Abstract

This article studies experimental design in settings where the experimental units are large aggregate entities (e.g., markets), and only one or a small number of units can be exposed to the treatment. In such settings, randomization of the treatment may result in treated and control groups with very different characteristics at baseline, inducing biases. We propose a variety of synthetic control designs (Abadie, Diamond and Hainmueller, 2010, Abadie and Gardeazabal, 2003) as experimental designs to select treated units in non-randomized experiments with large aggregate units, as well as the untreated units to be used as a control group. Average potential outcomes are estimated as weighted averages of treated units, for potential outcomes with treatment—and control units, for potential outcomes without treatment. We analyze the properties of estimators based on synthetic control designs and propose new inferential techniques. We show that in experimental settings with aggregate units, synthetic control designs can substantially reduce estimation biases in comparison to randomization of the treatment.

1. Introduction

Consider the problem of a ride-sharing company choosing between two compensation plans for drivers (Doudchenko et al., n.d.; Jones and Barrows, 2019). The company can either keep the current compensation plan or adopt a new one with higher incentives. In order to estimate the effect of a change in compensation plans on profits, the company’s data science unit designs an experimental evaluation where the new plan is deployed at a small scale, say, in one of the local markets (cities) in the country. In this setting, a randomized control trial—or A/B test, where drivers in a local market are randomized into the new plan (active treatment arm) or the status-quo (control treatment arm)—is problematic. On the one hand, such an experiment...
raises equity concerns, as drivers in the same local market but in different treatment arms obtain different compensations for the same jobs. On the other hand, if drivers in the active treatment arm respond to higher incentives by working longer hours, they will effectively steal business from drivers in the control arm of the experiment, which will result in biased experimental estimates.

A possible approach to this problem is to assign an entire local market to treatment, and use the rest of the local markets, which remain under the current compensation plan during the experimental periods, as potential comparison units. In this setting, using randomization to assign the active treatment allows ex-ante (i.e., pre-randomization) unbiased estimation of the effect of the active treatment. However, ex-post (i.e., post-randomization) biases can be large if, at baseline, the treated unit is different from the untreated units in the values of the features that affect the outcomes of interest.

As in the ride-sharing example where there is only one treated local market, large biases may arise more generally in randomized studies when either the treatment arm or the control arm contains a small number of units, so randomized treatment assignment may not produce treated and control groups that are similar in their features. In those cases, the fact that estimation biases would have averaged out over alternative treatment assignments is of little comfort to a researcher who is, in practice, limited to one assignment only.

To address these challenges, we propose the use of the synthetic control method (Abadie, Diamond and Hainmueller, 2010, Abadie and Gardeazabal, 2003) as an experimental design to select treated units in non-randomized experiments, as well as the untreated units to be used as a comparison group. We adopt the name synthetic control designs to refer to the resulting experimental designs.

1 A randomized evaluation across many markets is a potential solution to the problem of experimental interference between drivers. In practice, however, large-scale market-level randomized evaluations are often unfeasible. In the context of the ridesharing company example, large-scale market-level randomized evaluations (i) could be prohibitively expensive, (ii) could still raise substantial equity concerns, (iii) could create great disappointment among the large number of drivers in the treated cities if the program is rolled back after experimentation, and (iv) in some cases, the number of cities where the company operates could be too small for effective randomization.

2 While we leave the “experimental” qualifier implicit in “synthetic control design”, it should be noted that the synthetic control designs proposed in this article differ from observational synthetic control designs (e.g., Abadie, Diamond and Hainmueller, 2010, Abadie and Gardeazabal, 2003, Doudchenko and Imbens, 2016), for which the identity of the treated unit(s) is taken as given.

3 See, e.g., Abadie (2021), Amjad, Shah and Shen (2018), Arkhangelsky et al. (2021), Doudchenko and Imbens
In our framework, the choice of the treated unit (or treated units, if multiple treated units are desired) aims to accomplish two goals. First, it is often useful to select the treated units such that their features are representative of the features of an aggregate of interest, like an entire country market. The treatment effect for the treated units selected in this way may more accurately reflect the effect of the treatment on the aggregate of interest. Second, the treated units should not be idiosyncratic in the sense that their features cannot be closely approximated by the untreated units. Otherwise, the reliability of the estimate of the effect on the treated unit may be questionable. We show how to achieve these two objectives, whenever they are possible to achieve, using synthetic control methods.

While we are aware of the extensive use of synthetic control methods for experimental design in data science units, especially in the technology industry, the academic literature on this subject is at a nascent stage. There are, however, a few publicly available studies that are connected to this article. Aside to the present article, to our knowledge, Doudchenko et al. (n.d.) and Doudchenko et al. (2021) are the only other publicly available studies on the topic of experimental design with synthetic controls. The focus of Doudchenko et al. (n.d.) is on statistical power, which they calculate by simulation of the estimated effects of placebo interventions on historical (pre-experimental) data. That is, the selection of treated units is based on a measure of statistical power implied by the distribution of the placebo estimates for each unit. As a result, estimates based on the procedure in Doudchenko et al. (n.d.) target the effect of the treatment for the unit or units that are most closely tracked in the placebo distribution. In the same spirit, the target parameter in Doudchenko et al. (2021) is the treatment effect for a weighted average of treated units that can be closely matched in their pre-treatment outcomes by a weighted average of untreated units. In the present article, we aim to take a different perspective on the problem of unit selection in experiments with synthetic controls; one that takes into account the extent to which different sets of treated and control units approximate an aggregate causal effect of interest chosen by the analyst, such as the average treatment effect for (2016) for background material on synthetic controls and related methods.

See, in particular, Jones and Barrows (2019), which also provides the basis for the ride-sharing example above.
the relevant population. The inferential methods in the present article also differ from those in the related literature. In particular, Doudchenko et al. (2021) propose a permutation procedure for inference that requires that potential outcomes without the treatment are independent and identically distributed (i.i.d.) in time. In contrast, the inferential procedure proposed in the present article allows for time series dependence and non-stationarity in outcomes, which are pervasive features of time-series data. Another important difference between the present article and Doudchenko et al. (n.d.) and Doudchenko et al. (2021) is that Doudchenko et al. (n.d.) and Doudchenko et al. (2021) make use of pre-treatment outcomes only to select treated and control units, while our method allows the use of other observed features of the units. Agarwal, Shah and Shen (2021) propose synthetic interventions, a framework related to synthetic controls, and apply it to estimate treatment effect heterogeneity in an experimental setting with multiple treatments. Their work primarily focuses on the analyses of experimental data, but not on the design of experiments. Bottmer et al. (2021) is also related to the present article in the sense that they study synthetic control estimation in an experimental setting. Their article, however, considers only the case when the treatment is randomized, and is not concerned with issues of experimental design.

2. Synthetic Control Designs

We consider a setting with $T$ time periods and $J$ units, which may represent $J$ local markets as in the ride-sharing example in the previous section. Let $T_0$ be the number of pre-experimental periods, with $1 \leq T_0 < T$. At the end of period $T_0$, a researcher designs an experiment to conduct during periods $T_0 + 1, T_0 + 2, \ldots, T$. Using information available at $T_0$, the experimenter aims to select the set of units that will be administered treatment (intervention) during the experimental periods.

To define causal parameters, we formally adopt a potential outcomes framework. For any $j \in \{1, \ldots, J\}$ and any $t \in \{T_0 + 1, \ldots, T\}$, let $Y_{jt}^I$ be the potential outcome for unit $j$ at time $t$ when the unit is exposed to treatment starting at $T_0 + 1$. Similarly, for any $j \in \{1, \ldots, J\}$ and any $t \in \{1, \ldots, T\}$, let $Y_{jt}^N$ be the potential outcome for unit $j$ at time $t$ under no treatment. We assume that the outcome variable of interest is scaled so that it does not depend on the size of
the unit. In the ridesharing example, $Y_{jt}^I$ and $Y_{jt}^N$ could measure net income divided by market size, under the active and the control treatment, respectively. Unit-level treatment effects are defined as

$$\alpha_{jt} = Y_{jt}^I - Y_{jt}^N,$$

for $j = 1, \ldots, J$ and $t = T_0 + 1, \ldots, T$. The unit-level treatment effect $\alpha_{jt}$ represents the effect of switching at time $T_0 + 1$ to the active treatment on the outcome of unit $j$ at time $t > T_0$. We aim to estimate the average treatment effect

$$\tau_t = \sum_{j=1}^{J} f_j \cdot (Y_{jt}^I - Y_{jt}^N),$$

for $t = T_0 + 1, \ldots, T$. In this expression, $f_1, \ldots, f_J$ represents a set of known positive weights that are relevant to the definition of the average. In the ride-sharing example of the previous section, $f_j$ could represent the size of local market $j$ as a share of the national market. Without loss of generality, and because it is the case in many applications, we can assume that the weights $f_j$ sum to one,

$$\sum_{j=1}^{J} f_j = 1.$$ 

In the case when units are equally weighted, we set $f_j = 1/J$ for $j = 1, \ldots, J$. We use the notation $f$ for a vector that collects the values of $f_j$ for all the units, i.e., $f = (f_1, \ldots, f_J)$.

At time $T_0$, in order to estimate the treatment effect $\tau_t$ for $t = T_0 + 1, \ldots, T$, the experimenter chooses $w = (w_1, \ldots, w_J)$ and $v = (v_1, \ldots, v_J)$, such that

$$\sum_{j=1}^{J} w_j = 1,$$

$$\sum_{j=1}^{J} v_j = 1,$$

$$w_j \geq 0, \ v_j \geq 0, \ \text{and} \ w_j v_j = 0, \ \forall j = 1, \ldots, J.$$ 

Units with $w_j > 0$ are units that will be assigned to the intervention of interest from $T_0 + 1$ to $T$. These units are chosen to estimate average outcomes under the intervention of interest. Units with $w_j = 0$ constitute an untreated reservoir of potential control units (a “donor pool”).
Among units with $w_j = 0$, those with $v_j > 0$ are used to estimate average outcomes under no intervention.

The first goal of the experimenter is to choose $w_1, \ldots, w_J$ such that

$$\sum_{j=1}^{J} w_j Y^I_{jt} = \sum_{j=1}^{J} f_j Y^I_{jt},$$

for $t = T_0 + 1, \ldots, T$. If equation (1) holds, a weighted average of outcomes for the units selected for treatment reproduces the average outcome with treatment for the entire population of $J$ units.

In practice, however, the choice of $w_1, \ldots, w_J$ cannot directly rely on matching the population average of $Y^I_{jt}$, as in equation (1). The quantities $Y^I_{jt}$ are unobserved before time $T_0 + 1$, and will remain unobserved in the experimental periods for the units that are not exposed to the treatment. Instead, we aim to approximate equation (1) using predictors observed at $T_0$ of the values of $Y^I_{jt+1}, \ldots, Y^I_{jT}$. Note also that it is not possible to use the weights $w_1 = f_1, \ldots, w_J = f_J$, because it would leave no units in the donor pool, making the set of units with $v_j > 0$ empty and violating the second condition in (2).

The second goal of the experimenter is to choose $v_1, \ldots, v_J$ such that

$$\sum_{j=1}^{J} v_j Y^N_{jt} = \sum_{j=1}^{J} f_j Y^N_{jt},$$

or, alternatively,

$$\sum_{j=1}^{J} v_j Y^N_{jt} = \sum_{j=1}^{J} w_j Y^N_{jt}.$$  

If equations (2) or (3) hold, a weighted average of outcomes for the units in the donor pool reproduces the average outcome without treatment for the entire population of $J$ units (equation (2)), or for the units selected for treatment (equation (3)). Like in the previous case with treated outcomes, it is not feasible to directly choose $v_1, \ldots, v_J$ so that equation (2) or (3) is satisfied. Instead, we propose a variety of methods to approximate either (2) or (3) based on predictors of $Y^N_{jt+1}, \ldots, Y^N_{jT}$.

For the treated units, we define $Y_{jt} = Y^N_{jt}$ if $t = 1, \ldots, T_0$, and $Y_{jt} = Y^I_{jt}$ if $t = T_0 + 1, \ldots, T$. For the untreated units, we define $Y_{jt} = Y^N_{jt}$, for all $t = 1, \ldots, T$. That is, $Y_{jt}$ is the outcome
observed for unit $j = 1, \ldots, J$ at time $t = 1, \ldots, T$. We say that

$$
\sum_{j=1}^{J} w_{j} Y_{jt} \quad \text{and} \quad \sum_{j=1}^{J} v_{j} Y_{jt}
$$

are the synthetic treated and synthetic control outcomes, respectively. The difference between these two quantities is

$$
\tau_t(w, v) = \sum_{j=1}^{J} w_{j} Y_{jt} - \sum_{j=1}^{J} v_{j} Y_{jt},
$$

for $t = T_0 + 1, \ldots, T$. Suppose that equations (1) and (2) hold. Then, $\tau_t(w, v)$ is equal to the average treatment effect, $\tau_t$. If equation (3) holds instead, then $\tau_t(w, v)$ is equal to the average effect of the treatment on the treated ($w$-weighted),

$$
\tau_t^{T} = \sum_{j=1}^{J} w_{j} \cdot (Y_{jt}^{I} - Y_{jt}^{N})
$$

(Doudchenko et al., 2021).

We choose $w = (w_1, \ldots, w_J)$ and $v = (v_1, \ldots, v_J)$ to match the pre-intervention values of predictors of the potential outcomes $Y^{N}_{jt}$ and $Y^{I}_{jt}$ for $t > T_0$.

Let $X_j$ be a column vector of pre-intervention features of unit $j$. We see the features in $X_j$ as predictors of the values of $Y^{N}_{jt}$ and $Y^{I}_{jt}$ in the post-intervention periods, in a sense that will be made precise in Section 3. We use the notation

$$
\bar{X} = \sum_{j=1}^{J} f_j X_j.
$$

That is, $\bar{X}$ is the vector of population values for the predictors in $X_j$. For any real vector $x$, $\|x\|$ is the Euclidean norm of $x$, and $\|x\|_0$ is the number of non-zero coordinates of $x$. Let $m$ and $\overline{m}$ be positive integers such that $1 \leq m \leq \overline{m} \leq J - 1$. A simple selector of $w = (w_1, \ldots, w_J)$ and $v = (v_1, \ldots, v_J)$ is

$$
\min_{w_1, \ldots, w_J, \, v_1, \ldots, v_J} \left\| \bar{X} - \sum_{j=1}^{J} w_{j} X_j \right\|^2 + \left\| \bar{X} - \sum_{j=1}^{J} v_{j} X_j \right\|^2 \\
\text{s.t.} \sum_{j=1}^{J} w_j = 1,
$$
\[
\sum_{j=1}^{J} v_j = 1, \\
w_j, v_j \geq 0, \quad \forall j = 1, \ldots, J, \\
w_jv_j = 0, \quad \forall j = 1, \ldots, J, \\
m \leq \|w\|_0 \leq \overline{m}.
\]  
(4)

The first term of the objective function in (4) measures the discrepancies between the population average of the features in \(X_j\) (\(f\)-weighted) and the averages of the features for units assigned to the treatment group (\(w\)-weighted). The second term is analogous but with the second average taken over the units assigned to no intervention (\(v\)-weighted). The first four constraints require that the weights in \(w\), as well as the weights in \(v\), are non-negative and sum to one. They also require that any unit selected for treatment cannot be utilized as a control unit — so, if \(w_j > 0\), then \(v_j = 0\). The last constraint allows a minimum and maximum number of units assigned to treatment. This restriction is of practical importance in a variety of contexts, especially when experimentation is costly and the experimenter is restricted in the number of units that may receive the treatment. The values \(m = 1\) and \(\overline{m} = J - 1\) correspond to the unconstrained case. The last constraint in (4) is not the only conceivable restriction to the size or cost of the experiment. An explicit upper bound on the cost of an experiment would be given by \(c' d \leq B\), where the \(j\)-th coordinate of \(c\) is equal to the cost of assigning unit \(j\) to treatment, \(d\) is a \(J\)-dimensional vector with ones for coordinates with \(w_j > 0\), and zeros otherwise, and \(B\) is the experimenter’s budget.

Let \(w^* = (w^*_1, \ldots, w^*_J)\) and \(v^* = (v^*_1, \ldots, v^*_J)\) be a solution to the optimization problem in (4). In practice, we do not require optimality of \((w^*, v^*)\), as long as \((w^*, v^*)\) is feasible and satisfies \(\overline{X} - \sum_{j=1}^{J} w^*_jX_j \approx 0\) and \(\overline{X} - \sum_{j=1}^{J} v^*_jX_j \approx 0\), where 0 is a vector of zeros of the same dimension as \(X_j\). Suppose that units with \(w^*_j > 0\) are assigned to treatment in the experiment, and units with \(w^*_j = 0\) are kept untreated. A synthetic control estimator of \(\tau_t\) is \(\hat{\tau}_t = \tau_t(w^*, v^*)\), i.e.,

\[
\hat{\tau}_t = \sum_{j=1}^{J} w^*_jY_{jt} - \sum_{j=1}^{J} v^*_jY_{jt}.
\]  
(5)

This estimator is based on approximations to equations (1) and (2) that rely on \(X_j\), the observed
predictors of the potential outcomes, $Y^N_{jt}$, and $Y^I_{jt}$. Note that for every solution to (4) with $m \leq \|v\|_0 \leq \overline{m}$, there is another solution that swaps the roles of the treated and the untreated in the experiment without changing the objective value.

