# Do Mandated Health Insurance Benefits for Diabetes Save Lives?*

Jinyeong Son†

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**Abstract**

In response to the growing concern over diabetes, state-mandated health insurance benefits for diabetes have become popular since the late 1990s. However, little is known about whether these mandates improve the health of people with diabetes. In this paper, I use data from the restricted-use Multiple Cause of Death Mortality database and the Behavioral Risk Factor Surveillance System to investigate the effects of these mandates on diabetes-related mortality rates, along with the underlying mechanisms behind the estimated effects. Using a difference-in-differences framework that leverages variation in the enactment of mandates both across states and over time, I find that approximately 3.1 fewer diabetes-related deaths per 100,000 occur annually in mandate states than in non-mandate states. The mechanism analysis suggests higher utilization of the mandated medical benefits caused these mortality improvements. These findings can inform the ongoing policy debate on strengthening or weakening coverage mandates, including Essential Health Benefits under the Affordable Care Act.

***JEL Classification***: I13, I18, J11, K32

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†Department of Economics, The University of Texas at Austin, 2225 Speedway C3100, Austin, TX, 78712, U.S.A. Email: jinyeong.son@utexas.edu.

# 1 Introduction

State-mandated health insurance benefits are the benefits insurance providers must include in their plans to cover the treatment of specific diseases or certain medical services. These state regulations have grown tremendously in recent decades, resulting in more than 1,900 such statutes among all the states in the US (NCSL, 2018).[1] However, whether these mandated benefits improve the health outcomes of the insured is not obvious, and the efficacy of these benefits may also vary by the details of each mandate.[2] In this paper, I investigate whether mandated diabetes-related benefits affect the health outcomes, as measured by the associated mortality rates.

Diabetes is one of the most prevalent chronic diseases in the US, affecting more than one third of the population. The Centers for Disease Control and Prevention (CDC) estimates that more than 34 million people in the US (or 10.5% of the US population) have diabetes, and another 88 million US adults have pre-diabetes, a condition highly likely to develop into type 2 diabetes without proper treatment (CDC, 2020). Diabetes is also one of the most expensive diseases in the US: about 200,000 people in the US die from diabetes each year, making it the nation's seventh leading cause of death.[3] The total economic burden attributable to diagnosed diabetes was estimated to be $327 billion in 2017: $237 billion in direct medical costs, and $90 billion in productivity loss (ADA, 2018).

In response to this public health problem, most states have adopted regulations requiring private health insurance plans to provide coverage for diabetes treatment, particularly during the late 1990s and early 2000s. Interestingly, diabetes-related mortality rates have decreased since then. Figure 1 illustrates a striking correlation between the timing of the adoption of these man-

[1] According to Jensen and Morrisey (1999), state-level mandates were not common until the early 1970s: only 35 mandates existed among 41 states in 1970, implying less than one mandate for each state. However, the number of state-level mandates increased at roughly 25-fold (from 35 to 860 mandates) between 1970 and 1996 among these states. This number has more than doubled across all states since the early 2000s, meaning the average state currently has approximately 38 mandates. For more detail on the history of state-mandated health insurance benefits, see Jensen and Morrisey (1999).

[2] Empirical evidence on the effects of state-mandated benefits across different treatments (or diseases) is mixed. For instance, prior work finds the effects of mandates related to (in)fertility treatment on various outcomes such as birth rates, the probability of a multiple birth, and utilization of (in)fertility treatments (Bitler and Schmidt, 2012; Bundorf, Henne and Baker, 2007; Dills and Grecu, 2017; Schmidt, 2007). However, the effects were either limited or not found in the setting of mental health. Chatterji, Decker and Markowitz (2015) show no significant association exists between mandated benefits for autism and access to care, and Klick and Markowitz (2005) provide evidence that mental health mandates do not contribute to reducing suicide rates among adults.

[3] Stokes and Preston (2017) argue that deaths for which diabetes is assigned as the underlying cause are severely undercounted and find diabetes is the third leading cause of death in the US if this underestimation is corrected.

dates and diabetes-related mortality rates. The trend break of diabetes-related mortality observed in Figure 1 appears much clearer in comparison to a steady downward trend in all-cause mortality and seems to provide evidence of the mandate's impact on mortality.

Uncertainty remains, however, about the causal relationship between these two events. First, the mandates may not be binding if most plans already include the benefits even in the absence of the regulations, as documented in other settings (Gruber, 1994).[4] Second, due to an increase in total employment costs, the mandates may cause some employers to either reduce employment or drop offers of health insurance, eventually leading to a higher uninsured rate (Baicker and Levy, 2008; Gabel and Jensen, 1989, 1992; Kaestner and Simon, 2002; van der Goes, Wang and Wolchik, 2011).[5] Finally, the provision of mandated benefits for diabetes can generate an ex-ante moral-hazard problem, such as substituting medical benefits for healthy behaviors, thereby producing undesired offsetting effects of the mandates (Klick and Stratmann, 2006, 2007; Zweifel and Manning, 2000).[6] For these reasons, the extent to which the mandates caused the diabetes-related mortality declines in Figure 1 is unclear.

In this paper, I use data from the restricted-use Multiple Cause of Death Mortality database and the Behavioral Risk Factor Surveillance System to study the effects of diabetes mandates on diabetes-related mortality and to explore potential mechanisms behind the estimated effects. Specifically, I leverage variation in the enactment of the mandates both across states and over time, using a flexible difference-in-differences framework to compare changes in diabetes-related mortality rates in states with the mandates with those in states without the mandates over the relative years of mandate adoption.

My main results indicate approximately 3.1 fewer diabetes-related deaths per 100,000 occur annually in mandate states than in non-mandate states, which is equivalent to a 4.0% reduction in diabetes-related mortality rates relative to the pre-period mean mortality. These estimates im-

[4] Gruber (1994) finds mandates (alcoholism, drug abuse, mental illness, chiropractor) have little effect on the rate of insurance coverage among small firms, and provides some evidence that this lack of an effect for mandates is due to the fact that mandates are not effectively binding.

[5] Empirical evidence is mixed: some previous studies do not find such effects; for example, see Buchmueller, DiNardo and Valletta (2011); Gruber (1994); Gruber and Krueger (1991).

[6] In particular, Klick and Stratmann (2007) argue the existence of such an ex-ante moral-hazard effect in diabetes mandates by showing diabetics in adoption states tend to exhibit a higher increase in body mass index after the passage of the mandates.

ply approximately 9,500 lives are saved annually among the population living in mandate states, or about 10,100 lives if extrapolated nationally. This finding is robust to a number of different specifications, to possible scenarios generating biases such as contemporaneous mandates, to the dose-response approach addressing potential endogeneity, and to alternative estimators designed for a staggered adoption setting (e.g., the Sun and Abraham estimator). The results are also compatible with the findings from the medical literature in terms of the magnitude and timing of the effects (Block, y Keenoy and Gaal, 2008; Brown, 2000; Currie et al., 2012; He et al., 2017; Odnoletkova et al., 2016; Wishah, Al-Khawaldeh and Albsoul, 2015).

Besides the main results, I present supplemental analyses: potential mechanisms (or first-stage effects in a broader sense) and heterogeneity by different benefit components included in the mandates. First, I show evidence suggesting that the use of medical care covered by the mandates increases, providing reassuring evidence for the plausibility of the documented mortality improvements. Specifically, this evidence shows that diabetics make greater use of medical services for diabetes management in adoption states than in non-adoption states after the introduction of the mandates, with the estimated effects spanning a 2.4% to 12.3% increase across various covered benefit items. Second, heterogeneity analysis reveals the mortality improvements are similar across mandate states, regardless of the inclusion or exclusion of medication benefits. This evidence suggests other components of the mandates—such as mandated coverage for glucose monitoring equipment, diabetes self-management training, and nutritional therapy—may have been particularly important in driving the estimated effects, with limited moral-hazard effects through the substitution of medication for healthy behaviors.

This paper contributes to several distinct bodies of literature. First, this paper adds to a larger literature investigating the relationship between health insurance coverage and mortality. A number of studies document that health insurance coverage substantially reduces mortality in certain contexts (e.g., Card, Dobkin and Maestas, 2009; Miller, Johnson and Wherry, 2021; Goldin, Lurie and McCubbin, 2021), and recent work suggests that different types (or features) of insurance plans—along with differential coverage generosity—in a private market could have significant effects on mortality (Abaluck et al., 2021; Chandra, Flack and Obermeyer, 2021). This paper moves

the literature forward by providing evidence that state-level regulations on what medical benefits must be included in a basic market plan (or on minimum required benefits) could save lives, specifically from a certain targeted disease.

Second, within the setting of diabetes mandates, two previous studies examine the effects of diabetes mandates on ex-ante moral-hazard behaviors (Klick and Stratmann, 2007) and on infant health (Grecu and Spector, 2015). Although these studies provide evidence for the effects of the mandates on some important dimensions, they focus on either behavioral responses or second-hand effects on infants from the insured likely affected by the mandates. Furthermore, neither of the aforementioned papers provides evidence of the effects on the utilization of mandated benefits; instead, they focus on the reduced-form effects of the mandates. In contrast to previous studies, I focus on diabetes-related mortality rates, possibly the most important and clearly measurable health outcome, and find that the mandates contribute to reducing diabetes-related mortality by roughly 3.1 per 100,000 annually. This paper also presents suggestive evidence for the mechanisms behind the main results, which can also be viewed as evidence of the first-stage effects in a broader sense. The mechanism analysis in this paper indicates that the increased utilization of covered benefits among diabetics is likely the cause of the estimated mortality improvements. This evidence strengthens the veracity of the main results not only in this paper, but also in previous studies investigating the effects of diabetes mandates.

Third, to the best of my knowledge, this study is the first to investigate heterogeneity in the effects by covered benefit items. Prior research on state-mandated insurance benefits focuses on a dichotomous variation of the mandates, either adopted or not adopted, in examining the effects of mandated benefits. This paper further exploits variation in included benefit components to understand the heterogeneous effects among mandated states. Specifically, I find the inclusion of medication coverage does not cause a difference in lives saved in this setting, suggesting that other components (e.g., glucose monitoring equipment or self-management education) lead to the main results. However, the inclusion of medication coverage (or broadly, the detail of included benefit components) may generate differential effects in other settings. Therefore, this paper sheds light on the potential importance of such understudied variation in other settings of state-mandated

health insurance benefits.

Finally, this research contributes to a rich literature that examines the effects of state-mandated insurance benefits, including work on (in)fertility treatment (Bitler and Schmidt, 2012; Bundorf, Henne and Baker, 2007; Dills and Grecu, 2017; Schmidt, 2007), autism (Chatterji, Decker and Markowitz, 2015), mental health (Klick and Markowitz, 2005; McGuire and Montgomery, 1982), immunization (Chang, 2016), cervical cancer (Bitler and Carpenter, 2017), and breast cancer (Bitler and Carpenter, 2016).

The remainder of this paper is organized as follows. Section 2 provides background information on mandated health insurance benefits for diabetes. Section 3 describes the data. Section 4 explains the main empirical strategy. Section 5 reports the results. Section 6 presents the supplemental analyses. Section 7 concludes.

## 2 Background

### 2.1 What Is Diabetes?

Diabetes is a chronic health condition characterized by high levels of sugar in the bloodstream, which can induce serious complications, such as cardiovascular disease, kidney failure, blindness, or even death (ADA, 2009). As the seventh leading cause of death in the US, it claims more than 200,000 lives every year and remains a significant public health problem. One important characteristic of diabetes is that it is a manageable disease, meaning that early diagnosis and proper management of the disease can enable patients to minimize, delay, or even avoid severe complications.[7] This manageability is one of the critical grounds on which mandated insurance coverage for the disease is advocated, because it can help provide prompt and appropriate interventions for people with diabetes, thereby improving their health and/or saving their lives.

[7] No direct cure for diabetes currently exists, so rapid recovery of diabetes is generally infeasible.

## 2.2 Mandated Health Insurance Benefits for Diabetes

Mandated health insurance benefits for diabetes (usually called diabetes mandates) are state-regulated policies that require a health insurance provider to cover costs related to treating diabetes for the insured. As of 2020, 43 states and the District of Columbia have some form of diabetes mandates, and we refer to these states as *mandate-to-cover* states. Unlike mandate-to-cover states, state laws in Mississippi and Missouri demand that insurers offer at least one plan in the market that includes coverage of diabetes, but they do not necessarily include coverage in all plans. These states are called *mandate-to-offer* states. Only Alabama, Idaho, North Dakota, and Ohio have no form of mandate at all and are thus referred to as *no-mandate* states.[8] Figure 2 shows the legislative status for all states with enactment years, which is also the main identifying variation. Throughout the analysis in this paper, I exclude Mississippi and Missouri (i.e., mandate-to-offer states) due to their ambiguous treatment status.

Diabetes mandates have two institutional properties worth mentioning. First, in principle, they apply only to health insurance plans offered by employers or to private health insurance plans directly purchased by individuals in the market. Additionally, the roles of the state-regulated diabetes mandates are further limited by the Employment Retirement Income Security Act of 1974 (ERISA 1974). ERISA 1974 exempts self-insured plans (among employer-sponsored plans)[9] from any state-level insurance regulations, so only the fully insured (among employer-sponsored health plan holders)[10] and individuals who buy a private health insurance plan directly from the market are affected by the mandates. However, in my setting, legislative details suggest that the underlying coverage effects of the mandates could be expanded to other types of insurance plans, including Medicare beneficiaries, self-insured plan holders, and Medicaid recipients. I discuss this issue in greater detail in the next subsection.

Second, the benefits are standardized across mandate-to-cover states in that they consist of

[8] The state of Oregon introduced diabetes mandates in 2001, but repealed them as of January 1, 2018. It is currently one of five no-mandate states, but it is treated as a mandate-to-cover state in this paper, because the main analysis period is 1990 to 2013.

[9] Large employers typically assume the risk of paying the healthcare costs rather than actually purchase insurance for their employees, because they have enough financial resources to cover such expenses. For this reason, self-insured plans are common among employees working in large companies.

[10] In contrast to the self-insured, the fully insured mainly work in small- or medium-sized firms.

three key components to cover: medication, equipment, and self-management education.[11] However, the components that each state requires insurers to cover differ across mandate-to-cover states.[12] For example, Arizona includes medication and equipment, whereas Florida covers equipment and self-management education. In this paper, I classify all mandate-to-cover states into three types: medication, non-medication, and full-coverage states. Medication states refer to those that provide coverage for medication but not self-management education. By contrast, non-medication states are those that provide the benefits of self-management education but not medication. Full-coverage states cover both medication and non-medication benefits. Figure A1 in Online Appendix illustrates the classification of all mandate-to-cover states into these three categories. I use this variation to examine heterogeneity in the effects of diabetes mandates by components (or state type) in subsection 6.2.

To the best of my knowledge, this paper is the first study to document variation in covered benefit components across mandate-to-cover states and to use this information to study the heterogeneous effects of the mandates. I code whether and when a mandate-to-cover state incorporates each of the three benefit components, by extensively reviewing the relevant state statutes and tracking their legislative histories. Table A1 in Online Appendix shows the results of this effort, summarizing the detailed legislative status of the mandates with the dates each component was introduced, along with classification of all mandate-to-cover states into the three types.[13]

### 2.3 Underlying Coverage Increase by Mandates

Although I focus on estimating the intention-to-treat (ITT) effects of diabetes mandates on mortality, it is important to establish the existence of the underlying coverage increase of the mandates and to quantify its magnitude to provide more interpretation for reduced-form estimates via scaling exercises.

As briefly mentioned in the previous subsection, diabetes mandates (or more broadly, any state-

[11] Medication covers items such as insulin or oral diabetes pills. Equipment benefits include coverage of devices such as glucose monitors or insulin pumps. Self-management education includes any services to help diabetics learn about the disease and how to control and manage their blood sugar levels.

[12] All mandate-to-cover states commonly require equipment coverage.

[13] Data from Table A1 in Online Appendix in an electronic format and evidence related to this documentation are available upon request from the author.

level mandated benefit laws) are generally expected to affect particular types of insurance plans, namely non-self-insured private plans.[14] Detailed anecdotal and qualitative evidence presented in subsection A.1 in Online Appendix collectively imply that not all existing private health insurance plans included adequate diabetes benefits at the time of mandate enactment, thereby confirming the *existence* of the coverage effects.[15] Beyond this evidence, the results of the mechanism analysis in subsection 6.1, which can be viewed as the first-stage effects in a broader sense, further reduce this concern.

Next, I emphasize that the effects on actual coverage may operate through several channels in my setting. Specifically, those who have other types of insurance coverage—Medicare, self-insured private, and Medicaid plans—are also likely to benefit from the coverage mandates. First, many Medicare beneficiaries could be affected by the mandates because private supplemental plans (e.g., retiree coverage or Medigap) that most traditional Medicare enrollees have are subject to the mandates in most states. Second, after the mandates were enacted, some individuals with self-insured plans may have also been given access to similar benefits, given self-compliance behaviors among private self-insured firms or legal obligations to provide the benefits among public self-insured employers (e.g., state and local governments). Lastly, Medicaid agencies in some states expanded diabetes coverage concurrently with the enactment of the mandates, indicating that Medicaid recipients in those states also experienced an increase in coverage for diabetes. Online Appendix subsection A.2 describes each of these channels in detail, along with supporting evidence.

