

Inference under Staggered Adoption: Case Study of the Affordable Care Act

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Abstract

Panel data consists of a collection of N units that are observed over T units of time. A policy or treatment is subject to staggered adoption if different units take on treatment at different times (or never at all). Assessing the effectiveness of such a policy requires estimating the treatment effect, corresponding to the difference between outcomes for treated versus untreated units. We develop inference procedures that build upon a computationally efficient matrix estimator for treatment effects in panel data. Our routines return confidence intervals (CIs) both for individual treatment effects, as well as for more general bilinear functionals of treatment effects, with prescribed coverage guarantees. We apply these inferential methods to analyze the effectiveness of Medicaid expansion portion of the Affordable Care Act. Based on our analysis, Medicaid expansion has led to substantial reductions in uninsurance rates, has reduced infant mortality rates, and has had no significant effects on healthcare expenditures.

1 Introduction

Many datasets take the form of panel data, in which which a collection of N units (e.g., individuals, cities, states, countries, companies etc.), are observed over T time periods. Panel data arises in a very wide variety of applications, and the associated methodological literature is rich (e.g., see the book [Woo10] and references therein). It is frequently the case that some “treatment” is applied to a subset of the units. Here treatment should be understood, in a generic sense, as some form of intervention or policy that is applied. In the simplest case, the treatment is binary in nature (e.g., whether or not to vaccinate, or whether or not to join the EU). The framework of panel data with binary treatments has been used to study many problems, including—among many others—the effects of tax policy on smoking rates [ADH10]; the economic benefits of EU membership for a given country [Kop+24]; the effects of “right-to-carry” gun laws [DAW19]; and the effects of increases in minimum wage [CK93].

A fundamental issue underlying analysis of such panel data is the manner in which the treatment is adopted. Most straightforward is the randomized controlled trial, in which a randomly chosen subset of the units are given treatment at a common time, with all other units remaining untreated throughout time. In contrast, the focus of this paper is a more challenging

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setting: each unit can choose whether or not to adopt the treatment, and moreover can choose a time at which to do so. This set-up is known as *staggered adoption*. It leads to statistical inference problems that are challenging, both because of the observational nature of the data, and because of the differing adoption times. The Affordable Care Act (ACA) provides an archetypal example of panel data with staggered adoption. Here there are $N = 50$ states in total, and we can measure various features of a given state over a period of T time units. States can choose whether or not to take on the expanded Medicaid eligibility provided by the ACA. From its inception in 2010 through 2024, forty states have chosen to take on expanded Medicaid eligibility at different times; see [Figure 1](#) for a graphical illustration, and [Section 2.1](#) for additional background. On the methodological side, there are a wide variety of approaches to statistical inference with panel data under staggered adoption, including synthetic controls (e.g., [[AG03](#); [ADH15](#); [Li20](#); [DI16](#); [BMFR21b](#)])); linear panel models or fixed effects regression (e.g., [[IK21](#); [AI06](#)])); and approaches based on matrix completion (e.g., [[Ath+21](#); [Aba+24](#); [YW24](#)]). We examine the latter class of methods in this paper.

More specifically, this paper makes two primary contributions to the growing literature on panel data and staggered adoption. The first is methodological: building upon our recent work [[YW24](#)] on a low-rank matrix-based estimator for treatment effects, we show how to use its outputs to construct confidence intervals. We develop inferential procedures that are sufficiently flexible to handle heteroskedastic noise, and applicable to both individual treatment effects (ITEs), as well as to a more general notion of treatment effect as defined by a bilinear function. Our second contribution is to apply these inferential procedures so as to evaluate the effectiveness of Medicaid expansion component of the the Affordable Care Act (ACA). In particular, we study its effect on a variety of outcomes, including uninsurance rates, infant mortality, and expenditures. Our inferential procedures allow for fine-grained probing of these effects at the individual state level over each time period.

The remainder of the paper is organized as follows. [Section 2](#) provides some context on the ACA and its significance, and then lays out the framework of panel data with staggered adoption, including the treatment effects to be estimated. [Section 3](#) reviews our matrix-based estimator of treatment effects, and provides simple schemes to compute confidence intervals; we state some informal guarantees on its coverage properties, with all mathematical details deferred to the Appendices. In [Section 4](#), we use these inferential routines to analyze the causal effects of the Medicaid expansion in the ACA. We summarize and discuss future avenues of research in [Section 5](#).

2 Background and problem formulation

In this section, we provide background as well as a more precise formulation of panel data with staggered adoption. More specifically, in [Section 2.1](#), we provide relevant background on the Affordable Care Act, and a brief overview of some related work. [Section 2.2](#) is devoted to a more precise description of the framework of panel data with staggered adoption.

2.1 The Affordable Care Act

Among all developed nations, residents of the United States are expected (on average) to live the shortest lives, endure the most number of chronic health conditions, and experience the highest rates of infant and maternal mortalities. All of these negative outcomes occur despite the US spending the highest proportion of its GDP on healthcare, and nearly double the average of OECD nations [[GGWI23](#)]. A major contributing factor is lack of healthcare

access due to an inability to pay. For instance, in 2024, roughly 21% of Americans reported that they skipped seeking treatment due to costs; moreover, this rate of those skipping care nearly triples to 61% amongst those who do not have insurance [Lop+24]. In 2022, the US Census reported that nearly 26 million Americans lack health insurance [TDD23].

Medicaid Expansion of States

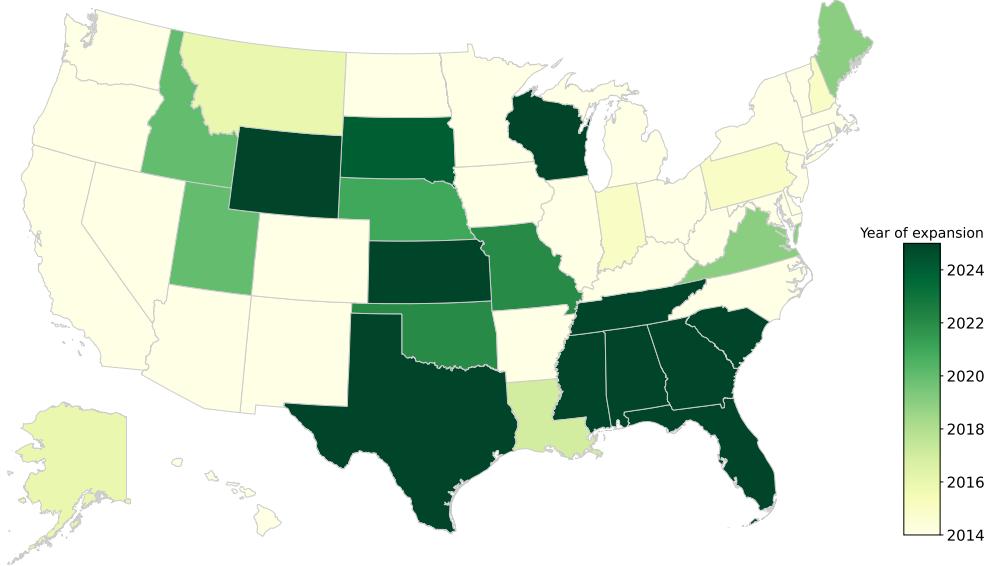


Figure 1. Adoption of Medicaid expansion by state. Darkness of colors indicates the time of adoption, with earlier adoption in lighter colors. The 10 states marked in very dark green (i.e., Alabama, Florida, Georgia, Kansas, Mississippi, South Carolina, Tennessee, Texas, Wisconsin and Wyoming) have not adopted the expansion.