In what follows, we take the weight selector in (4) as a starting point for synthetic control designs, and modify it in several respects. A second formulation of the synthetic control design is based on equations (1) and (3),

$$
\begin{align*}
\min_{w_1, \ldots, w_J, \ v_1, \ldots, v_J} & \quad \left\| \bar{X} - \sum_{j=1}^J w_j X_j \right\|^2 + \beta \left\| \sum_{j=1}^J w_j X_j - \sum_{j=1}^J v_j X_j \right\|^2 \\
\text{s.t.} & \quad \sum_{j=1}^J w_j = 1, \\
& \sum_{j=1}^J v_j = 1, \\
& \quad w_j, v_j \geq 0, \quad \forall j = 1, \ldots, J, \\
& \quad w_j v_j = 0, \quad \forall j = 1, \ldots, J, \\
& \quad m \leq \|w\|_0 \leq \overline{m}. 
\end{align*}
$$

The parameter $\beta > 0$ reflects the trade-off between selecting treated units to fit the aggregate value of the predictors $\bar{X}$, and selecting control units to fit the aggregate value of the treated units. A small value of $\beta$ favors designs with treated units that closely match $\bar{X}$. A large value of $\beta$, instead, favors designs with aggregate treated and aggregate control units that closely match each other. While it is possible to use data-driven selectors of $\beta$, the rule of thumb $\beta = 1$ provides a natural choice, which equally weights the two terms in the objective function in (6).

For this formulation of the synthetic control design, treatment assignment and estimation follow the same procedures as with the previous one. Large values for $\beta$ produce estimators that target the $w$-weighted average effect of the treatment on the treated, $\tau^T_t$ of Doudchenko et al. (2021). Small values of $\beta$ prioritize estimation of the average treatment effect, $\tau_t$.

In our third formulation of the synthetic control design, the experimenter selects a synthetic treated unit to match the average values of the characteristics in the population. However, unlike in the design in (6), the experimenter chooses multiple synthetic controls, one for each
unit that contributes to the synthetic treated unit. For any $J$-dimensional vector of non-negative coordinates, $\mathbf{w} = (w_1, \ldots, w_J)$, let $\mathcal{J}_w$ be the set of the indices with non-zero coordinates, $\mathcal{J}_w = \{ j : w_j > 0 \}$. Our next version of the synthetic control design is:

$$\min_{w_j, \forall j = 1, 2, \ldots, J, \quad v_{ij}, \forall i, j = 1, 2, \ldots, J} \left\| \mathbf{X} - \sum_{j=1}^{J} w_j \mathbf{X}_j \right\|^2 + \xi \sum_{j=1}^{J} w_j \left\| \mathbf{X}_j - \sum_{i=1}^{J} v_{ij} \mathbf{X}_i \right\|^2$$

s.t. \[ \sum_{j=1}^{J} w_j = 1, \]
\[ w_j \geq 0, \quad \forall j = 1, \ldots, J, \]
\[ \sum_{i=1}^{J} v_{ij} = 1, \quad \forall j \in \mathcal{J}_w, \]
\[ v_{ij} = 0, \quad \forall i \in \mathcal{J}_w, \quad j = 1, \ldots, J, \]
\[ v_{ij} \geq 0, \quad \forall j \in \mathcal{J}_w, \quad i = 1, \ldots, J, \]
\[ v_{ij} = 0, \quad \forall j \notin \mathcal{J}_w, \quad i = 1, \ldots, J, \]
\[ m \leq \| \mathbf{w} \|_0 \leq \overline{m}. \] (7)

The parameter $\xi > 0$ arbitrates potential trade-offs between selecting treated units to fit the aggregate value of the predictors $\mathbf{X}$, and selecting control units to fit the values of the predictors for the treated units. A small value of $\xi$ favors experimental designs with treated units that closely match $\mathbf{X}$. A large value of $\xi$, instead, favors designs where the values of the predictors for the treated units are closely matched by their respective synthetic controls.

Let $\{ w_j^*, v_{ij}^* \}_{i,j = 1, \ldots, J}$ be a solution of the optimization problem in (7). As before, we do not strictly require optimality of $\{ w_j^*, v_{ij}^* \}_{i,j = 1, \ldots, J}$, provided $\{ w_j^*, v_{ij}^* \}_{i,j = 1, \ldots, J}$ is feasible and $\mathbf{X} - \sum_{j=1}^{J} w_j^* \mathbf{X}_j \approx \mathbf{0}$ and $\mathbf{X}_j - \sum_{j=1}^{J} v_{ij}^* \mathbf{X}_j \approx \mathbf{0}$ for all $j$ such that $w_j^* > 0$. Assign units with $w_j^* > 0$ to treatment in the experiment, and keep units with $w_j^* = 0$ untreated. Let

$$v_{j}^* = \sum_{i=1}^{J} w_i^* v_{ij}^*. \quad (8)$$

Then,

$$\hat{\tau}_t = \sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_{j}^* Y_{jt}.$$
\begin{align*}
&= \sum_{j=1}^{J} w_j^* \left( Y_{jt} - \sum_{i=1}^{J} v_{ij}^* Y_{it} \right).
\end{align*}

Our next adjustment to the synthetic control design is motivated by settings where the units may be naturally divided in clusters with similar values in the predictors, $X_1, \ldots, X_J$. For example, weather patterns, which may be highly dependent across cities in the same region (e.g., Northeast, Midwest, etc, in the US), may influence the seasonality of the demand for ride-sharing services. In those cases, it is natural to treat each cluster (each region, in our example) as a distinct experimental design to ameliorate interpolation biases. Figure 1 illustrates this point. Panels (a) and (b) depict identical samples in the space of the predictors. In this simple example, we have two predictors only, and their values for each of the units are represented by the coordinates of the points in the figure, which are the same in the two panels. Red dots represent
units assigned to treatment. All other units are plotted as black dots. Panel (a) visualizes the result of treating the entire sample as one cluster. Three units are assigned to treatment. They closely reproduce the value of $\overline{X}$, but they all fall in the same central cluster, far away from observations in other clusters. In panel (b), assignment to treatment takes into account the clustered nature of the data, and one unit is treated per cluster. This provides a better approximation of the distribution of the predictor values for the entire sample, ameliorating concerns of interpolation biases.

Suppose we divide the set of $J$ available units into $K$ clusters. Let $\mathcal{I}_k$ be the set of indices for the units in cluster $k$. The cluster mean is

$$\overline{X}_k = \frac{\sum_{j \in \mathcal{I}_k} f_j X_j}{\sum_{j \in \mathcal{I}_k} f_j},$$

for each cluster $k = 1, \ldots, K$. For each index $i = 1, \ldots, J$, let $k(i)$ be the cluster that unit $i$ belongs to, i.e., $i \in \mathcal{I}_{k(i)}$. A clustered version of the synthetic control design in (7) is given by:

$$\min_{w_j, v_{ij}, v_{i,j}, j=1,2,\ldots,J} \sum_{k=1}^K \left( \sum_{j \in \mathcal{I}_k} f_j \right) \left\{ \| \overline{X}_k - \sum_{j \in \mathcal{I}_k} w_j X_j \|_2^2 + \xi \sum_{j \in \mathcal{I}_k} w_j \| X_j - \sum_{i,j \in \mathcal{I}_k} v_{ij} X_i \|_2^2 \right\}$$

s.t. $\sum_{j \in \mathcal{I}_k} w_j = 1$, $\forall k = 1, \ldots, K$,

$w_j \geq 0$, $\forall j = 1, \ldots, J$,

$\sum_{i=1}^J v_{ij} = 1$, $\forall j \in \mathcal{J}_w$,

$v_{ij} \geq 0$, $\forall j \in \mathcal{J}_w$, $i = 1, \ldots, J$,

$v_{ij} = 0$, $\forall j \notin \mathcal{J}_w$, $i = 1, \ldots, J$,

$v_{ij} = 0$, $\forall i \in \mathcal{J}_w$, $j = 1, \ldots, J$,

$v_{ij} = 0$, $\forall i, j$, such that $k(i) \neq k(j)$,

$m \leq \| w \|_0 \leq \bar{m}$.

We conclude this section by discussing other possible extensions to the synthetic control design. First, it is well known that synthetic control estimators may not be unique. Lack of uniqueness is typical in settings where the values of the predictors that a synthetic control is
targeting (i.e., $\overline{X}$ in equation (4), or $X_j$ for a treated unit in equation (7)) fall inside the convex hull of the values of $X_j$ for the units in the donor pool. To address this issue we adapt the penalized estimator of Abadie and L’Hour (2021) to the synthetic control designs proposed in this article. The penalized synthetic control estimator of Abadie and L’Hour (2021) is unique as long as predictor values for the units in the donor pool are in general quadratic position (see Abadie and L’Hour, 2021, for details). Moreover, penalized synthetic controls favor solutions where the synthetic units are composed of units that have predictor values, $X_j$, similar to the target values. Applying the penalized synthetic control of Abadie and L’Hour (2021) to the objective function of (4), we obtain

$$\min_{w_1,\ldots,w_J,\ v_1,\ldots,v_J} \left\| \overline{X} - \sum_{j=1}^J w_j X_j \right\|^2 + \left\| \overline{X} - \sum_{j=1}^J v_j X_j \right\|^2 + \lambda_1 \sum_{j=1}^J w_j \| \overline{X} - X_j \|^2 + \lambda_2 \sum_{j=1}^J v_j \| \overline{X} - X_j \|^2$$

s.t. $\sum_{j=1}^J w_j = 1,$

$\sum_{j=1}^J v_j = 1,$

$w_j, v_j \geq 0, \ \forall j = 1, \ldots, J,$

$w_j v_j = 0, \ \forall j = 1, \ldots, J,$

$m \leq \| w \|_0 \leq \overline{m}. \quad (9)$

Here, $\lambda_1$ and $\lambda_2$ are positive constants that penalize discrepancies between the target values of the predictor $\overline{X}$ and the values of the predictors for the units that contribute to their synthetic counterparts.$^5$

Other types of penalization are possible. In particular, Doudchenko and Imbens (2016), Doudchenko et al. (2021) and others have proposed synthetic control estimators that use ridge or elastic net regularization on the synthetic control weights (e.g., on $w_j$ and $v_j$ in design (4)). The synthetic control designs proposed in this article can incorporate regularization on the weights.

Finally, Abadie and L’Hour (2021), Arkhangelsky et al. (2021), and Ben-Michael, Feller

$^5$See Abadie and L’Hour (2021) for details on penalized synthetic control estimators. The synthetic control design in (9) is a penalized version of (4). Appendix A discusses how to apply the Abadie and L’Hour penalty to the other synthetic designs proposed in this article.
and Rothstein (2021) have proposed bias-correction techniques for synthetic control methods. Appendix A provides details on how to apply bias correction techniques in a synthetic control design.

3. Formal Results

We first introduce an extension of the linear factor model commonly employed in the synthetic control literature, which we use to analyze the properties of estimators that are based on a synthetic control design.

Assumption 1 Potential outcomes follow a linear factor model,

\begin{align*}
Y_{jt}^{N} &= \delta_t + \theta_t'Z_j + \lambda_t'\mu_j + \epsilon_{jt}, \quad (10a) \\
Y_{jt}^{I} &= \upsilon_t + \gamma_t'Z_j + \eta_t'\mu_j + \xi_{jt}, \quad (10b)
\end{align*}

where \(Z_j\) is a \((r \times 1)\) vector of observed covariates, \(\theta_t\) and \(\gamma_t\) are \((r \times 1)\) vectors of unknown parameters, \(\mu_j\) is a \((F \times 1)\) vector of unobserved covariates, \(\lambda_t\) and \(\eta_t\) are \((F \times 1)\) vectors of unknown parameters, and \(\epsilon_{jt}\) and \(\xi_{jt}\) are unobserved random shocks.

Equation (10a) is the linear factor model for potential outcomes under no treatment that is commonly employed in the literature as a benchmark model to analyze the properties of synthetic control estimators (see, e.g., Abadie, Diamond and Hainmueller, 2010, Ferman, 2020). Equation (10b) extends the linear factor structure to potential outcomes under treatment. The reason for extending the linear factor model to treated outcomes is that, in contrast to the usual setting of synthetic control estimation with observational data, experimental synthetic control designs require the choice of a treatment group in addition to the choice of a comparison group.

We employ the covariates in \(Z_j\) as well as pre-experimental values of the outcome variables \(Y_{jt}\) to construct the vectors of predictors, \(X_j\). In particular, let \(\mathcal{E} \subseteq \{1, \ldots, T_0\}\), \(T_\mathcal{E} = |\mathcal{E}|\), and let \(Y_j^\mathcal{E}\) be the \((T_\mathcal{E} \times 1)\) vector of \(T_\mathcal{E}\) pre-intervention outcomes for unit \(j\) and time indices in \(\mathcal{E}\). We define

\[\begin{bmatrix} Y_{j}^{\mathcal{E}} \\ Z_j \end{bmatrix},\]
for $j = 1, \ldots, J$. That is, the vector of predictors, $X_j$, collects the covariates in $Z_j$ and the pre-intervention outcome values, $Y_{jt}$, for the “fitting periods” in $E$. In practice, the values in $X_j$ are often scaled to make them independent of units of measurement, or to reflect the relative importance of each of the predictors (see, e.g., Abadie, 2021).

The next assumption gathers regularity conditions on model primitives.

**Assumption 2**

(i) Let $\lambda_{t,f}$ and $\eta_{t,f}$ be the $f$-th coordinates of $\lambda_t$ and $\eta_t$, respectively. There exists $\bar{\lambda}$ and $\bar{\eta}$, such that $|\lambda_{t,f}| \leq \bar{\lambda}$, for $t = 1, \ldots, T$, $f = 1, \ldots, F$ and $|\eta_{t,f}| \leq \bar{\eta}$, for $t = T_0 + 1, \ldots, T$, $f = 1, \ldots, F$.

(ii) $T_E \geq F$. Moreover, let $\lambda_E$ be the $(T_E \times F)$ matrix with rows equal to the $\lambda_t$’s indexed by $E$.

Let $\zeta_E$ be the smallest eigenvalue of $\lambda_E' \lambda_E$. Then, $\zeta = \zeta_E/T_E > 0$.

(iii) For $j = 1, \ldots, J$, $t = 1, \ldots, T$, $\epsilon_{jt}$ are mean zero and i.i.d. sub-Gaussian random variables with variance proxy $\bar{\sigma}^2$. Similarly, for $j = 1, \ldots, J$, $t = T_0 + 1, \ldots, T$, $\xi_{jt}$ are mean zero and i.i.d. sub-Gaussian random variables with variance proxy $\bar{\sigma}^2$.

Assumptions 2(i) and (ii) are regularity conditions similar to those in Abadie, Diamond and Hainmueller (2010). The restrictions in Assumption 2(iii) are similar to those invoked in Abadie, Diamond and Hainmueller (2010), Doudchenko and Imbens (2016), Chernozhukov, Wüthrich and Zhu (2021), and Arkhangelsky et al. (2021). Sub-Gaussianity is not strictly necessary, but it simplifies the form of our results. It can be relaxed by assuming bounded finite order moments (instead of bounding the entire moment generating function). Sub-Gaussianity is a relatively mild assumption. It holds for any Gaussian distribution, as well as any distribution with a bounded support. Distributions with heavy tails, including the Cauchy distribution, are not sub-Gaussian.

Unless otherwise noted, all probability statements are over the joint distribution of $\epsilon_{jt}$ and $\xi_{jt}$ and conditional on the values of the other components on the right-hand sides of equations (10a) and (10b). The next assumption pertains to the quality of the synthetic control fit. For concreteness, we focus on the base design in (4), and choose where $w^* = (w^*_1, \ldots, w^*_J)$ and
\( \mathbf{v}^* = (v_1^*, \ldots, v_J^*) \) so that the synthetic treated and synthetic control units reproduce the average values of \( \mathbf{X}_j \).

**Assumption 3** With probability one,

\[
\sum_{j=1}^{J} w_j^* Z_j = \sum_{j=1}^{J} f_j Z_j, \quad \sum_{j=1}^{J} w_j^* Y_{jt} = \sum_{j=1}^{J} f_j Y_{jt}, \quad \forall t \in \mathcal{E},
\]

and

\[
\sum_{j=1}^{J} v_j^* Z_j = \sum_{j=1}^{J} f_j Z_j, \quad \sum_{j=1}^{J} v_j^* Y_{jt} = \sum_{j=1}^{J} f_j Y_{jt}, \quad \forall t \in \mathcal{E}.
\]

Assumption 3 implies that the synthetic treated and control units defined by \( \mathbf{w}^* \) and \( \mathbf{v}^* \) provide a perfect fit for \( \mathbf{X} \). Assumption 3 is a strong restriction, which may only hold approximately in practice.

**Theorem 1** If Assumptions 1–3 hold, then for any positive integer \( q \),

\[
|E [\widehat{\tau}_t - \tau_t]| \leq \frac{\bar{\lambda}(\bar{\eta} + \bar{\lambda})F}{\zeta} J^{1/q} \sqrt{2(q \Gamma(q/2))}^{1/q} \frac{\bar{\sigma}}{\sqrt{T_\mathcal{E}}},
\]

(12)

where the expectation is taken over the distributions of \( \epsilon_{jt} \), for \( j = 1, \ldots, J, \ t = 1, \ldots, T \) and the distributions of \( \xi_{jt} \), for \( j = 1, \ldots, J, \ t = T_0 + 1, \ldots, T \). In (12), \( \Gamma(\cdot) \) stands for the gamma function.

The bias bound in Theorem 1 depends on the ratio between the scale of \( \epsilon_{jt} \), represented in (12) by \( \bar{\sigma} \), and the number of fitting periods \( T_\mathcal{E} \). Intuitively, the bias of the synthetic control estimator is small when a good fit in pre-intervention outcomes (Assumption 3) is obtained by implicitly fitting the values of the latent variables, \( \mu_j \). Overfitting happens when pre-intervention outcomes are instead fitted out of the variability in the individual transitory shocks, \( \epsilon_{jt} \). A small number of fitting periods, \( T_\mathcal{E} \), combined with enough variability in \( \epsilon_{jt} \) increases the risk of overfitting and, as a result, increases the bias bound. Similarly, for any fixed value of \( T_\mathcal{E} \), the bias bound increases with \( J \), reflecting the increased risk of over-fitting created by increased variability in \( \epsilon_{jt} \) over larger donor pools. Finally, the number of unobserved factors, \( F \), enters the bound linearly, which highlights the importance of including the observed predictors, \( Z_j \) — other than
pre-intervention outcomes — in the vector of fitting variables, $X_j$. Under the factor model in equations (10a) and (10b), observed predictors not included in $Z_j$ are shifted to $\mu_j$, increasing $F$ and the magnitude of the bias bound.