The discussion above implies that (1) the inclusion of all individuals, regardless of their insurance coverage type, in the analysis better reflects my institutional setting and that (2) the size of the first-stage effects could be bigger than one may think. One limitation of this paper is that I cannot present a single statistic combining all these complex channels due to data limitations and variation in the extent to which these channels are at work across mandate states.[16]

[14] I use the term, non-self-insured private plans, to collectively refer to both employer-sponsored fully insured plans and directly purchased plans.

[15] Beyond increasing the number of plans offering any diabetes coverage (an *extensive-margin* effect), mandates may have also caused plans with pre-existing diabetes coverage to increase the generosity of this coverage (an *intensive-margin* effect). This intensive-margin effect can arise from various features of the mandates. See subsection A.2 in Online Appendix for more.

[16] Whether and to what extent each channel is operative are also likely to differ across mandate states, given the differences in legislative details as well as population composition by insurance type in the pre-adoption periods.

# 3 Data

This paper uses data from two primary sources: the restricted-use Multiple Cause of Death Mortality (MCDM) database from the National Center for Health Statistics (NCHS, 2021) and the Behavioral Risk Factor Surveillance System (BRFSS) from the CDC (CDC, 2011).

The MCDM provides multiple causes of death for all deaths within the US based on death certificates filed in the Vital Statistics. Given that the MCDM is coded according to the International Classification of Diseases (ICD) codes,[17] I first used the ICD code for diabetes to identify all deaths related to diabetes, and then computed *age-adjusted state-level diabetes-related mortality* rates per 100,000 for all states and the District of Columbia over the years 1990 to 2013. Note that I use diabetes-related mortality that is measured based on *multiple* cause of death (MCOD) instead of *underlying* cause of death (UCOD), following the suggestions from medical literature (Israel, Rosenberg and Curtin, 1986; Park and Peters, 2014; Fedeli et al., 2015; Rodriguez et al., 2019)[18,19]. Figure A2 in Online Appendix visually provides the entire data for this key outcome variable. I focus on the period 1990 to 2013 because it provides sufficient pre-policy periods to assess the validity of the identification assumption, but avoids the potential confounding factor of the Affordable Care Act Medicaid expansions beginning in 2014.[20] Throughout the paper, all mortality rates are age-adjusted state-level rates per 100,000, but I call them mortality (rates) for brevity.

The BRFSS collects data on health-related risk behaviors, chronic health conditions, and the use of preventive services at the individual level. In particular, the diabetes module in the BRFSS includes a survey questionnaire that collects information about the respondents' use of healthcare services related to diabetes, such as whether they take insulin or check blood sugar levels for the management of diabetes.[21] I used the diabetes module to understand the mechanisms behind the

[17] It is worth highlighting that causes of deaths are classified in accordance with the ICD-9 until 1998, but thereafter with the ICD-10. The codes for diabetes are 250.0–250.9 in the ICD-9 and E10–E14 in the ICD-10. I consider this transition in the computation by using the conversion formula between the two versions provided by the CDC.

[18] I provide more discussion on why it is more appropriate to make the use of MCOD data in diabetes research in section C in Online Appendix.

[19] Mortality rates across states over different years must be adjusted to the same standard population for comparison. I used the year 2000 Standard Million Population of the US for this adjustment, in line with the current practices of the CDC. See https://wonder.cdc.gov/wonder/help/mcd.html for more details.

[20] Previous work (e.g., Borgschulte and Vogler, 2020; Miller, Johnson and Wherry, 2021) finds a significant reduction in the overall mortality rates in expansion states relative to non-expansion states.

[21] Online Appendix section B describes what specific questions (or variables) in the module are used for the analysis.

main findings: whether the higher utilization of healthcare services the mandates require insurance providers to cover leads to reduced diabetes-related mortality rates. The BRFSS data cover the years from 1995 to 2010, considering that most states started to participate in the module from 1995 and that substantial changes occurred in the BRFSS survey methodology in 2011. One limitation of the use of the diabetes module is that not every state administered the module each year during the sample period, because it is an optional module under the discretion of each state's health department.[22]

Finally, I obtained information on per-capita state government direct health/hospital expenditure from the Urban-Brookings Tax Policy Center (UBTPC, 2019),[23] the number of physicians per 10,000 from the National Center for Health Statistics (NCHS, 2016), and the state-level prevalence of diagnosed diabetes among adults from the United States Diabetes Surveillance System (USDSS, 2021).[24] Table 1 presents the summary statistics of the key variables used in this paper, separately by mandate status as well as collectively for all states. Panel (a) in Table 1 shows the state-level variables used for the main analysis, whereas Panel (b) displays the individual-level variables used for the mechanism analysis.[25]

## 4 Empirical Strategy

I examined the effects of diabetes mandates on diabetes-related mortality rates, using a difference-in-differences framework that compares changes in annual diabetes-related mortality rates in the states with the mandates to the states without the mandates before and after mandate adoption. My baseline specification takes the following form:

$$MR_{st} = \beta \times Adopt_s \times \mathbf{1}\left(t - t_s^* \geq 0\right) + f(X_{st}) + \gamma_t + \lambda_s + \epsilon_{st}. \tag{1}$$

[22] In section F in Online Appendix, I provide evidence that this drawback of missing data for some states (or the unbalanced panel structure) is not likely to affect the mechanism analysis.

[23] Per-capita state government direct health/hospital expenditure includes, but is not limited to, spending for community and public health programs, government-owned hospitals, and government payments to privately owned hospitals. These values are inflation adjusted to 2019 dollars.

[24] I imputed a few missing observations for these covariates using a simple linear regression.

[25] Given that most control units consist of later adoption states with a few no-mandate states, Table A2 in Online Appendix presents the summary statistics for the state-level variables during the baseline period (1990–1993) by three different groups: early adoption states, later adoption states, and no-mandate states.

The outcome variable, $MR_{st}$, denotes the diabetes-related mortality rate per 100,000 in state $s$ in year $t$. The treatment variable, $Adopt_s$, takes the value of 1 if state $s$ has the mandates. The post-period indicator variable, $\mathbf{1}(t - t_s^* \geq 0)$, equals 1 if year $t$ belongs to the post-periods for the mandate state $s$ whose enactment year is $t_s^*$, and 0 in all periods for no-mandate states. $f(X_{st})$ is a flexible set of controls that describes the time-varying state characteristics, including per-capita state government healthcare expenditure, the number of physicians per 10,000, and the prevalence of diagnosed diabetes among adults.[26] Year and state fixed effects are captured by $\gamma_t$ and $\lambda_s$, respectively. The coefficient of interest is $\beta$, which I interpret as the mean effects on the mortality among mandate-to-cover states relative to no-mandate states after the implementation of the mandates.

In addition, I also report estimates from a more flexible specification that allows the coefficient on the adoption of the mandates to vary by the relative years since the adoption, by estimating the following equation:

$$MR_{st} = \sum_{\substack{h=-8 \\ h\neq -1}}^{11} \{\beta_h \times Adopt_s \times \mathbf{1}(t - t_s^* = h)\} + f(X_{st}) + \gamma_t + \lambda_s + \epsilon_{st}. \tag{2}$$

The indicator variables, $\mathbf{1}(t - t_s^* = h)$, take the value of 1 if (and only if) $h$ years pass after the mandates are implemented in adoption states, and 0 in all periods for non-adoption states.[27] Because I normalize the coefficient on the year just prior to the introduction of the mandates to zero (i.e., $\beta_{-1} = 0$), each coefficient of $\beta_h$s can be interpreted as the change in the mortality rates in mandate-to-cover states relative to no-mandate states after $h$ years of the implementation, with all of the $\beta_h$s being estimated relative to the omitted year (i.e., $h = -1$).

Using this flexible model has two advantages. First, I can visually test the key identifying assumption, which is the parallel-trends assumption: in the absence of mandates, the mortality rates between mandate-to-cover states and no-mandate states would have evolved in parallel. Although this assumption is fundamentally untestable, plotting the $\beta_h$s of pre-periods (i.e., $\beta_{-8}$ to $\beta_{-2}$) can

[26] The inclusion of the state-by-year diabetes prevalence as a control addresses the endogeneity that state-level diabetes rates could simultaneously affect the mortality and the decision to adopt the mandates.

[27] I group all values such that $h \leq -8$ or $h \geq 11$ into single values $h = -8$ and $h = 11$, because these periods are not observable for some states.

provide visual evidence regarding whether any spurious correlations exist between the mandates and the mortality even before enactment. Second, this event-study allows the assessment of the time-evolving effects of the mandates. Ex ante, it may be expected that the mandates' effects do not appear immediately but instead become stronger over time, considering diabetes is a disease to be managed rather than rapidly recovered from. The flexible specification of equation (2) enables me to capture such time-varying effects.

# 5 Results

This section begins by providing the main results—that is, the effects of diabetes mandates on diabetes-related mortality rates. I then demonstrate that the main results are robust to a variety of robustness checks: (1) contemporaneous mandates and policy changes, (2) dose-response approach, (3) alternative difference-in-differences estimators, (4) comparison to medical literature, and (5) mean reversion.

## 5.1 Main Results

Table 2 reports the main results from estimating equation (1). Column (1) presents the estimates of the most parsimonious model controlling only for year and state fixed effects. In column (2), my baseline (or preferred) specification, I add controls for state-level healthcare resources and diabetes prevalence. In column (3), I repeat the baseline specification but estimate it without weights as a general model mis-specification test.[28] Finally, column (4) reports the results where the dependent variable is the natural logarithm of the mortality (as opposed to level). All specifications are estimated weighted by state population except for column (3), and standard errors are clustered at the state level.

Based on the baseline specification in Table 2 column (2), diabetes mandates lead to about 3.1 fewer diabetes-related deaths annually in mandate-to-cover states than in no-mandate states, which is equivalent to a 4.0% $\left(\frac{3.073}{77.06} \simeq 0.040\right)$ reduction in diabetes-related mortality rates over the

[28] Whether weighted or unweighted regressions are appropriate when estimating causal effects is not obvious; however, Solon, Haider and Wooldridge (2015) suggest comparison between the weighted and unweighted results as a general model mis-specification test.

pre-period sample mean. The results in column (3) from an unweighted regression are quantitatively very similar to the baseline estimates in column (2), which is reassuring evidence regarding the model specification. Finally, I find a 3.9% reduction in diabetes-related deaths using the log dependent variable, as reported in column (4). The similarity between the estimates in column (4) and the baseline estimates scaled by the pre-period outcome mean (i.e., 3.9% vs. 4.0%) broadly confirms that the main results are driven by most adoption states rather than a few large, effective states.[29]

Figure 3 displays the event-study figure, which corresponds to the flexible version of the baseline specification, as outlined in equation (2).[30] The plot shows no evidence of systematic differences in trends prior to the mandates, providing support for the parallel-trends identification assumption.[31] In addition, I observe that diabetes-related deaths do not significantly decrease in the first two years after the implementation, $\beta_0$ and $\beta_1$, which could be due either to the nature of diabetes or to delayed effective dates for some states.[32] However, the coefficient estimated in the third year following the mandates, $\beta_2$, indicates diabetes-related mortality rates declined by 2.468. The estimates for the subsequent years become larger over time, ranging from $-2.956$ to $-6.609$, and all are statistically distinguishable from zero. Column (1) of Table A3 in Online Appendix provides the associated coefficient estimates, standard errors, and p-values.

### 5.2 Robustness Analyses

**Contemporaneous Mandates and Policy Changes** I examined the effects of diabetes mandates on several non-diabetes-related mortality rates to address the concern that some contemporaneous mandates and policy changes bias my baseline estimates. The idea behind this approach is that if any other efforts (including the passage of other mandated benefits) occurred at a time similar to the adoption of diabetes mandates and affected diabetes-related mortality, they more likely have

[29] It is possible that only a few mandate-to-cover states are driving the main results, due to their substantial reductions in diabetes-related mortality. In this case, the results from a level-outcome regression (once appropriately scaled to percent change term) would be very different from those from a log-outcome regression.

[30] The two aggregated end points, $h = -8$ and $h = 11$, are not displayed in the figure, because they are severely unbalanced.

[31] All coefficients in the pre-adoption periods, ($\beta_{-8}$ to $\beta_{-2}$), are statistically indistinguishable from zero.

[32] The enactment dates can be different from the effective dates for some states because they may have specified delayed effective dates. See Table A1 in Online Appendix for more details.

impacted non-diabetes-related mortality. Because these interventions aim to deal with diseases other than diabetes or to improve people's overall health status; thus, spillovers to diabetes mortality are likely only in the presence of any primary effects on targeted non-diabetes diseases or mortality.

Figure 4 plots $\beta$ coefficients from separate regressions from equation (1), where the dependent variables are the natural logarithm of various mortality rates, as indicated in each row. The binary full partition simply categorizes all deaths into either diabetes-related or non-diabetes-related deaths, whereas the top-10 leading-cause partition further classifies all non-diabetes-related deaths based on the cause of death. For comparison, I reproduce the estimates reported in column (4) of Table 2. As displayed in Figure 4, diabetes mandates only have a significant impact on diabetes-related mortality, thereby rendering its estimated coefficient a notable outlier within the distribution. Specifically, the coefficient estimates from all the other non-diabetes-related mortality rates are quantitatively small (or moderate) and statistically indistinguishable from zero, ranging from $-0.039$ (Alzheimer's) to 0.035 (external-cause), with p-values being no smaller than 0.146.[33] See Table A4 in Online Appendix for estimates, standard errors, and p-values shown in Figure 4.

**Dose-Response Approach** To attenuate potential endogeneity between the adoption of the mandates and mortality, I also conducted the dose-response difference-in-differences analysis by using the fraction of the state population with *any* insurance coverage in the prior year to the mandate adoption as a dose variable.[34] Figure A3 in Online Appendix displays variation in the dose variable across 45 treated states, with the range of the dose variable being 17.0%, the maximum fraction being 92.5% (Wisconsin), and the minimum fraction being 75.5% (Texas and Arizona). Using this variation, I estimated the following modified baseline equation, in which the treatment dose variable, $D_{(s,t_s^*-1)}$, is additionally interacted with $Adopt_s \times \mathbf{1}\left(t - t_s^* \geq 0\right)$:

$$MR_{st} = \beta \times D_{(s,t_s^*-1)} \times Adopt_s \times \mathbf{1}\left(t - t_s^* \geq 0\right) + f\left(X_{st}\right) + \gamma_t + \lambda_s + \epsilon_{st}. \tag{3}$$

[33] Beyond this quantitative analysis, qualitative analysis that investigates other mandates implemented in a similar timing to the adoption of diabetes mandates suggests that the benefits from these simultaneous mandates are unlikely associated with mortality. See section D in Online Appendix for more details.

[34] This definition of treatment dose is consistent with the discussions in subsection 2.3 that the coverage effects (or the first-stage effects) of diabetes mandates could be extensive beyond non-self-insured private plans.

Now, the coefficient of interest, $\beta$, can be interpreted as a change in the mortality in adoption states relative to non-adoption states after the mandates for a hypothetical adoption state with full coverage (i.e., the fraction insured with any coverage is 100%).

Table A5 in Online Appendix reports the results of estimating equation (3) across different specifications (as was done in Table 2). Column (2) in Table A5 indicates a hypothetical mandate state with 100% insurance coverage for their population would experience a decrease in diabetes-related mortality by 4.083 deaths. This decrease is approximately equivalent to preventing 3.466 diabetes-related deaths for the average adoption state if it is scaled by the post-period mean of the treatment dose (i.e., 84.9%). Figure 5 Panel (a) presents the estimates from the analogous dose-response difference-in-differences event-study specification, illustrating that the results are highly comparable in this alternative empirical strategy.

**Alternative Difference-in-Differences Estimators** Next, I demonstrate the robustness of the main estimates to two alternative estimators, the Sun and Abraham (SA) estimator (Sun and Abraham, 2021) and the stacked event-study (SES) estimator. I consider the SA estimator as the most appropriate approach in my setting because it is specifically tailored to estimate dynamic treatment effects, which are of interest as well as a source of bias, given possible cumulative or lagged effects arising from the nature of diabetes. To complement the SA estimator, I further employed the SES method, which uses a different set of control units (i.e., both not-yet-treated and never-treated units).

In essence, both estimators deal with potential biases arising from bad comparisons between already-treated units (as control groups) and later-treated units (as treatment groups) in a staggered adoption research design (Goodman-Bacon, 2021), by removing such comparisons from the estimation procedure. Specifically, the SA approach estimates cohort-specific treatment effects using only never-treated units as clean controls, while the SES estimator switches the staggered adoption setting to a clean two-group and two-period design by creating cohort-specific 2x2 datasets.[35]

Panel (b) in Figure 5 compares event-study estimates using each of the alternative approaches, along with the baseline estimates from the two-way fixed effects (TWFE) approach. The event-

[35]Cohorts are defined based on (initial) treatment timing.

study coefficient estimates are very similar across all three estimators, which I interpret as reassuring evidence of the reliability of the baseline TWFE estimator in my setting. Online Appendix section E provides simulation analysis results that prove these two estimators perform well in a setting comparable to my empirical environment and describes the two alternative methods with estimating equations in depth.

