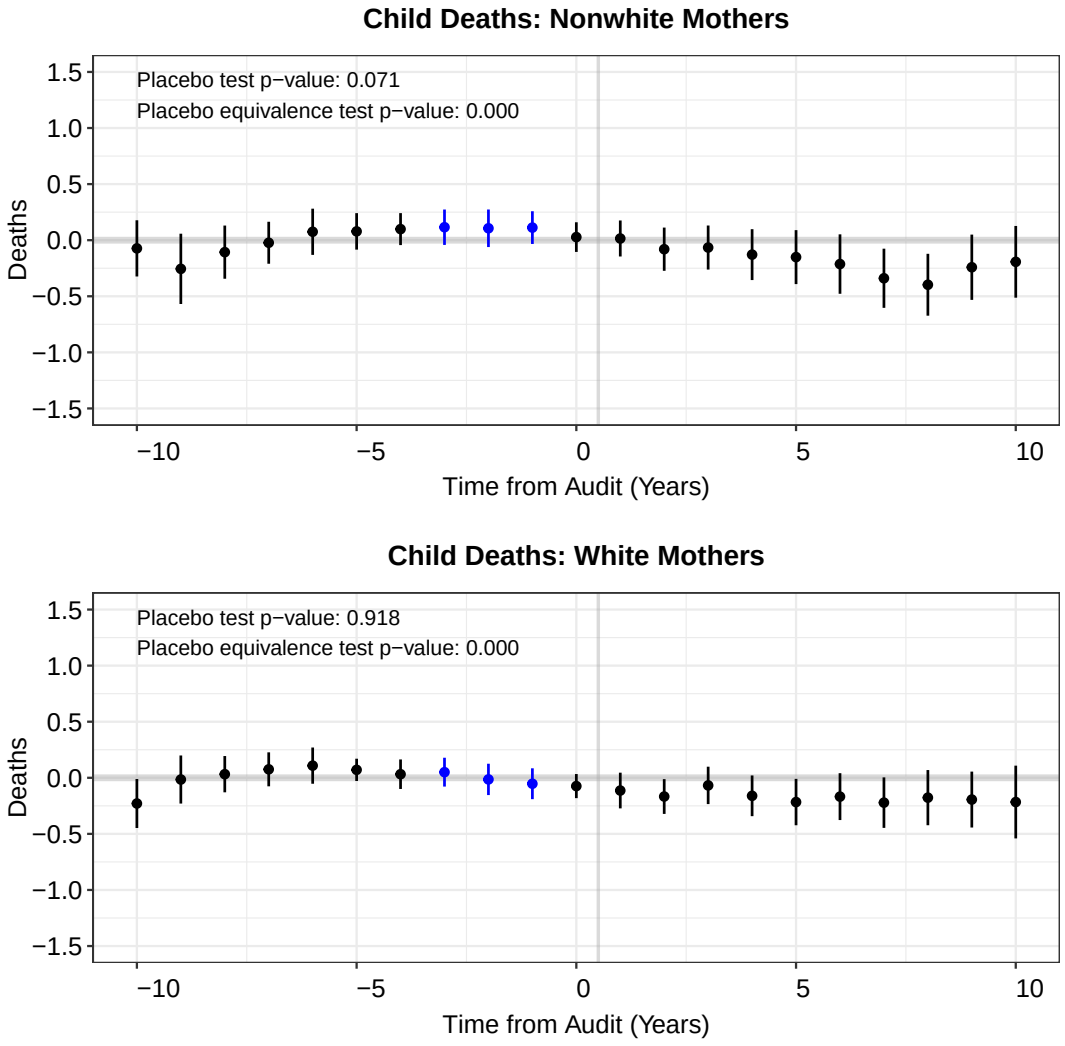
Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from Liu et al.'s (2022) fixed effects counterfactual estimator, for child deaths and infant deaths respectively. In addition, we report the p-value of a joint test of the significance of these placebo differences and the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure B3: Pre-Trend Tests for Audit Effects on Early-Life Mortality over Time, by Race of Mother (Fig. H3)



Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on Liu et al.'s (2022) fixed effects counterfactual estimator for child deaths and infant deaths, respectively. In addition, we report the p-value of a joint test of the significance of pre-treatment differences and p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure B4: Placebo Tests for Audit Effects on Early-Life Mortality over Time, by Race of Mother (Fig. H3)



Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from Liu et al.'s (2022) fixed effects counterfactual estimator, for child deaths and infant deaths respectively. In addition, we report the p-value of a joint test of the significance of these placebo differences, as well as the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

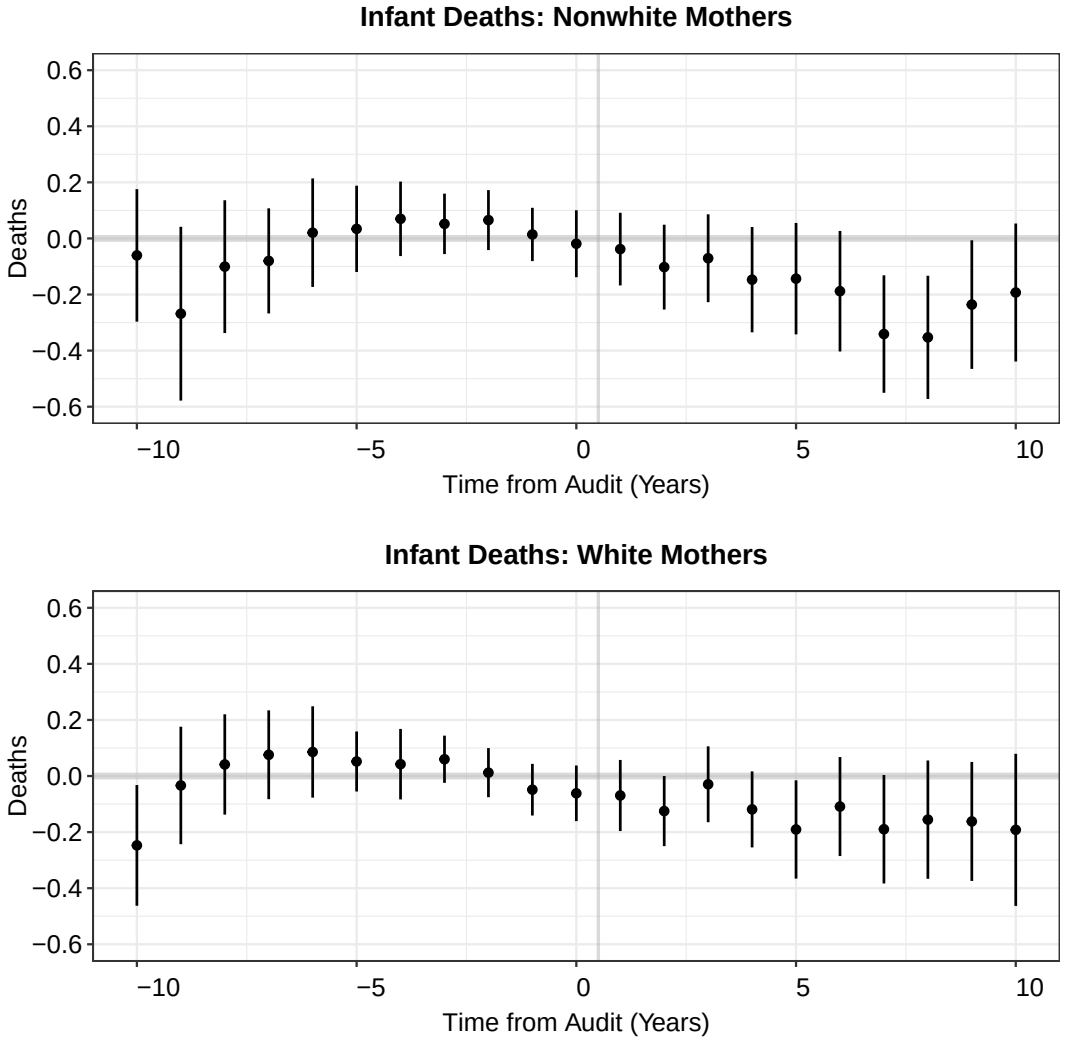
Table B1: Additional Pre-trends and Placebo Checks

	Pre-trend Tests:		Placebo Tests:	
	F-Test P-Value	Equivalence Test P-Value	T-Test P-Value	Equivalence Test P-Value
Child Deaths				
Overall	.424	.000	.807	.000
Nonwhite Mothers	.847	.000	.071	.000
White Mothers	.432	.000	.919	.000
Preventable	.668	.000	.923	.000
Non-Preventable	.518	.000	.911	.000
Infant Deaths				
Overall	.403	.000	.651	.000
Nonwhite Mothers	.887	.000	.089	.000
White Mothers	.402	.000	.942	.000
Preventable	.705	.000	.955	.000
Non-Preventable	.660	.000	.526	.000
Prenatal Care Coverage				
Overall	.116	.000	.236	.000
Nonwhite Mothers	.034	.000	.458	.000
White Mothers	.126	.000	.018	.000

Notes: Column 1 of the table reports p-values of joint tests of significance of pre-treatment differences of the dynamic treatment effects based on Liu et al.'s (2022) fixed effects counterfactual estimator, for child deaths, infant deaths, and prenatal coverage, overall and by subgroup, respectively. Column 2 reports analogous p-values of an equivalence test that such pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. Column 3 reports p-values of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from Liu et al.'s (2022) fixed effects counterfactual estimator, for the same outcomes. Column 4 reports p-values of analogous equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

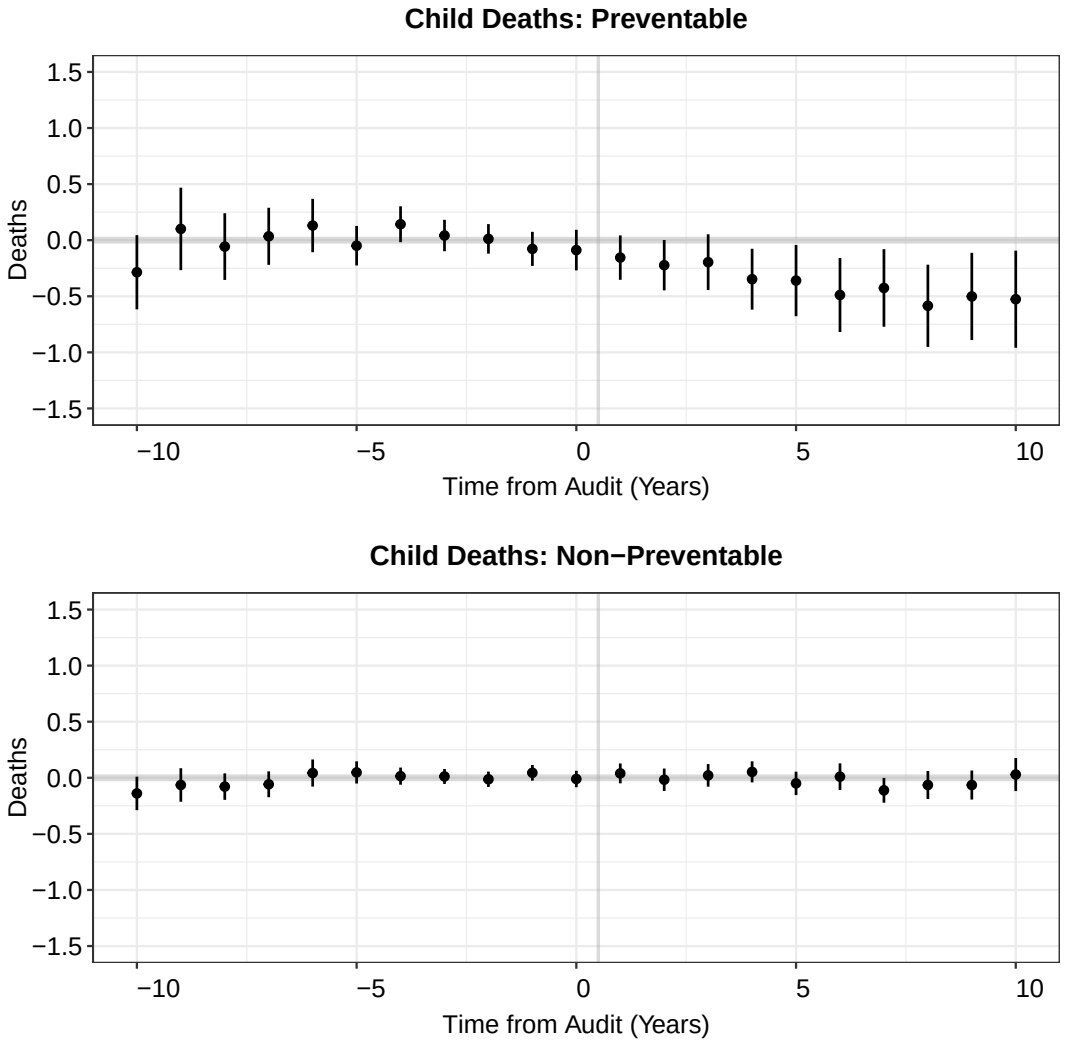
Appendix C: Additional Dynamic Treatment Effects Estimates

Figure C1: Audit Effects on Infant Mortality over Time, by Mother's Race (Fixed Effects Counterfactual Estimator)



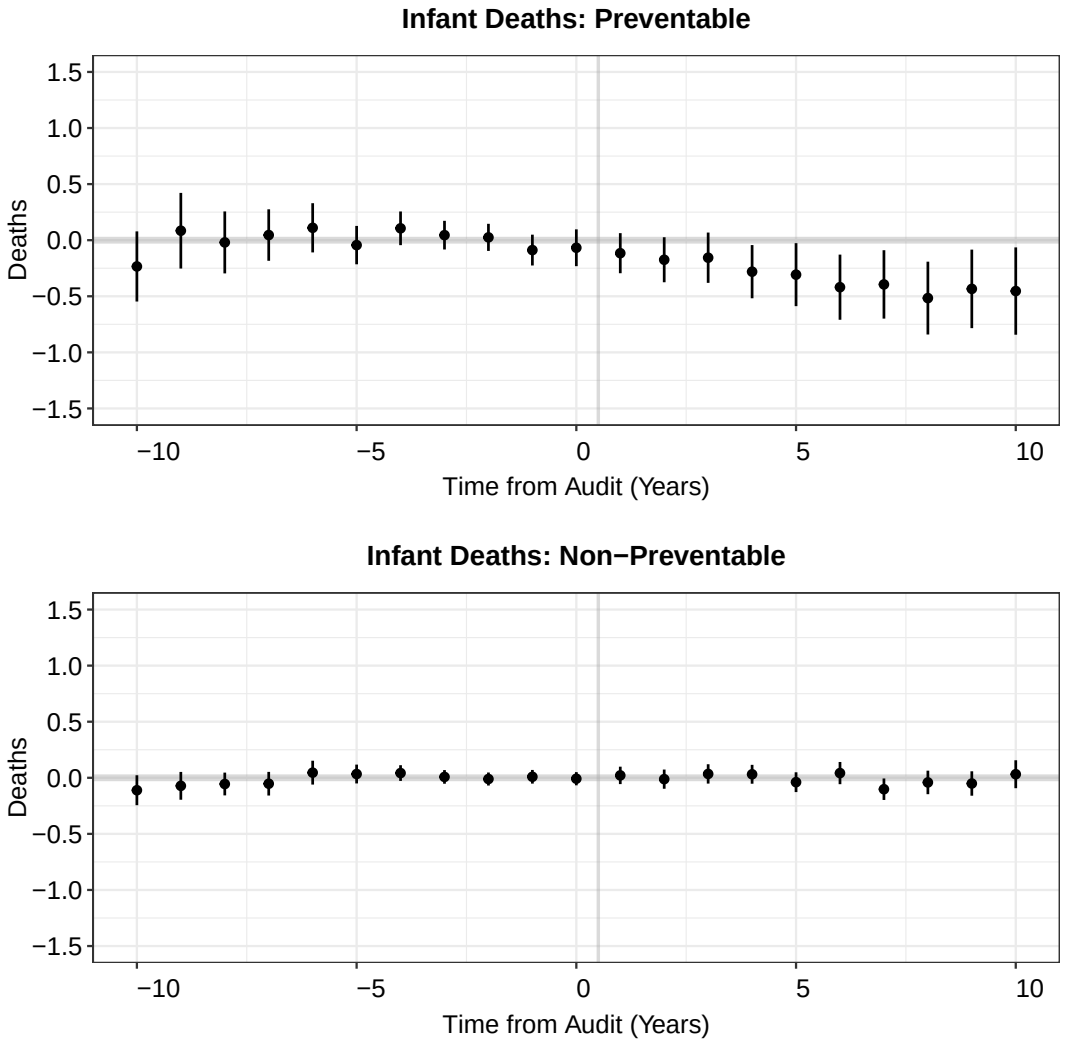
Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Block-bootstrapped standard errors allow for correlation of error terms at the municipality level.