The first major national reform to address these issues was the ACA, passed in 2010. Its primary goal is to ensure that more Americans have access to affordable health insurance, without it being tied to one's employment. One key component of the act was the expansion of eligibility for Medicaid, a government program established in 1965 to provide insurance to low income individuals and households. Medicaid provides coverage to individuals that live under the federal poverty line, and in 2014, it covered nearly 25% of Americans [Bala]. A key component of the ACA was an incentive to expand Medicaid coverage: the federal government provides funds to defray the additional costs of states choosing to expand coverage. This expanded coverage applies to households whose incomes amounted up to 138% of the federal poverty level [Balb]. As of 2024, 40 states have adopted the Medicaid expansion, whereas many of the remaining states are considering the expansion [Wei24]; see Figure 1 for a graphical illustration of the current adoption. Nonetheless, the ACA has remained politically contentious. In 2018, there were significant efforts made to repeal it, with various arguments made both in opposition and support. With the 2025 return of a Republican administration avowing to trim the government, it is quite possible that repeal of ACA will again be raised. For these reasons, understanding its effects, both on health outcomes and expenditures, has a timely importance.

Proponents of Medicaid expansion often point to statistics such as improved access to care and health outcomes in states that chose to expand access to Medicaid compared to those that did not opt in. For instance, Miller et. al. [MJW21] estimates that between 2014 and

2017, in the states that chose to expand Medicaid, there were approximately 19,200 fewer deaths amongst low-income adults in the age group 55–64; moreover, they estimate that there were approximately 15,600 preventable deaths in states that chose not to opt in. States that have not taken on expanded eligibility have uninsurance rates that are nearly double of those that did opt in [TDD24]. However, opponents often argue that the ACA “has not stopped the stampede of rising health care costs”, and that “nearly 30 millions Americans [are] still uninsured” [Moo18]. There is past work on analyzing the causal effect of Medicaid expansion: in 2008, Oregon enacted a lottery that provided Medicaid coverage to previously ineligible randomly selected low-income adults [Fin+12; Bai+13]. These studies documented mixed effects of such expansion: over the period 2008–2010, Medicaid expansion reduced uninsurance rates by 25%, led better self-reported health outcomes and higher utilization of healthcare, but produced no improvements in measured physical health. Our paper builds upon these results, in particular by developing inferential methods that can provide treatment effect estimates for observational data, and can be targeted at the state level, thereby helping identify which states benefit the most (or the least) from Medicaid expansion.

2.2 Panel data with staggered adoption

We now turn to a more precise formulation of the problem of estimating treatment effects in panel data under staggered adoption. In a panel data model, there are a total of N units observed over T time periods. For each unit index $i \in [N] := \{1, \dots, N\}$ and time $t \in [T] := \{1, \dots, T\}$, we observe a scalar outcome $Y_{i,t}$. There is an underlying binary treatment, and for each unit $i \in [N]$, we define $t_i \in [T] \cup \{+\infty\}$ to be the time at which unit i adopted the treatment, with $t_i = +\infty$ indicating non-adoption. With this set-up, we

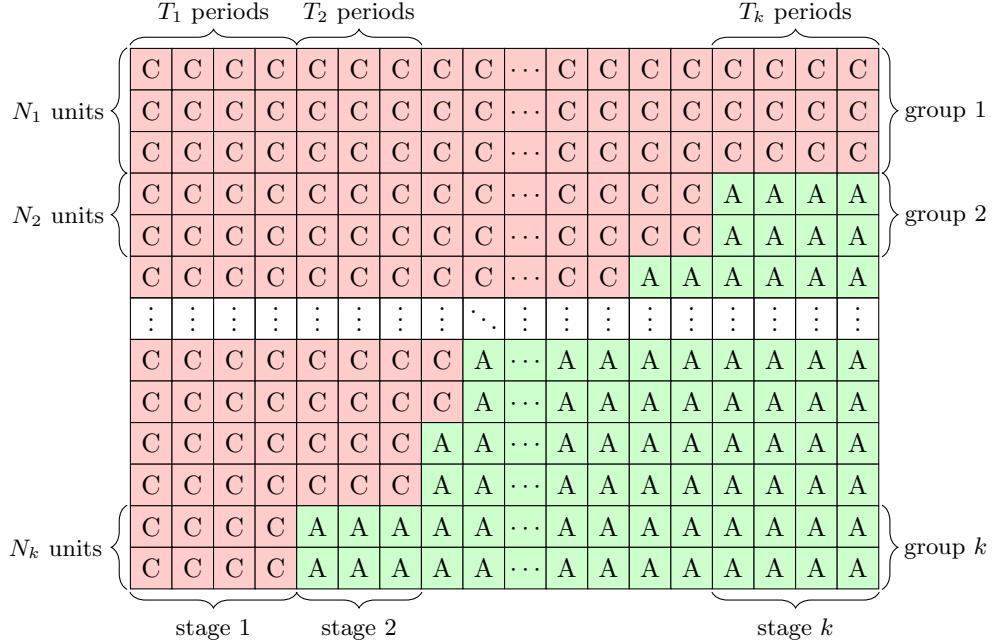


Figure 2. An example of panel data under staggered adoption design, with units sorted according to the first time of treatment. The labels C and A refer to “control” and “adopted” (i.e., treated) respectively.

can represent the full collections of observations $\{Y_{i,t}, i \in [N], t \in [T]\}$ as an $N \times T$ matrix, as shown in Figure 2. Each entry of this matrix can be labeled with a “C” for control (or

untreated) or a “A” for adopted (or treated), with the transition between “C” and “A” in each row demarcated by the time t_i .

Counterfactual outcomes: Let us now introduce the notion of counterfactual outcomes, following past work on this type of problem (e.g., [BJS24; BMFR21a; YW24]). We define a random matrix $\{Y_{i,t}(0), i \in [N], t \in [T]\}$ corresponding to the potential outcome of unit i and time t if it never undergoes treatment. We assume that for each $i \in [N]$ and time unit $t < t_i$, we observe $Y_{i,t} = Y_{i,t}(0)$, or equivalently

$$Y_{i,t} = \underbrace{\mathbb{E}[Y_{i,t}(0)]}_{:= M_{i,t}^*} + \varepsilon_{i,t},$$

where $\varepsilon_{i,t}$ is (by definition) a zero-mean random variable. By following this set-up, we are adopting the *no anticipation assumption*, meaning that observations $Y_{i,t}$ for times $t < t_i$ have the same statistical structure whether or not the treatment is applied in the future. On the other hand, our analysis *does not* impose any assumptions on the statistical structure of any observation $Y_{i,t}$ for $t \geq t_i$.