**Comparison to Medical Literature** I compared the main estimates with those from the medical literature to further probe whether the estimated effects are plausible in two important respects: magnitude and timing. For this comparison, I conducted back-of-the-envelope calculations using my baseline estimates, along with the summary statistics of the prevalence of diagnosed diabetes and the results from the mechanism analysis.[36] Table A6 in Online Appendix compares my estimates (scaled per 100 diabetics) in column (1) with those from diabetes intervention clinical studies in columns (2) through (4).[37] Overall, my estimates fell well within the range of estimated effects from the medical studies. In particular, the magnitude of my estimates was quite consistent with the previous findings in the medical papers studying similar interventions, as shown by a comparison between the first two columns in Table A6 in Online Appendix.

An additional important—but conceptually unrelated to the magnitude—issue is the timing of the appearance of effects. In event-study estimations, I observed that the survival gains from the mandates showed up two years after the mandates' implementation. An extensive review of diabetes clinical studies reveals active management of diabetes could provide health benefits such as improvements in glycemic control, total cholesterol levels, and body mass index within six months, and that, more importantly, such improvements generate reduced mortality rates within one to three years. These studies also find mortality improvements continue and become larger among treated subjects after they first emerge. Thus, the timing and time-varying patterns of the

[36] First, recall that my baseline estimate indicates a decrease in the mortality rate of 3.1 per 100,000 population. Next, the summary statistics displayed in Table 1 show the mean prevalence of diagnosed diabetes is 6.2% (among mandate states). Lastly, the mechanism analysis in Table 3 implies the mandates cause a 6.7% increase in the utilization across all benefits among diabetics. Combining these three parameters suggests the mandates lead to a mortality improvement of 3.1 when 415 ($\simeq 100,000 \times 0.062 \times 0.067$) diabetics are treated, which is equivalent to 0.8 lives saved per 100 diabetics.

[37] Comparison of the results between this paper and diabetes clinical studies requires re-scaling of the estimated effects in the unit of per 100 diabetics rather than per 100,000 population, because both individual samples used in the mechanism analysis and subjects analyzed in the medical literature are diabetics not the general population.

effects are also consistent with those from medical research (Block, y Keenoy and Gaal, 2008; Currie et al., 2012; He et al., 2017; Odnoletkova et al., 2016; Wishah, Al-Khawaldeh and Albsoul, 2015).[38]

**Mean Reversion** Finally, one spurious data pattern that could also lead to the main results is the mean reversion of diabetes-related mortality rates over time. Suppose a state experiences temporary high diabetes deaths due to unexpected shocks and then decides to adopt the mandates to curb this short-term escalation. Because the state's diabetes mortality will revert to the long-run average level, a spurious causality between the adoption of the mandates and mortality may appear by coincidence. To examine this potential concern, I investigated the relationship between the peak year for the mortality and the enactment year to understand whether the timing of these two events was correlated. Figure A4 in Online Appendix shows no evidence of such correlation. In addition, this analysis also shows most mandate states experienced their peaks after rather than prior to the mandates, which further alleviates the concern over the spurious causality.

# 6 Supplemental Analyses

Below, I present the supplemental results. First, I provide evidence about potential mechanisms behind the main results: the higher utilization of healthcare services that are required to cover by the mandates leads to reduced diabetes-related mortality. I then explore heterogeneity in the estimated effects between medication states and non-medication states. Finally, I examine racial disparity in the effects of the mandates on mortality between white and nonwhite groups.

## 6.1 Mechanism Analysis

To explore potential mechanisms behind the main estimates, I investigated whether utilization of the mandated benefits increased more in mandate-to-cover states than in no-mandate states.[39]

[38] As discussed in subsection 2.3, this paper cannot provide a single statistic of the coverage effects (or the first-stage effects) and, hence, the IV estimates—the mortality changes scaled by coverage changes. However, one could interpret the scaled estimate in Table A6 as an analogous IV estimate, assuming that either (1) an increase in utilization appears mostly among those who stand to benefit from the mandates or (2) those who increased their utilization in treating their diabetes are compliers in this setting (Imbens and Angrist, 1994).

[39] I assume that, ex ante, this channel is the most obvious one to drive the main results given the legislative intent of the enactment.

Specifically, I estimated the following regressions relating healthcare service utilization for the treatment of diabetes to the adoption of the mandates:

$$y_{ist} = \beta \times Adopt_s \times \mathbf{1}\left(t - t_s^* > 0\right) + g\left(z_{ist}\right) + \gamma_t + \lambda_s + \epsilon_{ist}. \tag{4}$$

The outcome variable, $y_{ist}$, describes healthcare utilization for the management of diabetes, and $i, s$, and $t$ indicate individual, state of residence, and year, respectively. $g\left(z_{ist}\right)$ controls for individual-level demographics such as gender, race, and age, in addition to the baseline state-level controls. The remaining variables are defined as before. Standard errors in all specifications are clustered at the state level, and all analyses use the BRFSS sampling weights. I estimated this equation among people with diabetes because the mandated benefits are available only to those diagnosed with diabetes.[40]

Table 3 presents the regression results for equation (4). Except for column (2), the dependent variables are indicator variables that take the value of 1 if a respondent takes insulin (column (1)), visits physicians specifically for diabetes treatment (column (3)), and participates in a diabetes-related education program (column (4)).[41] Column (2) uses how many times a respondent checks blood sugar levels daily as the dependent variable. It is worth highlighting that these dependent variables were carefully selected from the BRFSS diabetes module to match the benefit components offered by diabetes mandates.

Across all of the outcome variables, excluding the last column, the results in Table 3 show significant increases in the utilization of medical services for diabetes management among diabetics in mandate states relative to non-mandate states. For example, column (2) indicates that the daily frequency of checking blood sugar levels increases by 0.102, or about a 9.7% ($\frac{0.102}{1.048} \simeq 0.097$) increase over the relevant pre-period mean after the mandates were instituted.[42] I also obtained

[40] As opposed to equation (1), I exclude the enactment year (i.e., $t - t_s^* = 0$) in the post-period indicator in equation (4) to account for the fact that some questions in the BRFSS diabetes module are retrospective per se, or they could reflect respondents' past experiences depending on the interview month for a given survey year. I note that the change to a typical post-period indicator—$\mathbf{1}\left(t - t_s^* \geq 0\right)$—does not materially change the results. In addition, the event-study analysis is independent of this change.

[41] I estimated equation (4) with a linear probability model for these three indicator dependent variables.

[42] The results in column (2) are consistent with Li et al. (2010), who show that diabetes mandates are associated with an increase in the use of diabetes preventive care, such as self-monitoring of blood glucose.

qualitatively similar but quantitatively differential estimates for the other outcome variables: the coefficients relative to the corresponding sample means range from a 2.4% increase (column (3)) to a 12.3% increase (column (1)).

For the education outcome (column (4)), the sign on the coefficient is as expected but not statistically significant, with a p-value of 0.269. One explanation for this is the lack of variation in switching treatment status in the regression for column (4) since the dependent variable only became available in 2000, since which only a couple of states adopted the mandates. Another reason may be that the corresponding survey question asks whether the respondent has participated in diabetes-related education up to the point she/he is interviewed as opposed to for a given period (e.g., during the last year). Due to the nature of the question, more people (mechanically) answered yes to this question in later periods, which could make the regression unpowered.

Figure 6 displays estimates from the flexible version of equation (4). This event-study analysis yields estimates very similar to those from the mean effect specification reported in Table 3. As noted in section 3, each state's health department decides whether or not to administer the module; this implies that unbalanced panel data are relied upon for this analysis. Because the nature of the unbalanced panel data makes consistently interpreting each event-study coefficient difficult due to the differential sample composition across all relative years,[43] the results presented in Figure 6 should be interpreted with caution.

### 6.2 Heterogeneity by Different Benefit Components

Next, I examined the heterogeneous effects of the mandates among mandate-to-cover states, in particular, between medication states and non-medication states. Partially motivated by Klick and Stratmann (2007), who show diabetics in mandate states tended to exhibit a higher increase in body mass index after the passage of the mandates, I hypothesized that those living in medication states would be more likely to substitute medication for healthy behaviors, such as working out or consuming fewer calories. By contrast, non-pharmaceutical approaches, such as self-management training or nutritional advice, can complement—rather than replace—healthy behaviors, thereby

[43] The issue might not be trivial, particularly, when estimating regressions with sampling weights.

reinforcing healthy habits. That is, an ex-ante moral-hazard induced by the mandates could be more severe in medication states, partially crowding out the positive effects of the mandates.

To test this hypothesis, I separately identified the effects of the mandates among different types of mandate states (as depicted in Figure A1 in Online Appendix), by estimating the following specification:

$$MR_{st} = \beta_1 \times Mandate_{st} + \beta_2 \times Mandate_{st} \times NonMed_s + f(X_{st}) + \gamma_t + \lambda_s + \epsilon_{st}, \tag{5}$$

where $Mandate_{st}$ denotes the interaction term between the variables, $Adopt_s$ and $\mathbf{1}(t - t_s^* \geq 0)$.[44] $NonMed_s$ equals 1 if state $s$ belongs to the non-medication type, and the other variables are defined in the same fashion as before.[45] The coefficient of interest is $\beta_2$, which captures the additional effects of the mandates from not providing medication benefits. The sign of this coefficient is of particular interest because it implies which type of state saves more lives in this setting.

Table 4 reports the regression estimates for equation (5). For ease of interpretation, I show the total effects of the mandates for two different types of states ($\beta_1$ for medication states and $\beta_1 + \beta_2$ for non-medication states) in columns (1) and (2), along with the differences between them ($\beta_2 = (\beta_1 + \beta_2) - (\beta_1)$) in columns (3) and (4). The regressions are unweighted in odd columns and weighted by state population in even columns. Standard errors are clustered at the state level in all specifications.

A few patterns are worth noting. First, I found a larger reduction in diabetes-related mortality rates of $-3.940$ (unweighted) or $-3.329$ (weighted) in non-medication states, whereas the reduction was smaller in medication states, with an estimate of $-3.126$ (unweighted) or $-2.996$ (weighted). Second, the negative coefficients of $-0.814$ (unweighted) and $-0.333$ (weighted) in columns (3) and (4) in Table 4, which are the differences in the effects between the two types, are consistent with the original hypothetical prediction, but they are statistically indistinguishable from zero. Overall, I find no clear evidence suggesting the provision of medication generates differential mortality improvements among mandate states in this setting. This finding suggests other

[44] That is, $Mandate_{st} \equiv Adopt_s \times \mathbf{1}(t - t_s^* \geq 0)$. I define the interaction term for a simpler notation.

[45] The medication type includes both full-coverage and medication states, because both types cover medication benefits.

components of the mandates—such as mandated coverage for glucose monitoring equipment, diabetes self-management training, and nutritional therapy—may have been particularly important in driving the estimated effects, with limited moral-hazard effects through the substitution of medication for healthy behaviors.

### 6.3 Racial Disparity in Mortality Improvements

Last, I studied whether the effects of diabetes mandates on mortality differ across racial groups. Specifically, I consider the comparison between the white and nonwhite populations, given the well-documented disparities in insurance coverage and the prevalence of diabetes between the two groups (Keisler-Starkey and Bunch, 2020; CDC, 2020). The form of the analysis is to repeat the estimation of equations (1) and (2) by each racial group, separately.

Figure A5 in Online Appendix reports estimates of the race-specific effects of the mandates on mortality, with the mean effect coefficients being located in the upper right corner of the figure. The mandates reduce diabetes-related mortality for both groups, with an aggregated post-period coefficient of $-4.852$ for nonwhites and $-2.816$ for whites. While the estimates are not statistically distinguishable from one another due to the imprecision of the estimates (particularly for the nonwhite sample), the pattern of the point estimates broadly suggests that the impacts of diabetes mandates on mortality tend to be more substantial for the nonwhite group in the absolute magnitude,[46] and the racial gaps in mortality improvements persist over time.

## 7 Conclusion

This paper investigated the effects of diabetes mandates on diabetes-related mortality rates. To estimate the effects, I leveraged variation in the enactment of the mandates both across states and over time. The main estimates obtained from a difference-in-differences research design indicate approximately 3.1 fewer diabetes-related deaths per 100,000 occur annually in mandate states than in non-mandate states, which is equivalent to a 4.0% reduction in diabetes-related mortality rates

[46] In percent change terms (relative to the baseline mean), the mandates decrease the mortality for nonwhite and white populations by 4.1% and 3.9%, respectively.

relative to the pre-period mean mortality. These estimates imply approximately 9,500 lives are saved annually among the population living in mandate states, or about 10,100 lives if extrapolated nationally. Furthermore, I found these mortality improvements in mandate states were caused by the higher utilization of the mandated medical benefits among people with diabetes in those states.

This work connects to an ongoing policy debate. Recently, lawmakers in some states have debated either repealing diabetes mandates or making them more generous by increasing standard benefit components. For instance, Oregon abrogated its diabetes mandates (introduced in 2001) as of 2018.[47] Because the federal Essential Health Benefits under the Affordable Care Act also include similar diabetes mandates, more discussion has emerged over whether mandated benefits for diabetes improve the health of those that suffer from the disease. This paper informs current policy debates in three ways. First, it provides the first evidence of the effects of the mandates on mortality, which needs to be accounted for in any cost-benefit analysis of the mandates. Second, the findings of the mechanism analysis allow policymakers to understand the reduction in the mortality is likely driven by the achievement of the legislative intent of the mandates—improved management of diabetes through the provision of adequate coverage for necessary items. Finally, the evidence from the heterogeneity analysis can aid in determining what benefit components should be included in diabetes mandates.

One limitation of this paper is that my findings alone could not directly speak to any welfare implications of the mandates because one also needs to consider the costs of the mandates to draw any welfare conclusions. A comprehensive welfare analysis of diabetes mandates remains an important area for future work.[48]

[47] In contrast, three no-mandate states—Alabama, North Dakota, and Ohio—have considered or are considering the adoption of diabetes mandates.

[48] One way to draw a welfare implication of the main findings in this paper is to combine them with the evidence from Bailey (2013) and rely on the model proposed by Summers (1989). Bailey (2013) finds that obese workers fully pay for their own increased health costs caused by diabetes mandates in the form of lower wages, meaning that they fully value the mandated benefits according to the framework from Summers (1989). Under the assumption that the market is perfectly efficient without any information friction, the mandates would not be detrimental to welfare at least among these workers because they already incorporate all the expected value of the mandates, including potential mortality improvements, into their valuation.

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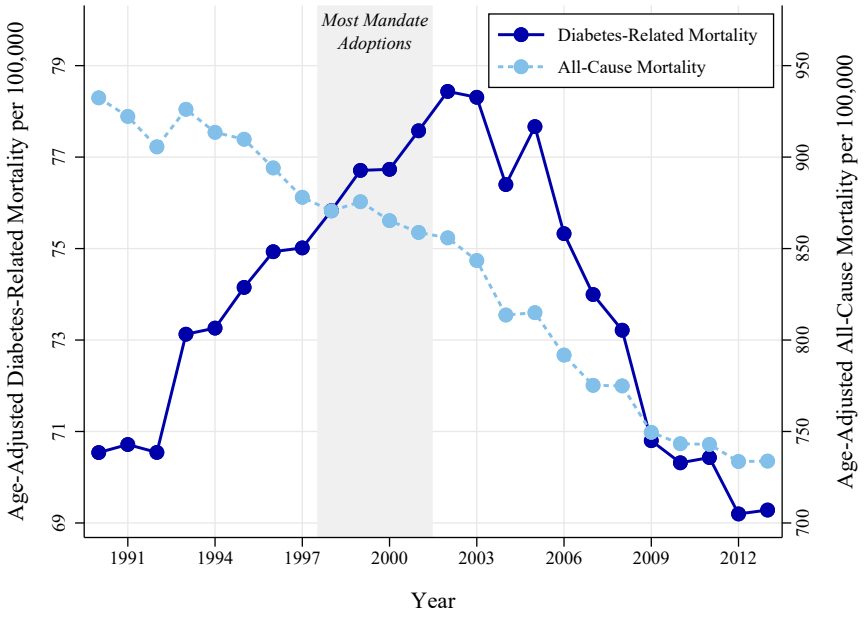
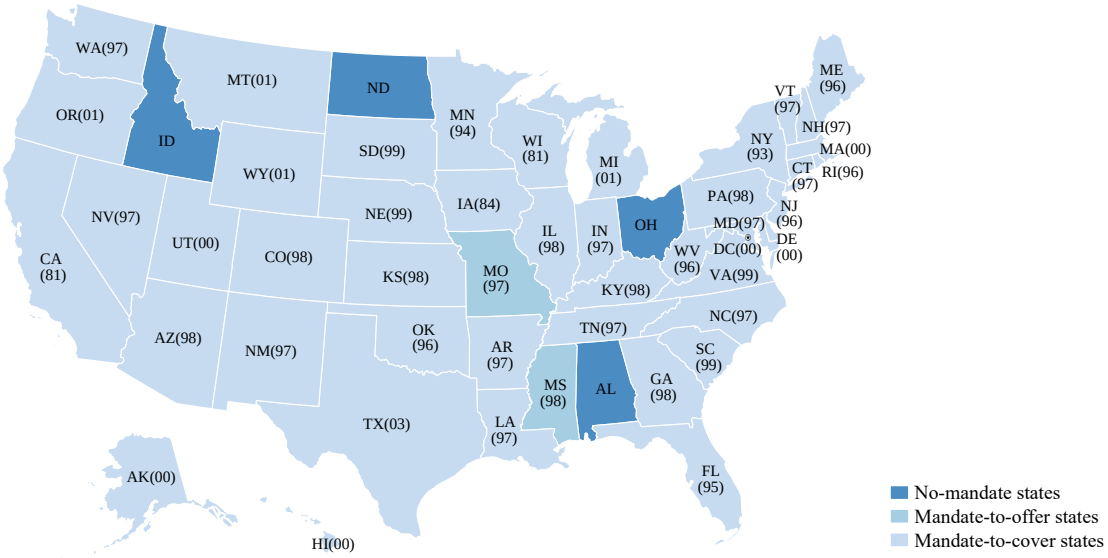
Figure 1: Motivation: Trends of Diabetes-Related Mortality and Adoption Timing of Diabetes Mandates




**NOTES**: The figure above shows trends of age-adjusted cause-specific mortality rates per 100,000 for the US from 1990 to 2013: diabetes-related mortality (dark blue line with left vertical axis) and all-cause mortality (light blue line with right vertical axis). The area shaded in gray represents the period that most states adopted diabetes mandates.