Figure C2: Audit Effects on Child Deaths over Time, by Cause of Death (Fixed Effects Counterfactual Estimator)



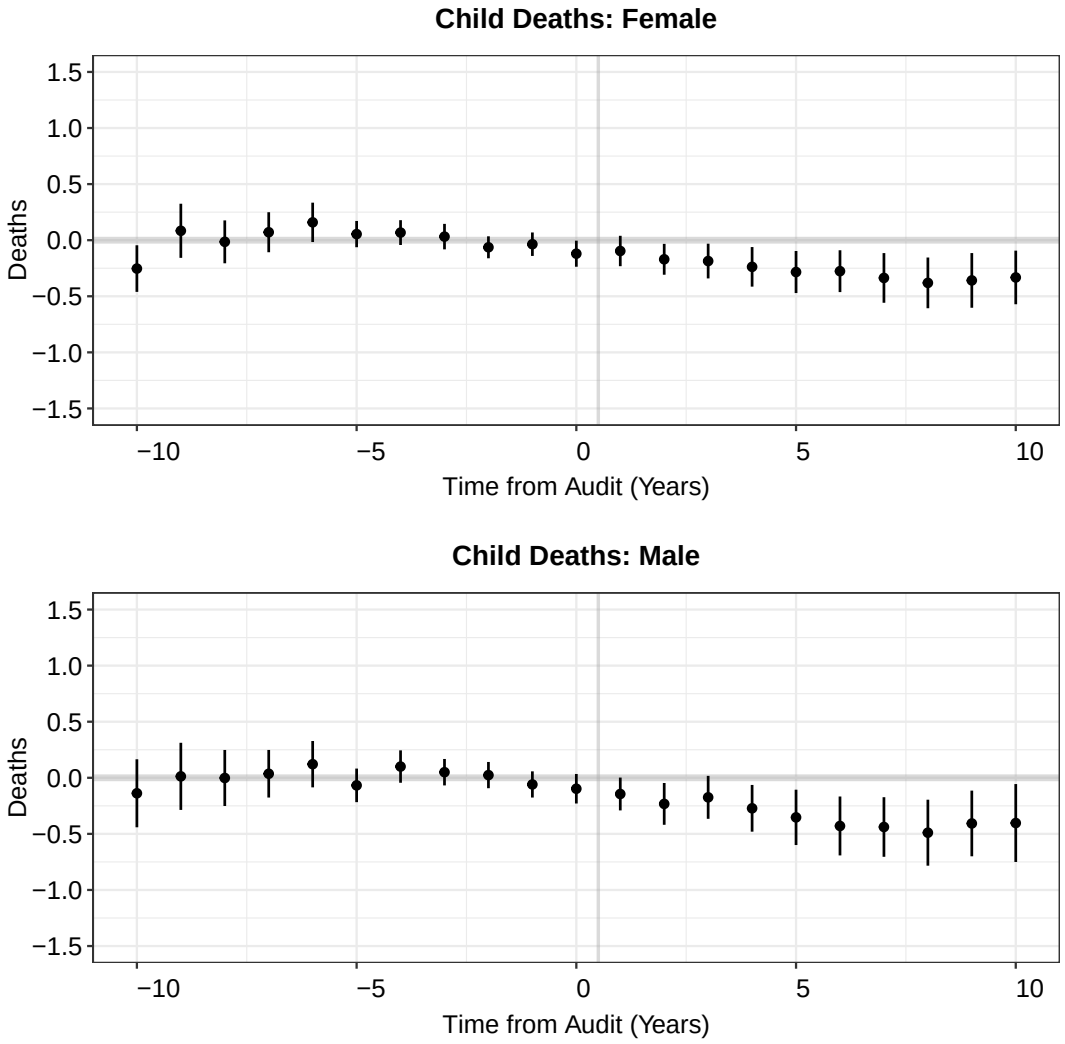
Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Block-bootstrapped standard errors allow for correlation of error terms at the municipality level.

Figure C3: Audit Effects on Infant Deaths over Time, by Cause of Death (Fixed Effects Counterfactual Estimator)



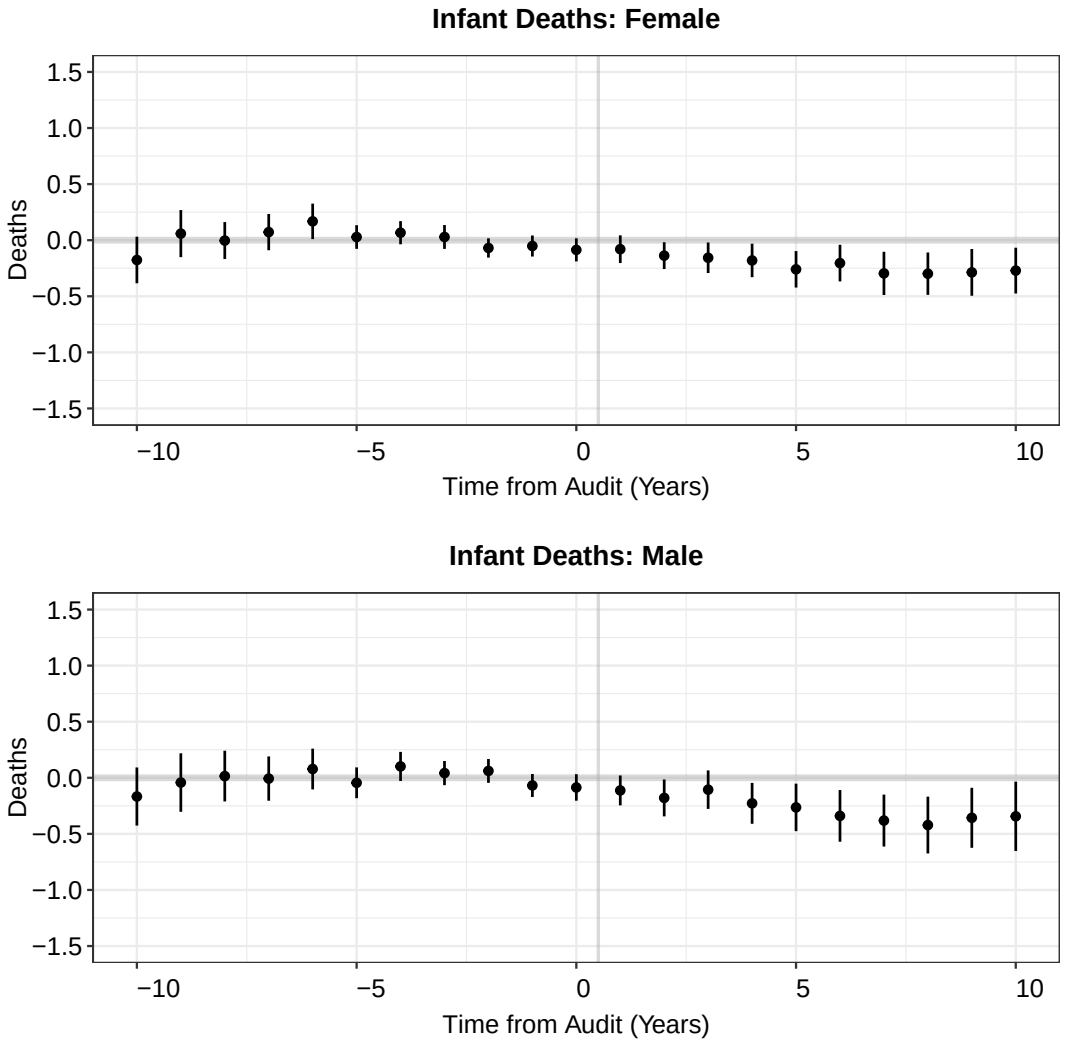
Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Block-bootstrapped standard errors allow for correlation of error terms at the municipality level.

Figure C4: Audit Effects on Child Mortality over Time, Female vs. Male (Fixed Effects Counterfactual Estimator)



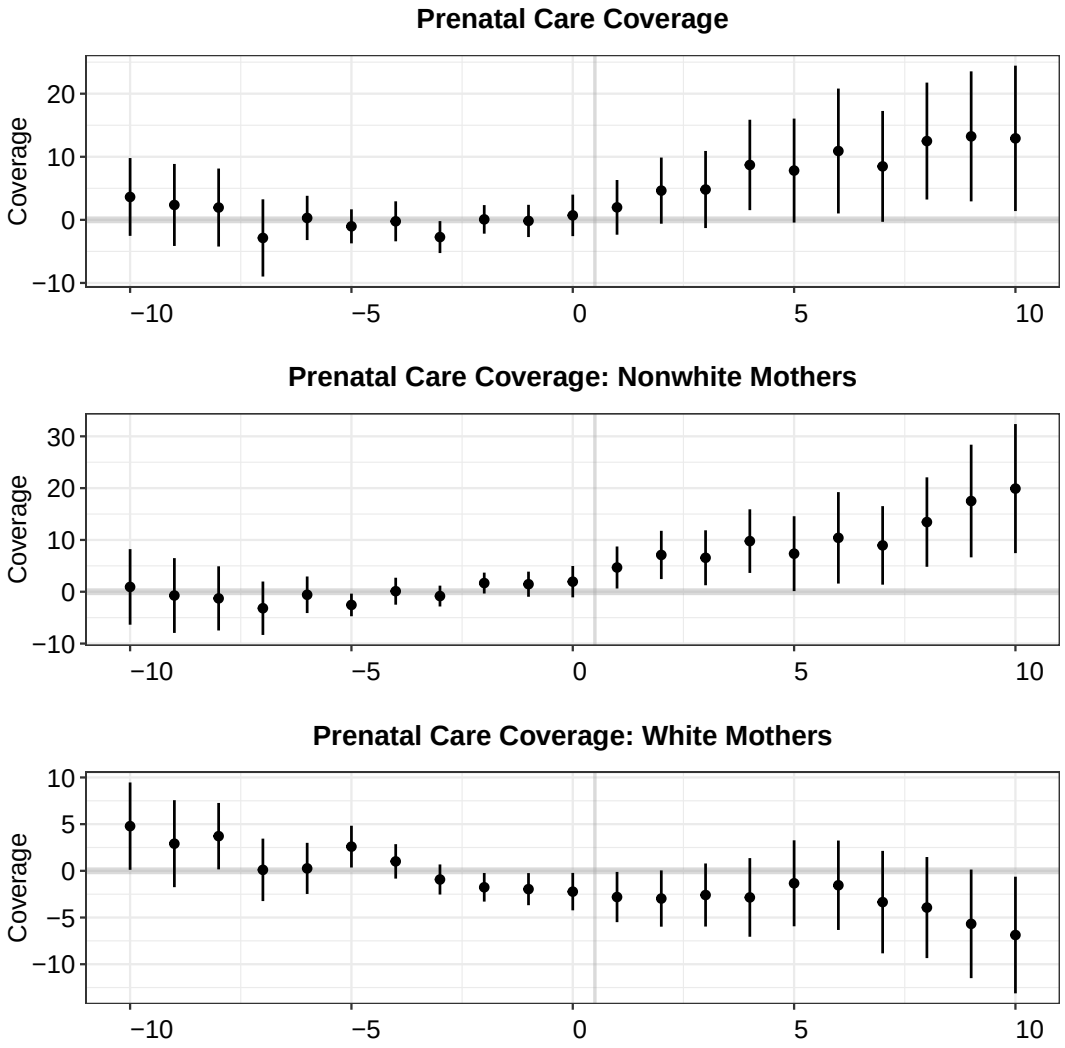
Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Block-bootstrapped standard errors allow for correlation of error terms at the municipality level.

Figure C5: Audit Effects on Infant Mortality over Time, Female vs. Male (Fixed Effects Counterfactual Estimator)



Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Block-bootstrapped standard errors allow for correlation of error terms at the municipality level.

Figure C6: Audit Effects on Prenatal Care Coverage over Time, Overall and by Mother's Race (Fixed Effects Counterfactual Estimator)



Notes: The figures report plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Block-bootstrapped standard errors allow for correlation of error terms at the municipality level.

Appendix D: Alternative Specifications

Table D1: Audit Effects on Early-Life Mortality and Prenatal Visits (Two-Way Fixed Effects Estimates)

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.53***	-0.43**	7.91*
	(0.16)	(0.14)	(3.22)
	[0.001]	[0.001]	[0.014]
Prevented Deaths	8,334	6,795	
	(3,445 – 13,223)	(2,622 – 10,969)	
Observed/Expected Mortality Ratio	0.934	0.937	
	(0.899 – 0.972)	(0.901 – 0.975)	
Nonwhite Mothers			
ATT	-0.21*	-0.15*	9.57**
	(0.08)	(0.07)	(3.04)
	[0.010]	[0.026]	[0.002]
Prevented Deaths	3,312	2,406	
	(783 – 5,841)	(288 – 4,524)	
Observed/Expected Mortality Ratio	0.949	0.955	
	(0.913 – 0.987)	(0.919 – 0.994)	
White Mothers			
ATT	-0.14	-0.11	-2.67
	(0.08)	(0.07)	(1.81)
	[0.065]	[0.087]	[0.141]
Prevented Deaths	2,187	1,755	
	(-138 – 4,512)	(-256 – 3,767)	
Observed/Expected Mortality Ratio	0.952	0.954	
	(0.905 – 1.003)	(0.907 – 1.007)	
Preventable Causes			
ATT	-0.32**	-0.28**	
	(0.12)	(0.10)	
	[0.006]	[0.008]	
Prevented Deaths	5,067	4,317	
	(1,140 – 8,694)	(1,107 – 7,527)	
Observed/Expected Mortality Ratio	0.940	0.942	
	(0.901 – 0.982)	(0.903 – 0.984)	
Non-Preventable Causes			
ATT	-0.01	-0.01	
	(0.04)	(0.03)	
	[0.780]	[0.839]	
Prevented Deaths	159	95	
	(-955 – 1,273)	(-824 – 1015)	
Observed/Expected Mortality Ratio	0.995	0.996	
	(0.964 – 1.029)	(0.964 – 1.031)	
Num. of municipalities	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370

Notes: Table shows results using two-way fixed effects estimations (with municipality and year fixed effects). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Table D2: Audit Effects on Early-Life Mortality and Prenatal Visits (Sun & Abraham Estimates)

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.48**	-0.34*	7.18*
	(0.17)	(0.14)	(3.04)
	[0.004]	[0.017]	[0.018]
Prevented Deaths	7,473	5,359	
	(2,369 – 12,576)	(959 – 9,759)	
Observed/Expected Mortality Ratio	0.940	0.949	
	(0.904 – 0.980)	(0.911 – 0.991)	
Nonwhite Mothers			
ATT	-0.28**	-0.18*	8.21**
	(0.10)	(0.09)	(2.82)
	[0.006]	[0.041]	[0.004]
Prevented Deaths	4,421	2,829	
	(1,265 – 7,577)	(122– 5535)	
Observed/Expected Mortality Ratio	0.933	0.948	
	(0.891 – 0.980)	(0.903 – 0.998)	
White Mothers			
ATT	-0.09	-0.05	-2.09
	(0.09)	(0.08)	(1.90)
	[0.301]	[0.468]	[0.272]
Prevented Deaths	1,382	857	
	(-1,236 – 4,000)	(-1,456 – 3,171)	
Observed/Expected Mortality Ratio	0.969	0.977	
	(0.915 – 1.030)	(0.920 – 1.041)	
Preventable Causes			
ATT	-0.27*	-0.20	
	(0.12)	(0.11)	
	[0.029]	[0.065]	
Prevented Deaths	4,159	3,171	
	(415 – 7,902)	(-196 – 6,539)	
Observed/Expected Mortality Ratio	0.950	0.957	
	(0.910 – 0.995)	(0.915 – 1.003)	
Non-Preventable Causes			
ATT	-0.06	-0.01	
	(0.05)	(0.04)	
	[0.209]	[0.716]	
Prevented Deaths	946	230	
	(-530 – 2,423)	(-1,007 – 1,467)	
Observed/Expected Mortality Ratio	0.973	0.992	
	(0.934 – 1.016)	(0.948 – 1.039)	
Num. of municipalities	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370

Notes: Table shows results using the Sun & Abraham (2021) method. The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets.. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Table D3: Audit Effects on Early-Life Mortality and Prenatal Visits
(Fixed Effects Counterfactual Estimator, Controlling for Births)

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.46**	-0.37**	9.79**
	(0.15)	(0.13)	(3.11)
	[0.002]	[0.003]	[0.002]
Prevented Deaths	7,202	5,786	
	(2,693 – 11,710)	(1,925 – 9,646)	
Observed/Expected Mortality Ratio	0.942	0.945	
	(0.910– 0.978)	(0.912– 0.981)	
Nonwhite Mothers			
ATT	-0.19*	-0.13	11.1***
	(0.08)	(0.07)	(3.05)
	[0.024]	[0.057]	[0.000]
Prevented Deaths	2,923	2,058	
	(383 – 5,463)	(-58 – 4,174)	
Observed/Expected Mortality Ratio	0.955	0.962	
	(0.919 – 0.994)	(0.925 – 1.001)	
White Mothers			
ATT	-0.10	-0.08	-2.67
	(0.07)	(0.06)	(1.70)
	[0.136]	[0.185]	[0.116]
Prevented Deaths	1,613	1,241	
	(-507 – 3,733)	(-596 – 3,078)	
Observed/Expected Mortality Ratio	0.964	0.967	
	(0.920 – 1.012)	(0.922 – 1.017)	
Preventable Causes			
ATT	-0.27*	-0.23*	
	(0.11)	(0.10)	
	[0.016]	[0.023]	
Prevented Deaths	4,297	3,583	
	(786 – 7,809)	(504 – 6,662)	
Observed/Expected Mortality Ratio	0.949	0.951	
	(0.910 – 0.990)	(0.913 – 0.993)	
Non-Preventable Causes			
ATT	0.01	0.01	
	(0.03)	(0.03)	
	[0.837]	[0.812]	
Prevented Deaths	-97	-95	
	(-1,027 – 833)	(-878 – 688)	
Observed/Expected Mortality Ratio	1.003	1.004	
	(0.976 – 1.031)	(0.975 – 1.034)	
Num. of municipalities	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370

Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual 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; p-values reported in brackets.. Rows 2 and 3 of each panel (respectively) 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 are reported in parentheses.