We next turn our attention to inference. We utilize a set of “blank periods,” $B \subseteq \{1, \ldots, T_0\} \setminus \mathcal{E}$, which comprise pre-intervention periods whose outcomes, $Y_{jt}$, have not been used to calculate $w^*$ or $v^*$. Because pre-intervention periods that are not in $\mathcal{E}$ or $B$ could always be discarded from the data, we can consider $B = \{1, \ldots, T_0\} \setminus \mathcal{E}$ only, without loss of generality. We therefore assume that the number of elements of $B$ is $T_B = |B| = T_0 - T_E$. We aim to test the null:

$$Y_{jt}^I = \delta_t + \theta_j'Z_j + \lambda_j'\mu_j + \xi_{jt}, \quad (13)$$

where $\xi_{jt}$ has the same distribution as $\epsilon_{jt}$.

Under the null hypothesis in (13), the distribution of $Y_{jt}^I$ is the same as the distribution of $Y_{jt}^N$, for $t = T_0 + 1, \ldots, T$, and $j = 1, \ldots, J$ (but the realized values of $Y_{jt}^I$ and $Y_{jt}^N$ may differ).

Recall from (5) that, for $t \in \{T_0 + 1, \ldots, T\}$, a synthetic control estimator is defined as

$$\hat{\tau}_t = \sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_j^* Y_{jt}. \quad (14)$$

Let $\hat{u}_t = \hat{\tau}_t, \forall t \in \{T_0 + 1, \ldots, T\}$ be the synthetic control estimator on the post-intervention periods. Similarly, for each $t \in B$ in the blank periods, let

$$\hat{u}_t = \sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_j^* Y_{jt}. \quad (15)$$

Such $\hat{u}_t$ for $t \in B$ are “placebo” treatment effects estimated for the blank periods. We study the properties of a test based on combinations from the set $\{\hat{u}_t : t \in B \cup \{T_0 + 1, \ldots, T\}\}$.

Let $\Pi$ be the set of all $(T - T_0)$-combinations of $B \cup \{T_0 + 1, \ldots, T\}$. That is, for each $\pi \in \Pi$, $\pi$ is a subset of indices from the blank periods and the post-intervention periods $B \cup \{T_0 + 1, \ldots, T\}$, such that $|\pi| = T - T_0$. The cardinality of $\Pi$ is $|\Pi| = (T - T_0)! / ((T - T_0)(T_0 - T_E)!)$. For each $\pi \in \Pi$, let $\pi(i)$ be the $i^{th}$ smallest value in $\pi$. We define the following $(T - T_0)$-dimensional
vector

$$\hat{e}_\pi = (\hat{u}_{\pi(1)}, \hat{u}_{\pi(2)}, ..., \hat{u}_{\pi(T - T_0)}).$$

In addition, let $$\hat{e} = (\hat{u}_{T_0 + 1}, ..., \hat{u}_T) = (\hat{\tau}_{T_0 + 1}, ..., \hat{\tau}_T).$$ This is a vector of treatment effect estimates from the post-intervention periods. For any $$(T - T_0)$$-dimensional vector $$e = (e_1, ..., e_{T - T_0})$$, we adopt the test statistic,

$$S(e) = \frac{1}{T - T_0} \sum_{t=1}^{T-T_0} |e_t|. \quad (14)$$

Other choices of test statistics are possible, such as those based on an $$L_p$$-norm of $$e$$ (Chernozhukov, Wüthrich and Zhu, 2021) and one-sided versions of the resulting test statistics (i.e., with $$e_t$$ or $$-e_t$$ replacing $$|e_t|$$ in equation (14)).

Let

$$\hat{p} = \frac{1}{|\Pi|} \sum_{\pi \in \Pi} 1\{S(\hat{e}_\pi) \geq S(\hat{e})\} \quad (15)$$

be the $$p$$-value based on (14). Theorem 2 below shows that if $$\lambda_t$$ are exchangeable random variables for $$t \in B \cup \{T_0 + 1, ..., T\}$$, then a test of the null hypothesis in (13) based on the $$p$$-value in (15) is exact.

**Theorem 2** Suppose that Assumptions 1–3 hold, that for $$j = 1, ..., J$$, $$t = 1, ..., T$$, $$\epsilon_{jt}$$ are continuously distributed, and that for $$t \in B \cup \{T_0 + 1, ..., T\}$$ the unknown parameters $$\lambda_t$$ are exchangeable random variables. Then,

$$\alpha - \frac{1}{|\Pi|} \leq \Pr(\hat{p} \leq \alpha) \leq \alpha,$$

for $$\alpha \in [0, 1]$$, where $$\Pr(\hat{p} \leq \alpha)$$ is taken over the distribution of $$\{\epsilon_{jt}, \lambda_t\}$$ for $$j = 1, ..., J$$ and $$t \in B \cup \{T_0 + 1, ..., T\}$$.

Note that, under the assumptions of Theorem 2, the potential outcome series $$Y_{jt}^N$$ is allowed to be non-stationary through the term $$\delta_t + \theta_j'Z_j$$ in equation (10a). This is in contrast with a related result in Doudchenko et al. (2021), which requires that the potential outcomes $$Y_{jt}^N$$ are i.i.d. in time. Exchangeability of $$\lambda_t$$ is a strong restriction. Theorem A.1 in Appendix A relaxes
this restriction by showing that conditional on $\bm{\lambda}$, (i.e., without resorting to exchangeability of $\bm{\lambda}$), the $p$-value in (15) is still approximately valid for large $T_{\varepsilon}$.

In some settings, the number of possible combinations, $|\Pi|$, could be very large, making exact calculation of $\hat{p}$ computationally expensive. In those instances, random samples of the combinations in $\Pi$ can be used to approximate the $p$-value in equation (15).

The inferential technique proposed in this article is related to, but distinct from, the permutation methods in Abadie, Diamond and Hainmueller (2010), Chernozhukov, Wüthrich and Zhu (2021), Chernozhukov, Wüthrich and Zhu (2019), Firpo and Possebom (2018), Lei and Candès (2020), and others. Inferential methods that reassign treatment across units (e.g., Abadie, Diamond and Hainmueller, 2010) are not appropriate for the designs of Section 2, which explicitly select treated and control units to satisfy an optimality criterion.

Similar to Chernozhukov, Wüthrich and Zhu (2021), our method is based on rearrangements of estimated treatment effects across time periods. But unlike Chernozhukov, Wüthrich and Zhu (2021), which proposes permutations over all periods including the pre-intervention periods, our inferential method permutes only over the blank periods and post-intervention periods, which are not used to estimate the weights in the synthetic control design. Relative to Chernozhukov, Wüthrich and Zhu (2021), the generative models of equations (10a) and (10b), which allow for unobserved factors, and the finite sample nature of the result in Theorem 2 and Theorem A.1 require both a novel testing procedure that takes advantage of the availability of blank periods and proof techniques that are, to our knowledge, new to the literature.

4. Simulation Study

4.1. Base Results

We report in this section the results of a simulation study that illustrates the behavior of the estimators proposed in this article. We consider a setting with $J = 15$ units, $r = 7$ observable covariates and $F = 11$ unobservable covariates. We simulate data for $T = 30$ periods in total, with $T_0 = 25$ pre-intervention periods, and $T - T_0 = 5$ experimental or post-intervention periods. We compute weights during the first $T_{\varepsilon} = 20$ periods, and leave periods $t = 21, \ldots, 25$ as blank periods. Periods $t = 26, \ldots, 30$ are the experimental periods.
We use the factor model in Assumption 1 to generate potential outcomes. For \( t = 1, \ldots, T \), we generate the series \( \delta_t \) and \( \upsilon_t \) as small-to-large re-arrangements of \( T \) i.i.d. Uniform \((0, 20)\) random variables. For \( j = 1, \ldots, J \), we set both \( Z_j \) and \( \mu_j \) to be random vectors of i.i.d. Uniform \((0, 1)\) random variables. For \( t = 1, \ldots, T \), we set \( \theta_t, \gamma_t, \lambda_t, \) and \( \eta_t \) to be random vectors of i.i.d. Uniform \((0, 10)\) random variables. Finally, for \( j = 1, \ldots, J \), and any \( t = 1, \ldots, T \), we set \( \epsilon_{jt} \) and \( \xi_{jt} \) to be i.i.d. Normal \((0, \sigma^2)\) random variables, with \( \sigma^2 = 1 \). In Online Appendix D we present additional simulation results of alternative values of the noise parameter \( \sigma^2 \).

Using the data generating process described above, we draw a single sample and conduct the synthetic control design in (4), with parameters \( \underline{m} = 1 \) and \( \overline{m} = 14 \), i.e., no constraint on the number of treated units. We report the results in Figures 2 and 3. In Figure 2, each blue dashed line represents one trajectory of the outcomes for the corresponding unit in the sample \( Y_{jt} \), for \( t = 1, \ldots, T \). The black solid line represents the trajectory of the synthetic treated unit \( \sum_{j=1}^{J} w^*_j Y_{jt} \), for \( t = 1, \ldots, T \). The black dashed line represents the trajectory of the synthetic control unit \( \sum_{j=1}^{J} v^*_j Y_{jt} \), for \( t = 1, \ldots, T \). The synthetic treated and synthetic control units closely track each other in the pre-experimental periods. They diverge in the experimental periods, when a treatment effect emerges as a result of the differences in the parameters of the data generating processes for \( Y_{jt}^N \) and \( Y_{jt}^I \). Figure 3 reports the difference between the synthetic treated and the synthetic control outcomes. The inferential procedure of Section 3 produces \( p \)-value equal to 0.0040 for the null hypothesis of no treatment effect in (13).

4.2. A Comparison of Synthetic Control Designs

In this section we compare the performance of different synthetic control designs under the same data generating process as in Section 4.1. We report the estimated average treatment effects and the synthetic treated and synthetic control weights for a single simulation. In Section 4.3, we compare the performance of different synthetic control designs over multiple simulations.

We consider five varieties of the synthetic control design:

1. **Unconstrained design**: This is the design in (4) without a cardinality constraint, so \( \underline{m} = 1 \) and \( \overline{m} = J - 1 = 14 \).

2. **Constrained design**: Same as the design in (4), but with \( \underline{m} = 1 \) and \( \overline{m} = 1, \ldots, 7 \).
Figure 2: Synthetic Treatment Unit and Synthetic Control Unit, when $\sigma^2 = 1$.

Note: The black solid line represents the synthetic treated outcome ($w^*$-weighted). The black dashed line represents the synthetic control outcome ($v^*$-weighted).

3. **Weakly-targeted design**: This is the design in (6). We vary $\beta$ from 0.01 to 100.

4. **Unit-level design**: This is the design in (7), which fits a different synthetic control to each unit assigned to treatment. We vary $\xi$ from 0.01 to 100.

5. **Penalized design**: This is the design in (9), with $\lambda = \lambda_1 = \lambda_2$. We vary $\lambda$ from 0.01 to 100. Section C in the Online Appendix discusses computational issues.

### 4.2.1. Average Treatment Effects

The first panel of Table 1 reports the average treatment effects, $\tau_t$. The first five columns in the second panel report average treatment effect estimates, $\hat{\tau}_t$, for periods $T_0 + 1 = 26$ through $T = 30$, under different synthetic control designs. The last three columns in the second panel report the mean absolute error (MAE) and the mean squared error (MSE), defined as

$$MAE = \frac{1}{T - T_0} \sum_{t=T_0+1}^{T} |\hat{\tau}_t - \tau_t|, \quad MSE = \frac{1}{T - T_0} \sum_{t=T_0+1}^{T} (\hat{\tau}_t - \tau_t)^2,$$

(16)

as well as the $p$-value in (15).

Because Table 1 reports outcomes for a single simulation only, the results may not be reflec-
Figure 3: Treatment Effect Estimate, when $\sigma^2 = 1$.

Note: This figure reports the difference between the synthetic treated and synthetic control outcomes of Figure 2. For the experimental periods, this is the treatment effect estimate.

tive of general patterns over many simulations, even for the particular data generating process employed to produce the data. However, we use a single simulation to illustrate patterns in the estimates that are induced directly by the features of their respective estimators. In Section 4.3 we report averages over a large number of simulations.

The Unconstrained estimates in Table 1 are fairly close to the true effects. The same is true for the Constrained estimates. As expected, the accuracy of the Constrained estimates improves as cardinality constraint is relaxed. For $\bar{m} = 7$ the Constrained and Unconstrained estimates coincide, because the Unconstrained estimator also uses seven treated units. Very large or very small values of $\beta$ and $\xi$ hinder the accuracy of estimates based on the Weakly-targeted design and the Unit-level design, respectively. Large values of $\beta$ and $\xi$ encourage designs that minimize the discrepancies between the characteristics of the treated units and the characteristics of their respective synthetic controls, at the potential cost of generating a synthetic treated unit that does not closely approximate $\overline{X}$. Conversely, very low values for $\xi$ encourage designs with a synthetic treated unit that closely reproduces $\overline{X}$, at the potential cost of choosing treated units with values of $X_j$ that cannot be closely approximated by a convex combination of untreated
Table 1: Results for a single simulation

| $\tau_t$ | $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ |
|----------|----------|----------|----------|----------|----------|
| $-15.56$ | $-17.76$ | $2.52$   | $-4.92$  | $-3.27$  |

| $\hat{\tau}_t$ | $MAE$ | $MSE$ | $\hat{\rho}$ |
| $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ |
|----------|----------|----------|----------|----------|
| Unconstrained |
| $m = 1$ | $-19.54$ | $-18.53$ | $1.84$ | $1.01$ | $0.61$ | $3.05$ | $13.41$ | $0.091$ |
| $m = 2$ | $-15.75$ | $-17.93$ | $2.32$ | $-11.18$ | $-4.14$ | $1.54$ | $8.01$ | $0.012$ |
| $m = 3$ | $-16.50$ | $-18.37$ | $2.44$ | $-0.60$ | $-2.89$ | $1.27$ | $4.02$ | $0.012$ |
| $m = 4$ | $-15.80$ | $-18.47$ | $1.97$ | $-1.31$ | $-3.87$ | $1.14$ | $2.85$ | $0.008$ |
| $m = 5$ | $-13.31$ | $-17.19$ | $3.71$ | $-7.70$ | $-2.91$ | $1.43$ | $2.93$ | $0.004$ |
| $m = 6$ | $-13.97$ | $-17.95$ | $3.94$ | $-8.51$ | $-3.18$ | $1.38$ | $3.50$ | $0.004$ |
| $m = 7$ | $-14.26$ | $-18.23$ | $3.57$ | $-8.96$ | $-3.66$ | $1.45$ | $3.90$ | $0.004$ |
| Constrained |
| $m = 1$ | $-19.54$ | $-18.53$ | $1.84$ | $1.01$ | $0.61$ | $3.05$ | $13.41$ | $0.091$ |
| $m = 2$ | $-15.75$ | $-17.93$ | $2.32$ | $-11.18$ | $-4.14$ | $1.54$ | $8.01$ | $0.012$ |
| $m = 3$ | $-16.50$ | $-18.37$ | $2.44$ | $-0.60$ | $-2.89$ | $1.27$ | $4.02$ | $0.012$ |
| $m = 4$ | $-15.80$ | $-18.47$ | $1.97$ | $-1.31$ | $-3.87$ | $1.14$ | $2.85$ | $0.008$ |
| $m = 5$ | $-13.31$ | $-17.19$ | $3.71$ | $-7.70$ | $-2.91$ | $1.43$ | $2.93$ | $0.004$ |
| $m = 6$ | $-13.97$ | $-17.95$ | $3.94$ | $-8.51$ | $-3.18$ | $1.38$ | $3.50$ | $0.004$ |
| $m = 7$ | $-14.26$ | $-18.23$ | $3.57$ | $-8.96$ | $-3.66$ | $1.45$ | $3.90$ | $0.004$ |
| Weakly-targeted |
| $\beta = 0.01$ | $-13.49$ | $-17.24$ | $3.67$ | $-3.79$ | $0.56$ | $1.74$ | $4.35$ | $0.067$ |
| $\beta = 0.1$ | $-14.48$ | $-17.24$ | $3.72$ | $-7.28$ | $-2.23$ | $1.23$ | $1.89$ | $0.004$ |
| $\beta = 1$ | $-13.11$ | $-17.56$ | $5.10$ | $-8.33$ | $-1.24$ | $2.13$ | $5.68$ | $0.016$ |
| $\beta = 10$ | $-16.95$ | $-18.38$ | $2.07$ | $-4.13$ | $-2.58$ | $0.79$ | $0.73$ | $0.008$ |
| $\beta = 100$ | $-16.39$ | $-20.24$ | $0.53$ | $-10.46$ | $-2.59$ | $2.31$ | $8.40$ | $0.044$ |
| Unit-level |
| $\xi = 0.01$ | $-13.63$ | $-19.49$ | $4.97$ | $-7.76$ | $2.40$ | $2.92$ | $10.57$ | $0.004$ |
| $\xi = 0.1$ | $-13.72$ | $-18.29$ | $5.07$ | $-8.10$ | $0.00$ | $2.28$ | $6.20$ | $0.024$ |
| $\xi = 1$ | $-16.16$ | $-18.38$ | $4.31$ | $-6.16$ | $-1.25$ | $1.26$ | $1.92$ | $0.012$ |
| $\xi = 10$ | $-17.60$ | $-16.72$ | $1.31$ | $-6.84$ | $-6.76$ | $1.94$ | $4.53$ | $0.016$ |
| $\xi = 100$ | $-18.58$ | $-18.43$ | $-1.12$ | $-1.45$ | $-11.76$ | $3.86$ | $21.40$ | $0.040$ |
| Penalized |
| $\lambda = 0.01$ | $-14.11$ | $-18.04$ | $3.62$ | $-8.90$ | $-3.75$ | $1.46$ | $3.88$ | $0.004$ |
| $\lambda = 0.1$ | $-13.20$ | $-16.64$ | $4.70$ | $-8.44$ | $-3.63$ | $1.91$ | $4.81$ | $0.004$ |
| $\lambda = 1$ | $-16.79$ | $-19.03$ | $3.56$ | $-0.62$ | $-2.94$ | $1.63$ | $4.56$ | $0.052$ |
| $\lambda = 10$ | $-22.88$ | $-22.42$ | $0.99$ | $-6.18$ | $1.68$ | $3.94$ | $20.75$ | $0.067$ |
| $\lambda = 100$ | $-22.98$ | $-25.58$ | $1.38$ | $-9.25$ | $4.51$ | $5.70$ | $39.40$ | $0.036$ |

Note: Unless otherwise noted, all designs use $m = 1$ and $\bar{m} = 14$ (unconstrained).

units. Finally, exceedingly large values of $\lambda$ damper the performance of the Penalized synthetic control designs. Large values of $\lambda$ encourage synthetic control and synthetic treated units that put their entire weights on the two units that are closest to $\bar{X}$ in the space of the predictors, worsening performance relative to designs with larger number of units in the treated and control groups. These patterns of performance for a single simulation are roughly in line with the ones that we show in Section 4.3 over a large number of simulations.
4.2.2. Synthetic Treated and Synthetic Control Weights

Tables 2 and 3 report the synthetic treated and synthetic control weights ($w^*$ and $v^*$, respectively) for the designs of Table 1. For the Unit-level design, synthetic control weights are aggregated as in (8). For the Unconstrained and Penalized designs, the synthetic treated and synthetic control weights can always be swapped without changing the objective values for their respective designs. For the Constrained design, the weights can be swapped when $\|v^*\|_0 \leq m$. When it is possible to swap synthetic treated and synthetic control weights, we choose the treated units so that the number of units with positive weights in $w^*$ is smaller than the number of units with positive weights in $v^*$. When $\|w^*\|_0 = \|v^*\|_0$, we determine whether or not to swap following a specific rule described in Section C in the Appendix.