**SOURCES**: State legislatures, the National Conference of State Legislatures, and the Multiple Cause of Death Mortality Data from the National Center for Health Statistics (NCHS, 2021).

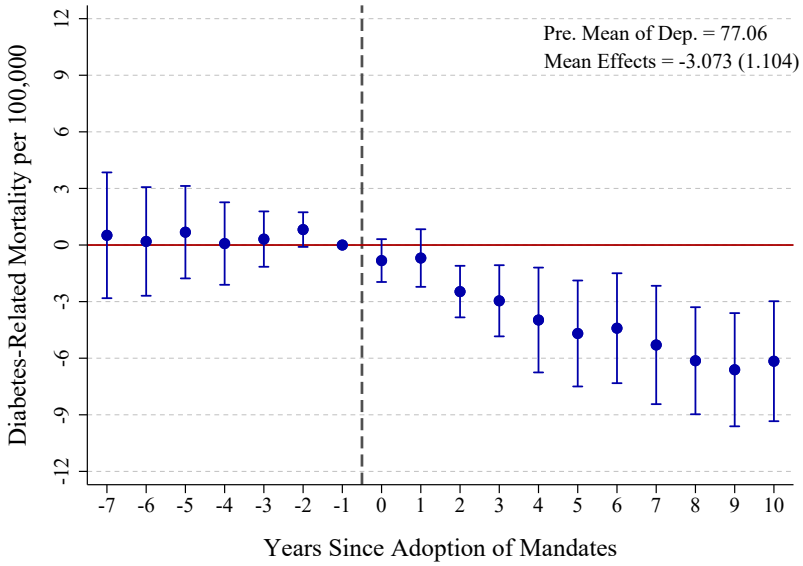
Figure 2: Identifying Variation: Legislative Status of Diabetes Mandates with Enactment Year




**NOTES**: The figure above shows the main identifying variation of the paper, the legislative status of diabetes mandates with enactment years in parentheses. The four states (Alabama, Idaho, North Dakota, and Ohio) do not have any form of the mandates (*No-mandate states* in the figure). State laws in Mississippi and Missouri demand that insurers offer at least one plan in the market that includes coverage of diabetes, but they do not necessarily include coverage in all plans (*Mandate-to-offer states* in the figure). All the other states (44 states + D.C.) require private health insurance plans (or fully insured and directly purchased plans) to cover the cost of treating diabetes for the insured (*Mandate-to-cover states* in the figure). See Table A1 in Online Appendix for more details.

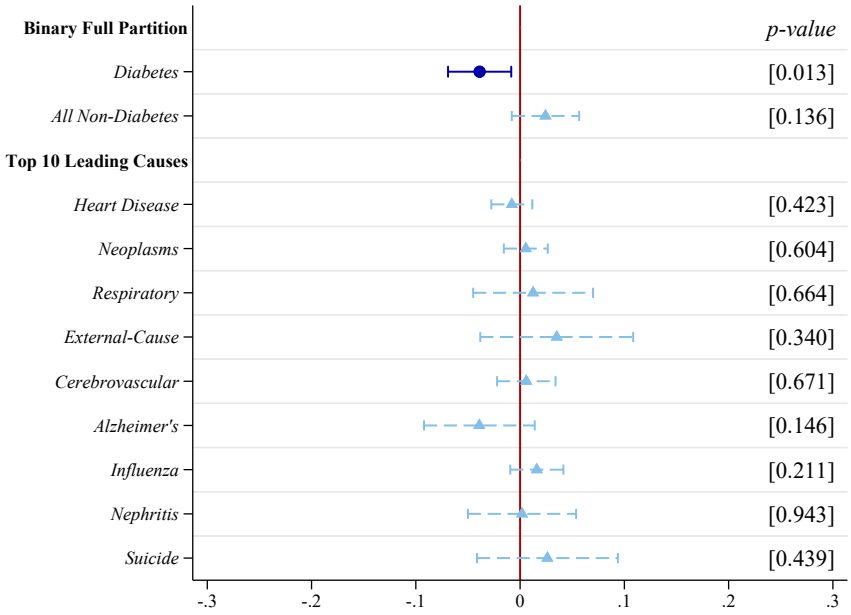
**SOURCES**: State legislatures and the National Conference of State Legislatures.

Figure 3: Main Event-Study Figure: Effects of Diabetes Mandates on Diabetes-Related Mortality




**NOTES**: The figure above plots coefficients on the adoption interacted with time indicators representing the number of years since the adoption of the mandates from a regression of equation (2), along with 95% confidence intervals calculated using standard errors clustered at the state level. The vertical dashed line in gray is plotted to distinguish between periods before and after the mandates. The two aggregated end points, $h = -8$ and $h = 11$, are not displayed in the figure. The dependent variable is the diabetes-related mortality rate per 100,000. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. The regression includes year fixed effects, state fixed effects, and state-level controls. The regression is estimated weighted by state population. The estimated coefficients, standard errors, and p-values can be found in column (1) of Table A3 in Online Appendix.

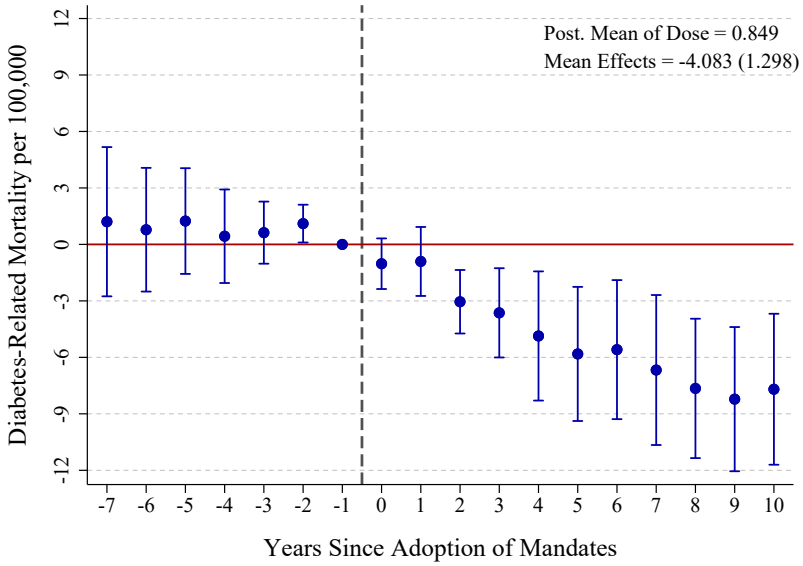
Figure 4: Robustness Analysis: Effects of Diabetes Mandates on Full Partition of All-Cause Mortality



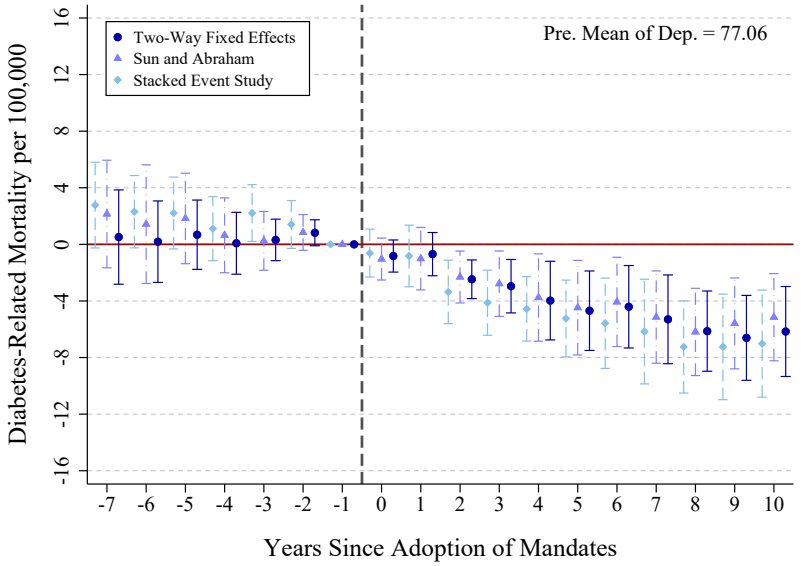

NOTES: The figure above plots coefficients on the adoption interacted with the post-period indicator from separate regressions of equation (1), where the dependent variables are the natural logarithm of cause-specific mortality rates, as indicated in each row. The p-values for each coefficient estimate are displayed in the right corner of the figure. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All regressions include year fixed effects, state fixed effects, and state-level controls. All regressions are estimated weighted by state population. The estimated coefficients, standard errors, and p-values can be found in Table A4 in Online Appendix.

Figure 5: Robustness Analysis: Results from Alternative Empirical Strategies

(a) Robustness to Dose-Response Approach

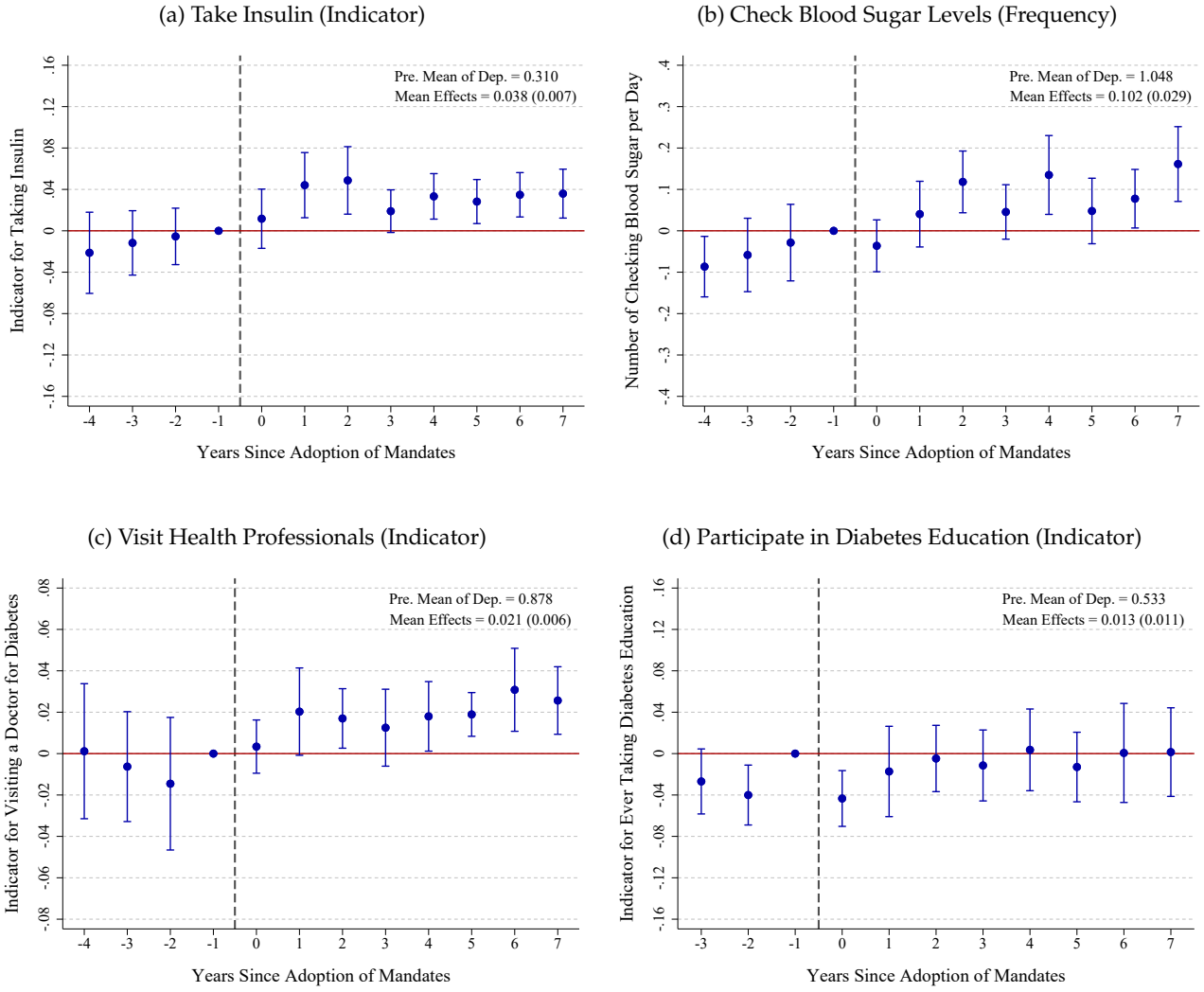



(b) Robustness to Sun and Abraham and Stacked Event-Study Estimators




**NOTES**: The figure above plots coefficients from alternative empirical strategies, along with 95% confidence intervals calculated using standard errors clustered at the state level: an event-study version of the dose-response specification in equation (3) in panel (a) and two alternative difference-in-differences estimators in equations (A2) and (A3) in panel (b). The vertical dashed line in gray is plotted to distinguish between periods before and after the mandates. The two aggregated end points, $h = -8$ and $h = 11$, are not displayed in the figure. The dependent variable is the diabetes-related mortality rate per 100,000. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All regressions are estimated weighted by state population. The estimated coefficients, standard errors, and p-values can be found in columns (2) through (4) of Table A3 in Online Appendix. See section E in Online Appendix for a detailed description of the estimation procedure for the two alternative estimators in panel (b).

Figure 6: Mechanism Analysis: Effects of Diabetes Mandates on Healthcare Utilization for Diabetes Treatment




**NOTES**: The figure above plots coefficients from regressions of an event-study version of equation (4), along with 95% confidence intervals calculated using standard errors clustered at the state level. The vertical dashed line in gray is plotted to distinguish between periods before and after the mandates. The two aggregated end points, $h = -5$ and $h = 8$ ($h = 8$ in panel (d)), are not displayed in the figure. The dependent variables are as indicated in each subpanel. The sample includes individuals with diabetes (i.e., diabetics) who have non-missing values for the indicated dependent variable in the BRFSS diabetes module survey from 1995 to 2010, with the exception of panel (d) (i.e., diabetes education), which covers the years since 2000. All regressions include year fixed effects, state fixed effects, state-level controls, and individual-level controls. All regressions are estimated weighted by the BRFSS sampling weights. The estimated coefficients, standard errors, and p-values can be found in Table A7 in Online Appendix.

Table 1: Summary Statistics

| | All States | | Mandate States | | No-Mandate States | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| **Panel (a): State Level for Main Analysis** | | | | | | |
| Diabetes-Related Mortality per 100,000 | 73.42 | 13.47 | 73.01 | 13.41 | 77.99 | 13.42 |
| Per-capita State Expenditure ($) | 681.57 | 325.23 | 681.38 | 322.60 | 683.81 | 355.21 |
| Number of Physicians per 10,000 | 25.45 | 8.52 | 25.84 | 8.70 | 21.13 | 4.23 |
| Diabetes Prevalence among Adults (%) | 6.22 | 2.10 | 6.21 | 2.07 | 6.31 | 2.44 |
| Obs. | 1,176 | | 1,080 | | 96 | |
| **Panel (b): Individual Level for Mechanism Analysis** | | | | | | |
| Insulin ( = 1 if Take) | 0.279 | 0.449 | 0.278 | 0.448 | 0.292 | 0.454 |
| Obs. | 262,437 | | 240,443 | | 21,994 | |
| Check Blood Sugar (Daily Frequency) | 1.370 | 1.719 | 1.369 | 1.704 | 1.382 | 1.878 |
| Obs. | 258,325 | | 236,652 | | 21,673 | |
| Visit Physicians ( = 1 if Visit) | 0.894 | 0.307 | 0.895 | 0.307 | 0.893 | 0.309 |
| Obs. | 254,260 | | 232,934 | | 21,326 | |
| Diabetes Education ( = 1 if Participate) | 0.545 | 0.498 | 0.544 | 0.498 | 0.557 | 0.497 |
| Obs. | 237,601 | | 217,773 | | 19,828 | |

**NOTES**: The table above reports the summary statistics by observation level: state-level (for main analysis) in panel (a) and individual-level (for mechanism analysis) in panel (b). Mandate states include only mandate-to-cover states. In panel (a), the unit of observation is the state $\times$ year, and the sample is the balanced panel over the years 1990 to 2013. In panel (b), the sample includes individuals with diabetes (i.e., diabetics) who have non-missing values for the indicated variable in the BRFSS diabetes module survey from 1995 to 2010, with the exception of the last variable (i.e, diabetes education), which covers the years since 2000. Both panels (a) and (b) report unweighted statistics. All dollar values are inflation adjusted to 2019 dollars.