For any index $i \in [N]$ and time $t \geq t_i$, we can define an individual treatment effect, with the full collection corresponding to unit-time indexed stochastic process

$$\tau_{i,t} = Y_{i,t} - \mathbb{E}[Y_{i,t}(0)] \equiv Y_{i,t} - M_{i,t}^*, \quad \text{for } i \in [N], \text{ and } t \geq t_i. \quad (1)$$

In words, the *individual treatment effect* $\tau_{i,t}$, or ITE for short, is the difference between the observed outcome $Y_{i,t}$ for a treated unit i at time t against the *average counterfactual outcome* $M_{i,t}^*$ associated with never adopting the treatment. Thus, for the unit i who adopts treatment at time t_i , the ITE $\tau_{i,t}$ for $t \geq t_i$ can be interpreted as the causal effect of adopting treatment. Recall that $Y_{i,t}$ is an observed quantity, so that all challenges associated with inference about $\tau_{i,t}$ lie in the unknown counterfactual outcomes $M_{i,t}^* = \mathbb{E}[Y_{i,t}(0)]$.

The main methodological contribution of this paper is to develop procedures for computing confidence intervals (CIs) for both any individual treatment effect $\tau_{i,t}$, as well as (more generally) for weighted bilinear functions defined on the treatment effect process. (See (??) for a more precise formulation.) These inference routines build upon a matrix-based estimator of the treatment effect from our past work [YW24]; this estimator is predicated upon an additional structural condition on the matrix of mean potential outcomes, which we now describe.

Low-rank factor model: Let $\mathbf{M}^* \in \mathbb{R}^{N \times T}$ be the full matrix of average potential outcomes for the control—that is, with entries $M_{i,t}^* = \mathbb{E}[Y_{i,t}(0)]$. The inferential routines of this paper build upon an estimation algorithm [YW24] designed to exploit low-rank structure in the matrix \mathbf{M}^* . In particular, we assume that it has a rank $r < \min\{N, T\}$, and so can be decomposed as

$$\mathbf{M}^* = \mathbf{U}^* \boldsymbol{\Sigma}^* (\mathbf{V}^*)^T = \sum_{j=1}^r \gamma_j^* \mathbf{U}_j^* (\mathbf{V}_j^*)^T \quad (2)$$

Here $\boldsymbol{\Sigma}^* := \text{diag}\{\gamma_1^*, \dots, \gamma_r^*\}$ is a diagonal matrix containing the singular values in non-increasing order $\gamma_1^* \geq \dots \geq \gamma_r^* > 0$. The matrices $\mathbf{U}^* \in \mathbb{R}^{N \times r}$ and $\mathbf{V}^* \in \mathbb{R}^{T \times r}$ contain the singular vectors, and can be written in terms of their columns as $\mathbf{U}^* = [\mathbf{U}_1^* \dots \mathbf{U}_r^*]$ and

$\mathbf{V}^* = [\mathbf{V}_1^* \cdots \mathbf{V}_r^*]$. Low-rank assumptions of the type (2) originated in the literature on factor models for panel data [BN02; Bai03; Bai09]. For panel data with treatments, there are various methods for estimating treatments that rely on a low-rank assumption; for example, see the papers [ADH10; BMFR21a] for methods based on synthetic controls, and the paper [BJS24] for a method based on differences-in-differences approaches. Our approach in this paper falls within the class of matrix completion methods (e.g., [Ath+21; Aba+24; YW24]).

A final comment to close this section: in general, without some type of structural condition, the treatment effect (1) is unidentifiable, since it depends on quantities—namely, $M_{i,t}^* = \mathbb{E}[Y_{i,t}(0)]$ for $t \geq t_i$ —for which we have no observations. The low rank assumption (2) is one way in which to enforce identifiability; it provides a strong coupling between these unobservable quantities and the other matrix entries $M_{i,t}^*$ for $t < t_i$, for which our observations are directly relevant.

3 Inferential routines for treatment effects

We now turn to methods for estimation and inference of treatment effects within our set-up. Recall from equation (1) the definition of the individual treatment effect $\tau_{i,t}$. Since the quantity $Y_{i,t}$ is observed, estimating and returning confidence intervals for $\tau_{i,t}$ is equivalent to doing so for the unknown mean counterfactual outcome $M_{i,t}^*$. For this reason, our discussion focuses on the matrix \mathbf{M}^* of these mean outcomes, but with the understanding that we can move freely back to the treatment effects.

In addition to confidence intervals (CIs) for the individual treatment effect (1), the methods to be described here can also be used to compute CIs for more general objects. For instance, given an arbitrary vector $w \in \mathbb{R}^N$, we can do so for the *weighted treatment effect on treated*, given by

$$\tau_{w,t} := \sum_{i=1}^N w_i \tau_{i,t} \mathbf{1}(t_i \geq t). \quad (3)$$

Even more generally, we can do so for an arbitrary bilinear form of the matrix \mathbf{M}^* ; see Section 3.3 and Appendix C for further discussion.

In the remainder of this section, we first describe our strategy reducing a general staggered design to a sequence of simpler four-block problems (Section 3.1). We describe how to perform estimation and inference in this simpler setting, before describing our complete algorithm in Section 3.2. Finally, Section 3.3 is devoted to discussion of coverage guarantees for our confidence intervals, with the technical details deferred to the appendices.

3.1 Reduction to four-block design

We tackle the general problem by reducing it to a sequence of simpler problems, ones that exhibit a structure that we refer to as 4-block design. Figure 3 provides an illustration of this structure and its relevance to the general problem. In the right panel, we show the generic structure of panel data under staggered adoption; by suitably re-ordering the rows, we can always convert it to a matrix with a staircase pattern, as shown here, that separates the treated and untreated blocks. A matrix in this staircase pattern can be further decomposed into a collection of four-block matrices; the left panel exhibits the extraction of this structure. See Appendix A for a more formal description of this partitioning procedure.

	T_1	T_2	T_3	T_4	T_5	T_6
N_1	$\mathbf{M}_{1,1}$	$\mathbf{M}_{1,2}$	$\mathbf{M}_{1,3}$	$\mathbf{M}_{1,4}$	$\mathbf{M}_{1,5}$	$\mathbf{M}_{1,6}$
N_2	$\mathbf{M}_{2,1}$	$\mathbf{M}_{2,2}$	$\mathbf{M}_{2,3}$	$\mathbf{M}_{2,4}$	$\mathbf{M}_{2,5}$	
N_3	$\mathbf{M}_{3,1}$	$\mathbf{M}_{3,2}$	$\mathbf{M}_{3,3}$	$\mathbf{M}_{3,4}$		
N_4	$\mathbf{M}_{4,1}$	$\mathbf{M}_{4,2}$	$\mathbf{M}_{4,3}$			
N_5	$\mathbf{M}_{5,1}$	$\mathbf{M}_{5,2}$				
N_6	$\mathbf{M}_{6,1}$					

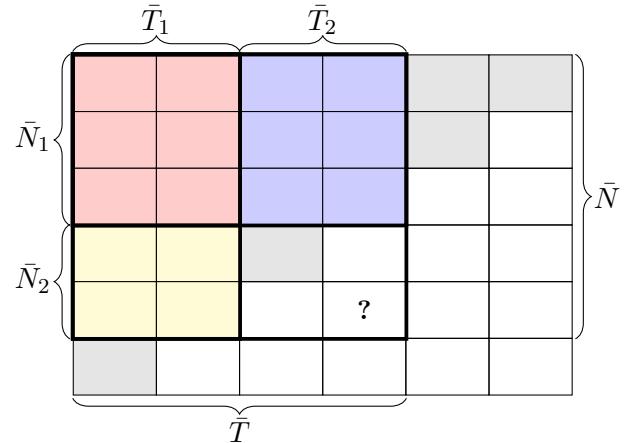


Figure 3. Left: After suitable re-ordering of the rows, panel data under staggered adoption can be converted to the “staircase” form shown here. Right: The staircase pattern can be decomposed into a number of smaller “four-block” designs, as shown here. Our procedure proceeds by first extracting these four-block designs, and then performing inference on each.