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

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.46**	-0.37**	9.43**
	(0.14)	(0.12)	(3.03)
Prevented Deaths	7,187	5,777	
	(2,747 – 11,627)	(2,010 – 9,543)	
Observed/Expected Mortality Ratio	0.943	0.946	
	(0.910 – 0.977)	(0.913 – 0.980)	
Nonwhite Mothers			
ATT	-0.19*	-0.13†	10.40***
	(0.08)	(0.07)	(3.00)
Prevented Deaths	2,946	2,102	
	(406 – 5,487)	(-34 – 4,240)	
Observed/Expected Mortality Ratio	0.954	0.961	
	(0.918 – 0.993)	(0.924 – 1.001)	
White Mothers			
ATT	-0.11	-0.09	-2.20
	(0.07)	(0.06)	(1.75)
Prevented Deaths	1,766	1,382	
	(-414 – 3,946)	(-501 – 3,265)	
Observed/Expected Mortality Ratio	0.961	0.964	
	(0.916 – 1.010)	(0.918 – 1.014)	
Preventable Causes			
ATT	-0.27*	-0.23*	
	(0.11)	(0.10)	
Prevented Deaths	4,281	3,602	
	(928 – 7,634)	(645 – 6,558)	
Observed/Expected Mortality Ratio	0.949	0.951	
	(0.912 – 0.988)	(0.914 – 0.991)	
Non-Preventable Causes			
ATT	0.01	0.01	
	(0.03)	(0.03)	
Prevented Deaths	-128	-142	
	(-1,101 – 845)	(-956 – 671)	
Observed/Expected Mortality Ratio	1.004	1.005	
	(0.976 – 1.033)	(0.976 – 1.037)	

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 audits' effect on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; † 10%, ∗ 5%, ∗∗ 1%, ∗ ∗ ∗ 0.1% significance levels. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

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

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.39*	-0.26*	9.06**
	(0.15)	(0.13)	(2.86)
	[0.012]	[0.047]	[0.002]
Prevented Deaths	6,051	4,097	
	(1,351 – 10,751)	(49 – 8,144)	
Observed/Expected Mortality Ratio	0.951	0.961	
	(0.916 – 0.989)	(0.925 – 1.000)	
Nonwhite Mothers			
ATT	-0.25*	-0.16	9.24***
	(0.10)	(0.09)	(2.80)
	[0.013]	[0.073]	[0.001]
Prevented Deaths	3,966	2,451	
	(843 – 7,088)	(-231 – 5,134)	
Observed/Expected Mortality Ratio	0.940	0.955	
	(0.897 – 0.987)	(0.910 – 1.005)	
White Mothers			
ATT	-0.05	-0.03	-1.51
	(0.08)	(0.07)	(1.83)
	[0.496]	[0.725]	[0.410]
Prevented Deaths	860	394	
	(-1,618 – 3,338)	(-1,797 – 2,585)	
Observed/Expected Mortality Ratio	0.980	0.989	
	(0.928 – 1.039)	(0.934 – 1.052)	
Preventable Causes			
ATT	-0.20	-0.15	
	(0.11)	(0.10)	
	[0.076]	[0.155]	
Prevented Deaths	3,185	2,285	
	(-331 – 6,701)	(-865 – 5,435)	
Observed/Expected Mortality Ratio	0.961	0.968	
	(0.922 – 1.004)	(0.928 – 1.013)	
Non-Preventable Causes			
ATT	-0.04	0.00	
	(0.05)	(0.04)	
	[0.402]	[0.915]	
Prevented Deaths	591	-65	
	(-791 – 1,973)	(-1,247 – 1,118)	
Observed/Expected Mortality Ratio	0.983	1.002	
	(0.946 – 1.024)	(0.960 – 1.048)	
Num. of municipalities	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370

Notes: Table shows results using the Sun & Abraham (2021) method, controlling for births. The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel report (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

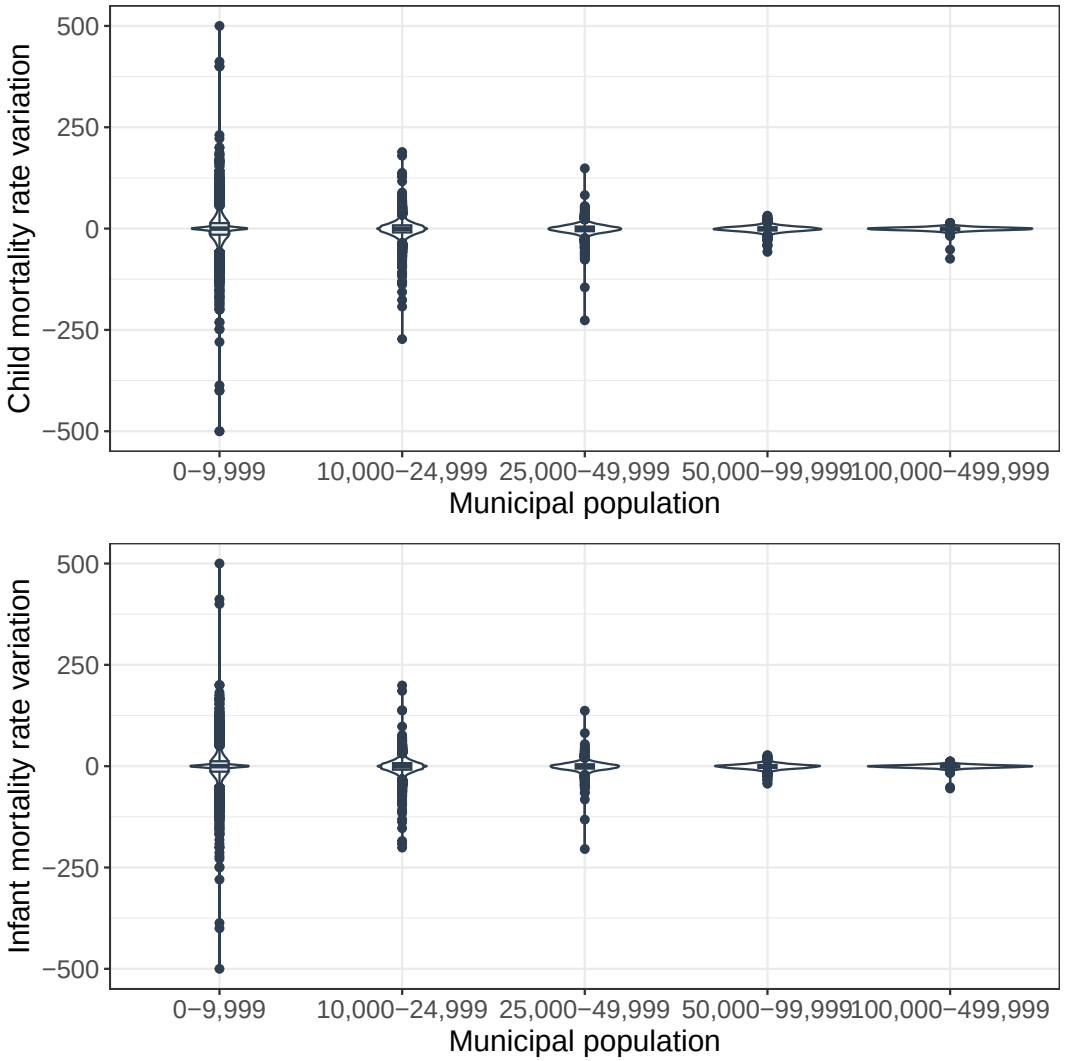
Table D6: Audit Effects on Fertility and Fetal Mortality Rates

	Dependent Variables:			
	Fetal Deaths	Fetal Mortality Rate	Births	Fertility Rate
	(1)	(2)	(3)	(4)
Overall Population				
ATT	-0.11	0.27	-4.09	-0.78*
	(0.09)	(0.19)	(2.85)	(0.31)
	[0.229]	[0.161]	[0.152]	[0.011]
Nonwhite Mothers				
ATT			4.16	
			(3.83)	
			[0.277]	
White Mothers				
ATT			-8.56**	
			(3.30)	
			[0.010]	
Num. of municipalities	5,203	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370	3,370

Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference (as presented in the Methods section). Each reports the ATT estimates of audits' effect on each outcome. Column 1 reports effects on the count of fetal deaths. Column 2 reports effects on the fetal mortality rate (i.e., fetal deaths per 1,000 births). Column 3 reports effects on the count of births. Column 4 reports effects on the fertility rate (i.e., births per 1,000 women aged 15-49 years). Fertility rate is not disaggregated by race due to data constraints (annual population estimates for women of reproductive age by race are unavailable). Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets.

Appendix E: Audit Effects on Mortality Rates

Figure E1: Annual Variation in Mortality Rates, by Population Strata (2000-2015)



Notes: This figure shows the year-to-year fluctuation in infant and child mortality rates (per 1,000 births) for Brazil's municipalities between 2000-2015, by population strata. These rates fluctuate substantially in municipalities with populations below 10,000.

Table E1: Audit Effects on Early-Life Mortality Rates
(Fixed Effects Counterfactual Estimator)

	Dependent Variables: Child Mortality Rate	Infant Mortality Rate
	(1)	(2)
Overall Population		
ATT	-0.55	-0.47
	(0.29)	(0.27)
	[0.058]	[0.080]
Prevented Deaths	3,723	3,170
	(-123 – 7,569)	(-379 – 6,719)
Observed/Expected Mortality Ratio	0.969	0.969
	(0.940 – 1.001)	(0.937 – 1.004)
Nonwhite Mothers		
ATT	-0.24	-0.16
	(0.16)	(0.14)
	[0.116]	[0.240]
Prevented Deaths	1,643	1,101
	(-404 – 3,690)	(-734 – 2,935)
Observed/Expected Mortality Ratio	0.974	0.979
	(0.944 – 1.007)	(0.946 – 1.014)
White Mothers		
ATT	0.10	0.11
	(0.15)	(0.14)
	[0.490]	[0.425]
Prevented Deaths	-682	-754
	(-2,617 – 1,254)	(-2,608 – 1,099)
Observed/Expected Mortality Ratio	1.016	1.021
	(0.972 – 1.065)	(0.971 – 1.077)
Preventable Causes		
ATT	-0.23	-0.19
	(0.23)	(0.22)
	[0.317]	[0.389]
Prevented Deaths	1,547	1,276
	(-1,481 – 4,575)	(-1,630 – 4,181)
Observed/Expected Mortality Ratio	0.981	0.982
	(0.946 – 1.019)	(0.944 – 1.024)
Non-Preventable Causes		
ATT	0.03	-0.01
	(0.11)	(0.10)
	[0.781]	[0.895]
Prevented Deaths	-214	86
	(-1,726 – 1,298)	(-1,188 – 1,360)
Observed/Expected Mortality Ratio	1.006	0.997
	(0.964 – 1.053)	(0.952 – 1.046)
Num. of municipalities	5,203	5,203
Num. of audited municipalities	1,833	1,833
Num. of unaudited municipalities	3,370	3,370

Notes: This table employs alternative dependent variables: infant and child mortality rates (per 1,000 births) — instead of counts. As shown in Figure E1, these rates fluctuate substantially in municipalities with populations below 10,000. Specifications employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference (as presented in the Methods section). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

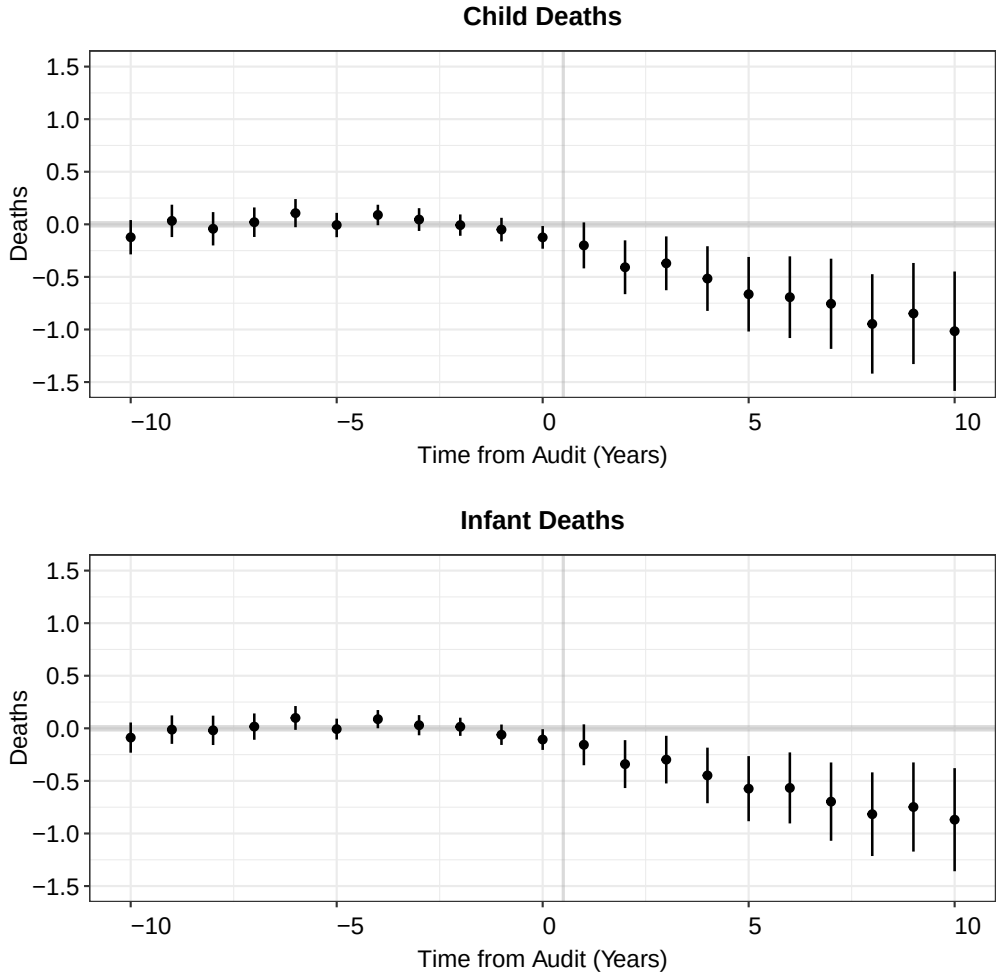
Table E2: Audit Effects on Early-Life Mortality Rates (Fixed Effects Counterfactual Estimator, Excludes Municipalities with <10,000 Citizens)

	Dependent Variables: Child Mortality Rate	Infant Mortality Rate
	(1)	(2)
Overall Population		
ATT	-0.70**	-0.62**
	(0.26)	(0.24)
	[0.008]	[0.008]
Prevented Deaths	4,363	3,878
	(1,156 – 7,570)	(997 – 6,760)
Observed/Expected Mortality Ratio	0.961	0.960
	(0.935 – 0.989)	(0.932 – 0.989)
Nonwhite Mothers		
ATT	-0.51**	-0.41**
	(0.18)	(0.15)
	[0.004]	[0.005]
Prevented Deaths	3,185	2,538
	(1,036 – 5,335)	(750 – 4,326)
Observed/Expected Mortality Ratio	0.947	0.949
	(0.914 – 0.982)	(0.917 – 0.985)
White Mothers		
ATT	-0.00	0.01
	(0.14)	(0.13)
	[0.999]	[0.967]
Prevented Deaths	0	-33
	(-1,686 – 1,687)	(-1,558 – 1,493)
Observed/Expected Mortality Ratio	1.000	1.001
	(0.959 – 1.045)	(0.958 – 1.049)
Preventable Causes		
ATT	-0.33	-0.29
	(0.20)	(0.18)
	[0.096]	[0.101]
Prevented Deaths	2,022	1,810
	(-356 – 4,399)	(-355 – 3,974)
Observed/Expected Mortality Ratio	0.973	0.973
	(0.943 – 1.005)	(0.942 – 1.006)
Non-Preventable Causes		
ATT	0.02	-0.02
	(0.09)	(0.08)
	[0.804]	[0.770]
Prevented Deaths	-137	143
	(-1,217 – 943)	(-818 – 1,104)
Observed/Expected Mortality Ratio	1.004	0.994
	(0.971 – 1.040)	(0.957 – 1.034)
Num. of municipalities	2,821	2,821
Num. of audited municipalities	1,088	1,088
Num. of unaudited municipalities	1,733	1,733