**SOURCES**: The National Center for Health Statistics (NCHS, 2021; NCHS, 2016), the Urban-Brookings Tax Policy Center (UBTPC, 2019), and the United States Diabetes Surveillance System (USDSS, 2021) for panel (a). The Centers for Disease Control and Prevention (CDC, 2011) for panel (b).

Table 2: Main Results: Effects of Diabetes Mandates on Diabetes-Related Mortality

| Dependent Variable: | Diabetes-Related Mortality per 100,000 | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Adopt_s \times \mathbf{1}(t - t_s^* \geq 0)$ | -3.897 | -3.073 | -3.355 | -0.039 |
| | (1.537) | (1.104) | (1.184) | (0.015) |
| | [0.015] | [0.008] | [0.007] | [0.013] |
| Main Effects | | | | |
| Year Fixed Effects | x | x | x | x |
| State Fixed Effects | x | x | x | x |
| Control Variables | | x | x | x |
| Weighted | x | x | | x |
| Pre-period Mean of Dep. | 77.06 | 77.06 | 73.50 | 4.33 |
| Dep. Variable | Level | Level | Level | Nat. Log |
| Obs. | 1,176 | | | |

**NOTES**: The table above reports estimates of the coefficient on the adoption interacted with the post-period indicator from regressions of equation (1). The dependent variable is the diabetes-related mortality rate per 100,000. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All specifications include year and state fixed effects. In addition to these controls, regressions in columns (2) to (4) also include the following state-level controls: per-capita state government healthcare expenditure, the number of physicians per 10,000, and the prevalence of diagnosed diabetes among adults. All regressions are estimated weighted by state population, with the exception of column (3). Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

Table 3: Mechanism Analysis: Effects of Diabetes Mandates on Healthcare Utilization for Diabetes Treatment

| Dependent Variable: | Insulin ( = 1 if Take) | Daily Frequency of Checking Blood Sugar | Health Professional ( = 1 if Visit) | Diabetes Education ( = 1 if Participate) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Adopt_s \times \mathbf{1}(t - t_s^* > 0)$ | 0.038 | 0.102 | 0.021 | 0.013 |
| | (0.007) | (0.029) | (0.006) | (0.011) |
| | [< 0.001] | [0.001] | [0.001] | [0.269] |
| Main Effects | | | | |
| Year Fixed Effects | x | x | x | x |
| State Fixed Effects | x | x | x | x |
| Control Variables | | | | |
| State Level Controls | x | x | x | x |
| Individual Level Controls | x | x | x | x |
| Weighted | x | x | x | x |
| Pre-period Mean of Dep. | 0.310 | 1.048 | 0.878 | 0.533 |
| Obs. | 262,437 | 258,325 | 254,260 | 237,601 |

**NOTES**: The table above reports estimates of the coefficient on the adoption interacted with the strict post-period indicator from regressions of equation (4). The dependent variables are as indicated in each column. The sample includes individuals with diabetes (i.e., diabetics) who have non-missing values for the indicated dependent variable in the BRFSS diabetes module survey from 1995 to 2010, with the exception of column (4) (i.e., diabetes education), which covers the years since 2000. All regressions include year fixed effects, state fixed effects, state-level controls, and individual-level controls. All regressions are estimated weighted by the BRFSS sampling weights. Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

Table 4: Comparison of Effects between Non-Medication States and Medication States

| Dependent Variable: | Diabetes-Related Mortality per 100,000 | | | |
|---|---|---|---|---|
| | Total Effects | | Non-Med vs. Med | |
| | (1) | (2) | (3) | (4) |
| $Mandate_{st}$ ($\beta_1$) | -3.126 | -2.996 | | |
| ( = Medication States) | (1.394) | (1.379) | | |
| | [0.030] | [0.035] | | |
| $\beta_1 + \beta_2$ | -3.940 | -3.329 | | |
| ( = Non-Medication States) | (2.334) | (2.124) | | |
| | [0.098] | [0.124] | | |
| $Mandate_{st} \times NonMed_s$ ($\beta_2$) | | | -0.814 | -0.333 |
| ( = Difference) | | | (2.742) | (2.692) |
| | | | [0.768] | [0.902] |
| Main Effects | | | | |
| Year Fixed Effects | x | x | x | x |
| State Fixed Effects | x | x | x | x |
| Control Variables | x | x | x | x |
| Weighted | | x | | x |
| Pre-period Mean of Dep. | 72.77 | 72.00 | 69.17 | 75.22 |
| Obs. | 1,176 | | | |

**NOTES**: The table above reports estimates of the coefficients on the mandates and the interaction between the mandates and non-medication type indicator from regressions of equation (5). The dependent is the diabetes-related mortality rate per 100,000. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All regressions include year fixed effects, state fixed effects, and state-level controls. The regressions are estimated without weights for columns (1) and (3) and estimated with weights by state population for columns (2) and (4). Columns (1) and (2) report the total effects for each type, and columns (3) and (4) report the differences in the effects between the two types (i.e., $\beta_2 = (\beta_1 + \beta_2) - (\beta_1)$). In the row of the pre-period mean of the dependent variable, the means among non-medication type states are reported in odd columns (measured without weights in column (1) and with weights in column (3)), and analogous means for medication type states are reported in even columns. Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

# Do Mandated Health Insurance Benefits for Diabetes Save Lives?

**ONLINE APPENDIX**

Jinyeong Son

## A Underlying Coverage Effects of Diabetes Mandates

In this paper, I focus on estimating the reduced-form effects of the adoption of diabetes mandates on mortality. Below, I provide more detailed discussion of the first-stage effects, which I define as the marginal increase in insurance coverage for diabetes associated with the mandates.

### A.1 Existence of Coverage Effects among Individuals with Non-Self-Insured Plans

I begin by establishing the *existence* of the coverage increase among individuals who have private non-self-insured plans, which is the group of individuals that is, in principle, subject to state-level mandated benefits regulations. I conducted an extensive review of the legislative history of the enactment of diabetes mandates for all 50 states and the District of Columbia, based on various sources. These sources include, but are not limited to, (1) bill analysis or summary reports by the legislative staff, (2) testimonies at related public hearings, (3) internal reports regarding mandated benefits from states' departments of insurance, (4) remarks by the state bill sponsor, and (5) newspaper articles. Below are a few excerpts from each type of source.

- [MN, Bill Summary Report] *"Currently, some insurance companies do not pay for syringes to inject insulin, blood sugar testing strips, and other items diabetics need their health on a daily basis. Some plans provide partial coverage. For instance, a plan will cover insulin, but not the syringes a diabetic administer the drug."*
- [NH, Testimony at Hearing] *"And the doctors warned us that nothing would be covered. But I didn't believe that because we have the top the line Blue Cross/Blue Shield. Of course, it would be covered. We went to get all of these prescriptions filled and nothing was covered. Her syringes were not covered. Her strips were not covered."*
- [PA, Internal Report] *"Council staff estimates that the total cost for the diabetes-related benefits proposed in House Bill 656 would cost approximately $342 million annually if no coverage presently existed ... If one estimates that between 25 percent and 50 percent of diabetics already have access to the proposed benefits ... Therefore, we estimate that the cost of enacting the diabetes portion of House Bill656 may cost between $171 and $256 million annually."*
- [AK, Remarks by Bill Sponsor, then AK House Representative Lisa Murkowski] *"However, many insurance companies have chosen not to follow the good advice, ... about 30 percent of the insurers in the state do not provide for this kind of coverage."*

- [WA, Newspaper Article] *"Right now, insurance coverage for supplies and self-management education classes is spotty across Washington state. ... many insurance companies leave people with diabetes to fend for themselves. Some companies will pay for insulin but not the syringes to inject the insulin."*

This comprehensive evidence clearly suggests coverage of diabetes among private plans was limited at the time of or before the mandates. Table A8 summarizes analogous evidence for 16 states for which I was able to find the relevant information. It is important to emphasize that the absence of evidence for the remaining mandate states should not be interpreted as evidence of the absence of the first-stage effects for those states. In fact, legislative materials are not digitized or searchable for enactments in the 1990s, during which time most of the diabetes mandates came into law. This was the main reason I could not provide evidence for those mandate states.

## A.2 Complexity of Quantifying the Magnitude of Coverage Effects

As discussed in subsection 2.3, quantifying the size of the coverage effects of diabetes mandates through a single statistic is challenging, due to the complex channels through which the mandates may affect coverage and variation in the extent to which these channels are at work across mandate states. Below, I provide more in-depth discussion of each of these channels, along with supporting evidence.

- **Intensive Margin within Non-Self-Insured Plan Holders**: Diabetes mandates may have caused plans with pre-existing diabetes coverage to increase the generosity of this coverage (an *intensive-margin* effect). This intensive-margin effect can arise from various features of the mandates. For instance, diabetes mandates do not allow insurance providers to require an annual deductible or copayment that is greater than an annual deductible or copayment established for other benefits within the plan.[49] In some cases, they also ban insurance providers from placing a benefit ceiling on diabetes treatment.[50] More broadly, partial coverage for the disease could have been reduced because the enactment of the mandates eliminates the ambiguity and inconsistency regarding who is and what services are eligible for reimbursement. Specifically, prior to the mandates, type 2 diabetics—a group that comprises more than 90% of people with diabetes—were often denied coverage for necessary medications and supplies, such as insulin or blood glucose meters, because they are not considered insulin-dependent patients. However, insurers can no longer deny coverage of these benefits to them because their eligibility is clearly stipulated in the laws. All of these requirements

[49] For instance, the provision of the state of California 1367.51 (c) states that the copayments and deductibles for the benefits specified shall not exceed those established for similar benefits within the given plan.

[50] For instance, the provision of the state of Virginia 38.2-3418.10 D states that no insurer, corporation, or health maintenance organization shall impose any policy-year or calendar-year dollar or durational benefit limitations or maximums for benefits or services provided under this section.

within the mandates suggest that those who already had some coverage of diabetes could also have been affected.

- **Medicare beneficiaries**: Many Medicare beneficiaries could be effectively bound by the mandates, because they also hold supplemental *private* coverage through either a prior employer or a purchased Medigap plan. In fact, a very high percentage of Medicare beneficiaries have these private plans: Cabral and Mahoney (2019) show that more than 60% of Medicare beneficiaries carry retiree supplemental insurance or Medigap coverage, or both, using the restricted Medicare Current Beneficiary Survey from 1992 to 2005.[51] These two types of supplemental plans are subject to the mandates in most mandate states.

  Relatedly, the fact that Medicare beneficiaries are part of the population affected by the mandates was acknowledged during the enactment process. For instance, the Pennsylvania Health Care Cost Containment Council, which was requested to review the provisions of House Bill 656 (i.e., its bill for diabetes mandates) by its state congress, reported that *"however, one source estimated that 69% of the over age 65 population also have coverage through private health insurance ... the benefits proposed in House Bill 656 would not cover ... Medicare enrollees without private insurance coverage ... In all, it was estimated that approximately 146,000 people under age 65 and 176,000 people over age 65 would qualify for the benefits mandated by House Bill 656 ... ."*[52]

  Furthermore, the effects of diabetes mandates on Medicare beneficiaries through their supplemental coverage could be sizable in my setting because Medicare did not provide sufficient coverage for diabetes at least until the mid-2000s. For instance, only after 2006 did Medicare cover insulin and associated diabetic supplies (e.g., syringes) through the Part D drug benefit (Ashkenazy and Abrahamson, 2006). Medicare also places caps on the amount of diabetes supplies it will cover and requires a relatively higher copayment.[53] In addition, Zhang et al. (2009) show diabetes patients with private insurance tend to have better quality of care than those with Medicare. Given this limited or partial coverage for diabetes through Medicare during my sample period (1990–2013), some Medicare beneficiaries could have relied on their supplemental private insurance for diabetes treatment.

- **Self-Insured Plan Holders**: Individuals with self-insured plans may also enjoy similar benefits after the mandates through self-compliance behaviors among self-insured firms (Acs et al., 1996; Gutowski, Dicken and Rivera-Lowitt, 1996; Florida Insurance Committee, 2000; Jensen and Morrisey, 1999). That is, once a certain mandated insurance benefit is enacted,

[51] See Table 2 in Cabral and Mahoney (2019). Among all the Medicare beneficiaries, 73.6% are enrolled in traditional Medicare (or fee for service). Out of these fee-for-service beneficiaries, 46.3% have retiree supplemental plans, 47.9% have Medigap plans, and 15.8% have neither. Thus, we obtain $0.736 \times (1 - 0.158) \simeq 0.62$.

[52] The full report can be found here: https://www.phc4.org/reports/mandates/HB656/review_of_house_bill_656.htm.

[53] Medicare beneficiaries typically need to pay 20% of the Medicare-approved amount after the yearly Part B deductible for diabetes equipment and supplies, whereas the coinsurance rate within private insurance plans ranges from zero to 20%.

self-insured firms tend to provide the same, similar, or even better benefits to their employees, although they are not under legal obligation to do so. This behavior can be explained by the fact that self-insured firms compete in attracting workers against fully insured firms in labor markets. In addition, all state and subordinate government entities (e.g., state employee insurance programs or any insurance program of a university system, city, town, county, or school district) are usually obliged to abide by the requirements of their state's diabetes mandates, despite being self-insured employers. I note that state and local governments are one of the biggest self-insured employers, with their employees accounting for more than 10% of the total workforce in the US.

- **Medicaid Program Recipients**: For a handful of mandate states, Medicaid agencies expanded diabetes coverage concurrently with diabetes mandates because the laws included a Medicaid component. For instance, Indiana State Code IC 27-8-14.5-3 states *"As used in this chapter, insurer means any person who provides health insurance and issues health insurance plans in Indiana. The term includes the following: ... (5) The state Medicaid plan ... ."*

Apart from these complicated channels, whether and to what extent each channel is operative are also likely to differ across mandate states, given the differences in legislative details as well as population composition by insurance type in the pre-adoption periods. Due to these difficulties, I do not provide an *unreliable* measure of the coverage effects, and instead focus on estimating the intention-to-treat (ITT) effects of diabetes mandates on mortality.

## B BRFSS Survey Questions in the Diabetes Module

In this section, I describe the specific questions in the BRFSS diabetes module I used to construct the dependent variables for the mechanism analysis. Four dependent variables that closely correspond to the mandated benefits are used in the mechanism analysis: (1) take insulin, (2) check blood sugar levels, (3) visit physicians, and (4) participate in diabetes education. To create each outcome variable, I used the following questions from the module, with the actual variable names in brackets:

- **Take Insulin**: *"Are you now taking insulin?"* [INSULIN]
- **Check Blood Sugar Levels**: *"About how often do you check your blood for glucose or sugar? Include times when checked by a family member or friend, but do not include times when checked by a health professional."* [BLDSUGAR]
- **Visit Physicians**: *"About how many times in the past 12 months have you seen a doctor, nurse, or other health professional for your diabetes?"* [DOCTDIAB]

- **Participate in Diabetes Education**: *"Have you ever taken a course or class in how to manage your diabetes yourself?"* [DIABEDU].

Note that in contrast to the first three variables that have been available since 1994 (i.e., the first year of the module being administered), the last variable (i.e., DIAEDU) has been available only since 2000.

## C Justification for the Use of Multiple Cause of Death Mortality

In this paper, I used diabetes-related mortality that is measured based on multiple cause of death (MCOD) instead of on underlying cause of death (UCOD). The main reason for this choice is that UCOD data suffer from a few significant shortcomings, particularly regarding diabetes research. First, diabetes is much more likely to be underreported as a UCOD than are other diseases, because (1) diabetes is considered a risk factor, as opposed to a *disease* per se (for some physicians), and (2) people with diabetes tend to have other comorbidities (e.g., heart disease). Given these two facts, some physicians are reluctant to identify diabetes as the UCOD due to the wording "*disease* or *injury* that initiated the events resulting in death" on the death certificate. This underreporting practice has been extensively documented in the prior epidemiology literature (McEwen et al., 2006; Will, Vinicor and Stevenson, 2001; McEwen et al., 2011; Stokes and Preston, 2017); for instance, Stokes and Preston (2017) argue diabetes would be the third leading UCOD in the US if this underestimation were corrected.