More formally, a panel data problem with four-block structure takes the following form. The N units can be divided into two subgroups: a subset of size N_1 that are never exposed to the treatment, with all the other $N_2 := N - N_1$ units receiving the treatment at the same time $T_1 + 1$. Consequently, both the potential outcome matrix \mathbf{M}^* and the observed matrix \mathbf{Y} can be partitioned as

$$\mathbf{M}^* = \begin{bmatrix} \mathbf{M}_a^* \in \mathbb{R}^{N_1 \times T_1} & \mathbf{M}_b^* \in \mathbb{R}^{N_1 \times T_2} \\ \mathbf{M}_c^* \in \mathbb{R}^{N_2 \times T_1} & \mathbf{M}_d^* \in \mathbb{R}^{N_2 \times T_2} \end{bmatrix}, \quad \mathbf{Y} = \begin{bmatrix} \mathbf{Y}_a & \mathbf{Y}_b \\ \mathbf{Y}_c & ? \end{bmatrix} = \begin{bmatrix} \mathbf{M}_a^* + \mathbf{E}_a & \mathbf{M}_b^* + \mathbf{E}_b \\ \mathbf{M}_c^* + \mathbf{E}_c & ? \end{bmatrix} \quad (4)$$

where $T_2 = T - T_1$, and $\mathbf{E}_a, \mathbf{E}_b, \mathbf{E}_c$ are the noise in the observation at each block. The blocks with a question mark are unobserved (counterfactual) outcomes.

3.1.1 A key subroutine: Four-block matrix estimation

Given a matrix in four-block form, our general procedure exploits a sub-routine, described formally as Algorithm `FourBlockEst`, designed to estimate the unknown block of the matrix \mathbf{M}^* . This particular procedure was proposed in the factor model literature [BN21], and is also a core sub-routine of our matrix estimator [YW24] for the more general setting of staggered adoption.

Recall the partially observed matrix \mathbf{Y} defined in equation (4), and define

$$\mathbf{Y}_{\text{left}} := \begin{bmatrix} \mathbf{Y}_a \\ \mathbf{Y}_c \end{bmatrix} \in \mathbb{R}^{N \times T_1}, \quad \mathbf{Y}_{\text{upper}} := [\mathbf{Y}_a \ \mathbf{Y}_b] \in \mathbb{R}^{N_1 \times T}$$

as its left and upper submatrices. Algorithm `FourBlockEst` takes as input the matrix \mathbf{Y} , and then operates separately on these submatrices of the observation matrix. It returns as output a matrix estimate $\widehat{\mathbf{M}}_d$ of the unknown block \mathbf{M}_d^* of the matrix \mathbf{M}^* ; see equation (4).

To understand the purpose of the different steps in Algorithm `FourBlockEst`, consider the singular value decomposition (SVD) of the matrix \mathbf{M}^* —say $\mathbf{M}^* = \mathbf{U}^* \Sigma^* (\mathbf{V}^*)^T$, where $\mathbf{U}^* \in \mathbb{R}^{N \times r}$ and $\mathbf{V}^* \in \mathbb{R}^{T \times r}$ are matrices of left and right singular vectors. The three main steps of the algorithm are:

Algorithm 1: FourBlockEst

- 1 **Input:** Data matrix $\mathbf{Y} \in \mathbb{R}^{N \times T}$, dimension parameters N_1 and T_1 , rank r
/ Step 1: Subspace Estimation */*
 - 2 Compute the truncated rank- r SVD $(\mathbf{U}_{\text{left}}, \boldsymbol{\Sigma}_{\text{left}}, \mathbf{V}_{\text{left}})$ of \mathbf{Y}_{left} .
 - 3 Partition $\widehat{\mathbf{U}} := \mathbf{U}_{\text{left}}$ into two submatrices $\widehat{\mathbf{U}}_1$ and $\widehat{\mathbf{U}}_2$, where $\widehat{\mathbf{U}}_1 \in \mathbb{R}^{N_1 \times r}$ consists of its top N_1 rows and $\widehat{\mathbf{U}}_2 \in \mathbb{R}^{N_2 \times r}$ consists of its bottom N_2 rows.
/ Step 2: Matrix Denoising */*
 - 4 Compute the truncated rank- r SVD $(\mathbf{U}_{\text{upper}}, \boldsymbol{\Sigma}_{\text{upper}}, \mathbf{V}_{\text{upper}})$ of $\mathbf{Y}_{\text{upper}}$.
 - 5 Partition $\widehat{\mathbf{V}} := \mathbf{V}_{\text{upper}}$ into two submatrices $\widehat{\mathbf{V}}_1$ and $\widehat{\mathbf{V}}_2$, where $\widehat{\mathbf{V}}_1 \in \mathbb{R}^{T_1 \times r}$ consists of its top T_1 rows and $\widehat{\mathbf{V}}_2 \in \mathbb{R}^{T_2 \times r}$ consists of its bottom T_2 rows.
 - 6 Compute the matrix estimate $\widehat{\mathbf{M}}_b := \mathbf{U}_{\text{upper}} \boldsymbol{\Sigma}_{\text{upper}} \widehat{\mathbf{V}}_2^\top$ of \mathbf{M}_b^* .
/ Step 3: Imputation of missing entries */*
 - 7 Compute the matrix $\widehat{\mathbf{M}}_d := \widehat{\mathbf{U}}_2 (\widehat{\mathbf{U}}_1^\top \widehat{\mathbf{U}}_1)^{-1} \widehat{\mathbf{U}}_1^\top \widehat{\mathbf{M}}_b$.
 - 8 **Output:** $\widehat{\mathbf{M}}_d$ as estimate of \mathbf{M}_d^* , along with intermediate quantities $(\widehat{\mathbf{U}}, \widehat{\mathbf{V}})$.
-

Left subspace estimation: The matrix $\widehat{\mathbf{U}}$ from Step 3 is an estimate of \mathbf{U}^* .

Right subspace estimation: Similarly, the matrix $\widehat{\mathbf{V}}$ in Step 5 is an estimate of \mathbf{V}^* .

Matrix denoising: Step 6 uses the appropriate components of $(\widehat{\mathbf{U}}, \widehat{\mathbf{V}})$ to compute a denoised estimate of \mathbf{M}_b^* .

Matrix imputation: Step 7 combines the denoised estimate with the subspace estimate to compute an estimate of \mathbf{M}_d^* .