Notes: This table employs alternative dependent variables: infant and child mortality rates (per 1,000 births) — instead of counts. Unlike Table E1, this table excludes municipalities with populations under 10,000, given that their rates fluctuate substantially (see Figure E1). Specifications employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference (as presented in the Methods section). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

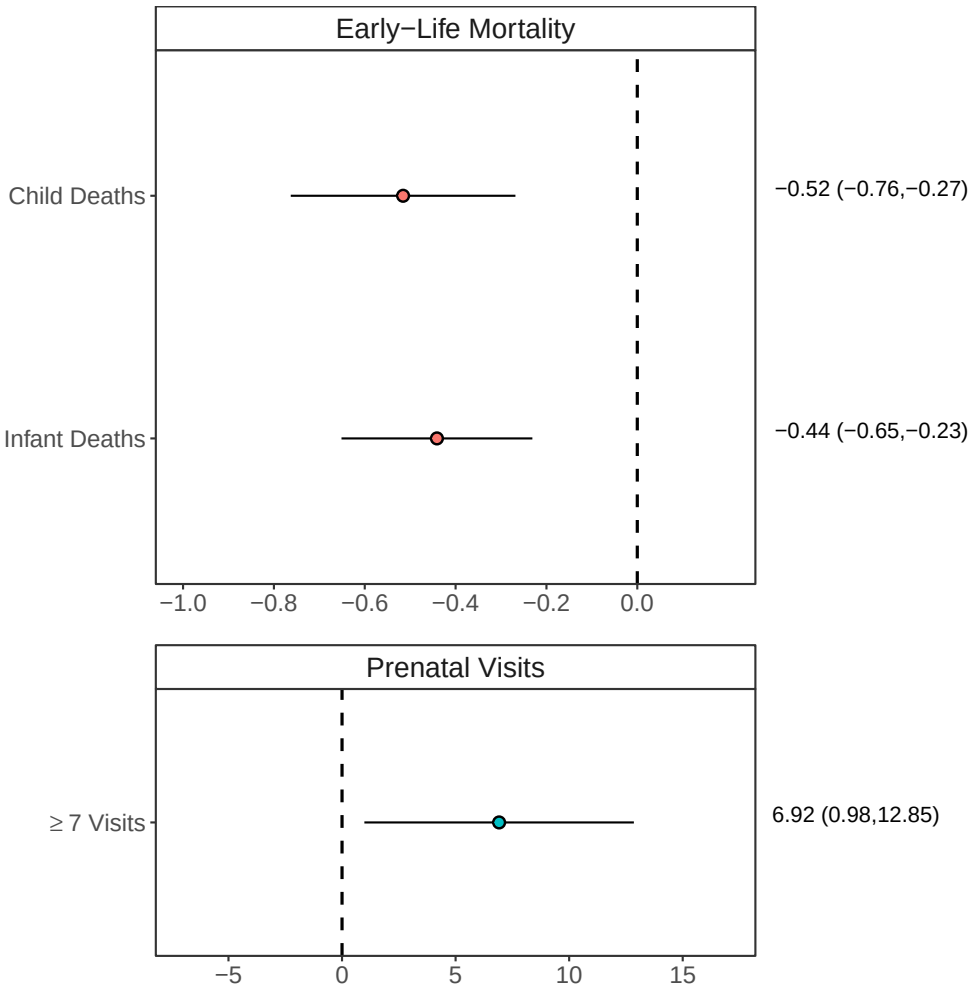
Appendix F: Robustness to Matrix Completion Estimator

Figure F1: Audit Effects on Early-Life Mortality over Time (MC Estimator)



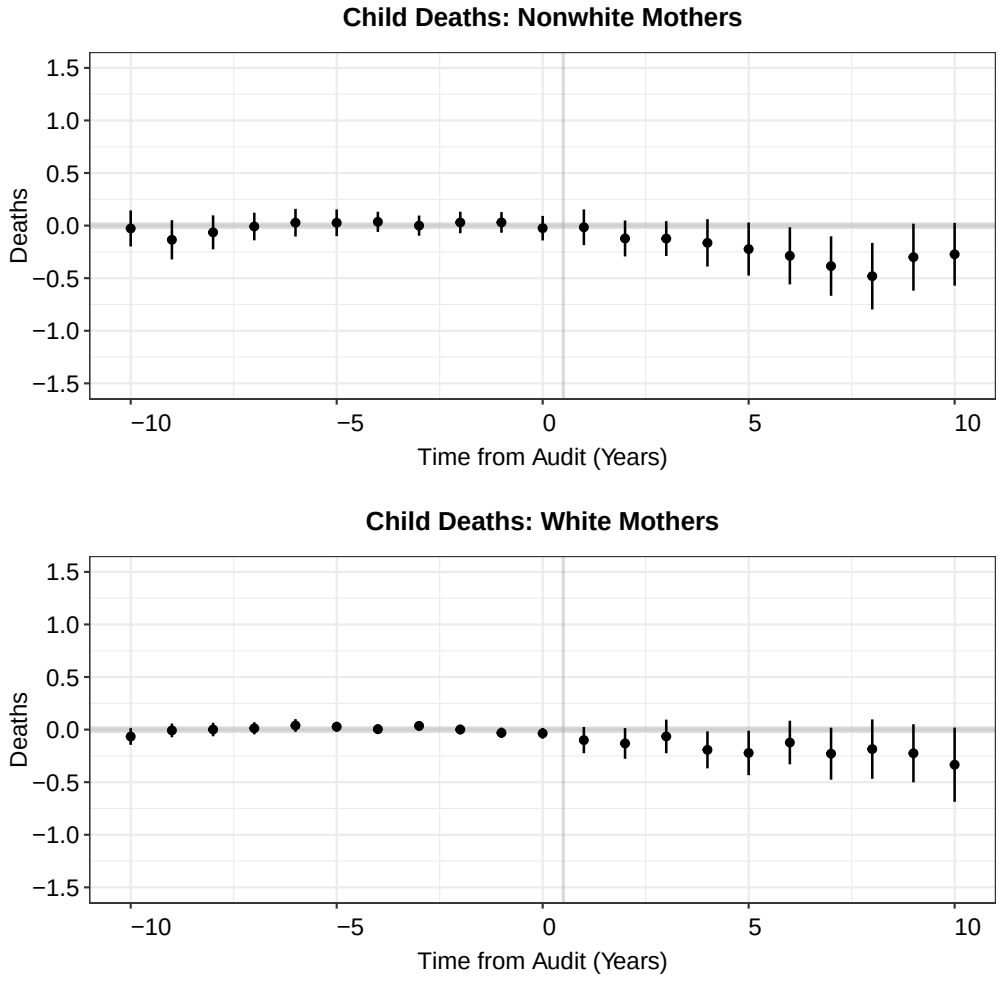
Notes: The figures report plots of dynamic treatment effects specifications that employ the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022). The MC estimator allows event study models under staggered treatment assignment to adjust flexibly for potential pre-trends. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Figure F2: Audit Effects on Early-Life Mortality (MC Estimator)



Notes: This figure reports plots of the overall Average Treatment Effect on the Treated, employing the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022). The MC estimator allows event study models under staggered treatment assignment to adjust flexibly for potential pre-trends. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Figure F3: Audit Effects on Child Mortality over Time, by Race of Mother (MC Estimator)



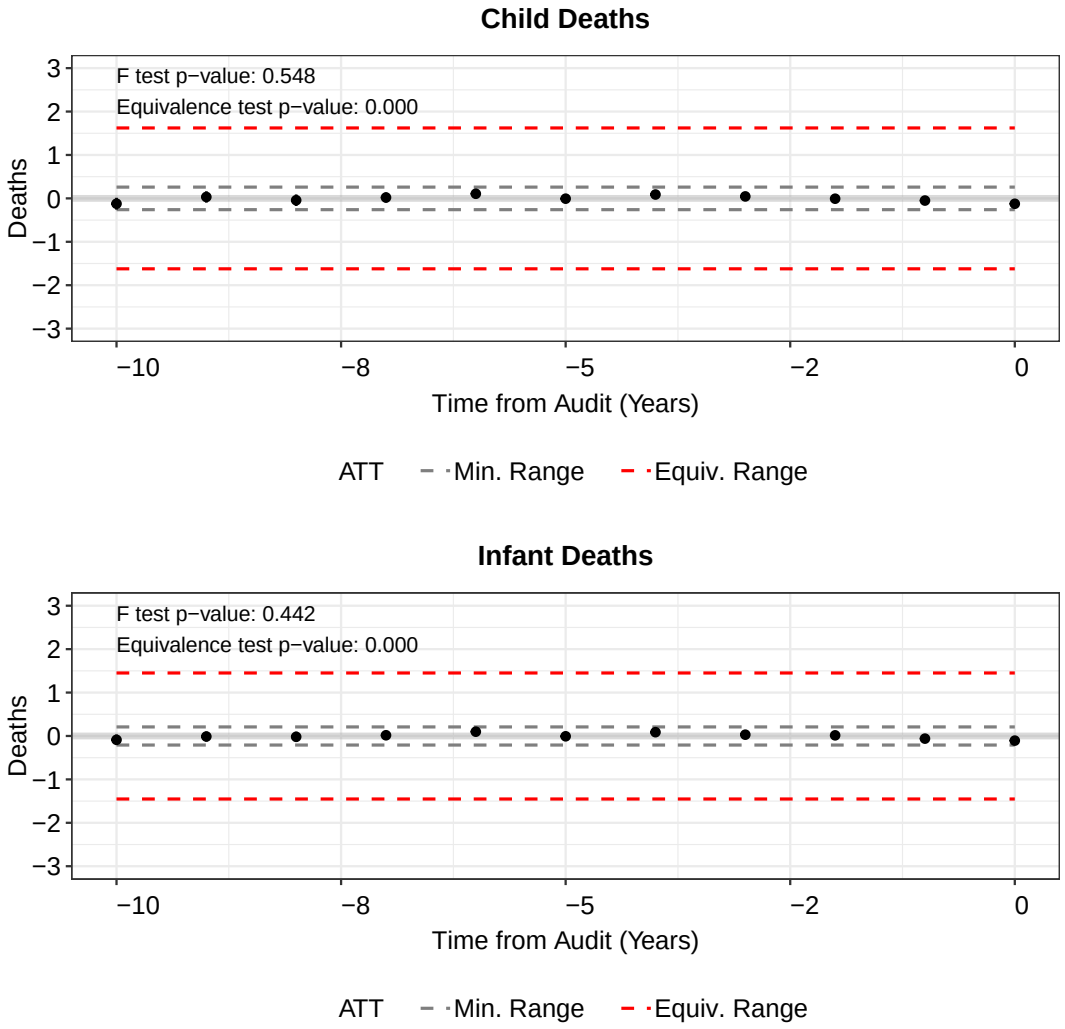
Notes: The figures report plots of dynamic treatment effects specifications that employ the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022). The MC estimator allows event study models under staggered treatment assignment to adjust flexibly for potential pre-trends. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Table F1: Audit Effects on Early-Life Mortality and Prenatal Care Coverage (MC Estimator)

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.52***	-0.44***	6.92*
	(0.13)	(0.11)	(3.03)
	[0.000]	[0.000]	[0.022]
Prevented Deaths	8,071	6,905	
	(4,200 – 11,942)	(3,616 – 10,194)	
Observed/Expected Mortality Ratio	0.936	0.936	
	(0.908 – 0.966)	(0.908 – 0.965)	
Nonwhite Mothers			
ATT	-0.20*	-0.16*	8.63**
	(0.08)	(0.07)	(2.77)
	[0.019]	[0.029]	[0.002]
Prevented Deaths	3,085	2,441	
	(506 – 5,664)	(249 – 4,634)	
Observed/Expected Mortality Ratio	0.952	0.955	
	(0.916 – 0.992)	(0.918 – 0.995)	
White Mothers			
ATT	-0.14	-0.11	-2.75
	(0.07)	(0.07)	(1.93)
	[0.055]	[0.083]	[0.154]
Prevented Deaths	2,220	1,772	
	(-44 – 4,484)	(-232 – 3,776)	
Observed/Expected Mortality Ratio	0.951	0.954	
	(0.906 – 1.001)	(0.906 – 1.006)	
Preventable Causes			
ATT	-0.35***	-0.31***	
	(0.09)	(0.08)	
	[0.000]	[0.000]	
Prevented Deaths	5,422	4,928	
	(2,543 – 8,302)	(2,330 – 7,525)	
Observed/Expected Mortality Ratio	0.936	0.934	
	(0.905 – 0.969)	(0.903 – 0.968)	
Non-Preventable Causes			
ATT	-0.04	-0.03	
	(0.04)	(0.03)	
	[0.208]	[0.277]	
Prevented Deaths	696	494	
	(-389 – 1,781)	(-396 – 1,383)	
Observed/Expected Mortality Ratio	0.980	0.982	
	(0.951 – 1.011)	(0.951 – 1.015)	
Num. of municipalities	4,927	4,927	4,927
Num. of audited municipalities	1,557	1,557	1,557
Num. of unaudited municipalities	3,370	3,370	3,370

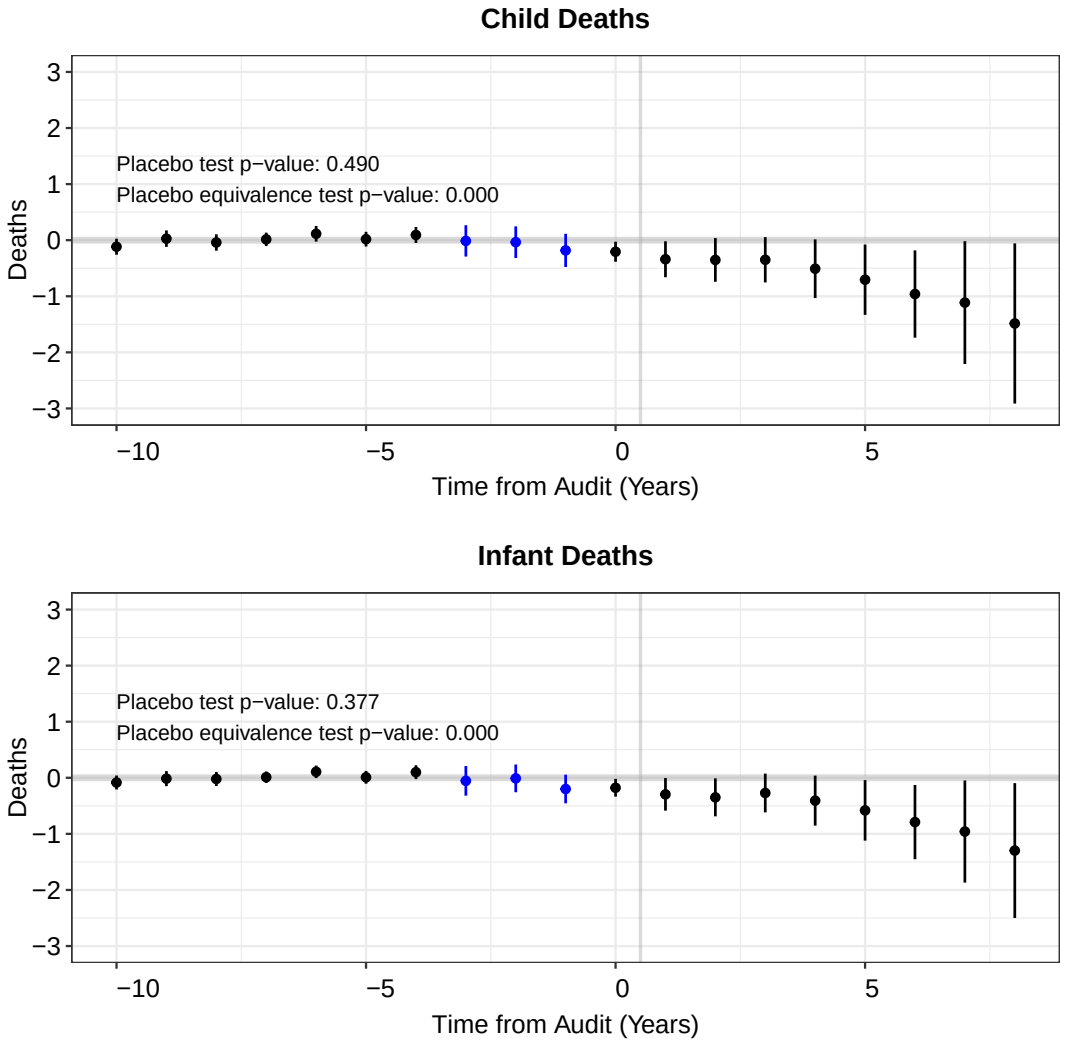
Notes: These specifications employ the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022). The MC estimator allows event study models under staggered treatment assignment to adjust flexibly for potential pre-trends. The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Figure F4: Pre-Trend Tests for Audit Effects on Early-Life Mortality over Time (Fig. F1, MC Estimator)