Second, in contrast to the MCOD section of the death certificate, where a physician lists all significant conditions contributing to death in any order, the UCOD section requires a physician to clarify the chain of events that directly caused the death and to place the UCOD at the end of the chain. However, many physicians make an error in this sequential UCOD reporting system, which leads to a substantial amount of inaccuracy in determining the UCOD, especially in diabetes-related deaths (Lu, Anderson and Kawachi, 2010; Foreman, Naghavi and Ezzati, 2016). Moreover, the rate of incorrect UCOD reporting in diabetes-related death varies widely across states (Cheng, Lu and Kawachi, 2012), and temporal changes exist in the practices of reporting diabetes as an UCOD as well (McEwen et al., 2011). As a result, UCOD data are likely to suffer from measurement errors and incomparability across states over time. In accordance with this concern, the prior medical literature has recommended that researchers use MCOD (as opposed to UCOD) data to characterize diabetes' contribution to mortality more completely and to avoid any potential misinterpretation (Israel, Rosenberg and Curtin, 1986; Park and Peters, 2014; Fedeli et al., 2015; Rodriguez et al., 2019). Based on this ground, I have chosen to use the MCOD mortality in this paper.

# D Qualitative Analysis of Contemporaneous Mandates

To the best of my knowledge, no systematic dataset documents all state-level mandate benefits *with adoption years* for each mandate. Nonetheless, using the Blue Cross Blue Shield Association's report (Laudicina, 2001), which is the most comprehensive source, as far as I am aware, I investigated what other mandates were also implemented one year before and after the year in which each state introduced diabetes mandates. Out of 45 mandate-to-cover states, the top five mandates involve breast reconstruction (24 states), emergency services (24 states), minimum maternity stay (19 states), mental health parity (14 states), and prostate cancer screening (14 states). Although I cannot completely rule out the possibility of spillovers from these simultaneous mandates to diabetes-related mortality, whether and in what direction these contemporaneous mandates would collectively affect the mortality is ex ante ambiguous. For instance, breast reconstruction and maternal minimum-stay benefits are medically unlikely to have an impact on both diabetes and any cause-specific mortality. Furthermore, Klick and Markowitz (2005) show mental health benefits are not effective in reducing mortality (specifically suicide rates). Finally, whether positive or negative interactions occur between prostate cancer and diabetes in the medical literature is unclear (Batty et al., 2010; Yeh et al., 2011; Bensimon et al., 2014; Lee, Giovannucci and Jeon, 2016).

# E Robustness to Alternative Estimators

In this paper, I demonstrate the robustness of the main estimates to two alternative estimators, the Sun and Abraham (SA) estimator and the stacked event-study (SES) estimator. Below, I first provide simulation analysis results that prove these two estimators perform well in a setting comparable to my empirical setting. Then, I explain each estimator in more detail and provide the estimating equation I used to obtain the results shown in Panel (b) of Figure 5.

## E.1 Simulation Analysis

**True Data Generating Process** I begin by validating that the SA and SES estimators work well in a setting that closely mimics my actual setting, even when heterogeneous treatment effects are present.

Specifically, I consider a setting in which we have 50 clusters ($s \in \{1, \ldots, 50\}$) and 24 periods ($t \in \{1, \ldots, 24\}$), and outcomes are generated by the following data generating process (DGP):

$$y_{st} = s + t + \tau_s^t + \varepsilon_{st}, \tag{A1}$$

where $\tau_s^t \equiv (t-8) \times Adopt_s \times \mathbf{1}(t - t_s^* \geq 0)$ and $\varepsilon_{st} \sim \mathrm{N}(0, 1^2)$.

The treatment timing, $t_s^*$, is randomly assigned to each unit $s$, allowing for four never-treated units (recall that four states have never adopted diabetes mandates). It is worth highlighting that

the true treatment effects (i.e., $\tau_s^t$s) are constructed to (1) depend on treatment timing (i.e., heterogeneous across cohorts defined by treatment timing) and (2) increase over time (i.e., dynamic treatment effects). To see this, consider two hypothetical states, $s$ and $s'$, whose treatment timings are given by 10 and 12, respectively (i.e., $t_s^* = 10$ and $t_{s'}^* = 12$). The magnitude of treatment effects in the year of treatment is 2 (i.e., $\tau_s^{10} = 2$) for state $s$ but 4 (i.e., $\tau_{s'}^{12} = 4$) for state $s'$, which implies that the size of treatment effects depends on treatment timing in a way that produces a larger treatment effect for units treated relatively later. It is clear that the treatment effects are dynamic (or increase over time) because they increase by one every year (e.g., $\tau_s^{10} = 2$ in year $t_s^*$ and $\tau_s^{10} = 3$ in year $t_s^* + 1$). Figure A6 Panel (a) displays the *true* average treatment effects (ATE) from the simulation data, which is the target I hope to obtain using the two alternative estimators.

**Simulation Results** Using the simulation data, I implement three estimators (TWFE, SA, and SES estimators) to see how closely each of them captures the true ATE. The simulation results shown in Figure A6 Panel (b) confirm the two alternative methods are effective in estimating the treatment effects, even in the presence of treatment effect heterogeneity and dynamics in a setting with relatively few clusters. By contrast, the TWFE estimator fails to obtain credible estimates from event time 5 and produces spurious upward pre-trends. Based on these results, I conclude that the two alternative approaches are a reliable method for checking robustness to potential issues in a staggered adoption research design, specifically within my empirical setting.

## E.2 Description of Two Alternative Estimators

**Sun and Abraham Estimator** The SA estimator relies on the following two steps: (1) obtain estimates for cohort-specific treatment effects and (2) aggregate these estimates to produce the overall treatment effects.[54] Below, I describe in more detail each step in the context of my empirical setting.

- **Step 1**: I estimate the full set of cohort-specific relative-time treatment effects, denoted by $\beta_{gh}$, by estimating the following equation:

$$MR_{st} = \sum_{g \in \mathcal{G}} \left[ \sum_{\substack{h=-8 \\ h \neq -1}}^{11} \left\{ \beta_{gh} \times Adopt_s \times \mathbf{1}\left(t - t_s^* = h\right) \times \mathbf{1}\left(t_s^* = g\right) \right\} \right] + f(X_{st}) + \gamma_t + \lambda_s + \epsilon_{st}, \tag{A2}$$

  where $\mathcal{G}$ represents a set of adoption years $t_s^*$, and standard errors are clustered at the state level.

- **Step 2**: To generate an aggregated relative-time coefficient (i.e., $\beta_h$), I take a weighted average of the cohort-specific estimates $\beta_{gh}$s obtained in the previous step, with weights equal to the

[54] Sun and Abraham (2021) provide a Stata package (*eventstudyinteract*) for readers to easily implement their estimator.

sample shares of each cohort in the relative period $h$. This weighting process can be written as:

$$\beta_h = \sum_{g \in \mathcal{G}} \left( \frac{N_h^g}{N_h} \right) \beta_{gh},$$

where $N_h$ is the total number of treated states observed for a relative year $h$, and $N_h^g$ is the number of treated states observed for a relative year $h$ within cohort $g$.

There are a few points worth noting regarding the SA estimator. First, the four never-treated states are repeatedly used as clean controls in each cohort-specific treatment effect estimation (i.e., Step 1 above): more generally, the SA estimator always uses never-treated units as effective controls.[55] Second, compared to the TWFE estimator which sometimes involves uninterpretable negative weights, the SA estimator uses an intuitively transparent weighting scheme, though there is no theoretically correct weighting rule. Finally, the SA estimator for each $\beta_h$ is asymptotically normal and consistent in a few standard assumptions, thereby directly constructing its asymptotic standard errors.[56]

**Stacked Event-Study Estimator** The SES estimator also involves the following two steps: (1) create cohort-specific datasets and stack them and (2) use the TWFE approach but with cohort by fixed effects. In my setting, the SES estimator can be implemented as follows.

- **Step 1**: For each $g \in \mathcal{G}$, I first generate a cohort-specific dataset $g$, in which I include all treated states within cohort $g$ and clean control states—both never-treated and not-yet treated states—within the relative-time window in consideration. Then, these cohort-specific datasets are stacked across cohorts, with a dataset identifier $g$.

- **Step 2**: Using the stacked data, the SES estimator regression takes the following TWFE specification with cohort-specific time and unit fixed effects:

$$MR_{st} = \sum_{\substack{h=-8 \\ h \neq -1}}^{11} \{\beta_h \times Adopt_s \times \mathbf{1}(t - t_s^* = h)\} + f(X_{st}) + \gamma_{gt} + \lambda_{gs} + \epsilon_{st}. \tag{A3}$$

There are a few things worth highlighting about the SES estimator. First, the SES estimator has been informally adopted as a remedy to the staggered setting in the applied literature (e.g., Cengiz et al., 2019; Deshpande and Li, 2019), as opposed to formally developed by econometricians. Second, it is efficient relative to the SA estimator in that it relies on a one-step OLS regression, but it suffers from unclear weighting for aggregation in return for this higher efficiency (Baker, Larcker and Wang, 2022).

[55] If no never-treated units exist, the SA estimator uses a last-treated cohort as control units and restricts the analysis period to be before the treated periods for the last-treated cohort.

[56] See Appendix C in Sun and Abraham (2021).

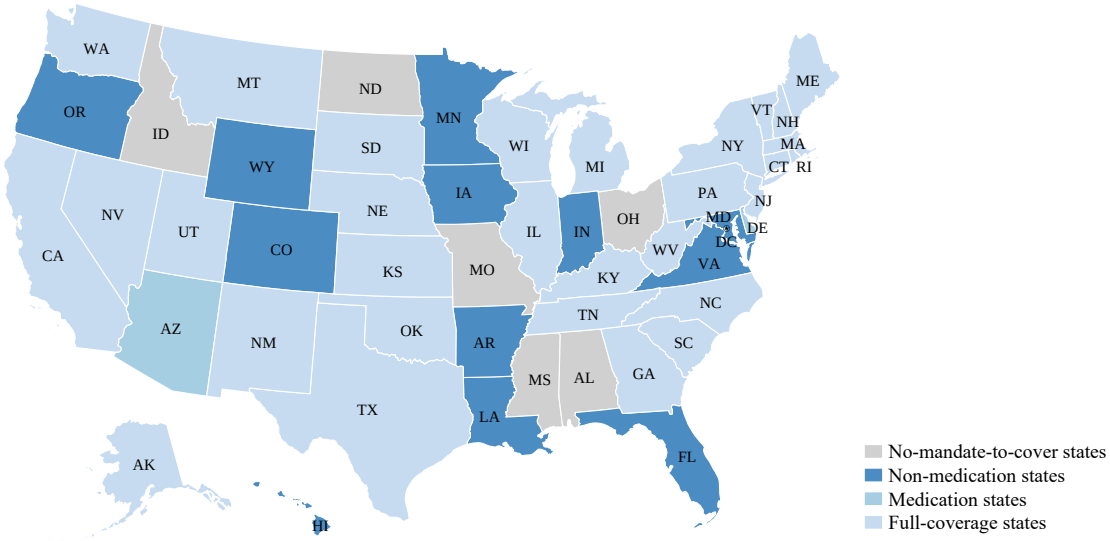
# F Potential Bias from Missing States in BRFSS Survey

As briefly discussed in section 3, a potential concern with the mechanism analysis is that the BRFSS diabetes module data are missing for some states.[57] To address this concern, first I estimated the baseline specification outlined in equation (1) with the indicator variable representing data are missing for state $s$ and year $t$ as the outcome variable. Second, I re-estimated the main regression in Table 2 column (2), excluding the observations to imitate the same missing structure as in the BRFSS diabetes module.[58] According to Table A9, the missing data are uncorrelated with mandate adoption, and the results from imitating the missing data structure are similar to the main estimates. These findings alleviate concerns that missing data may confound the mechanism analysis.

[57] Roughly 20% of the module data are missing at the state-year level.

[58] If a state $s$ is missing in year $t$ in the BRFSS diabetes module, I dropped the observation for the same state $s$ and the same year $t$ in this regression.

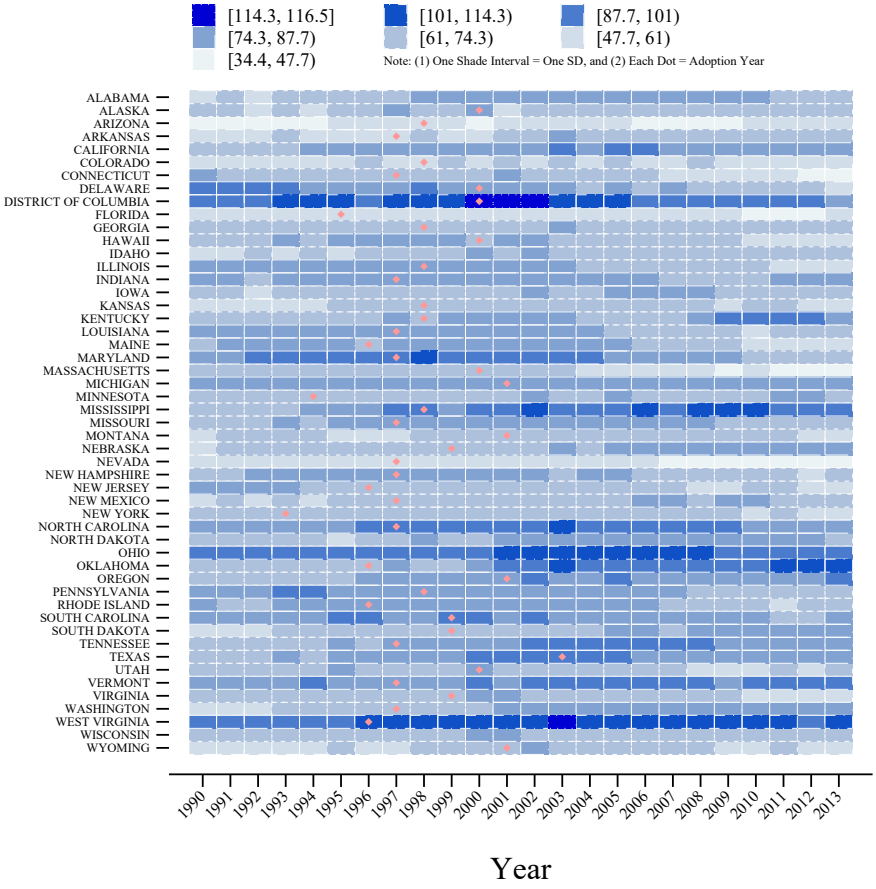
Figure A1: Different Benefit Components across Mandate-to-Cover States




**NOTES**: The figure above shows variation in benefit components across mandate-to-cover states. Mandated benefits for diabetes consist of three key components: medication, equipment, and self-management education. All mandate-to-cover states are classified into three types: medication, non-medication, and full-coverage states. Medication states refer to those that provide coverage for medication but not self-management education. By contrast, non-medication states are those that provide the benefits of self-management education but not medication. Full-coverage states cover both medication and non-medication benefits. No-mandate-to-cover states (states shaded in gray in the figure) include two mandate-to-offer states and four no-mandate states. See Table A1 for more details.

**SOURCES**: State legislatures and the National Conference of State Legislatures.

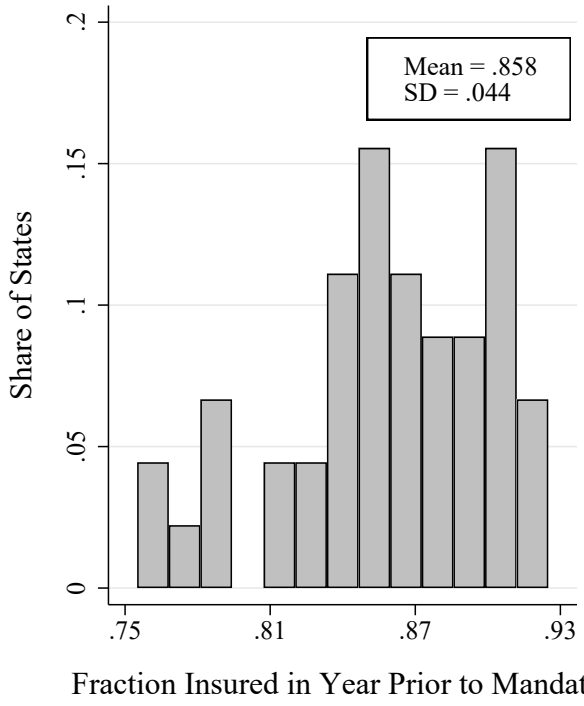
Figure A2: Visualization of Entire Data of Diabetes-Related Mortality




**NOTES**: The figure above shows age-adjusted diabetes-related mortality rates per 100,000 (the key dependent variable in the paper) for all states and D.C. from 1990 to 2013 (main analysis period). The range in each shade of color equals one standard deviation of the weighted distribution of the mortality. Each red dot represents the adoption year of the mandates for each state whose adoption year is 1990 or later.