As discussed in our previous paper [YW24], in the idealized case that \mathbf{M}^* is rank r , and we observe the blocks $\{\mathbf{M}_a^*, \mathbf{M}_b^*, \mathbf{M}_c^*\}$ without noise, then the output $\widehat{\mathbf{M}}_d$ of Algorithm **FourBlockEst** is guaranteed to be equivalent to \mathbf{M}_d^* . The same paper also analyzes its estimation-theoretic properties in the more realistic setting of noisy observations.

3.1.2 Confidence intervals for 4-block designs

Having specified our estimation procedure (Algorithm **FourBlockEst**), we now describe how to use its outputs for computing confidence intervals for each entry of \mathbf{M}^* . This routine, specified precisely as Algorithm **FourBlockConf**, involves three key steps.

Estimating the residuals: Use the output $\widehat{\mathbf{M}}$ to estimate the residuals $\mathbf{E} := \mathbf{Y} - \mathbf{M}^*$ via

$$\widehat{\mathbf{E}} := \begin{bmatrix} \mathbf{M}_a - \widehat{\mathbf{M}}_a & \mathbf{M}_b - \widehat{\mathbf{M}}_b \\ \mathbf{M}_c - \widehat{\mathbf{M}}_c & ? \end{bmatrix} \quad \text{where} \quad \begin{bmatrix} \widehat{\mathbf{M}}_a \in \mathbb{R}^{N_1 \times T_1} \\ \widehat{\mathbf{M}}_c \in \mathbb{R}^{N_2 \times T_1} \end{bmatrix} := \mathbf{U}_{\text{left}} \boldsymbol{\Sigma}_{\text{left}} \mathbf{V}_{\text{left}}^\top. \quad (5a)$$

The bottom right block of $\widehat{\mathbf{E}}$ is not defined, just as for \mathbf{E} .

Variance estimation: Use the estimated residuals $\widehat{\mathbf{E}}$ and the subspace estimates $(\widehat{\mathbf{U}}, \widehat{\mathbf{V}})$ to compute the scalar variance estimate

$$\widehat{\gamma}_{i,t} := \sum_{k=1}^{N_1} \widehat{E}_{k,t}^2 \left[\widehat{\mathbf{U}}_{i,\cdot} \underbrace{(\widehat{\mathbf{U}}_1^\top \widehat{\mathbf{U}}_1)^{-1}}_{\in \mathbb{R}^{r \times r}} \widehat{\mathbf{U}}_{k,\cdot}^\top \right]^2 + \sum_{s=1}^{T_1} \widehat{E}_{i,s}^2 \left[\widehat{\mathbf{V}}_{t,\cdot} (\widehat{\mathbf{V}}_1^\top \widehat{\mathbf{V}}_1)^{-1} \widehat{\mathbf{V}}_{s,\cdot}^\top \right]^2, \quad (5b)$$

where $\widehat{\mathbf{U}}_{i,\cdot} \in \mathbb{R}^r$ is the i^{th} row of $\widehat{\mathbf{U}}$, and $\widehat{\mathbf{V}}_{s,\cdot} \in \mathbb{R}^r$ is the s^{th} row of $\widehat{\mathbf{V}}$.

Algorithm 2: FourBlockConf

```

1 Input: Confidence level  $1 - \alpha \in (0, 1)$  and inputs of Algorithm FourBlockEst.
2 Call Algorithm FourBlockEst to compute  $(\widehat{\mathbf{M}}_d, \widehat{\mathbf{U}}, \widehat{\mathbf{V}})$ .
    // Residual Estimates
3 Estimate the residuals  $\widehat{\mathbf{E}}$  via equation (5a).
4 for  $i = N_1 + 1$  to  $N$  do
5   for  $t = T_1 + 1$  to  $T$  do
6     // Variance Estimates
7     Compute the variance estimate  $\widehat{\gamma}_{i,t}$  from equation (5b).
    // Confidence Intervals
8     Compute the interval  $\text{CI}_{i,t}^{(1-\alpha)} := [\widehat{M}_{i,t} \pm \Phi^{-1}(1 - \alpha/2) \widehat{\gamma}_{i,t}^{1/2}]$ .
9 Output: Return  $\text{CI}_{i,t}^{(1-\alpha)}$  as confidence interval  $M_{i,t}^*$  for each unobserved  $(i, t)$ .

```

Confidence intervals: Given a level $\alpha \in (0, 1)$, for each unobserved entry (i, t) , we construct the interval

$$\text{CI}_{i,t}^{(1-\alpha)} := \left[\widehat{M}_{i,t} \pm \Phi^{-1}(1 - \alpha/2) \widehat{\gamma}_{i,t}^{1/2} \right], \quad (5c)$$

where Φ denotes the CDF of the standard normal distribution.

As discussed in [Section 3.3](#), under appropriate regularity conditions, the interval $\text{CI}_{i,t}^{(1-\alpha)}$ is guaranteed to include the unknown matrix entry $M_{i,t}^*$ with probability converging to $1 - \alpha$. For short, we say that it is a confidence interval (CI) with coverage $1 - \alpha$. We also comment that the variance estimate (5b) is motivated by calculations of the asymptotic variance of the error in the matrix estimate $\widehat{\mathbf{M}}_d$ returned by Algorithm **FourBlockEst**.

3.2 Confidence intervals for general staggered design

With our two sub-routines (namely, Algorithms **FourBlockEst** and **FourBlockConf**) in place, we are now ready to describe our final inference routine, which applies to the general staggered design.

This routine is based on a partitioning scheme, described in [Appendix A](#), that takes as input a staggered design, returns a positive integer k , and an associated set of dimensions $\{N_i\}_{i=1}^k$ and $\{T_j\}_{j=1}^k$. Via these objects, for any pair of integers $i_0 \in [k]$ and $j_0 \in [k+2-i_0, k]$, we can extract an observation matrix \mathbf{Y}^{i_0, j_0} corresponding to a particular four-block problem; in particular, see equation (8) in [Appendix A](#) for the details. For each such pair of indices, we first perform matrix estimation by applying Algorithm **FourBlockEst** to this sub-problem, and then using its outputs, we apply Algorithm **FourBlockConf** to compute confidence intervals (CIs) for each of the unobserved entries within this sub-problem. Our construction ensures that each unobserved matrix entry in the full staggered matrix is covered by one of these four-block sub-problems. Thus, after running Algorithm **StaggeredConf**, we have produced a CI for each of the unobserved entries associated with the original staggered design.