The Constrained design imposes sparsity in the synthetic treatment weights through a hard cardinality constraint specified by the integer $m$. For $m = 7$, the Constrained and the Unconstrained weights coincide. For the Unit-level design, large values of $\xi$ generate sparsity in the synthetic treated weights. A sufficiently large value of $\xi$ produces a Unit-level design where the only treated unit is the unit that can be most closely fitted by a convex combination of the other ones. For large values of $\lambda$, the Penalized design behaves like a one-to-one matching design, assigning all the weight to one treated and one control unit. For small values of $\lambda$, the Penalized design weights are close to the Unconstrained design weights.

4.3. Performance Over Multiple Simulations

In this section, we compare the five synthetic control designs of Section 4.2 across averages over 1000 simulations that independently generate the model primitives (i.e., the factor loadings, covariates, and error terms) of Assumption 1. The data generating process is the same as in Section 4.1.

4.3.1. Average Treatment Effects

The first panel of Table 4 reports average treatment effects, $\tau_i$, averaged over 1000 simulations. The second panel reports estimated average treatment effects, mean absolute error, mean squared error, and $p$-value, all averaged over simulations, as well as rejection rates. Because the treatment
Table 2: Synthetic Treated Weights

| Parameter | \( w_1^* \) | \( w_2^* \) | \( w_3^* \) | \( w_4^* \) | \( w_5^* \) | \( w_6^* \) | \( w_7^* \) | \( w_8^* \) | \( w_9^* \) | \( w_{10}^* \) | \( w_{11}^* \) | \( w_{12}^* \) | \( w_{13}^* \) | \( w_{14}^* \) | \( w_{15}^* \) |
|-----------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|
| **Unconstrained** | | | | | | | | | | | | | | | | | |
| \( \bar{m} = 1 \) | 0.16 | 0.16 | 0.03 | 0 | 0 | 0 | 0.05 | 0.19 | 0 | 0 | 0.23 | 0 | 0 | 0.18 |
| \( \bar{m} = 2 \) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.19 | 0 | 0 | 0 | 0 | 0 | 0.35 | 0 | 0.14 |
| \( \bar{m} = 3 \) | 0 | 0 | 0.14 | 0 | 0 | 0 | 0 | 0.05 | 0.19 | 0 | 0 | 0 | 0 | 0.46 | 0 | 0.18 |
| \( \bar{m} = 4 \) | 0 | 0 | 0.12 | 0 | 0 | 0 | 0 | 0 | 0.16 | 0.36 | 0 | 0 | 0.36 | 0 | 0 | 0.18 |
| \( \bar{m} = 5 \) | 0.17 | 0.20 | 0 | 0 | 0 | 0 | 0 | 0.22 | 0 | 0 | 0.27 | 0 | 0 | 0.13 |
| \( \bar{m} = 6 \) | 0.15 | 0.17 | 0 | 0 | 0 | 0 | 0 | 0.08 | 0.19 | 0 | 0 | 0.26 | 0 | 0 | 0.15 |
| \( \bar{m} = 7 \) | 0.16 | 0.16 | 0.03 | 0 | 0 | 0 | 0 | 0.05 | 0.19 | 0 | 0 | 0.23 | 0 | 0 | 0.18 |
| **Weakly-targeted** | \( \beta = 0.01 \) | 0.13 | 0.14 | 0.03 | 0 | 0 | 0.02 | 0.06 | 0 | 0.12 | 0 | 0.06 | 0.12 | 0.09 | 0.08 | 0.14 |
| \( \beta = 0.1 \) | 0.15 | 0.17 | 0 | 0 | 0 | 0 | 0.05 | 0.07 | 0 | 0.14 | 0 | 0 | 0.15 | 0.11 | 0 | 0.16 |
| \( \beta = 1 \) | 0.14 | 0.18 | 0 | 0 | 0 | 0 | 0 | 0.09 | 0.17 | 0 | 0 | 0.29 | 0 | 0 | 0.14 |
| \( \beta = 10 \) | 0 | 0 | 0.09 | 0.12 | 0.05 | 0.11 | 0.13 | 0 | 0 | 0.11 | 0.13 | 0 | 0 | 0.08 | 0.18 | 0 |
| \( \beta = 100 \) | 0 | 0.15 | 0 | 0 | 0.16 | 0.09 | 0 | 0.10 | 0.05 | 0 | 0 | 0.30 | 0.10 | 0 | 0.05 |
| **Unit-level** | \( \xi = 0.01 \) | 0.08 | 0.13 | 0 | 0 | 0.06 | 0 | 0 | 0.04 | 0.14 | 0 | 0.10 | 0.16 | 0.09 | 0.07 | 0.12 |
| \( \xi = 0.1 \) | 0.1 | 0.13 | 0 | 0 | 0 | 0 | 0 | 0.12 | 0.13 | 0 | 0.10 | 0.27 | 0.06 | 0.09 |
| \( \xi = 1 \) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.27 | 0 | 0 | 0.21 | 0.33 | 0.19 | 0 |
| \( \xi = 10 \) | 0 | 0 | 0 | 0 | 0 | 0 | 0.51 | 0 | 0 | 0 | 0 | 0.49 | 0 | 0 | 0 |
| \( \xi = 100 \) | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| **Penalized** | \( \lambda = 0.01 \) | 0.15 | 0.17 | 0 | 0 | 0 | 0 | 0 | 0.07 | 0.19 | 0 | 0 | 0.26 | 0 | 0 | 0.15 |
| \( \lambda = 0.1 \) | 0.16 | 0.19 | 0 | 0 | 0 | 0 | 0 | 0.06 | 0.18 | 0 | 0 | 0.30 | 0 | 0 | 0.11 |
| \( \lambda = 1 \) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0.17 | 0.36 | 0 | 0 | 0.47 | 0 | 0 | 0 |
| \( \lambda = 10 \) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| \( \lambda = 100 \) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |

*Note:* Unless otherwise noted, all designs use \( \bar{m} = 1 \) and \( \bar{m} = 14 \).
| Table 3: Synthetic Control Weights |
|-----------------------------------|
|                                  | $v_1^*$ | $v_2^*$ | $v_3^*$ | $v_4^*$ | $v_5^*$ | $v_6^*$ | $v_7^*$ | $v_8^*$ | $v_9^*$ | $v_{10}^*$ | $v_{11}^*$ | $v_{12}^*$ | $v_{13}^*$ | $v_{14}^*$ | $v_{15}^*$ |
| Unconstrained                    | 0       | 0       | 0       | 0.10    | 0.09    | 0.17    | 0.11    | 0       | 0       | 0.17      | 0          | 0.19      | 0.18      | 0          | 0          |
| Constrained                      | $\bar{m} = 1$ | 0.12    | 0.14    | 0       | 0.02    | 0.05    | 0.01    | 0.03    | 0.04    | 0.15      | 0          | 0.15      | 0.10      | 0          | 0.14       |
|                                  | $\bar{m} = 2$ | 0.08    | 0.07    | 0.04    | 0.06    | 0       | 0.12    | 0.13    | 0       | 0       | 0.10      | 0          | 0.16      | 0.16      | 0.08       |
|                                  | $\bar{m} = 3$ | 0.14    | 0.15    | 0       | 0.01    | 0.01    | 0.04    | 0.07    | 0.02    | 0.15      | 0          | 0.15      | 0.11      | 0          | 0.15       |
|                                  | $\bar{m} = 4$ | 0.14    | 0.15    | 0       | 0.01    | 0.01    | 0.04    | 0.07    | 0.02    | 0.15      | 0          | 0.15      | 0.11      | 0          | 0.15       |
|                                  | $\bar{m} = 5$ | 0       | 0       | 0.02    | 0.11    | 0.07    | 0.11    | 0.10    | 0.06    | 0       | 0.16      | 0          | 0.17      | 0.19      | 0          |
|                                  | $\bar{m} = 6$ | 0       | 0       | 0.02    | 0.10    | 0.07    | 0.15    | 0.11    | 0       | 0       | 0.17      | 0          | 0.18      | 0.2       | 0          |
|                                 | $\bar{m} = 7$ | 0       | 0       | 0       | 0.10    | 0.09    | 0.17    | 0.11    | 0       | 0       | 0.17      | 0          | 0.19      | 0.18      | 0          |
| Weakly-targeted                  | $\beta = 0.01$ | 0       | 0       | 0       | 0.26    | 0.16    | 0       | 0       | 0.47    | 0.11      | 0          | 0          | 0          | 0          | 0          |
|                                  | $\beta = 0.1$ | 0       | 0       | 0.11    | 0.10    | 0.08    | 0       | 0       | 0.11    | 0.14      | 0.25      | 0          | 0          | 0          | 0.22       |
|                                  | $\beta = 1$ | 0       | 0       | 0.03    | 0.12    | 0.08    | 0.18    | 0.11    | 0       | 0       | 0.04      | 0.14      | 0          | 0.12      | 0.18       |
|                                  | $\beta = 10$ | 0.09   | 0.2     | 0       | 0       | 0       | 0       | 0.12    | 0.12    | 0       | 0.37      | 0          | 0          | 0          | 0.09       |
|                                  | $\beta = 100$ | 0.09   | 0       | 0.23    | 0.07    | 0       | 0       | 0.17    | 0       | 0       | 0.25      | 0.06      | 0          | 0          | 0.15       |
| Unit-level                       | $\xi = 0.01$ | 0       | 0       | 0.10    | 0.23    | 0       | 0.28    | 0.22    | 0       | 0       | 0.16      | 0          | 0          | 0          | 0          |
|                                  | $\xi = 0.1$ | 0       | 0       | 0.02    | 0.21    | 0.14    | 0.26    | 0.13    | 0       | 0       | 0.08      | 0          | 0          | 0.16      | 0          |
|                                  | $\xi = 1$ | 0.05   | 0.01    | 0.02    | 0.08    | 0.16    | 0.16    | 0.05    | 0       | 0.12    | 0.04      | 0          | 0          | 0.11      | 0.2        |
|                                  | $\xi = 10$ | 0       | 0       | 0.14    | 0.08    | 0.17    | 0       | 0       | 0.20    | 0.10      | 0.12      | 0          | 0          | 0.08      | 0.12       |
|                                  | $\xi = 100$ | 0       | 0       | 0.24    | 0       | 0       | 0       | 0       | 0.39    | 0.19      | 0.18      | 0          | 0          | 0          | 0          |
| Penalized                       | $\lambda = 0.01$ | 0       | 0       | 0       | 0.08    | 0.08    | 0.16    | 0.11    | 0       | 0       | 0.18      | 0          | 0.19      | 0.20      | 0          |
|                                  | $\lambda = 0.1$ | 0       | 0       | 0       | 0       | 0.02    | 0.08    | 0.09    | 0       | 0       | 0.06      | 0.27      | 0          | 0.16      | 0.31       |
|                                  | $\lambda = 1$ | 0.22   | 0.28    | 0       | 0       | 0       | 0       | 0       | 0.04    | 0       | 0.39      | 0.08      | 0          | 0          | 0          |
|                                  | $\lambda = 10$ | 0       | 0       | 0       | 0       | 0       | 0       | 0       | 0       | 0       | 0.07      | 0.36      | 0.57      | 0          | 0          |
|                                  | $\lambda = 100$ | 0      | 0       | 0       | 0       | 0       | 0       | 0       | 0       | 0       | 0          | 1          | 0          | 0          | 0          |

**Note:** Unless otherwise noted, all designs use $\bar{m} = 1$ and $\bar{m} = 14.$
Table 4: Average Treatment Effects (Averages over 1000 Simulations)

|                  | $\tau_t$ | $\hat{\tau}_t$ | MAE | MSE | $\hat{p}$ | $\hat{p} < 0.05$ |
|------------------|----------|-----------------|-----|-----|-----------|------------------|
|                  | $t = 26$ | $t = 27$        | $t = 28$ | $t = 29$ | $t = 30$  |                  |
|                  | $-13.05$ | $-10.92$        | $-7.65$  | $-5.17$ | $-2.22$   |                  |
| **Unconstrained**|          |                 |         |       |           |                  |
| $\bar{m} = 1$    | $-13.03$ | $-10.93$        | $-7.64$  | $-5.14$ | $-2.17$   | $0.96$           |
| $\bar{m} = 2$    | $-13.04$ | $-10.94$        | $-7.65$  | $-5.19$ | $-2.21$   | $1.69$           |
| $\bar{m} = 3$    | $-13.04$ | $-10.87$        | $-7.64$  | $-5.14$ | $-2.09$   | $1.28$           |
| $\bar{m} = 4$    | $-13.05$ | $-10.89$        | $-7.66$  | $-5.16$ | $-2.13$   | $1.10$           |
| $\bar{m} = 5$    | $-12.98$ | $-10.92$        | $-7.64$  | $-5.13$ | $-2.17$   | $1.02$           |
| $\bar{m} = 6$    | $-13.05$ | $-10.91$        | $-7.61$  | $-5.09$ | $-2.16$   | $0.97$           |
| $\bar{m} = 7$    | $-13.04$ | $-10.92$        | $-7.64$  | $-5.13$ | $-2.18$   | $0.94$           |
| **Constrained**  |          |                 |         |       |           |                  |
| $\beta = 0.01$   | $-13.14$ | $-10.97$        | $-7.86$  | $-5.24$ | $-2.39$   | $1.38$           |
| $\beta = 0.1$    | $-13.06$ | $-10.87$        | $-7.68$  | $-5.16$ | $-2.24$   | $1.04$           |
| $\beta = 1$      | $-13.13$ | $-10.90$        | $-7.67$  | $-5.20$ | $-2.23$   | $1.02$           |
| $\beta = 10$     | $-13.15$ | $-10.89$        | $-7.72$  | $-5.24$ | $-2.26$   | $1.11$           |
| $\beta = 100$    | $-13.15$ | $-10.92$        | $-7.68$  | $-5.23$ | $-2.24$   | $1.21$           |
| **Weakly-targeted** |       |                 |         |       |           |                  |
| $\rho = 0.01$    | $-13.02$ | $-10.85$        | $-7.72$  | $-5.11$ | $-2.35$   | $1.25$           |
| $\rho = 0.1$     | $-13.08$ | $-10.86$        | $-7.76$  | $-5.13$ | $-2.28$   | $1.07$           |
| $\rho = 1$       | $-13.00$ | $-10.82$        | $-7.67$  | $-5.09$ | $-2.25$   | $1.38$           |
| $\rho = 10$      | $-13.05$ | $-10.94$        | $-7.73$  | $-5.18$ | $-2.21$   | $2.23$           |
| $\rho = 100$     | $-13.05$ | $-10.89$        | $-7.67$  | $-5.10$ | $-2.15$   | $2.86$           |
| **Unit-level**   |          |                 |         |       |           |                  |
| $\xi = 0.01$     | $-13.01$ | $-10.91$        | $-7.66$  | $-5.13$ | $-2.23$   | $1.00$           |
| $\xi = 0.1$      | $-13.03$ | $-10.86$        | $-7.64$  | $-5.12$ | $-2.23$   | $1.26$           |
| $\xi = 1$        | $-12.95$ | $-10.85$        | $-7.64$  | $-5.06$ | $-2.12$   | $2.14$           |
| $\xi = 10$       | $-12.94$ | $-10.74$        | $-7.59$  | $-5.16$ | $-2.02$   | $3.69$           |
| $\xi = 100$      | $-12.98$ | $-10.68$        | $-7.60$  | $-5.26$ | $-1.99$   | $4.16$           |
| **Penalized**    |          |                 |         |       |           |                  |
| $\lambda = 0.01$ | $-13.01$ | $-10.91$        | $-7.66$  | $-5.13$ | $-2.23$   | $1.00$           |
| $\lambda = 0.1$  | $-13.03$ | $-10.86$        | $-7.64$  | $-5.12$ | $-2.23$   | $1.26$           |
| $\lambda = 1$    | $-12.95$ | $-10.85$        | $-7.64$  | $-5.06$ | $-2.12$   | $2.14$           |
| $\lambda = 10$   | $-12.94$ | $-10.74$        | $-7.59$  | $-5.16$ | $-2.02$   | $3.69$           |
| $\lambda = 100$  | $-12.98$ | $-10.68$        | $-7.60$  | $-5.26$ | $-1.99$   | $4.16$           |

Note: Unless otherwise noted, all designs use $\bar{m} = 1$ and $\bar{m} = 14$.