**SOURCES**: The Multiple Cause of Death Mortality Data from the National Center for Health Statistics (NCHS, 2021).

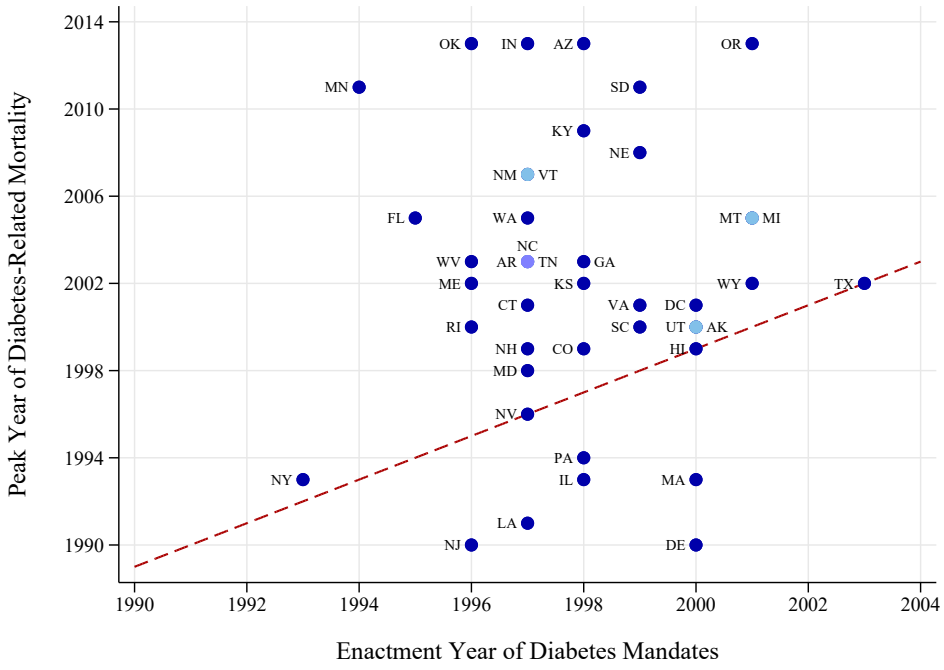
Figure A3: Variation in Dose Variable: Fraction of the Insured with Any Insurance Coverage




**NOTES**: The figure above shows variation in the dose variable (i.e., $D_{(s,t_s^*-1)}$) for estimating equation (3), which is the fraction of the insured with any type of insurance coverage in the year immediately prior to the adoption of the mandates.

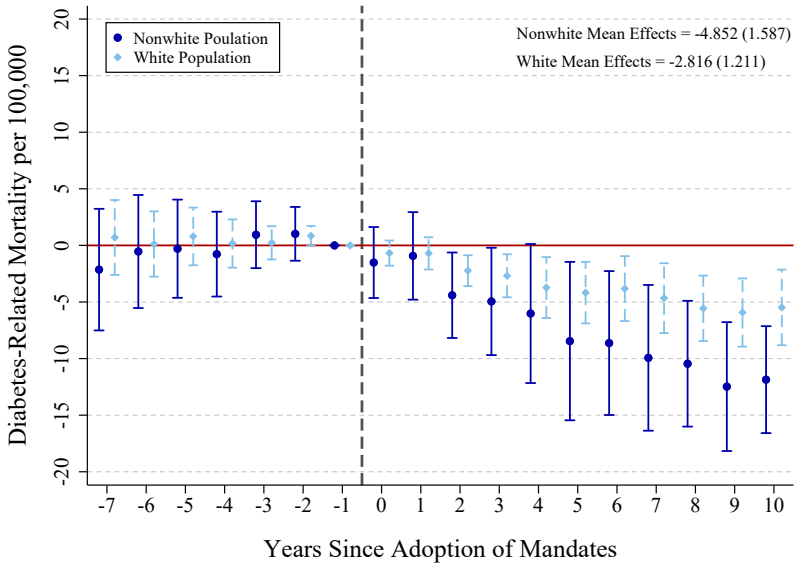
**SOURCES**: The Health Insurance Historical Tables from the United States Bureau of the Census (Census, 2021(a); Census, 2021(b); Census, 2021(c)).

Figure A4: Robustness Analysis: Mean Reversion




**NOTES**: The figure above shows the relationship between peak years for diabetes-related mortality and enactment years of diabetes mandates among all mandate-to-cover states, excluding three states that adopted the mandates before 1990. The red dashed line represents the graph of points given by the function, $peak\ year(vertical\ axis) = enactment\ year(horizontal\ axis) - 1$, to distinguish between states that experienced their peak in the mortality before and after the mandates. For some points where multiple states are placed, different colors are used.

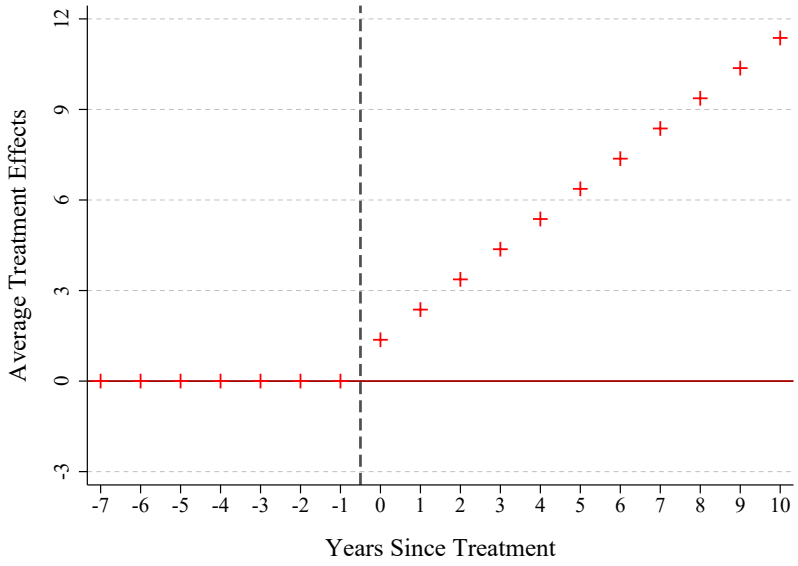
Figure A5: Racial Disparity: Heterogeneous Effects of Diabetes Mandates on Diabetes-Related Mortality by Race



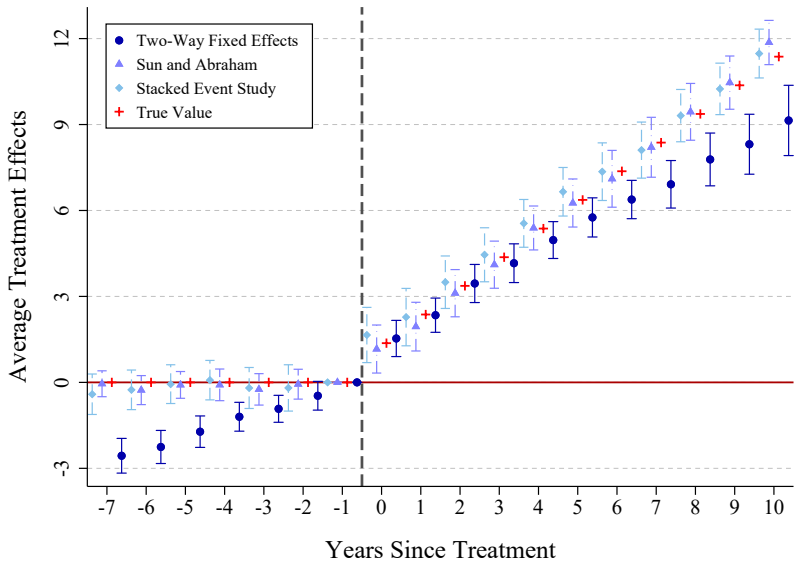

**NOTES**: The figure above plots coefficients on the adoption interacted with time indicators representing the number of years since the adoption of the mandates from separate regressions of equation (2) by race (i.e., white and nonwhite populations), along with 95% confidence intervals calculated using standard errors clustered at the state level. The vertical dashed line in gray is plotted to distinguish between periods before and after the mandates. The two aggregated end points, $h = -8$ and $h = 11$, are not displayed in the figure. The dependent variable is the diabetes-related mortality rate per 100,000 for each racial group. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All regressions include year fixed effects, state fixed effects, and state-level controls. All regressions are estimated weighted by state population for their corresponding racial group.

Figure A6: Simulation Analysis: True Data Generating Process and Performance of Three Estimators

(a) True Data Generating Process




(b) Performance of Three Estimators




**NOTES**: The figure above plots the true average treatment effects from the underlying data generating process outlined in equation (A1) in panel (a) and estimated event-study coefficients from three different estimators (i.e., baseline two-way fixed effects, Sun and Abraham, and Stacked Event-Study estimators) using the simulated data in panel (b).

Table A1: Detailed Legislative Status of Diabetes Mandates

| State | Medication | Equipment | Education | Type | Enactment Date | Enactment Year |
|---|---|---|---|---|---|---|
| AL | N/A | N/A | N/A | No mandate-to-cover | N/A | N/A |
| AK | 7/27/2000 | 7/27/2000 | 7/27/2000 | Full-coverage | 4/28/2000 | 2000 |
| AZ | 1/1/1999 | 1/1/1999 | N/A | Medication | 5/11/1998 | 1998 |
| AR | N/A | 8/1/1997 | 8/1/1997 | Non-medication | 4/9/1997 | 1997 |
| CA | 1/1/2000 | 1/1/2000 | 1/1/1982 | Full-coverage | 9/24/1981 | 1981 |
| CO | N/A | 7/1/1998 | 7/1/1998 | Non-medication | 4/13/1998 | 1998 |
| CT | 10/1/1997 | 10/1/1997 | 1/1/2000 | Full-coverage | 6/26/1997 | 1997 |
| DE | 9/29/2000 | 9/29/2000 | N/A | Medication | 6/30/2000 | 2000 |
| DC | N/A | 1/20/2001 | 1/20/2001 | Non-medication | 7/26/2000 | 2000 |
| FL | N/A | 7/1/1995 | 7/1/1995 | Non-medication | 6/14/1995 | 1995 |
| GA | 7/1/1998 | 7/1/1998 | 7/1/1998 | Full-coverage | 4/6/1998 | 1998 |
| HI | N/A | 7/1/2001 | 7/1/2001 | Non-medication | 6/19/2000 | 2000 |
| ID | N/A | N/A | N/A | No mandate-to-cover | N/A | N/A |
| IL | 1/1/1999 | 1/1/1999 | 1/1/1999 | Full-coverage | 8/13/1998 | 1998 |
| IN | N/A | 1/1/1998 | 1/1/1998 | Non-medication | 4/16/1997 | 1997 |
| IA | N/A | 7/1/1999 | 7/1/1984 | Non-medication | 5/16/1984 | 1984 |
| KS | 1/1/1999 | 1/1/1999 | 1/1/1999 | Full-coverage | 5/13/1998 | 1998 |
| KY | 7/15/1998 | 7/15/1998 | 7/15/1998 | Full-coverage | 4/9/1998 | 1998 |
| LA | N/A | 1/1/1998 | 1/1/1998 | Non-medication | 7/10/1997 | 1997 |
| ME | 7/4/1996 | 7/4/1996 | 7/4/1996 | Full-coverage | 4/2/1996 | 1996 |
| MD | N/A | 1/1/1998 | 1/1/1998 | Non-medication | 4/29/1997 | 1997 |
| MA | 8/2/2000 | 8/2/2000 | 8/2/2000 | Full-coverage | 5/4/2000 | 2000 |
| MI | 3/28/2001 | 3/28/2001 | 3/28/2001 | Full-coverage | 1/9/2001 | 2001 |
| MN | N/A | 8/1/1994 | 8/1/1997 | Non-medication | 4/29/1994 | 1994 |
| MS | N/A | 1/1/1999 | 1/1/1999 | No mandate-to-cover | 3/26/1998 | 1998 |
| MO | N/A | 1/1/1998 | 1/1/1998 | No mandate-to-cover | 7/1/1997 | 1997 |
| MT | 1/1/2002 | 1/1/2002 | 1/1/2002 | Full-coverage | 4/30/2001 | 2001 |
| NE | 10/1/1999 | 10/1/1999 | 10/1/1999 | Full-coverage | 4/28/1999 | 1999 |
| NV | 1/1/1998 | 1/1/1998 | 1/1/1998 | Full-coverage | 6/30/1997 | 1997 |
| NH | 1/1/1998 | 1/1/1998 | 1/1/1998 | Full-coverage | 6/19/1997 | 1997 |
| NJ | 1/5/1996 | 1/5/1996 | 1/5/1996 | Full-coverage | 1/5/1996 | 1996 |
| NM | 1/1/1998 | 1/1/1998 | 1/1/1998 | Full-coverage | 3/15/1997 | 1997 |
| NY | 1/1/1994 | 1/1/1994 | 1/1/1994 | Full-coverage | 7/21/1993 | 1993 |
| NC | 10/1/1997 | 10/1/1997 | 10/1/1997 | Full-coverage | 6/26/1997 | 1997 |
| ND | N/A | N/A | N/A | No mandate-to-cover | N/A | N/A |
| OH | N/A | N/A | N/A | No mandate-to-cover | N/A | N/A |
| OK | 11/1/1996 | 11/1/1996 | 11/1/1996 | Full-coverage | 4/23/1996 | 1996 |
| OR | N/A | 1/1/2002 | 1/1/2002 | Non-medication | 7/4/2001 | 2001 |
| PA | 2/13/1999 | 2/13/1999 | 2/13/1999 | Full-coverage | 10/16/1998 | 1998 |
| RI | 1/1/1997 | 1/1/1997 | 1/1/1997 | Full-coverage | 7/25/1996 | 1996 |
| SC | 1/1/2000 | 1/1/2000 | 1/1/2000 | Full-coverage | 6/11/1999 | 1999 |
| SD | 7/1/1999 | 7/1/1999 | 7/1/1999 | Full-coverage | 3/17/1999 | 1999 |
| TN | 1/1/1998 | 1/1/1998 | 1/1/1998 | Full-coverage | 5/30/1997 | 1997 |
| TX | 4/1/2005 | 4/1/2005 | 4/1/2005 | Full-coverage | 6/21/2003 | 2003 |
| UT | 7/1/2000 | 7/1/2000 | 7/1/2000 | Full-coverage | 3/14/2000 | 2000 |
| VT | 10/1/1997 | 10/1/1997 | 10/1/1997 | Full-coverage | 5/5/1997 | 1997 |
| VA | N/A | 7/1/1999 | 7/1/1999 | Non-medication | 3/4/1999 | 1999 |
| WA | 1/1/1998 | 1/1/1998 | 1/1/1998 | Full-coverage | 5/7/1997 | 1997 |
| WV | 6/7/1996 | 6/7/1996 | 6/7/1996 | Full-coverage | 4/1/1996 | 1996 |
| WI | 3/31/1988 | 11/19/1981 | 5/9/1984 | Full-coverage | 11/19/1981 | 1981 |
| WY | N/A | 7/1/2001 | 7/1/2001 | Non-medication | 2/28/2001 | 2001 |

**NOTES**: The table above reports the enactment dates of diabetes mandates and the effective dates of mandatory coverage of each benefit component for all states and D.C. If a state adopts each component in different years (e.g., California), the enactment year is defined as the state's earliest year introducing any benefit components. The enactment dates can be different from the effective dates (in component columns) for some states, because they may have specified delayed effective dates. All mandate-to-cover states are classified into three types: medication, non-medication, and full-coverage states. See the text for this classification. Empty cells (with N/A) indicate the corresponding component is not required to cover or is irrelevant for a given state. Figure 2 in the paper and Figure A1 in Online Appendix are based on the columns of enactment year and type, respectively.

**SOURCES**: State legislatures and the National Conference of State Legislatures.

Table A2: Summary Statistics: Baseline Periods (1990 to 1993)

| | Early States | | Later States | | No-Mandate States | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Diabetes-Related Mortality per 100,000 | 71.40 | 11.32 | 69.13 | 12.44 | 69.85 | 14.70 |
| Per-capita State Expenditure ($) | 547.86 | 198.34 | 602.55 | 290.94 | 562.38 | 271.39 |
| Number of Physicians per 10,000 | 22.59 | 5.26 | 22.34 | 9.88 | 17.63 | 3.38 |
| Diabetes Prevalence among Adults (%) | 3.66 | 0.55 | 3.62 | 0.86 | 3.31 | 0.78 |
| Obs. | 80 | | 88 | | 16 | |

**NOTES**: The table above reports the summary statistics for the state-level variables used in the main analysis. The unit of observation is the state $\times$ year, and the sample is the balanced panel over the years 1990 to 1993. Note that no state adopted diabetes mandates until 1993, with the exception of three always-treated states (California, Iowa, and Wisconsin) that are excluded from the table. Among mandate-to-cover states, early states implemented the mandates in 1997 or before, whereas later states implemented the mandates in 1998 or after. All dollar values are inflation adjusted to 2019 dollars.

**SOURCES**: The National Center for Health Statistics (NCHS, 2021; NCHS, 2016), the Urban-Brookings Tax Policy Center (UBTPC, 2019), and the United States Diabetes Surveillance System (USDSS, 2021).