3.3 Coverage guarantees

Thus far, we have described a routine (Algorithm **StaggeredConf**) that, for any level $\alpha \in (0, 1)$ and unobserved index (i, t) , produces an interval $\text{CI}_{i,t}^{(1-\alpha)}$. We now turn to the coverage

Algorithm 3: StaggeredConf

1 **Input:** Data matrix \mathbf{Y} , rank r , confidence level $1 - \alpha$.
 2 Extract the dimension information $\{N_i\}_{1 \leq i \leq k}$ and $\{T_j\}_{1 \leq j \leq k}$ from \mathbf{M} .
 3 **for** $i_0 = 1$ **to** k **do**
 4 **for** $j_0 = k + 2 - i_0$ **to** k **do**
 5 Construct the four-block data matrix \mathbf{Y}^{i_0, j_0} via equation (8).
 6 Call Algorithm **FourBlockEst** with input \mathbf{Y}^{i_0, j_0} and rank r to compute an
 estimate $\widehat{\mathbf{M}}_d$ for its bottom-right block.
 7 Extract the appropriate submatrix of $\widehat{\mathbf{M}}_d$, denoted by $\widehat{\mathbf{M}}^{i_0, j_0}$, as the estimate
 for the block $[\mathbf{M}^*]^{i_0, j_0}$.
 8 Call Algorithm **FourBlockConf** with input $\widehat{\mathbf{M}}^{i_0, j_0}$ and confidence level $1 - \alpha$
 to compute entry-wise confidence intervals for $[\mathbf{M}^*]^{i_0, j_0}$.
 9 **Output:** $\widehat{M}_{i,t}$ as the estimate, and $\text{CI}_{i,t}^{(1-\alpha)}$ as the confidence interval, for each
 unobserved entry $M_{i,t}^*$.

properties of this interval: in particular, we would like to ensure that it contains the unknown value $M_{i,t}^*$ with probability converging to $1 - \alpha$ as either N or T grow.

In order to provide a guarantee of this type, we need to impose certain assumptions on the problem.

Heteroskedastic noise For each $i \in [N]$ and $t < t_i$, the noise random variables $\varepsilon_{i,t}$ are independent, zero-mean sub-Gaussian random variables with variances $\text{var}(\varepsilon_{i,t}) = \sigma_{i,t}^2$. This assumption and mild variants of it are commonly made in the literature, e.g. the papers [BMFR21a; Ath+21; BJS24].

Theorem 1 (Informal version). *Under certain regularity conditions, for any four-block problem with rank $r \lesssim \min\{N_1, T_1\}$ and maximal noise level σ_{\max} , we have*

$$\mathbb{P}\left[\text{CI}_{i,t}^{(1-\alpha)} \ni M_{i,t}^*\right] \geq 1 - \alpha - \mathcal{O}\left(\frac{\sigma_{\max} \text{polylog}(N, T)}{\sqrt{\min\{N, T\}}}\right). \quad (6)$$

By quantifying the uncertainty of the estimator $\widehat{\mathbf{M}}_d$, we can show that $\widehat{M}_{i,t} - M_{i,t}^*$ is approximately a zero-mean Gaussian with variance

$$\gamma_{i,t}^* := \sum_{k=1}^{N_1} \sigma_{k,j}^2 \left[\mathbf{U}_{i,\cdot}^* (\mathbf{U}_1^{*\top} \mathbf{U}_1^*)^{-1} \mathbf{U}_{k,\cdot}^{*\top} \right]^2 + \sum_{k=1}^{T_1} \sigma_{i,k}^2 \left[\mathbf{V}_{j,\cdot}^* (\mathbf{V}_1^{*\top} \mathbf{V}_1^*)^{-1} \mathbf{V}_{k,\cdot}^{*\top} \right]^2, \quad (7)$$

where we partition the orthonormal matrices \mathbf{U}^* and \mathbf{V}^* into

$$\mathbf{U}^* := \begin{bmatrix} \mathbf{U}_1^* \in \mathbb{R}^{N_1 \times r} \\ \mathbf{U}_2^* \in \mathbb{R}^{N_2 \times r} \end{bmatrix}, \quad \mathbf{V}^* := \begin{bmatrix} \mathbf{V}_1^* \in \mathbb{R}^{T_1 \times r} \\ \mathbf{V}_2^* \in \mathbb{R}^{T_2 \times r} \end{bmatrix}.$$

This allows

$$\mathbb{P}\left(\widehat{M}_{i,t} \in \text{CI}_{i,t}^{(1-\alpha)}\right) = 1 - \alpha + f(N, T)$$

where $f(N, T) \rightarrow 0$ as either $N \rightarrow \infty$ or $T \rightarrow \infty$. See [Theorem 2](#) in the appendix for further details. Furthermore, we can build on these guarantees to establish estimation guarantees and confidence intervals for estimating bilinear forms of the matrix \mathbf{M}^* , i.e. for vectors $\mathbf{c}_1 \in \mathbb{R}^{N_2}$ and $\mathbf{c}_2 \in \mathbb{R}^{T_2}$, we want to estimate the quantity

$$\mathbf{c}_1^\top \mathbf{M}_d^* \mathbf{c}_2.$$

This subsumes the weighted treatment effect as described in [Equation \(3\)](#) as a special case by setting \mathbf{c}_1 to be a canonical basis vector.

The full description of the procedure is deferred to the appendix for simplicity of presentation; see [??](#) for further details.

However in some cases we may wish to estimate some population weighted effect, i.e. $\tau_{i,t}$ might denote the effect Medicaid expansion had on uninsurance rates for a state i in year t , but we want to estimate the number of Americans that had insurance because of this expansion in year t . Then, w_i would denote the population of state i in the year t , and $\tau_{w,t}$ would yield an estimate of such a quantity.

4 Application to the Affordable Care Act

We now return to the Affordable Care Act, first introduced in [Section 2.1](#), and apply the proposed procedures for inferring treatment effects. At a high level, we apply our method to three different outcomes: uninsurance rates, health expenditures, and infant mortality. When applied to any one of these outcomes, the final output of our procedure is an matrix estimate $\hat{\mathbf{M}}$ (or equivalently, a collection of estimates of treatment effects), along with confidence intervals for these quantities. Thus, our results can be viewed as a collection of time series, one for each state. For states that adopt treatment, we have an observed set of pre-treatment outcomes, an observed set of post-treatment outcomes, and our method provides estimates of the counterfactual outcomes that would have been observed if treatment *had not* been adopted, along with confidence intervals. [Figure 4](#) plots our results in this time series format for two different states, namely New York and Montana; see the figure caption for further details.

4.1 Data description

Let us provide some details of the data that underlies our analysis, and our casting of it within the panel data formulation. Recall that we have $N = 50$ states, and a subset of 40 of them have chosen to adopt Medicaid expansion at different times; see [Figure 1](#) for a graphical illustration. In most cases, a state that chose to adopt the expansion in a given year did so in January; accordingly, we assigned the first treatment period of those states to be the year of expansion. For other states with implementation in other months, we selected the year of the closest January as the first treatment period.

Specifically, we are interested in analyzing the causal effects of Medicaid expansion on three different outcomes: uninsurance rates, economics, and infant mortality rates. Each of these outcomes have been studied in some past work. For instance, the 2008 Oregon Medicaid expansion studies [[Fin+12](#); [Bai+13](#)] estimated that the effect of Medicaid expansion reduced uninsurance rates by about 25% in low-income adults. Some past work [[MJW21](#)] has reported that Medicaid expansion had reduced mortality rates, whereas another paper [[BBS18](#)] reported that infant mortality had risen in non-Medicaid expansion states and dropped in Medicaid expansion states between 2014 and 2016.