The effect is different from zero in the simulation of Table 4, smaller $p$-values and larger rejection rates reflect better performance of the testing procedure for a particular design.

In Table 4, the **Unconstrained** design has a strong relative performance. The performance of the **Constrained** design improves for larger $\bar{m}$, and is virtually identical to the performance of the **Unconstrained** design when $\bar{m} = 7$. The performance of **Weakly-targeted** and **Unit-level** designs is best when $\beta$ and $\xi$ take intermediate values. The **Penalized** design produces results similar to the **Unconstrained** design for small values of the penalization parameter $\lambda$. 
Table 5: Average Treatment Effects on the Treated (Averages over 1000 Simulations)

|                | $\tau_{t}^{T}$ t = 26 | $\tau_{t}^{T}$ t = 27 | $\tau_{t}^{T}$ t = 28 | $\tau_{t}^{T}$ t = 29 | $\tau_{t}^{T}$ t = 30 | MAE$^{T}$ | MSE$^{T}$ |
|----------------|------------------------|------------------------|------------------------|------------------------|------------------------|-----------|-----------|
| **Unconstrained** |                        |                        |                        |                        |                        |           |           |
| $\bar{m}$ = 1   | -12.81                 | -10.74                 | -7.63                  | -5.12                  | -2.38                  | -12.89    | -10.86    | 3.04      | 14.62    |
| $\bar{m}$ = 2   | -13.10                 | -10.86                 | -7.69                  | -5.31                  | -2.33                  | -13.04    | -10.94    | 1.85      | 5.39     |
| $\bar{m}$ = 3   | -13.02                 | -10.91                 | -7.71                  | -5.22                  | -2.14                  | -13.04    | -10.87    | 1.47      | 3.39     |
| $\bar{m}$ = 4   | -13.03                 | -10.87                 | -7.66                  | -5.17                  | -2.12                  | -13.05    | -10.89    | 1.24      | 2.47     |
| $\bar{m}$ = 5   | -13.08                 | -10.87                 | -7.62                  | -5.17                  | -2.19                  | -12.98    | -10.92    | 1.19      | 2.25     |
| $\bar{m}$ = 6   | -13.11                 | -10.91                 | -7.59                  | -5.18                  | -2.21                  | -13.05    | -10.91    | 1.14      | 2.05     |
| $\bar{m}$ = 7   | -13.04                 | -10.89                 | -7.66                  | -5.18                  | -2.25                  | -13.04    | -10.92    | 1.12      | 1.98     |
| **Weakly-targeted** |                |                        |                        |                        |                        |           |           |
| $\xi = 0.01$    | -13.02                 | -10.91                 | -7.64                  | -5.16                  | -2.22                  | -13.14    | -10.97    | 1.48      | 3.43     |
| $\xi = 0.1$     | -13.03                 | -10.90                 | -7.66                  | -5.15                  | -2.22                  | -13.06    | -10.87    | 1.17      | 2.17     |
| $\xi = 1$       | -13.05                 | -10.91                 | -7.62                  | -5.13                  | -2.25                  | -13.13    | -10.90    | 1.08      | 1.84     |
| $\xi = 10$      | -13.05                 | -10.88                 | -7.65                  | -5.12                  | -2.22                  | -13.15    | -10.89    | 1.03      | 1.64     |
| $\xi = 100$     | -13.04                 | -10.94                 | -7.65                  | -5.17                  | -2.21                  | -13.15    | -10.92    | 1.02      | 1.67     |
| **Unit-level**  |                        |                        |                        |                        |                        |           |           |
| $\xi = 0.01$    | -13.04                 | -10.90                 | -7.67                  | -5.16                  | -2.24                  | -13.02    | -10.85    | 1.33      | 2.84     |
| $\xi = 0.1$     | -13.04                 | -10.87                 | -7.68                  | -5.18                  | -2.26                  | -13.08    | -10.86    | 1.18      | 2.25     |
| $\xi = 1$       | -13.11                 | -10.86                 | -7.72                  | -5.17                  | -2.28                  | -13.00    | -10.82    | 1.43      | 3.21     |
| $\xi = 10$      | -13.06                 | -10.77                 | -7.62                  | -5.23                  | -2.19                  | -13.05    | -10.94    | 1.96      | 6.16     |
| $\xi = 100$     | -13.05                 | -10.74                 | -7.59                  | -5.17                  | -2.21                  | -13.05    | -10.89    | 2.35      | 8.79     |
| **Penalized**   |                        |                        |                        |                        |                        |           |           |
| $\lambda = 0.01$| -13.05                 | -10.90                 | -7.63                  | -5.18                  | -2.24                  | -13.01    | -10.91    | 1.16      | 2.12     |
| $\lambda = 0.1$| -13.03                 | -10.81                 | -7.62                  | -5.11                  | -2.19                  | -13.03    | -10.86    | 1.38      | 3.02     |
| $\lambda = 1$  | -13.02                 | -10.75                 | -7.64                  | -5.14                  | -2.25                  | -12.95    | -10.85    | 2.23      | 7.99     |
| $\lambda = 10$ | -12.87                 | -10.86                 | -7.63                  | -5.22                  | -2.28                  | -12.94    | -10.74    | 3.80      | 23.09    |
| $\lambda = 100$| -12.85                 | -10.85                 | -7.62                  | -5.20                  | -2.30                  | -12.98    | -10.68    | 4.22      | 28.53    |
4.3.2. Average Treatment Effects on the Treated

The first five columns in Table 5 report averages of $\tau^T_t$, the average effects of treatment on the treated units. These quantities depend on the weights for the treated units, which are different across different formulations of the synthetic control design. The next five columns report averages of $\hat{\tau}_t$. They are the same as in Table 4, yet we use them as estimators for $\tau^T_t$ in Table 5. The last two columns of Table 5 report the mean absolute error and the mean squared error, defined as in (16) but with $\tau^T_t$ replacing $\tau_t$.

The results in Table 5 are qualitatively similar to those for $\tau_t$ in Table 4, with one notable exception. As expected, for intermediate and large values of $\beta$, the Weakly-targeted design out-performs the other designs when the goal is to estimate $\tau^T_t$. This is because in the Weakly-targeted design the synthetic control weights are targeted to $\tau^T_t$ (and more so as $\beta$ becomes large).

4.3.3. Test size

In this section, we generate the model primitives under the null hypothesis (13). That is, we employ a data generating process such that the values of the common factors and the distributions of the idiosyncratic error variables are unaffected by the intervention.

We report the simulation results in Table 6, which organizes information in the same way as in Table 4. Because the data are generated from the same distribution under treatment and under no treatment, the average treatment effects in Table 6 are close to zero. The same is true for the averages of $\hat{\tau}_t$ for all designs. Under the null hypothesis (13), the $p$-value should approximately follow a uniform distribution between zero and one. The results in Table 6 show good behavior of our testing procedure under the null hypothesis: average $p$-values and rejection rates are close to 0.5 and 0.05, respectively.

4.3.4. Performance with Nonlinearities

In this section, we study the behavior of the estimators based on synthetic control designs under deviations from the linear model in (10a) and (10b). We consider a nonlinear data generating process,

$$Y_{jt}^N = \delta_t + \exp(\theta'_t Z_j) + \exp(X_t \mu_j) + \epsilon_{jt},$$

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Table 6: Average Treatment Effects Under the Null Hypothesis (13) (Averages over 1000 Simulations)

| $t$  | $\tau_t$ | $\hat{\tau}_t$ | MAE | MSE | $\hat{p}$ | $\hat{p} < 0.05$ |
|------|----------|-----------------|-----|-----|----------|------------------|
| 26   | -0.01    | 0.02            | 1.09| 1.90| 0.498    | 0.049            |
| 27   | 0.00     | -0.05           |     |     |          |                  |
| 28   | 0.00     | 0.03            |     |     |          |                  |
| 29   | 0.02     | 0.05            |     |     |          |                  |
| 30   | -0.01    |                 |     |     |          |                  |

Note: Unless otherwise noted, all designs use $\bar{m} = 1$ and $\bar{m} = 14$.

\[ Y_{jt} = v_t + \exp(\gamma_t'Z_j) + \exp(\eta_t'\mu_j) + \xi_{jt}. \]

The reason to study a nonlinear model is that nonlinearities may induce interpolation biases, affecting the relative performance of the different designs. All parameter values are the same as in the simulation setup of section 4.1, except for the values of $\theta_t$, $\gamma_t$, $\lambda_t$, and $\eta_t$, which are chosen to be random vectors of i.i.d. Uniform (0, 3) random variables, instead of Uniform (0, 10), to control the magnitude of the exponential components in the nonlinear design.

Tables 7 and 8 report the results for $\tau_t$ and $\tau_t^T$, respectively. In comparison to the results in Tables 4 and 5, we now see that the Unit-level and Penalized designs can easily match and in
Table 7: Average Treatment Effects, Nonlinear Model (Averages over 1000 Simulations)

| \( \tau \) | \( t = 26 \) | \( t = 27 \) | \( t = 28 \) | \( t = 29 \) | \( t = 30 \) |
|---|---|---|---|---|---|
| \(-13.18\) | \(-10.72\) | \(-7.96\) | \(-5.47\) | \(-2.43\) |

| \( \hat{\tau}_t \) | \( MAE \) | \( MSE \) | \( \hat{\mu} \) | \( \hat{\mu} < 0.05 \) |
|---|---|---|---|---|
| \( Unconstrained \) | \(-13.44\) | \(-10.91\) | \(-8.18\) | \(-5.85\) | \(-2.74\) | \(1.99\) | \(15.42\) | \(0.059\) | \(0.743\) |
| \( Constrained \) | \( \bar{m} = 1 \) | \(-15.70\) | \(-13.18\) | \(-10.50\) | \(-7.76\) | \(-4.78\) | \(3.51\) | \(32.78\) | \(0.061\) | \(0.717\) |
| | \( \bar{m} = 2 \) | \(-14.27\) | \(-11.86\) | \(-8.90\) | \(-6.44\) | \(-3.34\) | \(2.64\) | \(20.52\) | \(0.061\) | \(0.725\) |
| | \( \bar{m} = 3 \) | \(-13.69\) | \(-11.38\) | \(-8.38\) | \(-5.95\) | \(-2.97\) | \(2.23\) | \(17.84\) | \(0.058\) | \(0.745\) |
| | \( \bar{m} = 4 \) | \(-13.58\) | \(-11.09\) | \(-8.23\) | \(-5.89\) | \(-2.75\) | \(2.10\) | \(17.84\) | \(0.058\) | \(0.754\) |
| | \( \bar{m} = 5 \) | \(-13.37\) | \(-10.97\) | \(-8.14\) | \(-5.79\) | \(-2.88\) | \(2.05\) | \(15.82\) | \(0.060\) | \(0.747\) |
| | \( \bar{m} = 6 \) | \(-13.54\) | \(-11.03\) | \(-8.31\) | \(-5.86\) | \(-2.86\) | \(2.00\) | \(15.51\) | \(0.060\) | \(0.738\) |
| | \( \bar{m} = 7 \) | \(-13.49\) | \(-10.94\) | \(-8.17\) | \(-5.86\) | \(-2.78\) | \(1.98\) | \(15.32\) | \(0.058\) | \(0.743\) |
| \( Weakly-targeted \) | \( \beta = 0.01 \) | \(-11.67\) | \(-9.02\) | \(-6.37\) | \(-3.87\) | \(-1.00\) | \(2.59\) | \(20.13\) | \(0.116\) | \(0.604\) |
| | \( \beta = 0.1 \) | \(-12.08\) | \(-9.60\) | \(-6.87\) | \(-4.30\) | \(-1.46\) | \(2.15\) | \(15.75\) | \(0.082\) | \(0.680\) |
| | \( \beta = 1 \) | \(-12.52\) | \(-10.12\) | \(-7.34\) | \(-4.81\) | \(-1.93\) | \(1.96\) | \(13.01\) | \(0.057\) | \(0.758\) |
| | \( \beta = 10 \) | \(-13.00\) | \(-10.56\) | \(-7.83\) | \(-5.27\) | \(-2.34\) | \(2.19\) | \(16.70\) | \(0.031\) | \(0.859\) |
| | \( \beta = 100 \) | \(-13.25\) | \(-10.72\) | \(-8.01\) | \(-5.39\) | \(-2.58\) | \(2.45\) | \(20.81\) | \(0.024\) | \(0.876\) |
| \( Unit-level \) | \( \xi = 0.01 \) | \(-11.76\) | \(-9.15\) | \(-6.51\) | \(-3.91\) | \(-1.15\) | \(2.57\) | \(20.54\) | \(0.118\) | \(0.593\) |
| | \( \xi = 0.1 \) | \(-13.11\) | \(-10.59\) | \(-7.82\) | \(-5.15\) | \(-2.29\) | \(2.06\) | \(15.81\) | \(0.060\) | \(0.754\) |
| | \( \xi = 1 \) | \(-13.74\) | \(-11.12\) | \(-8.42\) | \(-5.75\) | \(-2.84\) | \(2.37\) | \(19.70\) | \(0.029\) | \(0.850\) |
| | \( \xi = 10 \) | \(-13.74\) | \(-11.20\) | \(-8.55\) | \(-5.89\) | \(-3.09\) | \(3.02\) | \(26.38\) | \(0.028\) | \(0.866\) |
| | \( \xi = 100 \) | \(-13.79\) | \(-11.16\) | \(-8.54\) | \(-5.90\) | \(-3.08\) | \(3.20\) | \(28.37\) | \(0.029\) | \(0.863\) |
| \( Penalized \) | \( \lambda = 0.01 \) | \(-13.40\) | \(-10.93\) | \(-8.32\) | \(-5.82\) | \(-2.83\) | \(1.97\) | \(16.13\) | \(0.056\) | \(0.757\) |
| | \( \lambda = 0.1 \) | \(-13.33\) | \(-10.80\) | \(-8.12\) | \(-5.56\) | \(-2.65\) | \(2.07\) | \(13.81\) | \(0.045\) | \(0.779\) |
| | \( \lambda = 1 \) | \(-13.32\) | \(-10.84\) | \(-8.15\) | \(-5.39\) | \(-2.60\) | \(3.08\) | \(27.44\) | \(0.056\) | \(0.758\) |
| | \( \lambda = 10 \) | \(-13.39\) | \(-10.82\) | \(-7.95\) | \(-5.34\) | \(-2.58\) | \(3.85\) | \(37.08\) | \(0.103\) | \(0.595\) |
| | \( \lambda = 100 \) | \(-13.35\) | \(-10.82\) | \(-8.00\) | \(-5.29\) | \(-2.57\) | \(4.10\) | \(41.23\) | \(0.117\) | \(0.562\) |

Note: Unless otherwise noted, all designs use \( m = 1 \) and \( m = 14 \).