Table A3: Robustness Analysis: Comparison to Alternative Empirical Strategies

| Dependent Variable: | Diabetes-Related Mortality per 100,000 | | | |
|---|---|---|---|---|
| | Baseline TWFE | Dose Specification | Sun and Abraham | Stacked Event-Study |
| | (1) | (2) | (3) | (4) |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -7)$ | 0.516 | 1.206 | 2.147 | 2.774 |
| | (1.658) | (1.971) | (1.940) | (1.525) |
| | [0.757] | [0.544] | [0.274] | [0.072] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -6)$ | 0.190 | 0.780 | 1.430 | 2.306 |
| | (1.432) | (1.634) | (2.139) | (1.286) |
| | [0.895] | [0.635] | [0.507] | [0.076] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -5)$ | 0.681 | 1.241 | 1.829 | 2.217 |
| | (1.219) | (1.396) | (1.631) | (1.279) |
| | [0.579] | [0.379] | [0.268] | [0.086] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -4)$ | 0.078 | 0.434 | 0.639 | 1.110 |
| | (1.088) | (1.236) | (1.350) | (1.140) |
| | [0.943] | [0.727] | [0.638] | [0.333] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -3)$ | 0.315 | 0.625 | 0.247 | 2.214 |
| | (0.730) | (0.821) | (1.061) | (1.010) |
| | [0.668] | [0.450] | [0.817] | [0.031] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -2)$ | 0.819 | 1.106 | 0.841 | 1.409 |
| | (0.457) | (0.499) | (0.645) | (0.852) |
| | [0.079] | [0.032] | [0.199] | [0.101] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 0)$ | -0.823 | -1.025 | -1.039 | -0.617 |
| | (0.565) | (0.669) | (0.755) | (0.853) |
| | [0.151] | [0.132] | [0.175] | [0.471] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 1)$ | -0.689 | -0.906 | -1.011 | -0.818 |
| | (0.759) | (0.911) | (1.127) | (1.096) |
| | [0.369] | [0.325] | [0.374] | [0.457] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 2)$ | -2.468 | -3.045 | -2.305 | -3.361 |
| | (0.680) | (0.839) | (0.936) | (1.130) |
| | [0.001] | [0.001] | [0.017] | [0.004] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 3)$ | -2.956 | -3.635 | -2.779 | -4.125 |
| | (0.938) | (1.179) | (1.181) | (1.159) |
| | [0.003] | [0.003] | [0.023] | [0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 4)$ | -3.974 | -4.864 | -3.758 | -4.557 |
| | (1.382) | (1.706) | (1.576) | (1.147) |
| | [0.006] | [0.006] | [0.021] | [< 0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 5)$ | -4.689 | -5.818 | -4.477 | -5.241 |
| | (1.397) | (1.772) | (1.705) | (1.367) |
| | [0.002] | [0.002] | [0.012] | [< 0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 6)$ | -4.409 | -5.589 | -4.069 | -5.573 |
| | (1.449) | (1.837) | (1.610) | (1.608) |
| | [0.004] | [0.004] | [0.015] | [0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 7)$ | -5.298 | -6.673 | -5.141 | -6.163 |
| | (1.560) | (1.982) | (1.666) | (1.862) |
| | [0.001] | [0.002] | [0.003] | [0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 8)$ | -6.133 | -7.650 | -6.189 | -7.248 |
| | (1.411) | (1.841) | (1.574) | (1.642) |
| | [< 0.001] | [< 0.001] | [< 0.001] | [< 0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 9)$ | -6.609 | -8.220 | -5.585 | -7.246 |
| | (1.493) | (1.905) | (1.639) | (1.879) |
| | [< 0.001] | [< 0.001] | [0.001] | [< 0.001] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 10)$ | -6.158 | -7.693 | -5.147 | -7.018 |
| | (1.581) | (1.994) | (1.572) | (1.906) |
| | [< 0.001] | [< 0.001] | [0.002] | [< 0.001] |
| Main Effects | | | | |
| Year Fixed Effects | x | x | x | x |
| State Fixed Effects | x | x | x | x |
| Control Variables | x | x | x | x |
| Weighted | x | x | x | x |
| Pre-period Mean of Dep. | | | 77.06 | |
| Post-period Mean of Dose | - | 0.849 | - | - |
| Obs. | 1,176 | 1,176 | 1,176 | 2,232 |

**NOTES**: The table above reports estimates of event-time indicator coefficients from four different estimating equations: column (1) from equation (2), column (2) from an event-study version of the dose-response specification in equation (3), column (3) from equation (A2), and column (4) from equation (A3). The two aggregated end points, $h = -8$ and $h = 11$, are not reported in the table. The dependent variable is the diabetes-related mortality rate per 100,000. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All regressions are estimated weighted by state population. Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

Table A4: Robustness Analysis: Effects of Diabetes Mandates on Full Partition of All-Cause Mortality

| Dependent Variable: | Log Value of Cause-Specific Mortality Rate per 100,000 | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Diabetes | Non-diabetes | Heart | Neoplasms | Respiratory | External | Cerebrovascular | Alzheimer's | Influenza | Nephritis | Suicide |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| $Adopt_s \times \mathbf{1}(t - t_s^* \geq 0)$ | -0.039 | 0.024 | -0.008 | 0.005 | 0.012 | 0.035 | 0.006 | -0.039 | 0.016 | 0.002 | 0.026 |
| | (0.015) | (0.016) | (0.010) | (0.010) | (0.029) | (0.036) | (0.014) | (0.026) | (0.013) | (0.026) | (0.034) |
| | [0.013] | [0.136] | [0.423] | [0.604] | [0.664] | [0.340] | [0.671] | [0.146] | [0.211] | [0.943] | [0.439] |
| Main Effects | | | | | | | | | | | |
| Year Fixed Effects | x | x | x | x | x | x | x | x | x | x | x |
| State Fixed Effects | x | x | x | x | x | x | x | x | x | x | x |
| Control Variables | x | x | x | x | x | x | x | x | x | x | x |
| Weighted | x | x | x | x | x | x | x | x | x | x | x |
| Pre-period Mean of Dep. | 4.33 | 6.73 | 5.96 | 5.42 | 4.48 | 3.28 | 4.60 | 3.22 | 4.03 | 4.00 | 2.46 |
| Obs. | | | | | | 1,176 | | | | | |

**NOTES**: The table above reports estimates of the coefficient on the adoption interacted with the post-period indicator from separate regressions of equation (1). The dependent variables are the natural logarithm of cause-specific mortality rates per 100,000: all causes of death outside of diabetes-related causes (column (2)), disease of heart (column (3), ICD-10 codes; I00–I09, I11, I13, I20–I51), malignant neoplasms (column (4), ICD-10 codes; C00–C97), chronic lower respiratory diseases (column (5), ICD-10 codes; J40–J47), external-cause (column (6), ICD-10 codes; V01-V99, Y85, U01-U02, X85-Y09, Y87.1, Y35, Y89.0, Y10-Y34, Y87.2, Y89.9, Y36, Y89.1), cerebrovascular diseases (column (7), ICD-10 codes; I60–I69), Alzheimer's disease (column (8), ICD-10 codes; G30), influenza and pneumonia (column (9), ICD-10 codes; J09–J18), nephritis, nephrotic syndrome and nephrosis (column (10), ICD-10 codes; N00–N07, N17–N19, N25–N27), suicide (column (11), ICD-10 codes; X60–X84, Y87). The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All regressions include year fixed effects, state fixed effects, and state-level controls. All regressions are estimated weighted by state population. Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

Table A5: Robustness Analysis: Effects of Diabetes Mandates on Diabetes-Related Mortality from Dose-Response Specification

| Dependent Variable: | Diabetes-Related Mortality per 100,000 | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $D_{(s,t_s^*-1)} \times Adopt_s \times \mathbf{1}(t - t_s^* \geq 0)$ | -5.336 | -4.083 | -4.337 | -0.052 |
| | (1.696) | (1.298) | (1.437) | (0.017) |
| | [0.003] | [0.003] | [0.004] | [0.004] |
| Main Effects | | | | |
| Year Fixed Effects | x | x | x | x |
| State Fixed Effects | x | x | x | x |
| Control Variables | | x | x | x |
| Weighted | x | x | | x |
| Pre-period Mean of Dep. | 77.06 | 77.06 | 73.50 | 4.33 |
| Post-period Mean of Dose | | | 0.849 | |
| Dep. Variable | Level | Level | Level | Nat. Log |
| Obs. | | | 1,176 | |

**NOTES**: The table above reports estimates of the coefficient on the adoption interacted with the post-period indicator and dose variable from regressions of equation (3). The dependent variable is the diabetes-related mortality rate per 100,000. The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1990 to 2013. All specifications include year and state fixed effects. In addition to these controls, regressions in columns (2) to (4) also include the following state-level controls: per-capita state government healthcare expenditure, the number of physicians per 10,000, and the prevalence of diagnosed diabetes among adults. All regressions are estimated weighted by state population, with the exception of column (3). Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

Table A6: Robustness Analysis: Comparison to Medical Literature

| Source Literature | This Paper (1) | Brown (2) | Currie et al. (3) | Odnoletkova et al. (4) |
|---|---|---|---|---|
| Life Savings (per 100 diabetics) | 0.8 | 0.7 ~ 1.7 | 1.22 | 0.69 |
| Treatment | Insurance Coverage of Diabetes | Comprehensive Care Programs | Take Medication | Participate in Diabetes Education |

**NOTES**: The table above compares the main estimates in this paper with those from medical literature. Column (1) reports the main results of this paper in a scaled measure, i.e., reduced mortality *per 100 diabetics* who increased their utilization in the mandated benefits. See footnote 36 in the text for this calculation. In column (2), comprehensive care programs include pharmaceutical treatment, training in self-monitoring of glucose concentrations, and diabetes education. In column (3), Currie et al. (2012) compare compliant diabetics who take medications for diabetes as prescribed with non-compliant diabetics who frequently fail to take the medications. In column (4), education programs help diabetics identify the gaps in the management of several diabetes risk factors and provide them strategies to close the gaps, such as lifestyle adjustments. Both the treatment and control groups in Odnoletkova et al. (2016) take some diabetes medications.

Table A7: Mechanism Analysis: Effects of Diabetes Mandates on Healthcare Utilization for Diabetes Treatment

| Dependent Variable: | Insulin ( = 1 if Take) | Daily Frequency of Checking Blood Sugar | Health Professional ( = 1 if Visit) | Diabetes Education ( = 1 if Participate) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -4)$ | -0.021 | -0.087 | 0.001 | - |
| | (0.020) | (0.036) | (0.016) | - |
| | [0.282] | [0.021] | [0.943] | - |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -3)$ | -0.012 | -0.058 | -0.006 | -0.027 |
| | (0.015) | (0.044) | (0.013) | (0.016) |
| | [0.454] | [0.192] | [0.636] | [0.092] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = -2)$ | -0.005 | -0.029 | -0.015 | -0.040 |
| | (0.014) | (0.046) | (0.016) | (0.014) |
| | [0.692] | [0.538] | [0.366] | [0.008] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 0)$ | 0.012 | -0.036 | 0.003 | -0.043 |
| | (0.014) | (0.031) | (0.006) | (0.013) |
| | [0.416] | [0.250] | [0.599] | [0.002] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 1)$ | 0.044 | 0.040 | 0.020 | -0.017 |
| | (0.016) | (0.039) | (0.010) | (0.022) |
| | [0.007] | [0.312] | [0.059] | [0.431] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 2)$ | 0.049 | 0.118 | 0.017 | -0.005 |
| | (0.016) | (0.037) | (0.007) | (0.016) |
| | [0.004] | [0.003] | [0.022] | [0.768] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 3)$ | 0.019 | 0.046 | 0.013 | -0.011 |
| | (0.010) | (0.033) | (0.009) | (0.017) |
| | [0.070] | [0.170] | [0.182] | [0.505] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 4)$ | 0.033 | 0.135 | 0.018 | 0.004 |
| | (0.011) | (0.047) | (0.008) | (0.020) |
| | [0.004] | [0.006] | [0.036] | [0.854] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 5)$ | 0.028 | 0.048 | 0.019 | -0.013 |
| | (0.011) | (0.039) | (0.005) | (0.017) |
| | [0.010] | [0.230] | [0.001] | [0.442] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 6)$ | 0.035 | 0.078 | 0.031 | 0.001 |
| | (0.011) | (0.035) | (0.010) | (0.024) |
| | [0.002] | [0.032] | [0.003] | [0.979] |
| $Adopt_s \times \mathbf{1}(t - t_s^* = 7)$ | 0.036 | 0.161 | 0.026 | 0.001 |
| | (0.012) | (0.045) | (0.008) | (0.021) |
| | [0.004] | [0.001] | [0.003] | [0.945] |
| Main Effects | | | | |
| Year Fixed Effects | x | x | x | x |
| State Fixed Effects | x | x | x | x |
| Control Variables | | | | |
| State Level Controls | x | x | x | x |
| Individual Level Controls | x | x | x | x |
| Weighted | x | x | x | x |
| Pre-period Mean of Dep. | 0.310 | 1.048 | 0.878 | 0.533 |
| Obs. | 262,437 | 258,325 | 254,260 | 237,601 |

**NOTES**: The table above reports estimates of event-time indicator coefficients from an event-study version of equation (4). The two aggregated end points, $h = -5$ and $h = 8$ ($h = 8$ in column (4)), are not reported in the table. The dependent variables are as indicated in each column. The sample includes individuals with diabetes (i.e., diabetics) who have non-missing values for the indicated dependent variable in the BRFSS diabetes module survey from 1995 to 2010, with the exception of column (4) (i.e., diabetes education), which covers the years since 2000. All regressions include year fixed effects, state fixed effects, state-level controls, and individual-level controls. All regressions are estimated weighted by the BRFSS sampling weights. Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.

Table A8: Anecdotal and Qualitative Evidence of Limited Coverage for Diabetes Before Diabetes Mandates

| State | Category | Specific Location in Source Material | Link |
|---|---|---|---|
| AK | Remarks by Bill Sponsor | See remarks by representative Murkowski in number 1462 | Link |
| CA | Bill Analysis Report | See the first paragraph in Purpose of the Bill (04/11/00- Assembly Committee) | Link |
| CT | News Article | See the article entitled Diabetes Patients | Link |
| HI | Internal Report | See the first paragraph on page 2 | Link |
| ME | Internal Report | See the first paragraph on page 3 in Executive Summary | Link |
| MA | News Article | See the entire article | Link |
| MI | Bill Analysis Report | See the second paragraph in Rationale | Link |
| MN | Bill Summary Report | See paragraphs in Diabetics Get Insurance Help | Link |
| NV | Remarks by Bill Sponsor | See remarks by assemblyman Herrera for Assembly Bill 477 | Link |
| NH | Testimony at Hearing | See the first paragraph on page 18 | Link |
| OH | Testimony at Hearing | See the paragraph starting with "The insurance industry stopped Ohio ... " | Link |
| PA | Internal Report | See paragraphs in Council Cost Estimates (in Review of House Bill 656) | Link |
| SC | Internal Report | See the last two paragraphs in AIM 3.4 on page 45 | Link |
| UT | Internal Report | See the second and third paragraphs in Executive Summary | Link |
| VA | Internal Report | See paragraphs in Social Impact on page 7 | Link |
| WA | News Article | See the article entitled Diabetes Legislation Worth Passing | Link |

**NOTES**: The table above provides anecdotal and qualitative evidence of limited coverage of diabetes (within non-self-insured private plans) before the enactment of diabetes mandates from various sources. Note that Ohio is one of four no-mandate states but is included in the table because its current status also suggests inadequate coverage of diabetes in the absence of the mandates. All sources were accessible as of August 2022. All materials are archived separately by the author and can be provided upon request. See section A for more details.

Table A9: Robustness Analysis: Missing Data in Mechanism Analysis

| Dependent Variable: | Missing Indicator | Diabetes Mortality |
|---|---|---|
| | (1) | (2) |
| $Adopt_s \times \mathbf{1}(t - t_s^* \geq 0)$ | 0.057 | -2.354 |
| | (0.061) | (0.932) |
| | [0.350] | [0.015] |
| Main Effects | | |
| Year Fixed Effects | x | x |
| State Fixed Effects | x | x |
| Control Variables | | x |
| Weighted | | x |
| Pre-period Mean of Dep. | 0.20 | 82.98 |
| Obs. | 784 | 630 |

**NOTES**: The table above reports estimates of the coefficient on the adoption interacted with the post-period indicator from regressions of equation (1). The dependent variables are the indicator for missing data in column (1) and diabetes-related mortality rate per 100,000 in column (2). The unit of observation is the state $\times$ year. The sample is the balanced panel over the years 1995 to 2010 in column (1) and the unbalanced panel (imitating the same missing data structure as the BRFSS data used in the mechanism analysis) over the years 1995 to 2010 in column (2). All regressions include year and state fixed effects, and column (2) further includes state-level controls. Column (1) is estimated without weights, but column (2) is estimated with weights by state population. Robust standard errors clustered at the state level are reported in parentheses, and p-values are reported in brackets.