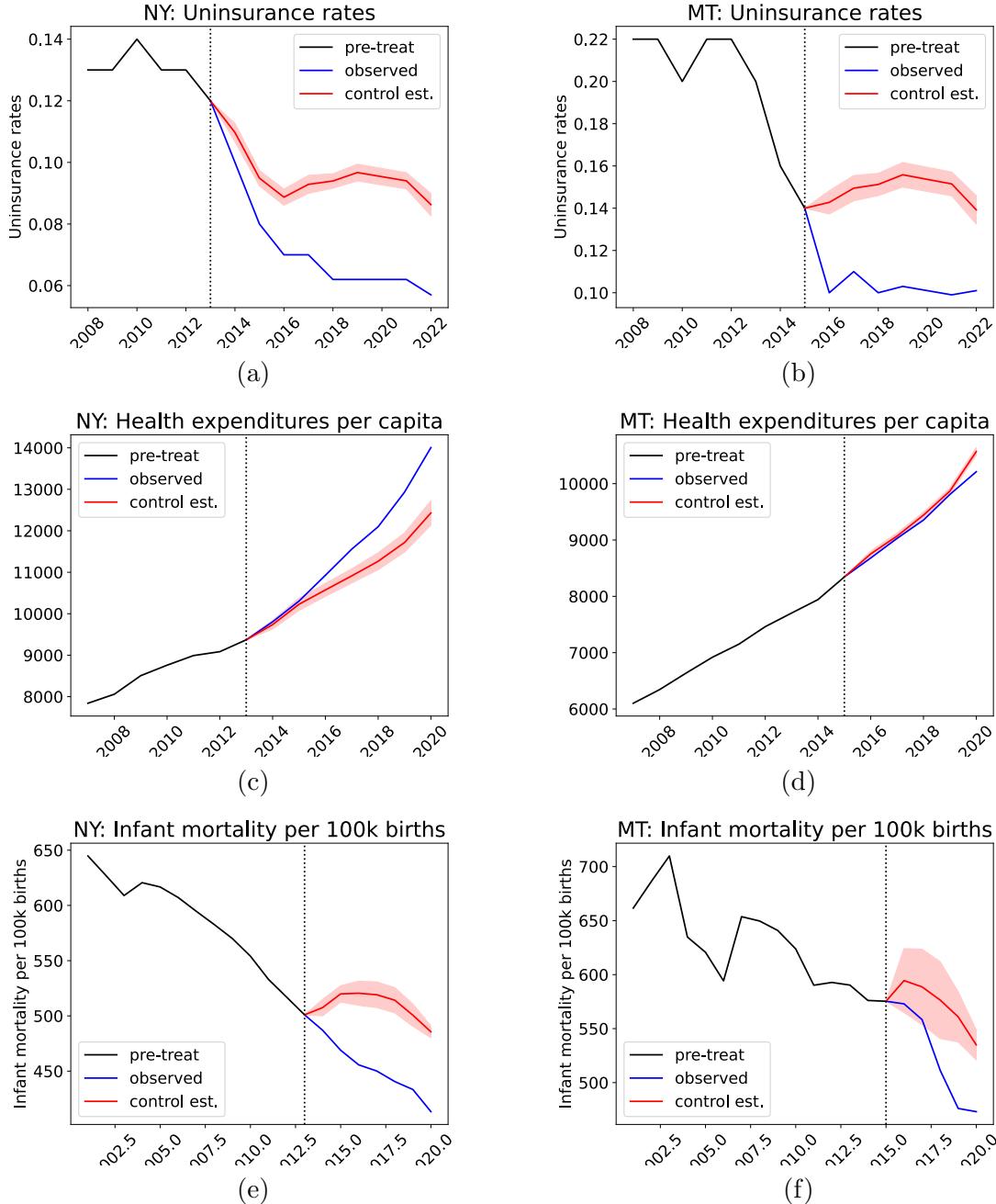


Figure 4. Results of Medicaid expansion over time for New York (NY) and Montana (MT) on uninsurance rates (plots (a) and (b)), health expenditures per capita (plots (c) and (d)), and infant mortalities per 100,000 births (plots (e) and (f)). Black represents values prior to the adoption of the expansion, and blue represents the observed measurements after adoption. Red denotes the estimated counterfactual of the outcomes under not adopting the expansion, and the shaded red denotes the 95% confidence intervals.

Uninsurance rates: Our data for the uninsured rates is collected by the Kaiser Family Foundation (KFF) which contains uninsurance rates for each state between 2008 and 2022. Our goal is to evaluate whether or not the Medicaid expansion has achieved its stated goal of

reducing uninsurance rates in states that have adopted the policy.

Healthcare spending: As with any policy intervention, the benefits of Medicare expansion need to be considered in conjunction with its costs. Some of the arguments against the ACA lie within the economics of the act. One driving factor for its passage was to address the extremely high cost of care within the United States. However, in the years following its passage, health care costs have continued to grow; for this reason, opponents of ACA argue that Medicaid expansion is ineffective in addressing these issues. We use our inferential methods in an attempt to address this important question, in particular via the outcome variable defined as the effect of Medicaid expansion on healthcare spending per capita. The data is taken from the Kaiser Family Foundation.

Infant mortality: As mentioned in the introduction, being uninsured plays a significant role in an individual's decision to seek medical care. It is natural to expect that failure to seek medical care could lead to affect health outcomes and potentially higher mortality rates. In order to address this question, we studied the effects of Medicare expansion on infant mortality rates, where "infant" is defined as a baby less than one year in age. This data is taken from the CDC Wonder project [DCP21]. Major causes of infant mortality include birth defects, premature births as well as other factors; screening and treating these issues often relies on repeated checkups. Lack of insurance can reduce one's access to the requisite medical care, leading to increases in infant mortality.

4.2 Findings

We now turn to a more detailed discussion of our findings.

4.2.1 Uninsurance rates

Table 1 provides a summary of our results on the analysis of uninsurance rates. The first three columns denote the number of states that have a statistically significant effect in the specific direction, i.e. in 2022 there were 0 states with a significant positive effect, 36 states with a significant negative effect, and 3 states with no significant effects. The fourth column represents the average effect across states that have adopted treatment (i.e. the average treatment effect on treated). The final column represents the population-level effect, i.e. in 2022 we estimate that approximately 6.5 million Americans are insured as a result of the Medicaid expansion. We do not have access to uninsurance rates in 2020 (likely due to the Covid-19 pandemic) and so we do not report any results for this year.

Based on our analysis, the results in Table 1 show that adopting the expansion policy had a substantial effect of reducing uninsurance rates in the states that have adopted it. Concretely, for every year between 2014 and 2022, nearly every adopting state exhibits statistically significant reduction in uninsurance rates. The estimated average effect from 2015-2022 is between 2-3% across the states that have implemented expansion, resulting in an estimated 6-7 million Americans that are now covered as a consequence of the Medicaid expansion. Consequently, based on our analysis, the Medicaid expansion component of the ACA has delivered in reducing uninsurance rates in those states that have adopted it.