Some cases improve the performance of the Unconstrained design, especially for the estimation of \( \tau^T_i \). By fitting each treated unit with a unit-specific synthetic control, the Unit-level design can ameliorate interpolation biases. The Penalized design selects synthetic treated and control units close to \( X \) in the space of the predictors, which can reduce interpolation biases at the potential cost of lower precision for large values of \( \lambda \) (in which case, the Penalized design employs a small number of units in the synthetic treated and synthetic control). As in Table 8, when the parameter of interest is \( \tau^T_i \), the weakly targeted design easily outperforms the unconstrained estimator for large values of \( \beta \).
Table 8: Average Treatment Effects on the Treated, Nonlinear Model (Averages over 1000 Simulations)

| Model Type          | $\tau_t^{T}$ | $\tilde{\tau}_t$ | $\text{MAE}^T$ | $\text{MSE}^T$ |
|---------------------|---------------|-------------------|----------------|---------------|
|                     | $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ | $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ | $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ |
| Unconstrained       | -13.29  | -10.65  | -7.96   | -5.47   | -2.40   | -13.44  | -10.91  | -8.18   | -5.85   | -2.74   | 2.31    | 20.07  |
| Constrained         | $\bar{m} = 1$ | -13.41  | -10.81  | -8.01   | -5.25   | -2.61   | -15.70  | -13.18  | -10.50  | -7.76   | -4.78   | 3.53    | 29.28  |
|                     | $\bar{m} = 2$ | -13.19  | -10.88  | -7.93   | -5.42   | -2.47   | -14.27  | -11.86  | -8.90   | -6.44   | -3.34   | 2.86    | 26.06  |
|                     | $\bar{m} = 3$ | -13.17  | -10.82  | -8.07   | -5.54   | -2.44   | -13.69  | -11.38  | -8.38   | -5.95   | -2.97   | 2.52    | 20.96  |
|                     | $\bar{m} = 4$ | -13.29  | -10.82  | -8.03   | -5.59   | -2.31   | -13.58  | -11.09  | -8.23   | -5.89   | -2.75   | 2.44    | 20.98  |
|                     | $\bar{m} = 5$ | -13.23  | -10.77  | -7.88   | -5.58   | -2.30   | -13.37  | -10.97  | -8.14   | -5.79   | -2.88   | 2.39    | 20.72  |
|                     | $\bar{m} = 6$ | -13.36  | -10.71  | -7.99   | -5.50   | -2.36   | -13.54  | -11.03  | -8.31   | -5.86   | -2.86   | 2.32    | 19.91  |
|                     | $\bar{m} = 7$ | -13.31  | -10.68  | -7.97   | -5.46   | -2.39   | -13.49  | -10.94  | -8.17   | -5.86   | -2.78   | 2.30    | 19.91  |
| Weakly-targeted     | $\xi = 0.01$ | -13.14  | -10.67  | -7.97   | -5.47   | -2.45   | -11.67  | -9.02   | -6.37   | -3.87   | -1.00   | 2.68    | 21.06  |
|                     | $\xi = 0.1$  | -13.19  | -10.66  | -7.92   | -5.47   | -2.45   | -12.08  | -9.60   | -6.87   | -4.30   | -1.46   | 2.31    | 17.03  |
|                     | $\xi = 1$    | -13.22  | -10.76  | -7.93   | -5.46   | -2.45   | -12.52  | -10.12  | -7.34   | -4.81   | -1.93   | 1.91    | 10.68  |
|                     | $\xi = 10$   | -13.28  | -10.70  | -7.95   | -5.49   | -2.49   | -13.00  | -10.56  | -7.83   | -5.27   | -2.34   | 1.33    | 3.86   |
|                     | $\xi = 100$  | -13.24  | -10.74  | -8.03   | -5.43   | -2.54   | -13.25  | -10.72  | -8.01   | -5.39   | -2.58   | 1.14    | 2.56   |
| Unit-level          | $\xi = 0.01$ | -13.20  | -10.76  | -7.98   | -5.44   | -2.48   | -11.76  | -9.15   | -6.51   | -3.91   | -1.15   | 2.68    | 21.11  |
|                     | $\xi = 0.1$  | -13.35  | -10.81  | -8.07   | -5.53   | -2.55   | -13.11  | -10.59  | -7.82   | -5.15   | -2.29   | 1.99    | 10.94  |
|                     | $\xi = 1$    | -13.40  | -10.77  | -8.05   | -5.47   | -2.56   | -13.74  | -11.12  | -8.42   | -5.75   | -2.84   | 1.47    | 4.45   |
|                     | $\xi = 10$   | -13.35  | -10.66  | -7.99   | -5.38   | -2.71   | -13.74  | -11.20  | -8.55   | -5.89   | -3.09   | 1.49    | 4.30   |
|                     | $\xi = 100$  | -13.39  | -10.63  | -7.95   | -5.37   | -2.69   | -13.79  | -11.16  | -8.54   | -5.90   | -3.08   | 1.56    | 4.55   |
| Penalized           | $\lambda = 0.01$ | -13.32  | -10.69  | -8.07   | -5.49   | -2.42   | -13.40  | -10.93  | -8.32   | -5.82   | -2.83   | 2.25    | 18.48  |
|                     | $\lambda = 0.1$ | -13.17  | -10.72  | -8.09   | -5.59   | -2.55   | -13.33  | -10.80  | -8.12   | -5.56   | -2.65   | 1.87    | 10.06  |
|                     | $\lambda = 1$ | -13.40  | -10.80  | -7.98   | -5.35   | -2.66   | -13.32  | -10.84  | -8.15   | -5.39   | -2.60   | 1.97    | 8.89   |
|                     | $\lambda = 10$ | -13.41  | -10.89  | -8.00   | -5.32   | -2.64   | -13.39  | -10.82  | -7.95   | -5.34   | -2.58   | 2.79    | 17.48  |
|                     | $\lambda = 100$ | -13.42  | -10.87  | -8.02   | -5.26   | -2.63   | -13.35  | -10.82  | -8.00   | -5.29   | -2.57   | 3.06    | 21.54  |
Table 9: Treatment Effects Estimates Using Randomized Assignments and Difference-in-Means estimators (Averages over 1000 Simulations)

| $\tau_t$ | $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ |
|----------|---------|---------|---------|---------|---------|
| $\hat{\tau}_t$ | -13.05 | -10.92 | -7.65 | -5.17 | -2.22 |

| $\tau_t$ | $t = 26$ | $t = 27$ | $t = 28$ | $t = 29$ | $t = 30$ |
|----------|---------|---------|---------|---------|---------|
| $\hat{\tau}_t$ | $\bar{m} = 1$ | -12.90 | -10.65 | -7.17 | -4.90 | -2.08 | 5.73 | 52.34 |
|           | $\bar{m} = 2$ | -12.87 | -10.67 | -7.33 | -5.05 | -1.98 | 4.54 | 32.58 |
|           | $\bar{m} = 3$ | -12.68 | -10.51 | -7.22 | -4.87 | -1.85 | 3.83 | 23.41 |
|           | $\bar{m} = 4$ | -12.99 | -10.94 | -7.78 | -5.13 | -2.16 | 3.38 | 17.72 |
|           | $\bar{m} = 5$ | -12.93 | -10.75 | -7.45 | -5.14 | -2.17 | 3.10 | 15.00 |
|           | $\bar{m} = 6$ | -12.91 | -10.85 | -7.53 | -5.09 | -2.12 | 3.08 | 15.78 |
|           | $\bar{m} = 7$ | -12.91 | -10.66 | -7.53 | -5.02 | -2.07 | 2.87 | 12.90 |

Note: In this table, $\bar{m}$ stands for the maximum number of treated units allowed in the randomized control trial.

5. Comparison to Randomized Control Trials

Using randomization to assign treatment allows ex-ante (pre-randomization) unbiased estimation of the average treatment effect. However, as we show below, ex-post (post-randomization) biases can be large, especially when only a small number of units are treated.

Table 9 reports performance measures of a randomized design for the same simulation setup as in Section 4.3. For seven treated units, randomization of the treatment produces MAE and MSE equal to 2.87 and 12.90, substantially larger than the 0.94 and 1.41 values for the Constrained design in Table 4. For one treated unit only, the difference is even more stark; randomization of the treatment produces MAE and MSE equal to 5.73 and 52.34, substantially larger than the 2.93 and 13.29 for the Constrained design in Table 4. These results underscore the appropriateness of synthetic control designs in experimental studies with a limited number of aggregate units.

6. Conclusions

Experimental design methods have largely been concerned with settings where a large number of experimental units are randomly assigned to a treatment arm, and a similarly large number of...
experimental units are assigned to a control arm. This focus on large samples and randomization has proven to be enormously useful in large classes of problems, but becomes inadequate when treating more than a few units is unfeasible, which is often the case in experimental studies with large aggregate units (e.g., markets). In that case, randomized designs may produce estimators that are substantially biased (post-randomization) relative to the average treatment effect or to the average treatment effect on the treated. Large biases can be expected when the unit or units assigned to treatment fail to approximate average outcomes under treatment for the entire population, or when the units in the control arm fail to approximate the outcomes that treated units would experience without treatment.

In this article we have applied synthetic control techniques, widely used in observational studies, to the design of experiments when treatment can only be applied to a small number of experimental units. The synthetic control design optimizes jointly over the identities of the units assigned to the treatment and the control arms, and over the weights that determine the relative contribution of those units to reproduce the counterfactuals of interest. We propose various designs aimed to estimate average treatment effects, analyze the properties of such designs and the resulting estimators, and devise inferential methods to test a null hypothesis of no treatment effects. In addition, we report simulation results that demonstrate the applicability and computational feasibility of the methods proposed in this article. We show that synthetic control design can substantially overperform randomized designs in experimental settings with a small number of treated units.

Corporate researchers and academic investigators are often confronted with settings where interventions at the level of micro-units (i.e., customers, workers, or families) are unfeasible, impractical or ineffective (see, e.g., Duflo, Glennerster and Kremer, 2007, Jones and Barrows, 2019). There is, in consequence, a wide range of potential applications of experimental design methods for large aggregate entities, like the ones proposed in this article.

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Appendix

A. Approximate Validity when $\lambda_t$ are not Exchangeable

Recall that in Theorem 2 we have shown that when $\lambda_t$ are exchangeable for $t \in B \cup \{T_0 + 1, \ldots, T\}$ the $p$-value in (15) is exact. In this section, we discuss the case when $\lambda_t$ are not necessarily exchangeable. We show below in Theorem A.1 that the $p$-value in (15) is approximately valid when $T_\epsilon$ is large.

**Theorem A.1** Suppose that Assumptions 1–3 hold, and that for $j = 1, \ldots, J$, $t = 1, \ldots, T$, $\epsilon_{jt}$ are continuously distributed with (a version of) the probability density function bounded by a constant $\kappa < \infty$. Then, the $p$-values of equation (15) are approximately valid. In particular, there is an event $C$, such that conditional on $C$, for any $\alpha \in (0, 1]$, we have

$$\alpha - 2z_2 - \frac{1}{\|I\|} \leq \Pr(\hat{p} \leq \alpha) \leq \alpha + 2z_2,$$

and the event $C$ happens with probability equal or greater than

$$\Pr(C) = 1 - 2J \exp\left(-\frac{z_1^2 \kappa^2}{8\sigma^2 \lambda^4 F^2 T_\epsilon}\right) - \frac{z_1}{z_2} \cdot 2\epsilon \sqrt{2J \min\{T - T_0, T_0 - T_\epsilon\}} \kappa,$$

where $z_1, z_2$ are arbitrary positive constants.

We defer the proof of Theorem A.1 to Section B.1 in the Online Appendix. A limitation of the result in Theorem A.1 is that there are values of the parameters of the data generating for which the result of the theorem provides a tight bound on test size only for large values of $T_\epsilon$.

B. Proofs

B.1. Proof of Theorem 1

**Proof of Theorem 1.** For any period $t = T_0 + 1, \ldots, T$ we decompose $\hat{\tau}_t$ as follows,

$$\hat{\tau}_t = \tau_t = \left(\sum_{j=1}^J w_j^* Y_{jt}^I - \sum_{j=1}^J v_j^* Y_{jt}^N\right) - \left(\sum_{j=1}^J f_j Y_{jt}^I - \sum_{j=1}^J f_j Y_{jt}^N\right) = \left(\sum_{j=1}^J w_j^* Y_{jt}^I - \sum_{j=1}^J f_j Y_{jt}^I\right) - \left(\sum_{j=1}^J v_j^* Y_{jt}^N - \sum_{j=1}^J f_j Y_{jt}^N\right). \quad \text{(B.1)}$$
The first term in (B.1) measures the difference between the synthetic treatment outcome and the aggregated treatment outcomes. The second term measures the difference between the synthetic control outcome and the aggregate control outcomes. We bound these two terms separately.

From (10b), we obtain

\[
\sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} f_j Y_{jt}^I = \gamma_t \left( \sum_{j=1}^{J} w_j^* Z_j - \sum_{j=1}^{J} f_j Z_j \right) + \eta_t \left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} f_j \mu_j \right) + \left( \sum_{j=1}^{J} w_j^* \xi_{jt} - \sum_{j=1}^{J} f_j \xi_{jt} \right)
\]

(B.2)

Similarly, using expression (10a), we obtain

\[
\sum_{j=1}^{J} w_j^* Y_{jt}^E - \sum_{j=1}^{J} f_j Y_{jt}^E = \theta_E \left( \sum_{j=1}^{J} w_j^* Z_j - \sum_{j=1}^{J} f_j Z_j \right) + \lambda_E \left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} f_j \mu_j \right) + \left( \sum_{j=1}^{J} w_j^* \epsilon_{jt}^E - \sum_{j=1}^{J} f_j \epsilon_{jt}^E \right)
\]

where \( \theta_E \) is the \((T_E \times r)\) matrix with rows equal to the \( \theta_t \)'s indexed by \( E \), and \( \epsilon_{jt}^E \) is defined analogously. Pre-multiplying by \( \eta_t (\lambda_E \lambda_E)^{-1} \lambda_E' \) yields

\[
\eta_t (\lambda_E' \lambda_E)^{-1} \lambda_E' \left( \sum_{j=1}^{J} w_j^* Y_{jt}^E - \sum_{j=1}^{J} f_j Y_{jt}^E \right) = \eta_t (\lambda_E' \lambda_E)^{-1} \lambda_E' \theta_E \left( \sum_{j=1}^{J} w_j^* Z_j - \sum_{j=1}^{J} f_j Z_j \right) + \eta_t \left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} f_j \mu_j \right) + \eta_t (\lambda_E' \lambda_E)^{-1} \lambda_E' \left( \sum_{j=1}^{J} w_j^* \epsilon_{jt}^E - \sum_{j=1}^{J} f_j \epsilon_{jt}^E \right).
\]

(B.3)

Subtract (B.3) from (B.2) and apply Assumption 3 to obtain

\[
\sum_{j=1}^{J} w_j^* Y_{jt}^I - \sum_{j=1}^{J} f_j Y_{jt}^I = -\eta_t (\lambda_E' \lambda_E)^{-1} \lambda_E' \sum_{j=1}^{J} w_j^* \epsilon_{jt}^E + \eta_t (\lambda_E' \lambda_E)^{-1} \lambda_E' \sum_{j=1}^{J} f_j \epsilon_{jt}^E + \left( \sum_{j=1}^{J} w_j^* \xi_{jt} - \sum_{j=1}^{J} f_j \xi_{jt} \right).
\]

(B.4)
Only the first term on the right-hand side of (B.4) has a non-zero mean (because the weights, \(w_j^*\), depend on the error terms \(\epsilon_j^\mathcal{E}\)). Therefore,

\[
\left| E \left[ \sum_{j=1}^{J} w_j^* Y_j^I - \sum_{j=1}^{J} f_j Y_j^I \right] \right| = E \left[ \eta_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_{\mathcal{E}} \sum_{j=1}^{J} w_j^* \epsilon_j^\mathcal{E} \right].
\]

Using the same line of reasoning for the second term on the right-hand side of (B.1), we obtain

\[
\left| E \left[ \sum_{j=1}^{J} v_j^* Y_j^N - \sum_{j=1}^{J} f_j Y_j^N \right] \right| = E \left[ \lambda_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_{\mathcal{E}} \sum_{j=1}^{J} v_j^* \epsilon_j^\mathcal{E} \right].
\]

For any \(t \geq T_0 + 1 \) and \(s \in T_\zeta\), under Assumption 2 (ii), we apply Cauchy-Schwarz inequality and the eigenvalue bound on the Rayleigh quotient to obtain

\[
(\eta_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_s)^2 \leq (\eta_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \eta_t) (\lambda_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_s) \leq \left( \frac{\eta_t^2 F}{T_\zeta \zeta} \right) \left( \frac{\lambda_t^2 F}{T_\zeta \zeta} \right).
\]

Similarly,

\[
(\lambda_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_s)^2 \leq \left( \frac{\lambda_t^2 F}{T_\zeta \zeta} \right)^2.
\]  

Let

\[
\bar{\epsilon}_{jt}^\mathcal{E} = \eta_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_{\mathcal{E}} \epsilon_j^\mathcal{E} = \sum_{s \in \mathcal{E}} \eta_t'(\lambda'_{\mathcal{E}} \lambda_{\mathcal{E}})^{-1} \lambda_{s} \epsilon_{js}.
\]

Because \(\bar{\epsilon}_{jt}^\mathcal{E}\) is a linear combination of independent sub-Gaussians with variance proxy \(\sigma^2\), it follows that \(\bar{\epsilon}_{jt}^\mathcal{E}\) is sub-Gaussian with variance proxy \((\bar{\eta}_t F/\zeta)^2 \sigma^2/T_\zeta\). Let \(q \geq 1\) be any positive integer. We obtain

\[
\left| E \left[ \sum_{j=1}^{J} w_j^* Y_j^I - \sum_{j=1}^{J} f_j Y_j^I \right] \right| \leq E \left[ \sum_{j=1}^{J} w_j^* |\bar{\epsilon}_{jt}^\mathcal{E}| \right] \leq E \left[ \left( \sum_{j=1}^{J} |\bar{\epsilon}_{jt}^\mathcal{E}|^q \right)^{1/q} \right].
\]
\[
\left( E \left[ \sum_{j=1}^J |\epsilon_{jt}|^q \right] \right)^{1/q} \\
= \left( \sum_{j=1}^J E \left[ |\tilde{\epsilon}_{jt}|^q \right] \right)^{1/q} \\
\leq \left( J (2 \bar{\eta} F/\zeta)^2 \frac{\sigma^2}{TE} q^{1/q} \Gamma(q/2) \right)^{1/q} \\
= \frac{\bar{\lambda} \bar{\eta} F}{\zeta} J^{1/q} \sqrt{2} \sigma (q \Gamma(q/2))^{1/q} \frac{1}{\sqrt{TE}}.
\]

The first and third inequalities are implied by Jensen’s Inequality, the second inequality is implied by Holder’s inequality, and the last inequality is bounding the absolute moments of sub-Gaussian random variables (see, e.g., Rigollet and Hütter, 2019, Lemma 1.4). An analogous argument yields,

\[
\left| E \left[ \sum_{j=1}^J \hat{u}_{jt}^* Y_{jt} - \sum_{j=1}^J f_j Y_{jt} \right] \right| \leq \frac{\bar{\lambda}^2 F}{\zeta} J^{1/q} \sqrt{2} \sigma (q \Gamma(q/2))^{1/q} \frac{1}{\sqrt{TE}}.
\]

Equations (1) and (1) directly yield the result of the theorem.