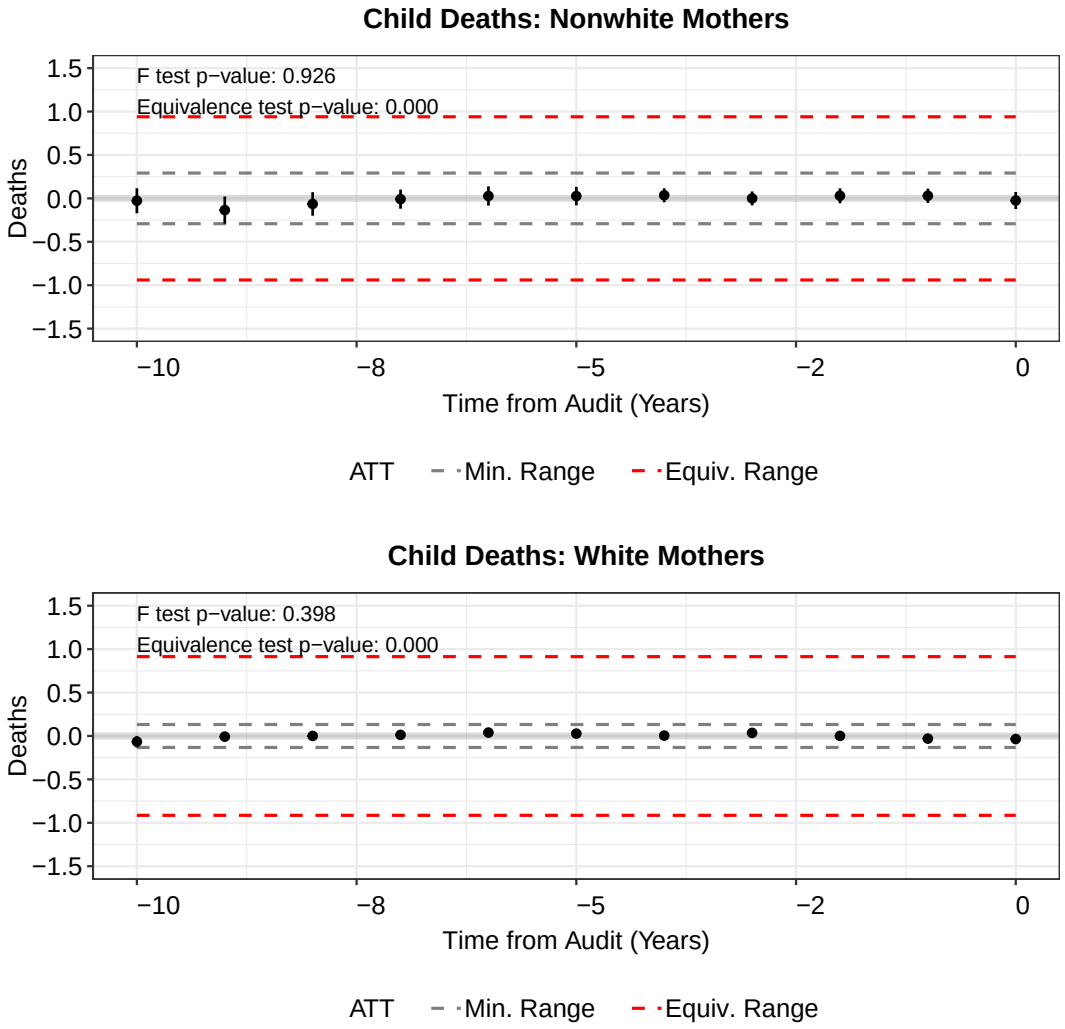
Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022), for child deaths and infant deaths, respectively. In addition, we report the p-value of a joint test of the significance of pre-treatment differences and p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure F5: Placebo Tests for Audit Effects on Early-Life Mortality over Time (Fig. F1, MC Estimator)



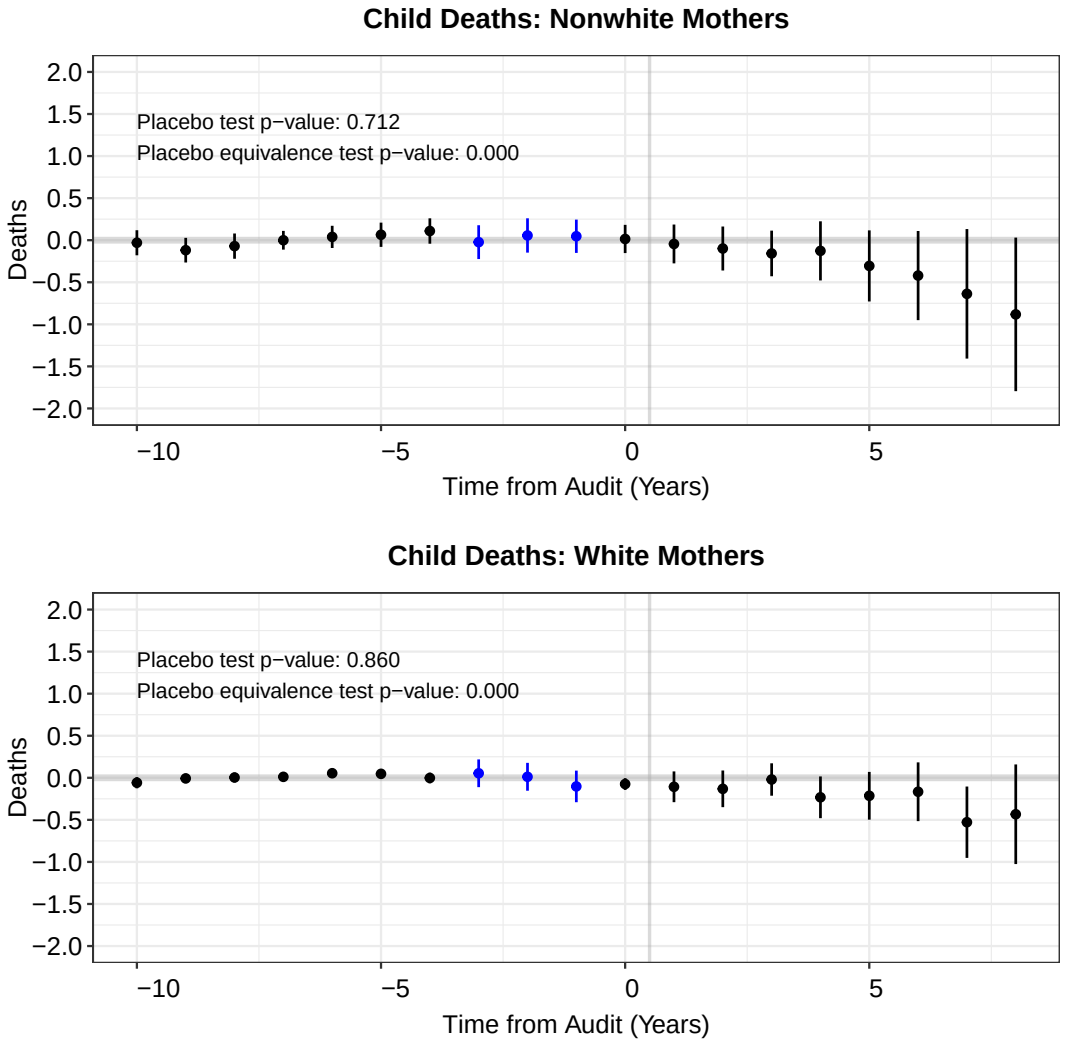
Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022), for child deaths and infant deaths respectively. In addition, we report the p-value of a joint test of the significance of these placebo differences and the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure F6: Pre-Trend Tests for Audit Effects on Early-Life Mortality over Time, by Race of Mother (Fig. F3, MC Estimator)



Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022), for child deaths and infant deaths, respectively. In addition, we report the p-value of a joint test of the significance of pre-treatment differences and p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure F7: Placebo Tests for Audit Effects on Early-Life Mortality over Time, by Race of Mother (Fig. F3, MC Estimator)



Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022), for child deaths and infant deaths respectively. In addition, we report the p-value of a joint test of the significance of these placebo differences, as well as the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Table F2: Additional Pre-trends and Placebo Checks (MC Estimator)

	Pre-trend Tests:		Placebo Tests:	
	F-Test P-Value	Equivalence Test P-Value	T-Test P-Value	Equivalence Test P-Value
Child Deaths				
Overall	.548	.000	.490	.000
Nonwhite Mothers	.926	.000	.712	.000
White Mothers	.398	.000	.860	.000
Preventable	.530	.000	.239	.000
Non-Preventable	.542	.000	.179	.000
Infant Deaths				
Overall	.442	.000	.377	.000
Nonwhite Mothers	.934	.000	.889	.000
White Mothers	.280	.000	.991	.000
Preventable	.627	.000	.208	.000
Non-Preventable	.694	.000	.359	.000
Prenatal Care Coverage				
Overall	.286	.000	.759	.000
Nonwhite Mothers	.038	.000	.479	.000
White Mothers	.107	.000	.692	.000

Notes: Column 1 of the table reports p-values of joint tests of significance of pre-treatment differences of the dynamic treatment effects based on the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022), for child deaths, infant deaths, and prenatal coverage, overall and by subgroup, respectively. Column 2 reports analogous p-values of an equivalence test that such pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. Column 3 reports p-values of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from the matrix completion (MC) method, for the same outcomes. Column 4 reports p-values of analogous equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Table F3: Audit Effects on Early-Life Mortality Rate (MC Estimator)

	Dependent Variables:	
	Child Mortality Rate	Infant Mortality Rate
	(1)	(2)
Overall Population		
ATT	-0.59*	-0.53*
	(0.29)	(0.26)
	[0.044]	[0.043]
Prevented Deaths	3,934	3,593
	(106 – 7,762)	(115 – 7,071)
Observed/Expected Mortality Ratio	0.968	0.965
	(0.938 – 0.999)	(0.934 – 0.999)
Nonwhite Mothers		
ATT	-0.14	-0.11
	(0.18)	(0.16)
	[0.422]	[0.466]
Prevented Deaths	970	764
	(-1,397 – 3,337)	(-1,290 – 2,817)
Observed/Expected Mortality Ratio	0.985	0.985
	(0.949 – 1.023)	(0.948 – 1.027)
White Mothers		
ATT	0.06	0.09
	(0.17)	(0.15)
	[0.721]	[0.549]
Prevented Deaths	-400	-621
	(-2,598 – 1,798)	(-2,650 – 1,408)
Observed/Expected Mortality Ratio	1.009	1.017
	(0.960 – 1.064)	(0.963 – 1.078)
Preventable Causes		
ATT	-0.33	-0.28
	(0.23)	(0.22)
	[0.158]	[0.203]
Prevented Deaths	2,224	1,914
	(-864 – 5,312)	(-1,035 – 4,864)
Observed/Expected Mortality Ratio	0.973	0.973
	(0.937 – 1.011)	(0.935 – 1.015)
Non-Preventable Causes		
ATT	-0.02	-0.08
	(0.12)	(0.11)
	[0.875]	[0.462]
Prevented Deaths	127	522
	(-1,450 – 1,703)	(-870 – 1,914)
Observed/Expected Mortality Ratio	0.996	0.981
	(0.953 – 1.044)	(0.934 – 1.033)
Num. of municipalities	4,927	4,927
Num. of audited municipalities	1,557	1,557
Num. of unaudited municipalities	3,370	3,370

Notes: This table employs alternative dependent variables: infant and child mortality rates (per 1,000 births) — instead of counts. As shown in Figure E1, these rates fluctuate substantially in municipalities with populations below 10,000. Specifications employ the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Table F4: Audit Effects on Early-Life Mortality Rate (MC Estimator, Excludes Municipalities with <10,000 Citizens)

	Dependent Variables: Child Mortality Rate	Infant Mortality Rate
	(1)	(2)
Overall Population		
ATT	-0.65*	-0.61*
	(0.27)	(0.25)
	[0.018]	[0.014]
Prevented Deaths	4,039	3,779
	(693 – 7,386)	(780 – 6,776)
Observed/Expected Mortality Ratio	0.964	0.961
	(0.936 – 0.994)	(0.932 – 0.992)
Nonwhite Mothers		
ATT	-0.43**	-0.37*
	(0.17)	(0.14)
	[0.010]	[0.011]
Prevented Deaths	2,668	2,294
	(644 – 4,692)	(534 – 4,053)
Observed/Expected Mortality Ratio	0.955	0.954
	(0.924 – 0.989)	(0.922 – 0.989)
White Mothers		
ATT	0.00	0.01
	(0.14)	(0.13)
	[0.998]	[0.934]
Prevented Deaths	-3	-67
	(-1,705 – 1,700)	(-1,650 – 1,516)
Observed/Expected Mortality Ratio	1.000	1.002
	(0.959 – 1.045)	(0.957 – 1.052)
Preventable Causes		
ATT	-0.39	-0.37*
	(0.20)	(0.19)
	[0.052]	[0.048]
Prevented Deaths	2,410	2,309
	(-23 – 4,843)	(21 – 4,597)
Observed/Expected Mortality Ratio	0.968	0.965
	(0.938 – 1.000)	(0.933 – 1.000)
Non-Preventable Causes		
ATT	0.02	-0.02
	(0.10)	(0.09)
	[0.856]	[0.788]
Prevented Deaths	-110	142
	(-1,291 – 1,072)	(-897 – 1,182)
Observed/Expected Mortality Ratio	1.003	0.994
	(0.967 – 1.043)	(0.955 – 1.038)
Num. of municipalities	2,647	2,647
Num. of audited municipalities	914	914
Num. of unaudited municipalities	1,733	1,733

Notes: This table employs alternative dependent variables: infant and child mortality rates (per 1,000 births) — instead of counts. Unlike Table F3, this table excludes municipalities with populations under 10,000, given that their rates fluctuate substantially (see Figure E1). Specifications employ the matrix completion (MC) method developed by Athey et al. (2021) and Liu et al. (2022). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Appendix G: Results for Neonatal Mortality

Table G1: Descriptive Statistics - Neonatal Deaths

	All Municipalities, Annual 2000-2015	All Municipalities, Annual 2000-2002	Pre-Intervention Difference, Annual 2000-2002
	(1)	(2)	(3)
Neonatal Deaths			
Overall	4.4	5.4	0.4
	[8.8]	[11.1]	(0.3)
			[0.580]
White	1.7	2.2	0.1
	[4.3]	[5.4]	(0.1)
			[0.477]
Nonwhite	1.8	1.6	0.0
	[4.7]	[4.5]	(0.1)
			[0.833]
Preventable	3.3	4.1	0.1
	[7.0]	[8.9]	(0.2)
			[0.600]
Non-Preventable	0.9	1.0	0.0
	[1.9]	[2.2]	(0.1)
			[0.853]
Num. of municipalities	5203	5203	5203
Num. of observations	83248	15609	15609

Notes: Columns 1 and 2 present the mean of each variable and its standard deviation in brackets, the first column for the overall period 2000-2015, and the second for the baseline period preceding the start of the audit program (2000-02). 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%, $**$ 1%, $***$ 0.1% significance levels. As with all analyses in this study, the sample excludes municipalities with populations above 500,000, which were ineligible for the CGU's randomized anti-corruption audits.