Table 1: Results for uninsurance rates

	Positive	Negative	Null	ATET (SE)	Population Eff.	(SE)
2014	1	23	2	-1.7% (0.1)	-3,100,000	(200,000)
2015	1	28	0	-2.3% (0.1)	-5,000,000	(100,000)
2016	2	28	1	-2.4% (0.1)	-5,800,000	(200,000)
2017	2	28	2	-2.7% (0.1)	-6,600,000	(300,000)
2018	1	30	1	-2.8% (0.1)	-6,800,000	(100,000)
2019	0	29	5	-2.6% (0.1)	-6,700,000	(200,000)
2021	1	36	0	-2.9% (0.1)	-7,500,000	(200,000)
2022	0	36	3	-2.2% (0.2)	-6,500,000	(400,000)

Infant mortality effect (2019)

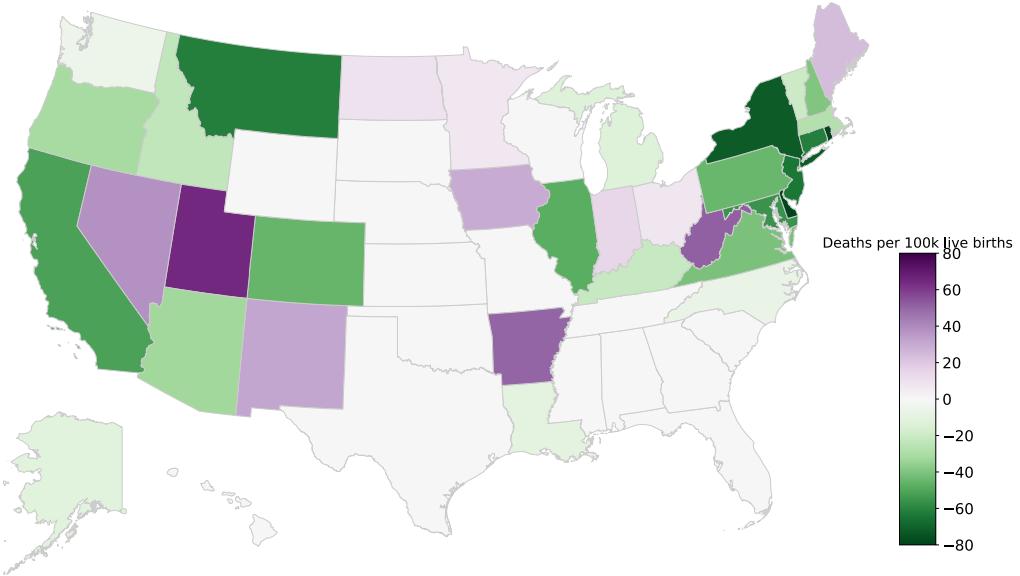


Figure 5. Estimated causal effect of the Medicaid expansion on infant mortality rates in 2019. Measured in infant deaths per 100,000 live births. Green (respectively purple) shading indicates a decrease (respectively increase) in infant mortality. As per Table 2, for this 2019 data, a total of 3 states reported a significant increase, whereas 22 states reported a significant decrease.

4.2.2 Infant mortality

We report the results of our analysis in Table 2 and provide a visualization of the estimated effects in Figure 5. To clarify, the measured outcome is actually a moving average with window size 3 of infant mortalities per 100,000 births. The decision to do so was made on the basis of the fact that certain states have birth population adjusted infant mortalities that vary quite dramatically year to year because within the United States, infant mortalities are relatively rare events. We can see that Medicaid expansion has moderate effects on reducing infant mortalities. Every year, there are around 5 states that have significant increases in newborn deaths, 10 states that have no significant changes, and 15-20 states that have significant decreases. The average reduction across states between 2014 and 2020 is between 20-30 infant mortalities per 100,000 live births. For reference, in 2019 across the United States there were

560 reported infant deaths per 100,000 live births. The final column reports the estimated effect of number of lives saved each year due to the policy. Our analysis estimates that between 2014 and 2020 approximately 5000 newborns would have died if not for the adoption of Medicaid expansion.

Figure 4 indicates a trend break for infant mortality: prior to 2014 infant mortality is trending downwards but our method estimates that infant mortality would have increased under the counterfactual. This may lead one to doubt the validity of our causal claims, but we remark that past studies [BBS18] have highlighted the fact that infant mortality rates have, indeed, increased for non-Medicaid expansion states between 2014-2016. Our method uses the states that did not adopt Medicaid expansion to impute the counterfactual, so it is quite reasonable that we would estimate the infant mortality rates to increase under the control for the states that did adopt the expansion.

Table 2: Results of infant mortalities per 100,000 births

	Positive	Negative	Null	ATET (SE)	Population Eff. (SE)
2014	5	12	9	-12 (3)	-270 (60)
2015	6	17	6	-22 (2)	-640 (50)
2016	5	15	11	-26 (5)	-770 (100)
2017	5	16	11	-30 (3)	-870 (60)
2018	4	18	10	-31 (3)	-880 (70)
2019	3	22	9	-33 (7)	-920 (140)
2020	6	20	10	-19 (5)	-750 (100)

4.2.3 Healthcare spending

Our analysis of the effects of expansion of health expenditures per capita are reported in Table 3. As before, the first three columns report the number of states with statistically significant effects in each direction. We can see from the table that its unclear whether the expansion caused an increase in health expenditures per capita. Between 2014 and 2020

Table 3: Results of yearly health expenditures per capita

	Positive	Negative	Null	ATET (SE)
2014	12	3	11	\$ 68 (14)
2015	9	7	13	\$ 26 (16)
2016	12	7	12	\$ 65 (17)
2017	11	8	13	\$ 79 (18)
2018	14	10	8	\$ 79 (17)
2019	14	11	9	\$ 111 (19)
2020	12	12	12	\$ -1 (24)

there is a mix of states that have reported significant increases, decreases, and null effects, on healthcare spending. Similarly in this vein, the average effect of the policy on expenditures is quite small, an increase of around \$100 for most of the years in that time period. For reference, in 2019, the health expenditures per capita across the entire United States was almost \$12,000. Therefore, our analysis suggests that as opposed to suggestions to ACA

would increase healthcare costs and spending, Medicaid expansion has negligible effects on healthcare expenditures per capita.

5 Conclusion

In this paper we provide new methodology for estimating individual treatment effects and significance testing based on prior work in matrix completion. Under a low-rank assumption on the matrix of outcomes under controls, we are able to estimate the counterfactual for units that undergo treatment and, consequently, provide estimates of the treatment effect for each unit. We relax the noise conditions assumed in prior work to allow for more general, heteroskedastic noise settings. Additionally, we establish confidence intervals that allows us to test whether the estimated effects are significant or not. Our works also extends towards estimation and inferential procedures from the weighted treatment effect. There are several interesting future directions to consider. Many papers, including ours, in the causal panel data setting relies on a factor model assumption. However there is little work considering the robustness of causal conclusions to this structural assumption. Additionally, many applied practitioners in these settings will have access to further covariates $X_{i,t}$ for each unit and time period. Methods to incorporate this covariate information with our matrix completion approach for causal effect estimation would be an interesting avenue to study.

To motivate our method, we apply it to analyze the causal effect of Medicaid expansion, passed as part of the Affordable Care Act, for states that have adopted it. We find that this policy has substantially reduced uninsurance rates in the states that have implemented this policy: our analysis reports that in 2022, an estimated 6.5 million Americans now have insurance because of the adoption of this policy. We also find that the expansion has negligible effects on increasing healthcare spending. Finally, our method also reports that the expansion has played a moderate role in reducing infant mortality rates: we estimate that between 2014 and 2020, there are approximately 5,000 newborn babies who would have passed away if not for Medicaid expansion.

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