**B.2. Proof of Theorem 2**

**Proof of Theorem 2.** Recall that

\[
\hat{u}_t = \sum_{j=1}^J w_{jt}^* Y_{jt} - \sum_{j=1}^J v_{jt}^* Y_{jt},
\]

for \( t \in B \cup \{T_0 + 1, \ldots, T\} \). For \( t \in \{T_0 + 1, \ldots, T\} \), \( \hat{u}_t \) are the post-intervention estimates of the treatment effects; and for \( t \in B \), \( \hat{u}_t \) are the placebo treatment effects estimated for the blank periods. Let

\[
u_t = \sum_{j=1}^J w_{jt}^* \epsilon_{jt} - \sum_{j=1}^J v_{jt}^* \epsilon_{jt}
\]

for \( t \in B \), and

\[
u_t = \sum_{j=1}^J w_{jt}^* \xi_{jt} - \sum_{j=1}^J v_{jt}^* \epsilon_{jt}
\]

for \( t \in \{T_0 + 1, \ldots, T\} \). Note that
\[
\sum_{j=1}^{J} w_j^* Y_j^E - \sum_{j=1}^{J} v_j^* Y_j^E = \theta_E \left( \sum_{j=1}^{J} w_j^* Z_j - \sum_{j=1}^{J} v_j^* Z_j \right) + \lambda_E \left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} v_j^* \mu_j \right) + \left( \sum_{j=1}^{J} w_j^* \epsilon_j^E - \sum_{j=1}^{J} v_j^* \epsilon_j^E \right).
\]

Assumption 3 implies
\[
\left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} v_j^* \mu_j \right) = -\left( \lambda_E' \lambda_E \right)^{-1} \lambda_E' \left( \sum_{j=1}^{J} w_j^* \epsilon_j^E - \sum_{j=1}^{J} v_j^* \epsilon_j^E \right).
\]

It follows that
\[
\hat{u}_t = \sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_j^* Y_{jt}
\]
\[
= -\lambda_t' (\lambda_E' \lambda_E)^{-1} \lambda_E' \sum_{j=1}^{J} (w_j^* - v_j^*) \epsilon_j^E + u_t,
\]

for \( t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\} \). Since for \( t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\} \), the \( u_t \) are exchangeable, and \( \lambda_t \) are exchangeable for \( t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\} \) and independent of \( \epsilon_{jt} \), then \( \hat{u}_t \) are exchangeable for \( t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\} \), and the result of the theorem follows from Theorem D.1 in Chernozhukov, Wüthrich and Zhu (2021).

C. Swapping Treated and Control Weights

Recall that when it is possible to swap synthetic treated and synthetic control weights, we choose the treated units so that the number of units with positive weights in \( w^* \) is smaller than the number of units with positive weights in \( v^* \). When \( \|w^*\|_0 = \|v^*\|_0 \), we determine whether or not to swap using the following rule. For the Unconstrained design, we choose the treated group to be the one with the smallest index among the units with positive weights. In particular, in the Unconstrained design in Tables 2 and 3, the unit in the synthetic treated with lowest index is the 1st unit, and the unit in the synthetic control with the lowest index is the 4th unit. We use the same procedure based on lowest index for Constrained with \( \overline{m} = 7 \) (highest value) and Penalized with \( \lambda = 0.01 \) (lowest value). Then, starting from \( \overline{m} = 7 \) and for smaller values of \( \overline{m} \), we assign to the treated group the set of weights that is most similar to the weights obtained for
\|\mathbf{w}^*\|_0 \leq \bar{m} + 1 \text{ (in terms of what units obtain positive weights). In those cases where the two sets of swappable weights for } \|\mathbf{w}^*\|_0 \leq \bar{m} \text{ are equally similar to the synthetic treated weights for } \|\mathbf{w}^*\|_0 \leq \bar{m} + 1, \text{ we select the set of weights with the smallest index. We follow the analogous procedure for } \lambda > 0.01, \text{ starting from smaller values of } \lambda.
A. Designs Based on Penalized and Bias-corrected Synthetic Control Methods

Consider the design problem in (7),

\[
\begin{align*}
\left\| \mathbf{X} - \sum_{j=1}^{J} w_j \mathbf{X}_j \right\|^2 + & \sum_{j=1}^{J} w_j \left\| \mathbf{X}_j - \sum_{i=1}^{J} v_{ij} \mathbf{X}_i \right\|^2 . \\
& \text{(a)} \\
\sum_{i=1}^{J} v_{ij} \left\| \mathbf{X} - \mathbf{X}_j \right\|^2, \\
& \text{(b)} \\
\sum_{i=1}^{J} v_{ij} \left\| \mathbf{X}_j - \mathbf{X}_i \right\|^2 .
\end{align*}
\]  

(A.1)

To apply the penalized synthetic control method of Abadie and L’Hour (2021) to this design, we replace the term (a) in (A.1) with

\[
\left\| \mathbf{X} - \sum_{j=1}^{J} w_j \mathbf{X}_j \right\|^2 + \lambda_1 \sum_{j=1}^{J} w_j \left\| \mathbf{X} - \mathbf{X}_j \right\|^2,
\]  

(A.2)

and the terms (b) with

\[
\left\| \mathbf{X}_j - \sum_{i=1}^{J} v_{ij} \mathbf{X}_i \right\|^2 + \lambda_2 \sum_{i=1}^{J} v_{ij} \left\| \mathbf{X}_j - \mathbf{X}_i \right\|^2 .
\]  

(A.3)

Here, \( \lambda_1 \) and \( \lambda_2 \) are positive constants that penalize discrepancies between the target values of the predictors (\( \mathbf{X} \) in (A.2) and \( \mathbf{X}_j \) in (A.3)) and the values of the predictors for the units that contribute to their synthetic counterparts.

All designs of Section 2 depend on terms akin to (a) and (b) in (A.1). These terms can be adapted as in (A.2) and (A.3) to implement the penalized synthetic control design of Abadie and L’Hour (2021).

For all the designs in Section 2, the bias-corrected estimator of Abadie and L’Hour (2021) is

\[
\tilde{\tau}_t^{BC} = \sum_{j=1}^{J} w_j^*(Y_{jt} - \hat{\mu}_0(X_j)) - \sum_{j=1}^{J} v_{ij}^*(Y_{jt} - \hat{\mu}_0(X_j)),
\]

where \( t \geq T_0 + 1 \) and the terms \( \hat{\mu}_0t(X_j) \) are the fitted values of a regression of untreated outcomes, \( Y_{jt}^N \), on units characteristics, \( X_j \). To avoid over-fitting biases, \( \hat{\mu}_0t(X_j) \) can be cross-fitted for the untreated.
B. Additional Proofs

B.1. Proof of Theorem A.1

B.1.1. Definitions

First, define \( T_p = \min\{T - T_0, T_0 - T_e\} \). Next, recall that

\[
\hat{u}_t = \sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_j^* Y_{jt},
\]

for \( t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\} \). For \( t \in \{T_0 + 1, \ldots, T\} \), \( \hat{u}_t \) are the post-intervention estimates of the treatment effects; and for \( t \in \mathcal{B} \), \( \hat{u}_t \) are the placebo treatment effects estimated for the blank periods.

Let

\[
u_t = \sum_{j=1}^{J} w_j^* \epsilon_{jt} - \sum_{j=1}^{J} v_j^* \epsilon_{jt} \quad \text{(B.1)}
\]

for \( t \in \mathcal{B} \), and

\[
u_t = \sum_{j=1}^{J} w_j^* \xi_{jt} - \sum_{j=1}^{J} v_j^* \epsilon_{jt} \quad \text{(B.2)}
\]

for \( t \in \{T_0 + 1, \ldots, T\} \). For each \( \pi \in \Pi \), similar to our definition of \( \bar{e}_\pi \), define the \( (T - T_0) \)-dimensional vector

\[ e_\pi = (u_{\pi(1)}, u_{\pi(2)}, \ldots, u_{\pi(T - T_0)}). \]

In addition, let \( e = (u_1, \ldots, u_{T - T_0}) = (\tau_{T_0+1}, \ldots, \tau_T) \). It is useful to observe that, under the null hypothesis in (13), the random variables \( u_t \) for \( t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\} \) are independent and identically distributed.

Next, define the following two functions. Let

\[
\hat{F}(x) = \frac{1}{|\Pi|} \sum_{\pi \in \Pi} 1 \{ S(\bar{e}_\pi) < x \},
\]

and

\[
\tilde{F}(x) = \frac{1}{|\Pi|} \sum_{\pi \in \Pi} 1 \{ S(e_\pi) < x \}.
\]
The proof of Theorem A.1 proceeds in four steps. In step one, we define a high probability event, $C_1$, such that $u_t$ and $\widehat{u}_t$ are close to each other under $C_1$. In step two, we define a high probability event, $C_2$, such that many components of $\{S(e_\pi)\}_{\pi \in \Pi}$ are well-separated from $S(e)$ under $C_2$. In step three, we show that, conditional on $C_1$ and $C_2$, the ordering of $S(e_\pi)$ and $S(e)$ will be the same as the ordering of $S(\widehat{e}_\pi)$ and $S(\widehat{e})$ for most $\pi \in \Pi$, which implies that $\widehat{F}(S(\widehat{e}))$ and $\tilde{F}(S(e))$ are also close to each other. In step four, we conclude the proof by linking $\widehat{F}(S(\widehat{e}))$ to the estimated $p$-value, and $\tilde{F}(S(e))$ to the nominal level $\alpha$.

B.1.2. Lemmas for the Proof of Theorem A.1

Lemma B.2 (Corollary 2, Bobkov and Chistyakov (2014)) Let $X_1, X_2, \ldots, X_n$ be independent and continuously distributed random variables with densities $f_{X_1}, f_{X_2}, \ldots, f_{X_n}$. For any $k \in \{1, 2, \ldots, n\}$, let $\Lambda_{X_k}$ be the smallest upper bound on the probability density $f_{X_k}$. Suppose that for any $k \in \{1, 2, \ldots, n\}$, $\Lambda_{X_k} \leq \kappa$. Then for any $a_1, a_2, \ldots, a_n$ such that $\sum_{k=1}^n a_k^2 = 1$,

$$
\Lambda(a_1X_1 + a_2X_2 + \ldots + a_nX_n) \leq \sqrt{e}\kappa.
$$

Lemma B.3 Let $X$ be a continuously distributed random variable with a density $f_X$. Let $\Lambda_X$ be the smallest upper bound on the probability density $f_X$.

1. The random variable $|X|$ has a density $f_{|X|}$ bounded by $\Lambda_{|X|} \leq 2\Lambda_X$;

2. For any constant $a \neq 0$, the random variable $aX$ has a density, $f_{aX}$, bounded by $\Lambda_{aX} \leq \Lambda_X/a$;

Proof of Lemma B.3. To prove 1, note that for any $v \geq 0$,

$$
f_{|X|}(v) = f_X(v) + f_X(-v) \leq 2\Lambda_X.
$$

To prove 2, note that for any $v \geq 0$,

$$
f_{aX}(v) = \frac{1}{|a|} f_X(v/a) \leq \frac{1}{|a|} \Lambda_X.
$$

\hfill \blacksquare
Lemma B.4 Recall that $u_t$ is defined as (B.1) and (B.2), for the blank periods and the post-intervention periods, respectively. The probability density of $u_t$ can be bounded by

$$\Lambda_{u_t} \leq \frac{1}{2} \sqrt{e^J \kappa}$$

Proof of Lemma B.4. Fix any $t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\}$. Using Lemma B.2 (Bobkov and Chistyakov, 2014, Corollary 2), let there be $J$ variables $\epsilon_{jt}$ for any $j \in \{1, 2, \ldots, J\}$. For any $j \in \{1, 2, \ldots, J\}$, define $a_j = \frac{w_j^* - v_j^*}{\sqrt{\sum_{j=1}^{J} (w_j^* - v_j^*)^2}}$ such that $\sum_{j=1}^{J} a_j^2 = 1$. Using $a_j$, we can write $u_t$ as $u_t = \sqrt{\sum_{j=1}^{J} (w_j^* - v_j^*)^2} \cdot \sum_{j=1}^{J} a_j \epsilon_{jt}$. Then we have, either when $J$ is even,

$$\Lambda_{u_t} \leq \frac{1}{\sqrt{\sum_{j=1}^{J} (w_j^* - v_j^*)^2}} \cdot \Lambda_{\sum_{j=1}^{J} a_j \epsilon_{jt}}$$

$$\leq \frac{1}{\sqrt{\sum_{j=1}^{J} (w_j^* - v_j^*)^2}} \cdot \sqrt{e^J \kappa}$$

$$\leq \frac{1}{\sqrt{\sum_{j=1}^{J} (\frac{2}{J})^2}} \cdot \sqrt{e^J}$$

$$= \frac{\sqrt{J}}{2} \sqrt{e^J},$$

where the first inequality is due to Lemma B.3 Part 2; the second inequality is due to Lemma B.2; the third inequality is due to convexity and Jensen’s inequality, and the worst case is taken when $w_j^* = 2/J$ for one half of total units and $v_j^* = 2/J$ for the other half. Or, when $J$ is odd,

$$\Lambda_{u_t} \leq \frac{1}{\sqrt{\sum_{j=1}^{J} (w_j^* - v_j^*)^2}} \cdot \Lambda_{\sum_{j=1}^{J} a_j \epsilon_{jt}}$$

$$\leq \frac{1}{\sqrt{\sum_{j=1}^{J} (w_j^* - v_j^*)^2}} \cdot \sqrt{e^J \kappa}$$

$$\leq \frac{1}{\sqrt{\sum_{j=1}^{J} (\frac{2}{J})^2 + \sum_{j=1}^{J-1} (\frac{2}{J-1})^2}} \cdot \sqrt{e^J}$$

$$= \sqrt{\frac{J^2 - 1}{J}} \cdot \sqrt{e^J}$$

$$\leq \frac{\sqrt{J}}{2} \sqrt{e^J},$$
where the first inequality is due to Lemma B.3 Part 2; the second inequality is due to Lemma B.2; the third inequality is due to convexity and Jensen’s inequality, and the worst case is taken when \(w^*_j = 2/(J + 1)\) for \((J + 1)/2\) of total units and \(v^*_j = 2/(J - 1)\) for the other \((J - 1)/2\) of total units.

**B.1.3. Proof of Theorem A.1**

**Proof of Theorem A.1.** (Step one.) Note that

\[
\sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_j^* Y_{jt} = \theta \left( \sum_{j=1}^{J} w_j^* Z_j - \sum_{j=1}^{J} v_j^* Z_j \right) \\
+ \lambda \left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} v_j^* \mu_j \right) + \left( \sum_{j=1}^{J} w_j^* \epsilon_j^* - \sum_{j=1}^{J} v_j^* \epsilon_j^* \right).
\]

Assumption 3 implies

\[
\left( \sum_{j=1}^{J} w_j^* \mu_j - \sum_{j=1}^{J} v_j^* \mu_j \right) = -(\lambda' \lambda \lambda')^{-1} \lambda' \left( \sum_{j=1}^{J} w_j^* \epsilon_j^* - \sum_{j=1}^{J} v_j^* \epsilon_j^* \right).
\]

It follows that

\[
\hat{u}_t = \sum_{j=1}^{J} w_j^* Y_{jt} - \sum_{j=1}^{J} v_j^* Y_{jt} \\
= - \lambda'_t (\lambda_\mathcal{E} \lambda_\mathcal{E})^{-1} \lambda'_\mathcal{E} \sum_{j=1}^{J} (w_j^* - v_j^*) \epsilon_j^* + u_t,
\]

for \(t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\}\). We next define an event

\[
C_1 = \left\{ \forall t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\}, \left| \lambda'_t (\lambda_\mathcal{E} \lambda_\mathcal{E})^{-1} \lambda'_\mathcal{E} \sum_{j=1}^{J} (w_j^* - v_j^*) \epsilon_j^* \right| \leq z_1 \right\}
\]

\[
= \left\{ \max_{t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\}} \left| \lambda'_t (\lambda_\mathcal{E} \lambda_\mathcal{E})^{-1} \lambda'_\mathcal{E} \sum_{j=1}^{J} (w_j^* - v_j^*) \epsilon_j^* \right| \leq z_1 \right\}.
\]

Note that,

\[
\max_{t \in \mathcal{B} \cup \{T_0 + 1, \ldots, T\}} \left| \lambda'_t (\lambda_\mathcal{E} \lambda_\mathcal{E})^{-1} \lambda'_\mathcal{E} \sum_{j=1}^{J} (w_j^* - v_j^*) \epsilon_j^* \right|
\]
\[
\max_{t \in B \cup \{T_0 + 1, \ldots, T\}} \sum_{j=1}^{J} |w_j^* - v_j^*| \sum_{s \in \mathcal{E}} |\lambda_j'(\lambda_j' \lambda_{E})^{-1} \lambda_s| \epsilon_{js} \\
\leq \sum_{j=1}^{J} |w_j^* - v_j^*| \sum_{s \in \mathcal{E}} \frac{\chi^2}{T \mathcal{E} \xi} |\epsilon_{js}|,
\]
where the first inequality is due to (B.5), and the second inequality is because \(|w_j^* - v_j^*| \geq 0\) and \(\epsilon_{js} \geq 0\). Therefore,

\[
\Pr(C_1) \geq 1 - \Pr \left( \sum_{j=1}^{J} \frac{|w_j^* - v_j^*|}{2} \sum_{s \in \mathcal{E}} \frac{\chi^2}{T \mathcal{E} \xi} |\epsilon_{js}| > \frac{z_1}{2} \right) \\
\geq 1 - \sum_{j=1}^{J} \Pr \left( \sum_{s \in \mathcal{E}} \frac{\chi^2}{T \mathcal{E} \xi} |\epsilon_{js}| > \frac{z_1}{2} \right) \\
\geq 1 - 2J \exp \left( -\frac{z_1^2 \xi^2}{8\sigma^2 \lambda^4 F^2 T \mathcal{E}} \right),
\]
where the first inequality follows from union bound, and the second inequality is the Chernoff bound for sub-Gaussian random variables.