Table G2: Audit Effects on Neonatal Mortality
(Fixed Effects Counterfactual Estimator)

	Dependent Variable: Neonatal Deaths
	(1)
Overall Population	
ATT	-0.18*
	(0.09)
	[0.043]
Prevented Deaths	2,842
	(86 – 5,599)
Observed/Expected Mortality Ratio	0.960
	(0.925 – 0.999)
Nonwhite Mothers	
ATT	-0.01
	(0.05)
	[0.799]
Prevented Deaths	212
	(-1,415 – 1,838)
Observed/Expected Mortality Ratio	0.994
	(0.951 – 1.041)
White Mothers	
ATT	-0.04
	(0.04)
	[0.354]
Prevented Deaths	622
	(-693 – 1,937)
Observed/Expected Mortality Ratio	0.975
	(0.926 – 1.029)
Preventable Causes	
ATT	-0.12
	(0.08)
	[0.127]
Prevented Deaths	1,859
	(-526 – 4,244)
Observed/Expected Mortality Ratio	0.966
	(0.925 – 1.010)
Non-Preventable Causes	
ATT	-0.01
	(0.02)
	[0.481]
Prevented Deaths	208
	(-370 – 785)
Observed/Expected Mortality Ratio	0.986
	(0.948 – 1.027)
Num. of municipalities	5,203
Num. of audited municipalities	1,833
Num. of unaudited municipalities	3,370

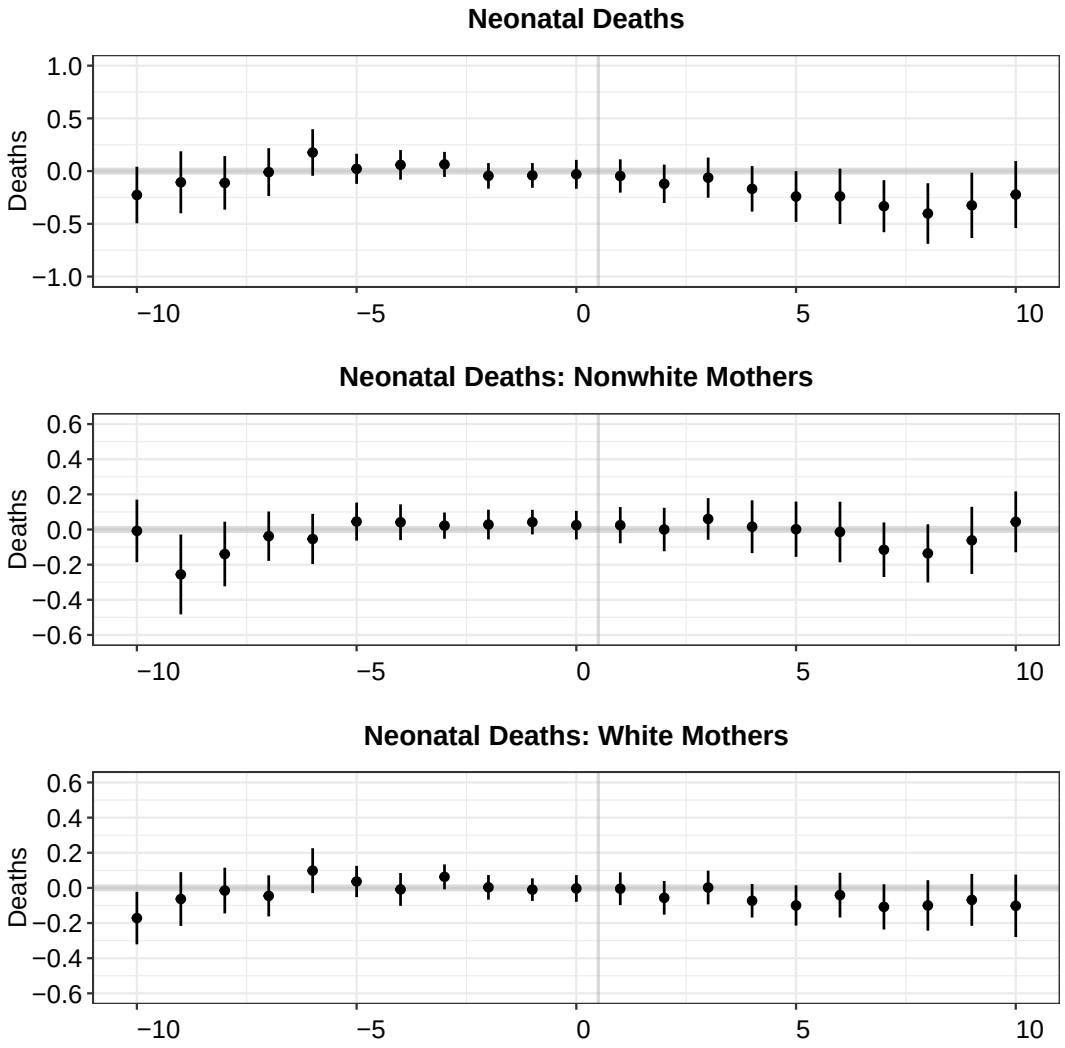
Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual method. The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest; standard errors clustered at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Table G3: Audit Effects on Neonatal Mortality
(Sun & Abraham Estimates)

	Dependent Variable: Neonatal Deaths
	(1)
Overall Population	
ATT	-0.14
	(0.10)
	[0.131]
Prevented Deaths	2,253
	(-673 – 5,179)
Observed/Expected Mortality Ratio	0.968
	(0.930 – 1.010)
Nonwhite Mothers	
ATT	-0.06
	(0.06)
	[0.349]
Prevented Deaths	935
	(-1,021 – 2,891)
Observed/Expected Mortality Ratio	0.974
	(0.925 – 1.029)
White Mothers	
ATT	-0.03
	(0.05)
	[0.603]
Prevented Deaths	436
	(-1,204 – 2,076)
Observed/Expected Mortality Ratio	0.982
	(0.921 – 1.052)
Preventable Causes	
ATT	-0.09
	(0.08)
	[0.273]
Prevented Deaths	1,377
	(-1,084 – 3,839)
Observed/Expected Mortality Ratio	0.974
	(0.932 – 1.021)
Non-Preventable Causes	
ATT	-0.01
	(0.03)
	[0.799]
Prevented Deaths	112
	(-747 – 970)
Observed/Expected Mortality Ratio	0.992
	(0.936 – 1.055)
Num. of municipalities	5,203
Num. of audited municipalities	1,833
Num. of unaudited municipalities	3,370

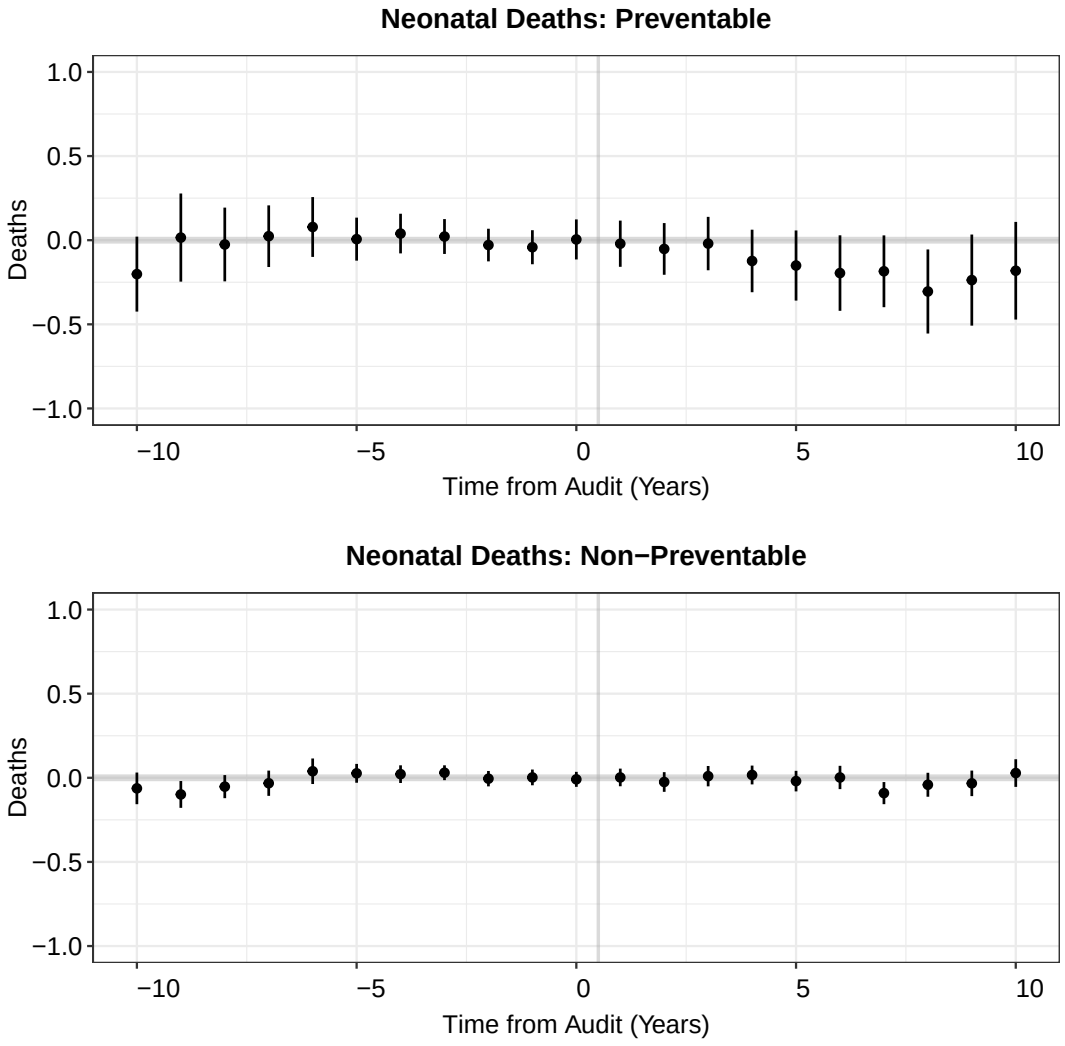
Notes: The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest, using Sun & Abraham's (2021) method. Standard errors clustered at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.

Figure G1: Audit Effects on Neonatal Mortality over Time, Overall and by Mother's Race (Fixed Effects Counterfactual Estimator)



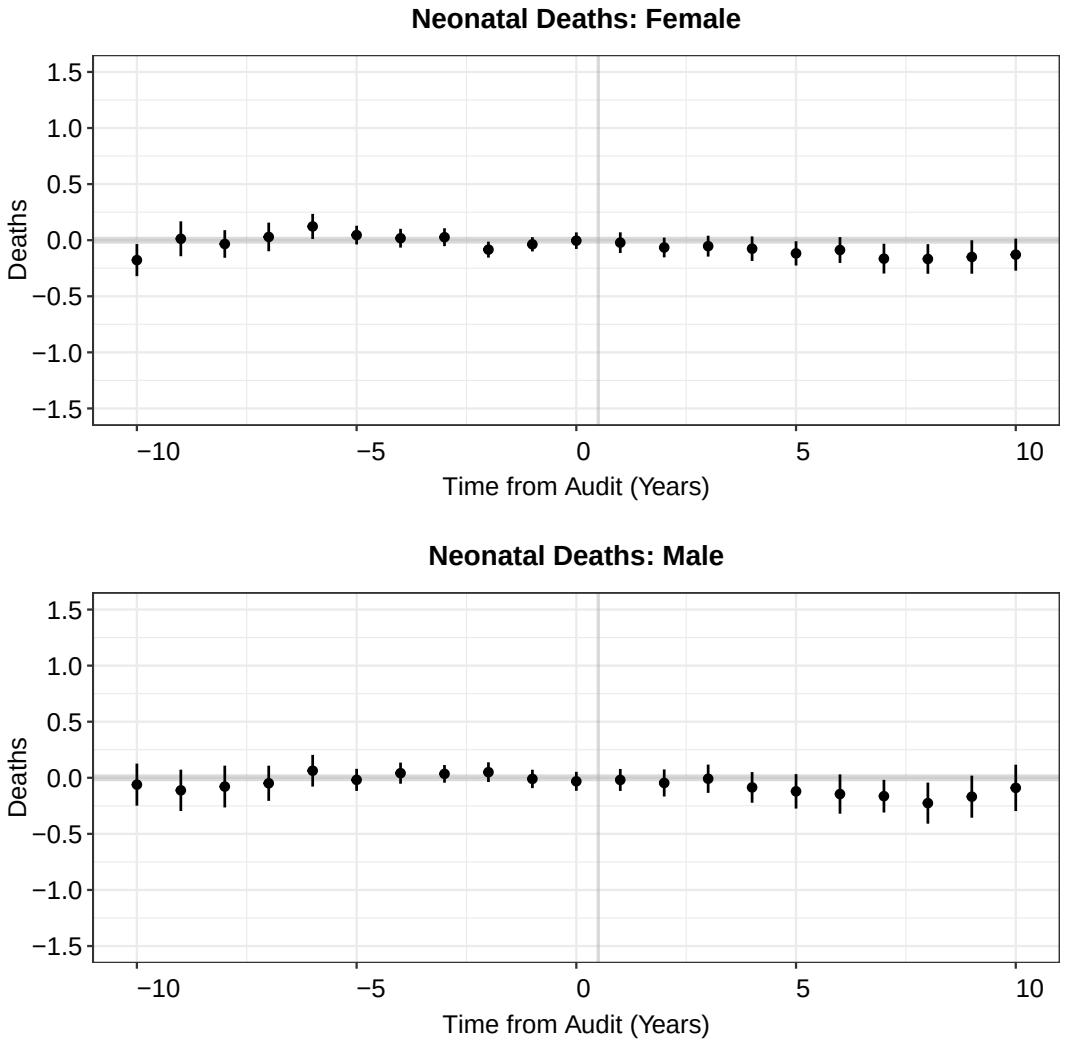
Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual method. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Figure G2: Audit Effects on Neonatal Deaths over Time, by Cause of Death (Fixed Effects Counterfactual Estimator)



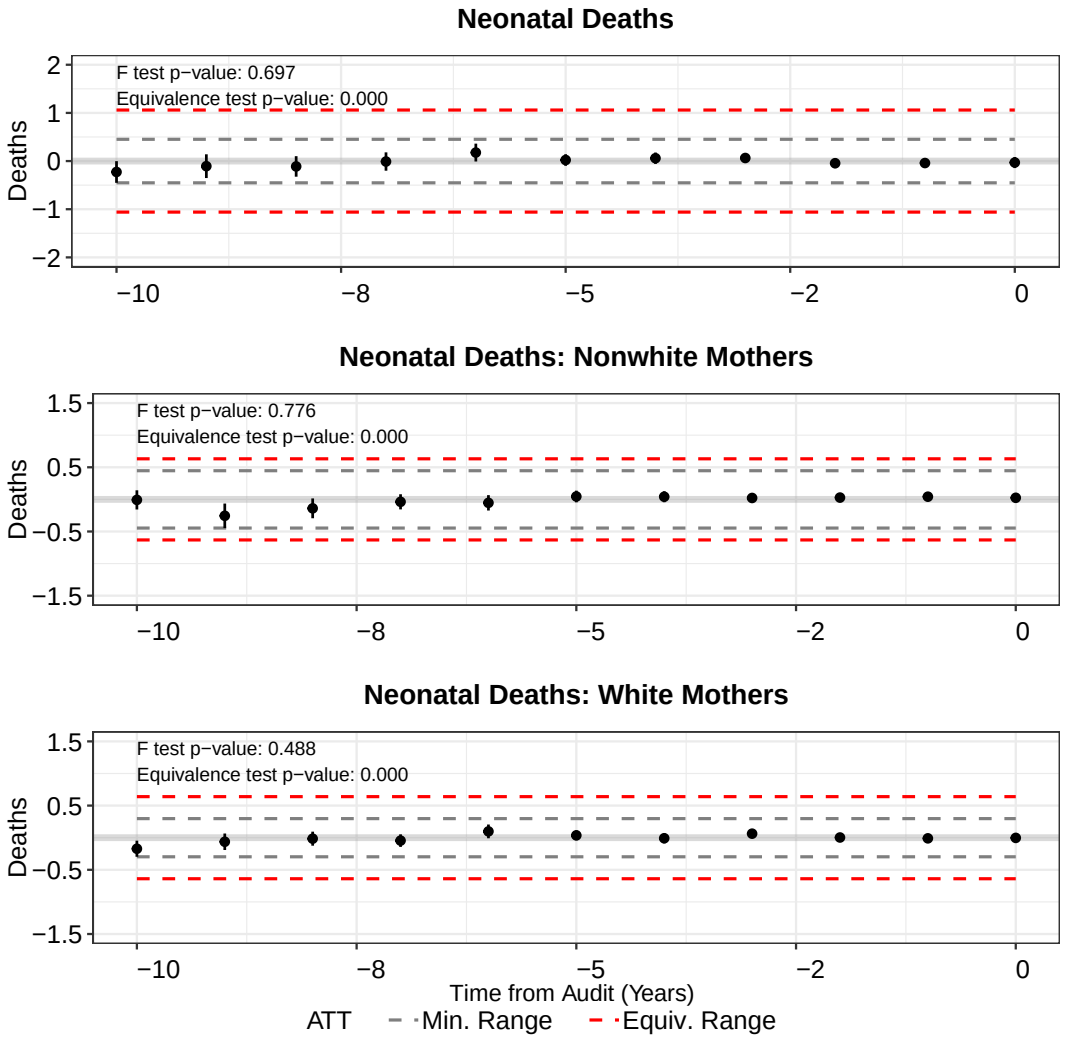
Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual method. Coefficients and 95% confidence intervals are estimated as described in the Methods section, where the outcome captures neonatal death counts at the municipality level. Changes are measured relative to the death count before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Figure G3: Audit Effects on Neonatal Mortality over Time, Female vs. Male (Fixed Effects Counterfactual Estimator)



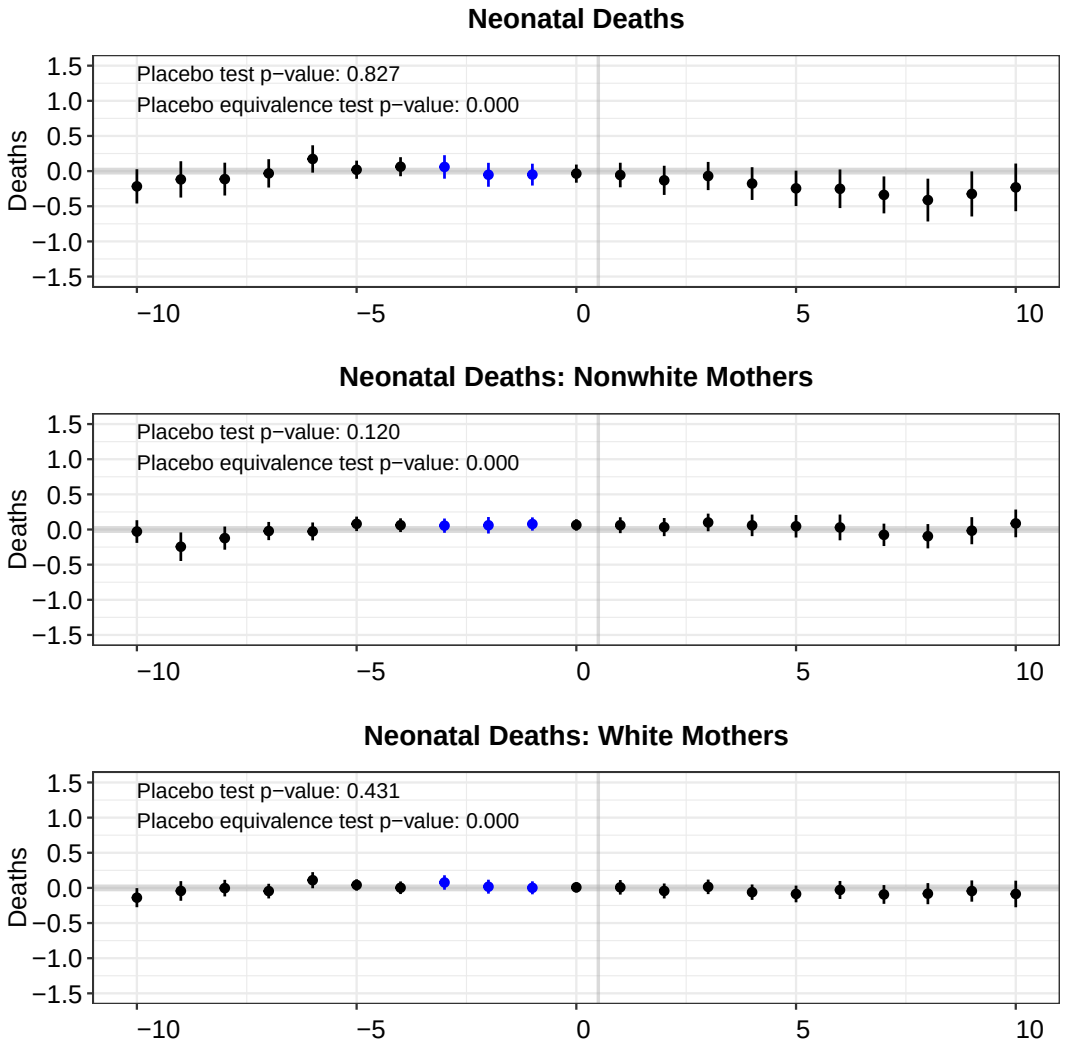
Notes: These specifications employ Liu et al.'s (2022) fixed effects counterfactual method. Coefficients and 95% confidence intervals are estimated as described in the Methods section. Changes are measured relative to the death count in the year before the Audit ($t = -1$). Standard errors are clustered at the municipality level.