(Step two.) Define \(\tilde{z}_1 = 2z_1 > 0\). For each \(k \in \{0, 1, 2, \ldots, T - T_0\}\), we define the following sets of permutations. First, define \(\Pi_0 = \{\pi_0\}\), where \(\pi_0\) is defined as the set of post-intervention indices \(\pi_0 = \{T_0 + 1, \ldots, T\}\). Then, for any \(k \in \{1, 2, \ldots, T - T_0\}\), define

\[
\Pi_k = \left\{ \pi \in \Pi \left| \left| \pi \setminus \pi_0 \right| = k \right. \right\}
\]
to be the set of \((T - T_0)\)-combinations with exactly \(k\) many indices from the blank periods. Using the above definitions, we can decompose \(\Pi\) into

\[
\Pi = \bigcup_{k=0}^{T_p} \Pi_k,
\]
where \(T_p = \min\{T - T_0, T_0 - T_0 - T_E\}\).

Then, for any \(k \in \{1, 2, \ldots, T_p\}\) and \(\pi \in \Pi_k\), we focus on the following indicator

\[
1 \left\{ \left| \sum_{t \in \pi \setminus \pi_0} u_t - \sum_{t \in \pi_0 \setminus \pi} |u_t| \right| \leq 2kz_1 \right\}.
\]
The above indicator involves a total of $2k$ many $|u_t|$’s. Intuitively, it is obtained by canceling out common terms in $S(e_x)$ and $S(e)$.

Below we focus on the properties of the sum of such indicators. First, focus on the probability density of $\left| \sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| \right|$. We have

$$\Lambda |\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t|| \leq 2\Lambda \sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} \frac{1}{2\sqrt{k}} |u_t|$$

$$\leq \frac{2}{\sqrt{2k}} \Lambda |\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} \frac{1}{\sqrt{2k}} |u_t||$$

$$\leq \frac{2}{\sqrt{2k}} \sqrt{e} \Lambda |u_t|$$

$$\leq \frac{2}{\sqrt{2k}} \sqrt{e} \sqrt{J} \kappa$$

$$= \frac{2\sqrt{J}}{\sqrt{k}} e \kappa,$$

where the first inequality is due to Lemma B.3-1; the second inequality is due to Lemma B.2-2; the third inequality is due to Lemma B.2; the last inequality is due to Lemma B.4 and B.3-1.

We obtain

$$\Pr \left( \left| \sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| \right| \leq 2kz_1 \right) \leq 2e \sqrt{2Jkz_1 \kappa}.$$ 

Next, due to Markov inequality, for any constant $z_2 > 0$, we have

$$\Pr \left( \sum_{k=1}^{T_p} \sum_{\pi \in \Pi_k} \mathbb{1} \left\{ \left| \sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| \right| \leq 2kz_1 \right\} \geq |\Pi| z_2 \right)$$

$$\leq \frac{1}{|\Pi| z_2} \sum_{k=1}^{T_p} \sum_{\pi \in \Pi_k} \mathbb{E} \left[ \mathbb{1} \left\{ \left| \sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| \right| \leq 2kz_1 \right\} \right]$$

$$\leq \frac{\sum_{k=1}^{T_p} |\Pi_k| 2e \sqrt{2Jkz_1 \kappa}}{|\Pi| z_2}.$$ 

To conclude step two, define the event

$$C_2 = \left\{ \sum_{k=1}^{T_p} \sum_{\pi \in \Pi_k} \mathbb{1} \left\{ \left| \sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| \right| \leq 2kz_1 \right\} < |\Pi| z_2 \right\}. \quad (B.3)$$
The probability that event $C_2$ happens is at least
\[
\Pr(C_2) \geq 1 - \frac{\sum_{k=1}^{T_p} |\Pi_k| \sqrt{k} 2e\sqrt{2Jz_1\kappa}}{|\Pi| z_2}.
\]

(Step three.) Conditional on event $C_2$, fewer than $|\Pi|z_2$ of the absolute value terms in (B.3) are such that $|\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| | \leq 2kz_1$. For all the others, $|\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| | > 2kz_1$.

Conditional on event $C_1$, we know that $|\hat{u}_t - u_t| \leq z_1$ for any $t \in B \cup \{T_0 + 1, \ldots, T\}$. So we have that $\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| > 2kz_1$ implies
\[
S(\hat{\pi}) - S(\pi) = \frac{1}{T - T_0} \sum_{t \in \pi} |\hat{u}_t| - \frac{1}{T - T_0} \sum_{t \in \pi_0} |\hat{u}_t|
\]
\[
= \frac{1}{T - T_0} \left( \sum_{t \in \pi \setminus \pi_0} |\hat{u}_t| - \sum_{t \in \pi_0 \setminus \pi} |\hat{u}_t| \right)
\]
\[
\geq \frac{1}{T - T_0} \left( \sum_{t \in \pi \setminus \pi_0} (|u_t| - z_1) - \sum_{t \in \pi_0 \setminus \pi} (|u_t| + z_1) \right)
\]
\[
> \frac{1}{T - T_0} (2kz_1 - 2kz_1)
\]
\[
= 0,
\]
where the first equality is due to definition $S(\pi) = \frac{1}{T - T_0} \sum_{t \in \pi} |u_t|$. Similarly, $C_1$ and $\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| < -2kz_1$ implies
\[
S(\hat{\pi}) - S(\pi) = \frac{1}{T - T_0} \left( \sum_{t \in \pi \setminus \pi_0} |\hat{u}_t| - \sum_{t \in \pi_0 \setminus \pi} |\hat{u}_t| \right)
\]
\[
\leq \frac{1}{T - T_0} \left( \sum_{t \in \pi \setminus \pi_0} (|u_t| + z_1) - \sum_{t \in \pi_0 \setminus \pi} (|u_t| - z_1) \right)
\]
\[
< \frac{1}{T - T_0} (-2kz_1 + 2kz_1)
\]
\[
= 0.
\]

Combining both cases, we know that conditional on $C_1$ and when $|\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| | > 2kz_1$, the ordering of $S(\pi)$ and $S(\hat{\pi})$ is the same as the ordering of $S(\hat{e}_\pi)$ and $S(\hat{e})$. As a
result, for those $\pi$ such that $|\sum_{t \in \pi \setminus \pi_0} |u_t| - \sum_{t \in \pi_0 \setminus \pi} |u_t| | > 2kz_1$, we have $\mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} = \mathbb{1}\{S(e_\pi) \geq S(e)\}$. There are at most $|\Pi|z_2$ many $\pi$’s that contribute to the following summation,

$$\left| \sum_{k=1}^{T_p} \sum_{\pi \in \Pi_k} \left( \mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} - \mathbb{1}\{S(e_\pi) \geq S(e)\} \right) \right| < |\Pi|z_2.$$

Note that $S(\tilde{\pi}_{\pi_0}) = S(\tilde{e})$ and $S(e_{\pi_0}) = S(e)$, so $\mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} = \mathbb{1}\{S(e_\pi) > S(e)\}$ is always true. Combining $\pi_0$ we have

$$\left| \sum_{\pi \in \Pi} \left( \mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} - \mathbb{1}\{S(e_\pi) \geq S(e)\} \right) \right| = \sum_{k=0}^{T_p} \sum_{\pi \in \Pi_k} \left( \mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} - \mathbb{1}\{S(e_\pi) \geq S(e)\} \right) < |\Pi|z_2.$$

We conclude step three using the following block of inequalities. For any $\alpha \in (0, 1]$,

$$\left| \Pr \left( 1 - \tilde{F}(S(\tilde{e})) \leq \alpha \right) - \Pr \left( 1 - \tilde{F}(S(e)) \leq \alpha \right) \right| = \left| \Pr \left( 1 - \frac{1}{|\Pi|} \sum_{\pi \in \Pi} \mathbb{1}\{S(\tilde{\pi}) < S(\tilde{e})\} \leq \alpha \right) - \Pr \left( 1 - \frac{1}{|\Pi|} \sum_{\pi \in \Pi} \mathbb{1}\{S(e_\pi) < S(e)\} \leq \alpha \right) \right|$$

$$= \left| \Pr \left( \sum_{\pi \in \Pi} \mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} \leq \alpha|\Pi| \right) - \Pr \left( \sum_{\pi \in \Pi} \mathbb{1}\{S(e_\pi) \geq S(e)\} \leq \alpha|\Pi| \right) \right|$$

$$\leq E \left[ \sum_{\pi \in \Pi} \mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} \leq \alpha|\Pi| \right] - E \left[ \sum_{\pi \in \Pi} \mathbb{1}\{S(e_\pi) \geq S(e)\} \leq \alpha|\Pi| \right]$$

$$\leq \Pr \left( \alpha|\Pi| - \sum_{\pi \in \Pi} \mathbb{1}\{S(e_\pi) \geq S(e)\} \leq \sum_{\pi \in \Pi} \left( \mathbb{1}\{S(\tilde{\pi}) \geq S(\tilde{e})\} - \mathbb{1}\{S(e_\pi) \geq S(e)\} \right) \right),$$

where the second inequality is due to the following: $|\mathbb{1}\{a \leq c\} - \mathbb{1}\{b \leq c\}| \leq \mathbb{1}\{|c-b| \leq |a-b|\}$. Conditional on events $C_1$ and $C_2$, we obtain

$$\left| \Pr \left( 1 - \tilde{F}(S(\tilde{e})) \leq \alpha \right) - \Pr \left( 1 - \tilde{F}(S(e)) \leq \alpha \right) \right| \leq \Pr \left( \alpha|\Pi| - \sum_{\pi \in \Pi} \mathbb{1}\{S(e_\pi) \geq S(e)\} \leq |\Pi|z_2 \right).$$

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\[ \leq \frac{2|\Pi|z_2}{|\Pi|} = 2z_2, \quad (B.4) \]

where the last inequality is because \( \sum_{\pi \in \Pi} 1\{S(e_\pi) \geq S(e)\} \) is a discrete uniform distribution over \( \{1, 2, ..., |\Pi|\} \), and that there are at most \( 2|\Pi|z_2 \) many integers centered around \( \alpha|\Pi| \).

(Step four.) Note that, for any \( \alpha \in (0, 1], \)

\[ \alpha - \frac{1}{|\Pi|} \leq \Pr \left( 1 - \hat{F}(S(e)) \leq \alpha \right) \leq \alpha. \]

So conditional on events \( C_1 \) and \( C_2 \), (B.4) implies

\[ \Pr \left( 1 - \hat{F}(S(\hat{e})) \leq \alpha \right) \leq \Pr \left( 1 - \hat{F}(S(e)) \leq \alpha \right) + 2z_2 \leq \alpha + 2z_2 \]

and

\[ \Pr \left( 1 - \hat{F}(S(\hat{e})) \leq \alpha \right) \geq \Pr \left( 1 - \hat{F}(S(e)) \leq \alpha \right) - 2z_2 \geq \alpha - 2z_2 - \frac{1}{|\Pi|}. \]

Combining both parts, conditional on \( C = C_1 \cap C_2 \), we have

\[ \alpha - 2z_2 - \frac{1}{|\Pi|} \leq \Pr(\hat{p} \leq \alpha) = \Pr \left( 1 - \hat{F}(S(\hat{e})) \leq \alpha \right) \leq \alpha + 2z_2, \]

and \( C \) happens with probability at least

\[ \Pr(C_1 \cap C_2) \geq (1 - \Pr(C_1)) + (1 - \Pr(C_2)) - 1 \]

\[ \geq 1 - 2J \exp \left( -\frac{z_1^2 \xi^2}{8\sigma^2 \lambda^4 F^2 T\epsilon} \right) - \frac{\sum_{k=1}^{T_p} |\Pi_k| \sqrt{k}}{|\Pi|} \cdot \frac{z_1}{z_2} \cdot 2e \sqrt{2J \kappa}, \]

which finishes the proof. \( \blacksquare \)

C. Implementations of the Optimization Formulations

To computationally solve (4), i.e., the Unconstrained design, we propose two methods. The first method is by enumeration, which takes advantage of the objective function of (4) being separated between \( w \) and \( v \). If we knew which units were to receive treatment and which units were to receive control, then we could decompose (4) into two classical synthetic control problems and
solve both of them efficiently. We brute force enumerate all the possible combinations of the
treatment units and control units. Because the two groups of treated and control units can
be swapped (see Section 4.2.2), we only enumerate combinations such that the cardinality of
the treated group is smaller than or equal to the cardinality of the control group. When the
 cardinality of the treated group is equal to the cardinality of the control group, we prioritize the
treated group to be the one with the smallest index among the units with positive weights.
The second method solves a constrained optimization problem, by converting it into the canonical
form of a Quadratic Constraint Quadratic Program (QCQP), which we detail below. The decision
variables are \( w_j \) and \( v_j, \forall j = 1, \ldots, J \). For simplicity, we write it in a vector form \( \bar{W} = (w_1, w_2, \ldots, w_J, v_1, v_2, \ldots, v_J) \).
Let \( M \) be the dimension of the predictors \( X_j \). Let \( X \) be an \( M \times J \) matrix, each column of which
is \( X_j \), which stands for the predictors of unit \( j \).
Define \( P^0 = \{ P^0_{k,l} \}_{k,l=1,\ldots,2J} \in \mathbb{R}^{2J \times 2J} \), such that \( P^0 \) has only two diagonal blocks, while the two off-diagonal blocks are zero. Define for any \( k, l = 1, \ldots, 2J \),
\[
P^0_{k,l} = \begin{cases} 
\sum_{i=1}^{M} X_{i,k} X_{i,l}, & k, l = 1, \ldots, J; \\
\sum_{i=1}^{M} X_{i,(k-J)} X_{i,(l-J)}, & k, l = J + 1, \ldots, 2J; \\
0, & \text{otherwise.}
\end{cases}
\]
Define \( q^0 \in \mathbb{R}^{2J} \), such that for any \( k = 1, \ldots, 2J \)
\[
q^0_k = \begin{cases} 
-2 \sum_{i=1}^{M} X_{i,k} \cdot \left( \sum_{j=1}^{J} f_j X_{i,j} \right), & k = 1, \ldots, J; \\
-2 \sum_{i=1}^{M} X_{i,k-J} \cdot \left( \sum_{j=1}^{J} f_j X_{i,j} \right), & k = J + 1, \ldots, 2J.
\end{cases}
\]
Further define \( e_1 = (1, 1, \ldots, 1, 0, 0, \ldots, 0)' \) whose first \( J \) elements are 1 and last \( J \) elements 0; and \( e_2 = (0, 0, \ldots, 0, 1, 1, \ldots, 1)' \) whose first \( J \) elements are 0 and last \( J \) elements 1.
Finally, define \( P^1 = \{ P^1_{k,l} \}_{k,l=1,\ldots,2J} \in \mathbb{R}^{2J \times 2J} \) such that \( P^1 \) only has non-zero values in the two
off-diagonal blocks, i.e., for any $k, l = 1, \ldots, 2J$,

$$
P_{k,l}^1 = \begin{cases} 1, & k = l + J; \\ 1, & k = l - J; \\ 0, & \text{otherwise.} \end{cases}
$$

Using the above notations we re-write the (non-convex) QCQP as follows,

$$
\min \ W' P_0 W + q^0 W \\
\text{s.t. } e_1' W = 1, \\
\quad e_2' W = 1, \\
\quad \tilde{W}' P^1 \tilde{W} = 0, \\
\quad \tilde{W} \geq 0.
$$

(C.1)

The first computational method (enumeration) solves two synthetic control problems in each iteration. The synthetic control problems can be efficiently solved. We implement the synthetic control problem using the “lseii” function from “limSolve” package in R 4.0.2. For the second computational method (quadratic programming), the problem (C.1) is implemented using Gurobi 9.0.2 in R 4.0.2. Since the QCQP is non-convex, the computation leads to some numerical errors up to 0.001 in finding the treated and control weights. So we round the treated and control weights to the nearest 2-digits in the implementation of the QCQP. Moreover, for all the weights that are less than or equal to 0.01, we trim the weights to zero. This is because smaller weights suffer from greater impacts of numerical errors, and that numerical errors could make zero weights to be non-zero, thus having a non-negligible impact on the swapping rule.

To conclude, we compare the treated and control weights calculated from both methods. Both methods yield the same treated and control weights up to some negligible rounding error, while the first method takes longer computational time.

The other designs are computationally implemented using either one of the above two methods. The Constrained design is implemented using the enumeration method. In cases when the cardinality constraint $\bar{m}$ is small, this brute force enumeration is very efficient. The Weakly-targeted design is implemented using the quadratic programming method. In the QCQP formulation,
the objective function has both a different quadratic term $P^0$ and a different linear term $q^0$. The *Unit-Level* design is implemented using the enumeration method. The *Penalized* design is implemented using the quadratic programming method. In the QCQP formulation, the objective function has the same quadratic term $P^0$ and a different linear term $q^0$.

D. Additional Simulation Results

In Section 4.1 the idiosyncratic shocks are i.i.d. Normal with variance $\sigma^2 = 1$. Figures 4 and 5 report results for $\sigma^2 = 5$ and $\sigma^2 = 10$, respectively. Figures 6 and 7 report differences between the outcomes for the synthetic treated and the synthetic control units for the same values for $\sigma^2$. As the value of $\sigma^2$ increases, the quality of the post-treatment estimation and inference deteriorates, and the $p$-value for the null hypotheses of in (13) increases. The deterioration in pre-treatment fit in Figures 4 and 5 provides a diagnosis of the accuracy of the respective estimates.
Figure 4: Synthetic Treatment Unit and Synthetic Control Unit, when $\sigma^2 = 5$.

Figure 5: Synthetic Treatment Unit and Synthetic Control Unit, when $\sigma^2 = 10$. 

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Figure 6: Treatment Effect Estimate, when $\sigma^2 = 5$.

Figure 7: Treatment Effect Estimate, when $\sigma^2 = 10$. 

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