Figure G4: Pre-Trend Tests for Audit Effects on Neonatal Mortality over Time, Overall and by Mother's Race



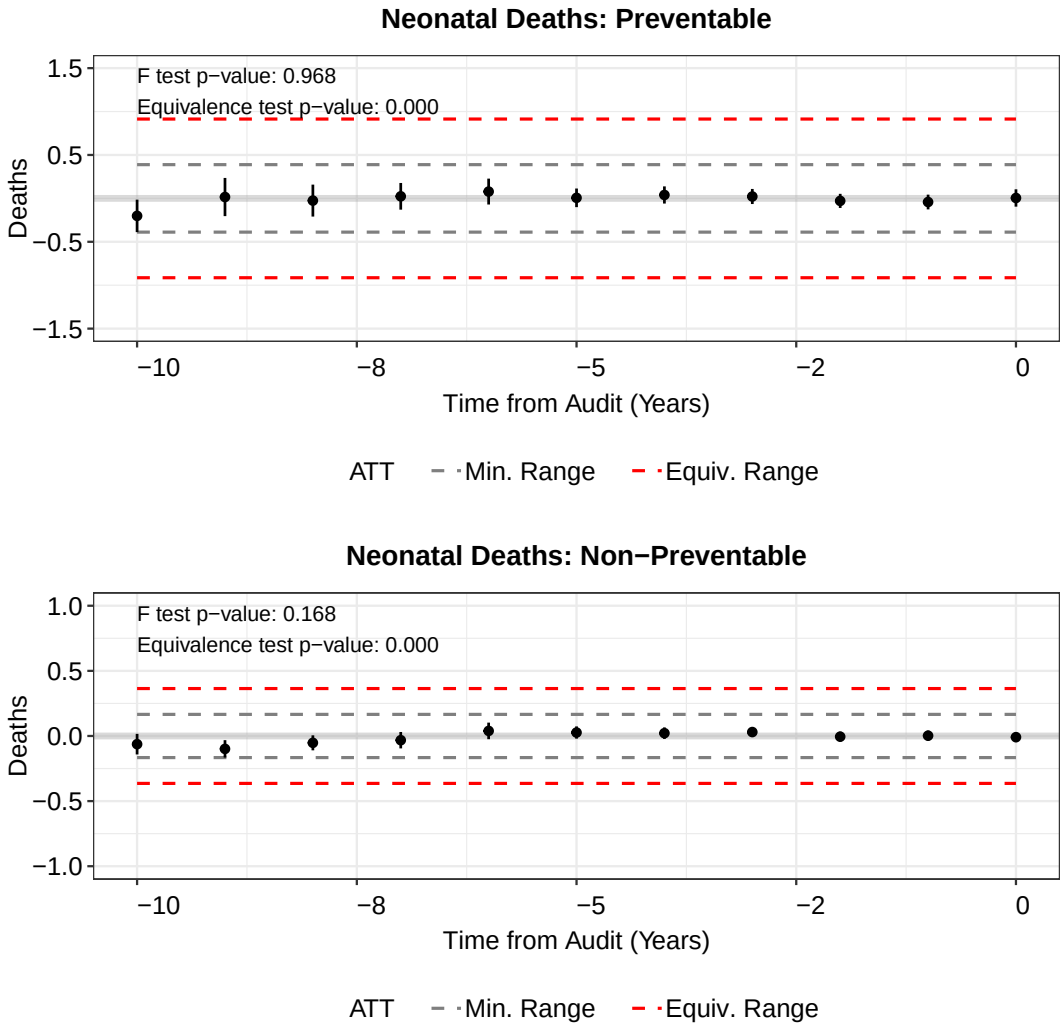
Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on Liu et al.'s (2022) fixed effects counterfactual estimator. In addition, we report the p-value of a joint test of significance of pre-treatment differences, as well as p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure G5: Placebo Tests for Audit Effects on Neonatal Mortality over Time, Overall and by Mother's Race



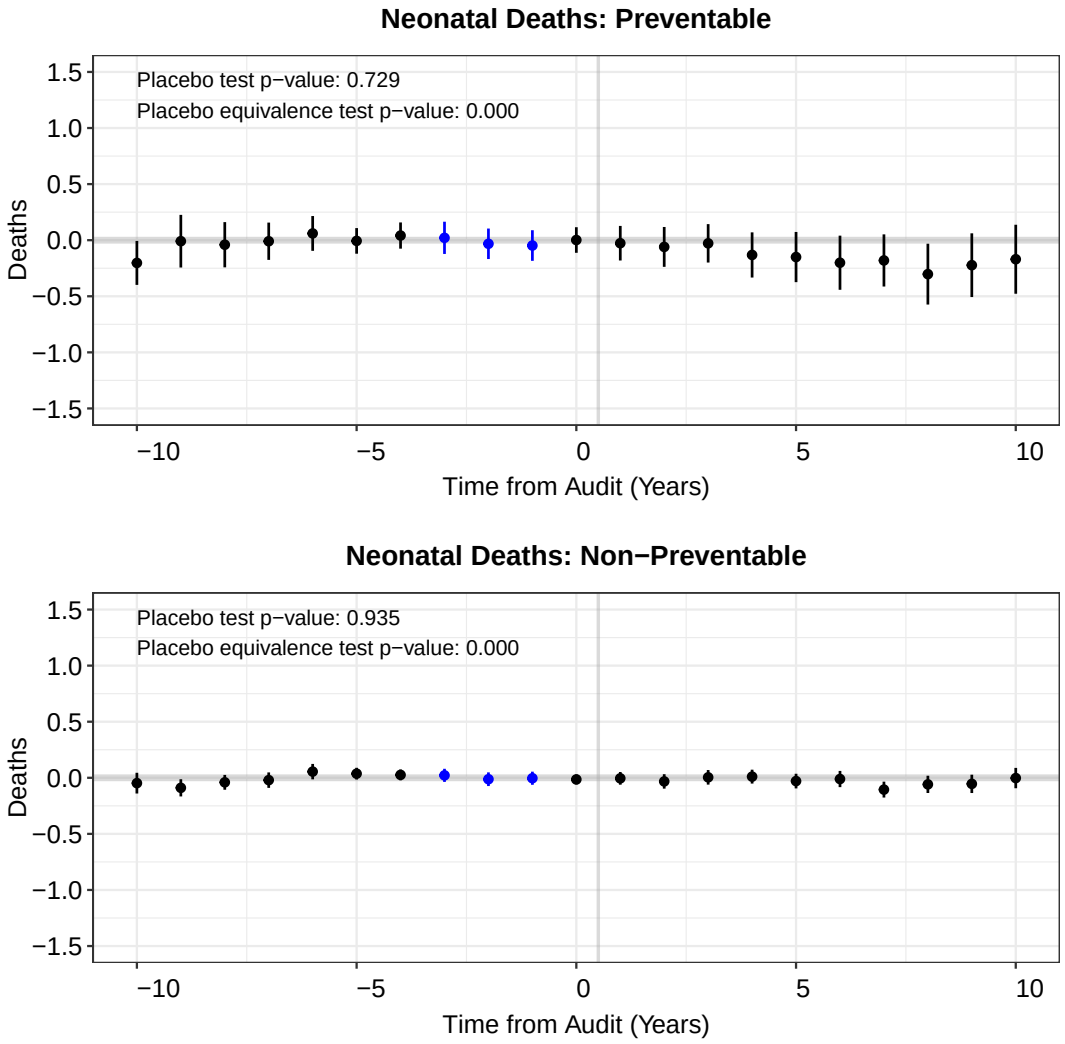
Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from Liu et al.'s (2022) fixed effects counterfactual estimator. In addition, we report the p-value of a joint test of significance of these placebo differences, as well as the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure G6: Pre-Trend Tests for Audit Effects on Neonatal Mortality over Time, by Cause of Death



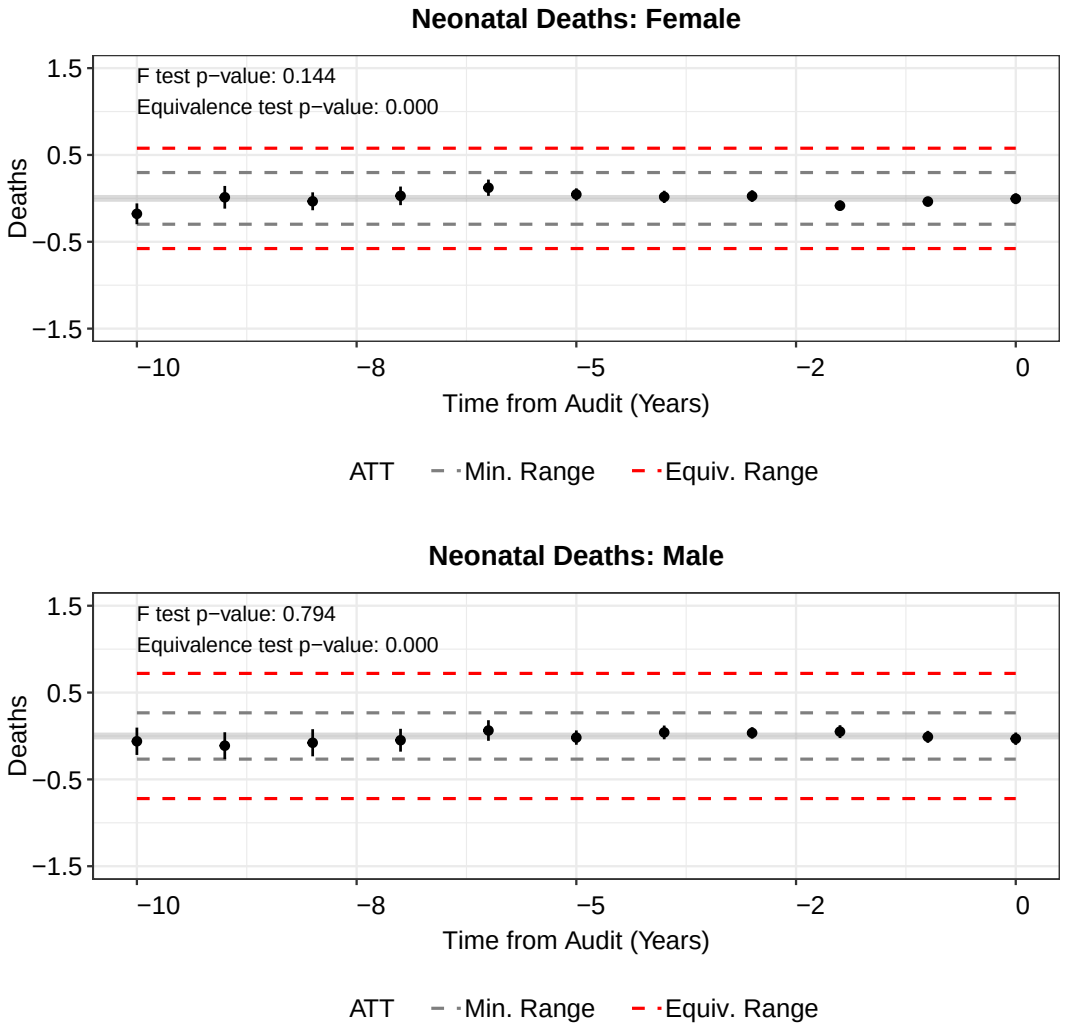
Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on Liu et al.'s (2022) fixed effects counterfactual estimator. In addition, we report the p-value of a joint test of significance of pre-treatment differences, as well as p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure G7: Placebo Tests for Audit Effects on Neonatal Mortality over Time, by Cause of Death



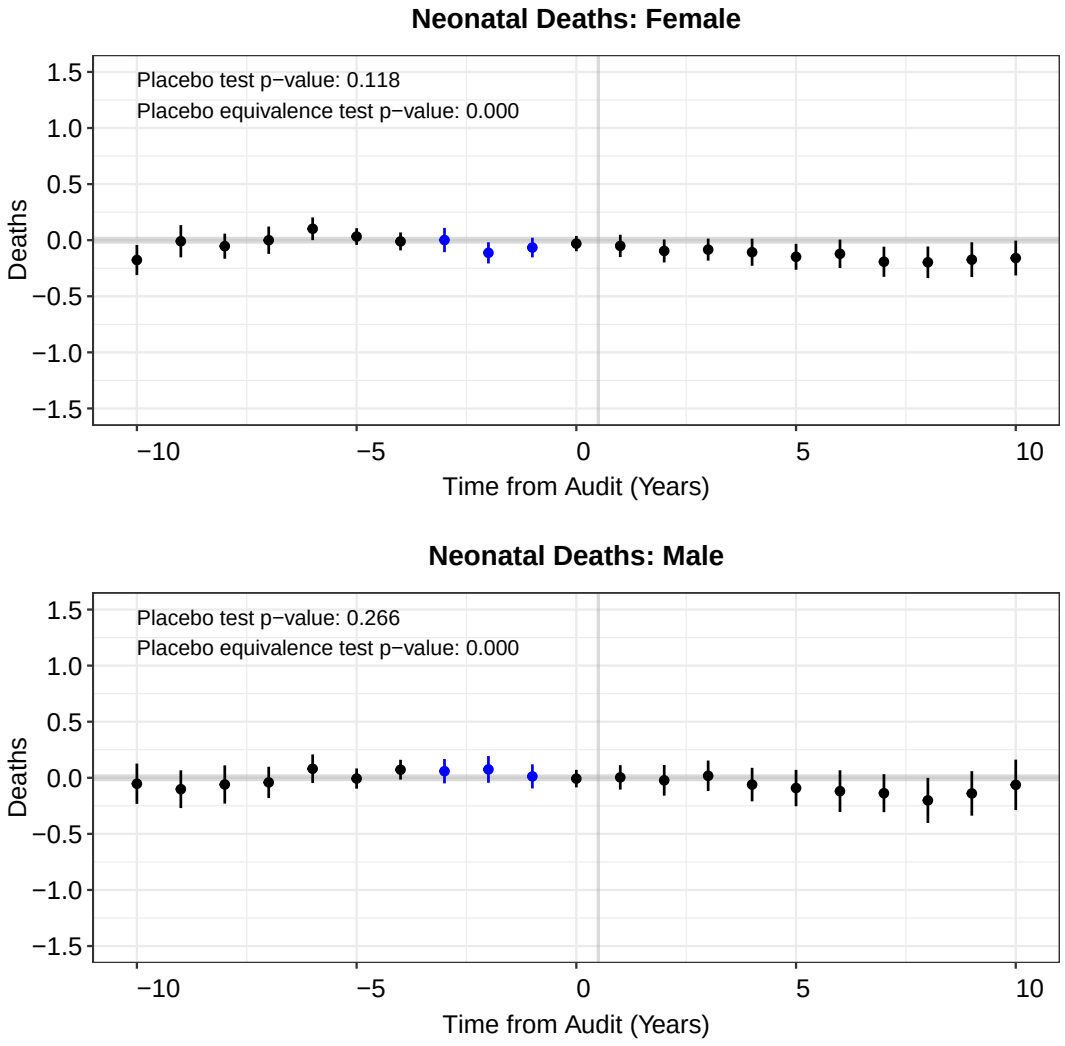
Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from Liu et al.'s (2022) fixed effects counterfactual estimator. In addition, we report the p-value of a joint test of significance of these placebo differences, as well as the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

Figure G8: Pre-Trend Tests for Audit Effects on Neonatal Mortality over Time, Female vs. Male



Notes: The figures report plots and 95% confidence intervals of pre-trend tests of the dynamic treatment effects based on Liu et al.'s (2022) fixed effects counterfactual estimator. In addition, we report the p-value of a joint test of significance of pre-treatment differences, as well as p-values of an equivalence test that pre-treatment differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

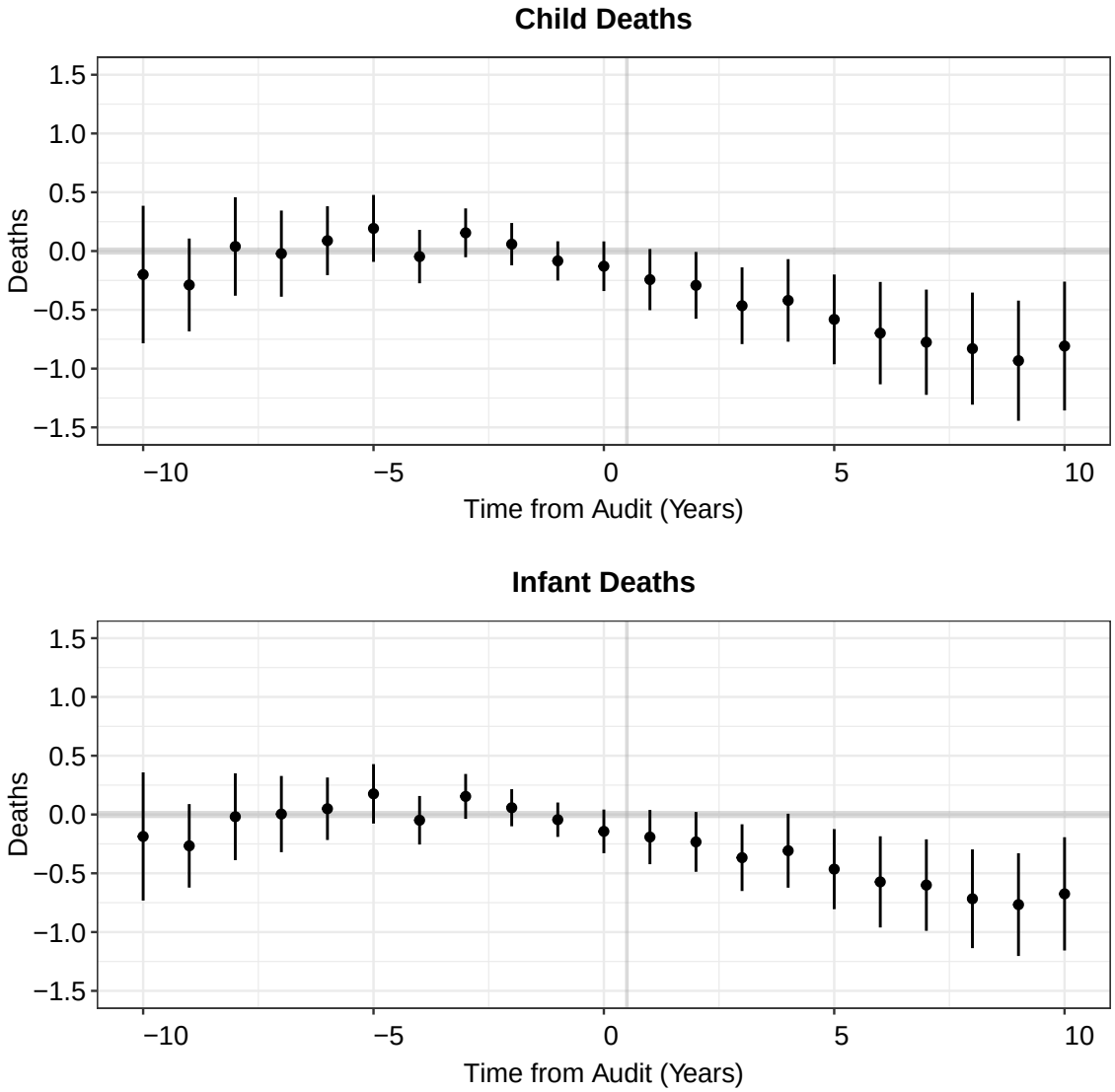
Figure G9: Placebo Tests for Audit Effects on Neonatal Mortality over Time, by Female vs. Male



Notes: The figures report plots and 95% confidence intervals of placebo tests that assume that treatment occurs in years $t-3$, $t-2$, and $t-1$ before actual treatment assignment using the dynamic treatment effects estimates from Liu et al.'s (2022) fixed effects counterfactual estimator. In addition, we report the p-value of a joint test of significance of these placebo differences, as well as the p-values of an equivalence test that placebo differences between treated and control units are greater in absolute value than a pre-defined effect size. For details, see Methods section.

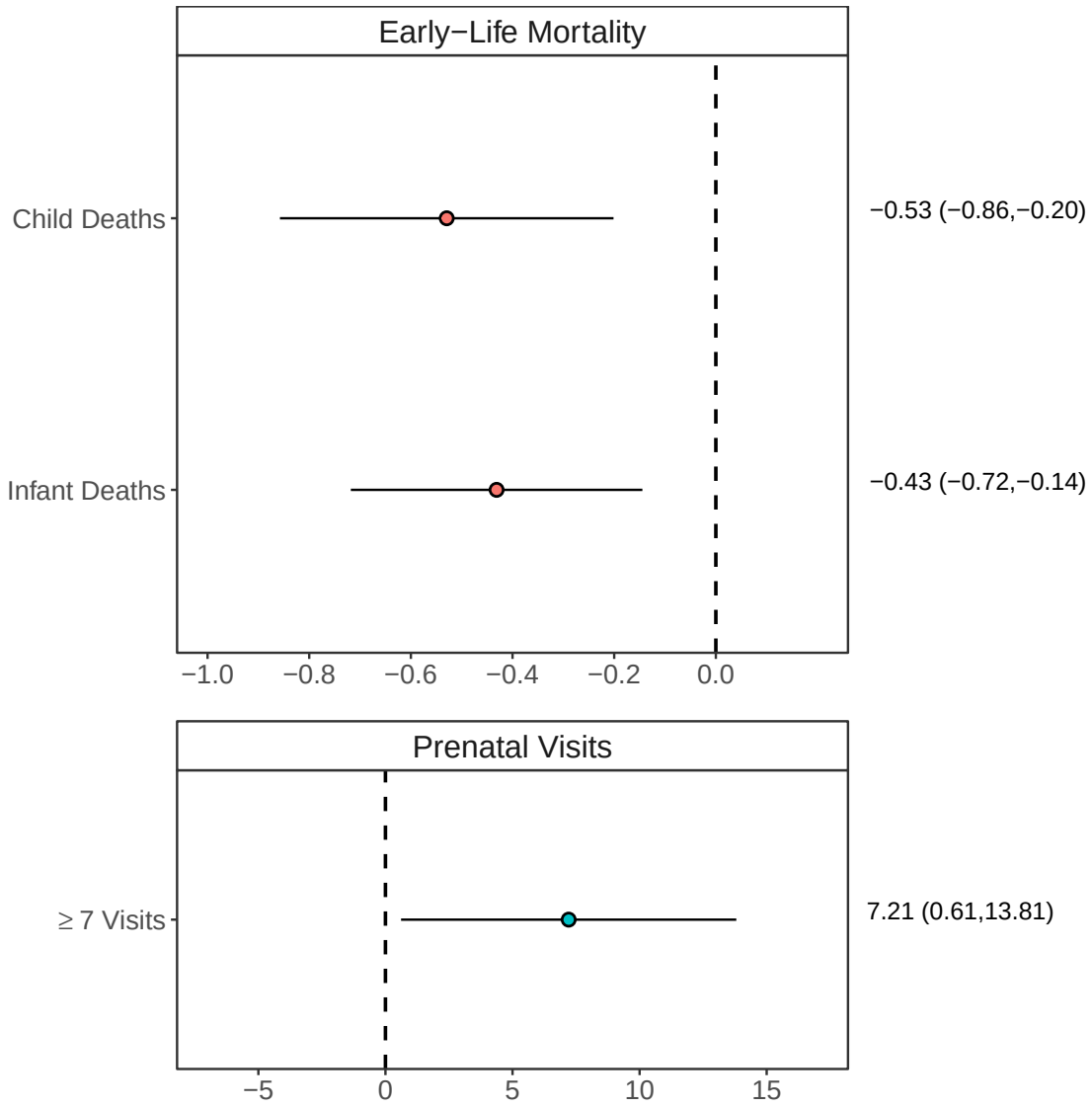
Appendix H: Alternative Coding of Treatment Variable

Figure H1: Audit Effects on Early-Life Mortality over Time (No Lag)



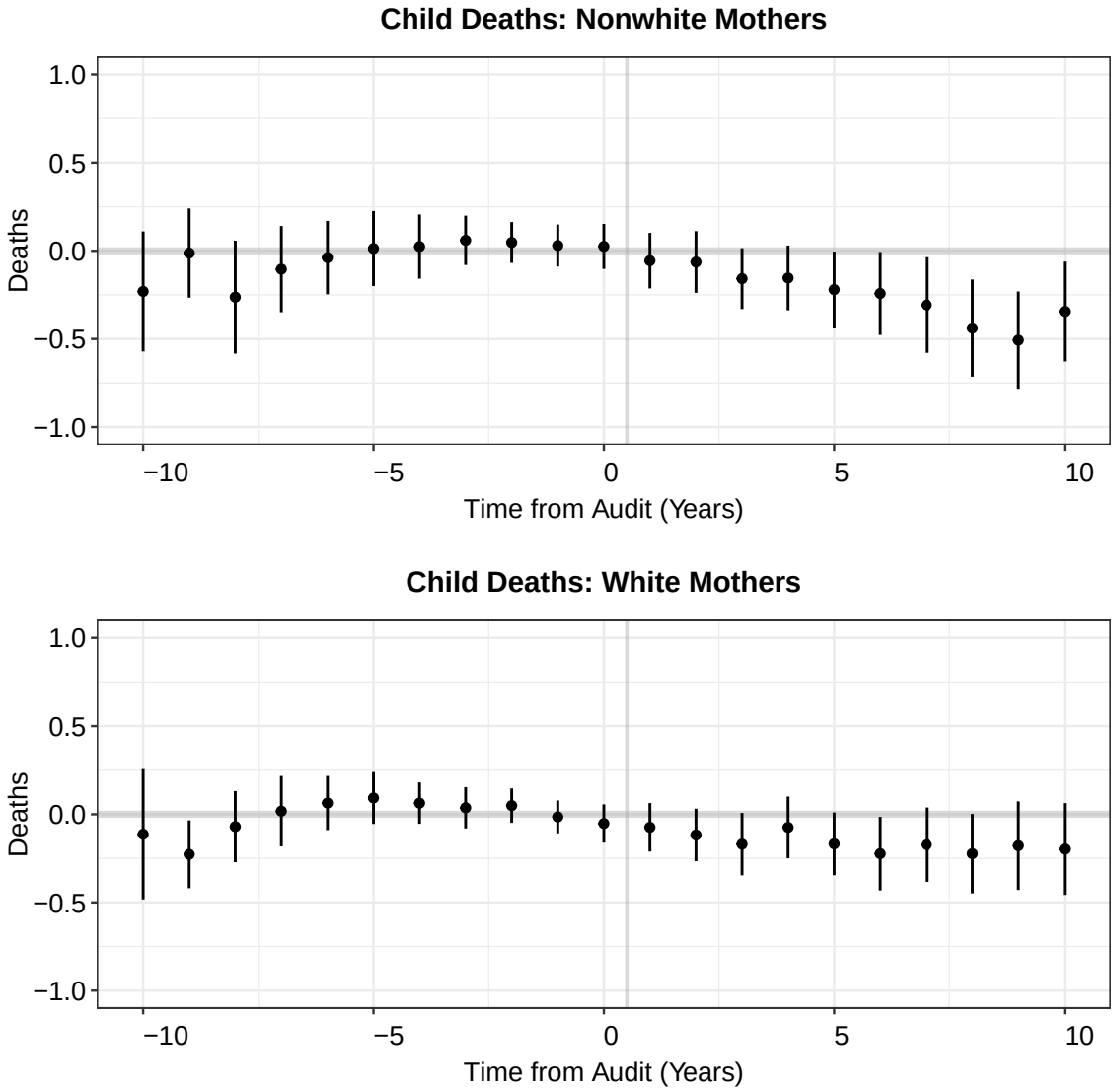
Notes: This figure is identical to Figure 1, except it employs an alternative coding of the treatment variable. More specifically, treatment is coded as contemporaneous instead of lagged (i.e., municipalities selected for lotteries in year t are coded as treated in year t and subsequent years). It reports plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Figure H2: Audit Effects on Early-Life Mortality and Prenatal Care Coverage (No Lag)



Notes: This figure is identical to Figure 2, except it employs an alternative coding of the treatment variable. More specifically, treatment is coded as contemporaneous instead of lagged (i.e., municipalities selected for lotteries in year t are coded as treated in year t and subsequent years). It reports plots of the overall Average Treatment Effect on the Treated, employing Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Figure H3: Audit Effects on Child Mortality over Time, by Race of Mother (No Lag)



Notes: This figure is identical to Figure 3, except it employs an alternative coding of the treatment variable. More specifically, treatment is coded as contemporaneous instead of lagged (i.e., municipalities selected for lotteries in year t are coded as treated in year t and subsequent years). It reports plots of dynamic treatment effects specifications that employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference. Coefficients and 95% confidence intervals are estimated as discussed in the Methods section. Block-bootstrapped standard errors allow for the correlation of error terms at the municipality level.

Table H1: Audit Effects on Early-Life Mortality and Prenatal Care Coverage (No Lag)

	Dependent Variables:		
	Child Deaths	Infant Deaths	Prenatal Care Coverage
	(1)	(2)	(3)
Overall Population			
ATT	-0.53**	-0.43**	7.21*
	(0.17)	(0.15)	(3.37)
	[0.002]	[0.003]	[0.032]
Prevented Deaths	9,277	7,558	
	(3,527 –15,026)	(2,533 – 12,582)	
Observed/Expected Mortality Ratio	0.935	0.938	
	(0.899 – 0.974)	(0.901 – 0.978)	
Nonwhite Mothers			
ATT	-0.20*	-0.15*	9.27**
	(0.08)	(0.07)	(2.90)
	[0.015]	[0.039]	[0.001]
Prevented Deaths	3,551	2,571	
	(679 – 6,423)	(136 – 5,007)	
Observed/Expected Mortality Ratio	0.951	0.958	
	(0.915 – 0.990)	(0.921 – 0.998)	
White Mothers			
ATT	-0.13	-0.11	-3.33
	(0.08)	(0.07)	(2.05)
	[0.081]	[0.132]	[0.104]
Prevented Deaths	2,320	1,892	
	(-469 – 5,108)	(-572 – 4,356)	
Observed/Expected Mortality Ratio	0.955	0.956	
	(0.905 – 1.010)	(0.905 – 1.014)	
Preventable Causes			
ATT	-0.32**	-0.27*	
	(0.12)	(0.11)	
	[0.009]	[0.016]	
Prevented Deaths	5,559	4,662	
	(1,361 – 9,756)	(877 – 8,447)	
Observed/Expected Mortality Ratio	0.942	0.945	
	(0.903 – 0.985)	(0.904 – 0.989)	
Non-Preventable Causes			
ATT	-0.00	-0.00	
	(0.04)	(0.03)	
	[0.963]	[0.961]	
Prevented Deaths	32	27	
	(-1,316 – 1,380)	(-1,084 – 1,140)	
Observed/Expected Mortality Ratio	0.999	0.999	
	(0.965 – 1.035)	(0.964 – 1.037)	
Num. of municipalities	5,203	5,203	5,203
Num. of audited municipalities	1,833	1,833	1,833
Num. of unaudited municipalities	3,370	3,370	3,370

Notes: This table is identical to Table 1, except it employs an alternative coding of the treatment variable. More specifically, treatment is coded as contemporaneous instead of lagged (i.e., municipalities selected for lotteries in year t are coded as treated in year t and subsequent years). These specifications employ Liu et al.'s (2022) fixed effects counterfactual estimator for causal inference (as presented in the Methods section). The first row in each panel reports the ATT estimates of audits' effect on each of the main outcomes of interest. Block-bootstrapped standard errors allowing for correlation of error terms at the municipality level are reported in parentheses; $*$ 5%, $**$ 1%, $***$ 0.1% significance levels; p-values reported in brackets. Rows 2 and 3 of each panel (respectively) 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 (from row 1); 95% confidence intervals are reported in parentheses.