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Trajectory Balancing: A General Reweighting Approach to Causal Inference with Time-Series Cross-Sectional Data

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Abstract

We introduce trajectory balancing, a general reweighting approach to causal inference with time-series cross-sectional (TSCS) data. We focus on settings where one or more units is exposed to treatment at a given time, while a set of control units remain untreated. First, we show that many commonly used TSCS methods imply an assumption that each unit's non-treatment potential outcomes in the post-treatment period are linear in that unit's pre-treatment outcomes and its time-invariant covariates. Under this assumption, we introduce the *mean balancing* method that reweights control units such that the averages of the pre-treatment outcomes and covariates are approximately equal between the treatment and (reweighted) control groups. Second, we relax the linearity assumption and propose the *kernel balancing* to seek approximate balance on a kernel-based feature expansion of the pre-treatment outcomes and covariates. The resulting approach inherits the ability of synthetic control and latent factor models to tolerate time-varying confounders, but (1) improves feasibility and stability with reduced user discretion; (2) accommodates both short and long pre-treatment time periods with many or few treated units; and (3) balances on the high-order "trajectory" of pre-treatment outcomes rather than their period-wise average. We illustrate this method with simulations and two empirical examples.

Keywords: causal inference, time-series cross-sectional data, panel data, difference-in-differences, synthetic control, interactive fixed effects, kernel balancing, reweighting

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1. Introduction

Causal inference with observational data is an everyday challenge for many applied researchers. A wide variety of tools attempt to make causal claims using time-series cross-sectional (TSCS) data or panel data, in which over-time variations in the outcomes and treatment status in at least a subset of the units can assist in making credible inferences.

One popular set of tools for estimation in this context are the difference-in-differences (DID) and two-way fixed effects models. While time-invariant confounders are "differenced out" under these approaches, they share a common assumption that there exists *no unobserved time-varying confounders* influencing both the outcome and the treatment. This assumption, often referred to as "parallel trends" in the DID tradition, requires that average change in the non-treatment outcome is the same among the controls and treated, making their paths parallel. This assumption is not directly testable, and remains a core challenges of DID and fixed effects approaches.[1]

In addition, Imai and Kim (2013) show that fixed effects models carry additional modeling assumptions, including (1) the treatment effect is homogeneous; (2) the treatment only affects the contemporaneous outcome and (3) past outcomes do not affect future treatment.To alleviate these concerns, in this paper, we focus on a *generalized DID setting* in which all units under consideration begin as untreated and a subset of units receive a treatment that begins at a given time. In this particular setting, because only two histories of treatment status are under consideration, we can allow the treatment to have a long-lasting effect on the outcome, as long as we make the direct comparison between potential outcomes under the two treatment histories.

Two approaches attempt to deal with the seemingly intractable problem of unobserved time-varying confounders in this setting, both by taking advantage of information in the pre-treatment outcomes. The synthetic control method (SCM) is a weighting-based approach that finds weights on control units that form a "synthetic control" unit whose pre-treatment history closely matches that of a single treated unit (Abadie and Gardeazabal 2003, Abadie, Diamond and Hainmueller

[1]We further note that these models may have hierarchical/multilevel elements, random or mixed effects, or data structures that account for auto-regression of many forms, but these modeling assumptions do not typically weaken the core identifying assumptions of no unobserved time-varying confounders.

2010, henceforth, ADH 2010). One interpretation of the SCM is as follows: assume that there exist one or several omitted time varying confounders and some time-fixed linear combination of these confounders affect the outcomes of both the treated and control units. Because these confounders appear in varying degrees in the control units, weighting the control units to make their averaged pre-treatment trend match that of the treated effectively replicates the combination of confounders that must be influencing the treated unit as well. Hence, the time-varying confounders are differenced out.

A closely related approach explicitly assumes a latent factor model (LFM), a data generating process (DGP) for which the SCM would be approximately unbiased under reasonable assumptions. While we examine the details below, in brief this approach presumes the scenario described above based on an interactive fixed effects (IFE) model: a set of time-varying influences (i.e., latent factors) exist, and each unit takes some (fixed) linear combination of them (Bai 2009). As described in Xu (2017), predicting treated counterfactuals using a LFM provides an alternative estimation procedure to that of synthetic control with greater flexibility. Both the SCM and LFM are attractive in that they allow omitted time-varying confounders of a particular form, but also have several important limitations we seek to address.

In this paper, we introduce *trajectory balancing* as a general solution to causal inference with observational data in a generalized DID setting. First, we show that a surprising variety of existing modeling approaches imply an assumption that the non-treatment potential outcomes in the post-treatment period are linear in the pre-treatment outcomes and covariates. We refer to this as the *Linearity in Prior Outcomes* (LPO) assumption. As a first contribution, the LPO assumption naturally suggests a simpler and more general estimation procedure: obtain equal means for the treatment and control groups, on the period-wise pre-treatment outcomes, and optionally on auxiliary covariates. Because of linearity, if the treated and control groups have equal means on these features, then they have equal means in any functions linear in these features, without having to estimate the corresponding coefficients. Therefore, under the proposed group of trajectory balancing approaches, we thus first consider such a *mean balancing* procedure. It chooses weights for the control units such that the mean-balance constraints are satisfied while the entropy or maximum

empirical likelihood of weights are maximized. An estimate of the Average Treatment Effects on the Treated (ATT) is obtained by taking the difference between the average of treated outcome and the weighed average of control outcome in the post-treatment period. This procedure is in the spirit of the SCM as it seeks balance on pre-treatment outcomes and covariates between the treated and controls. It is different in that it first seeks exact balance on feature when it is feasible; when exact balance is infeasible, it seeks balance on the first P principal components of the features, where P is chosen automatically. This approach sidesteps several additional challenges of the estimation procedures of SCM and LFMs. First, unlike the SCM, it can accommodate multiple treated units in a single run with little increase in computational time or risk of non-convergence. Second, it does not require a large number of pre-treatment periods, which is necessary for both the SCM and LFMs to work properly.

Our second contribution is to overcome the limitations inherent in the LPO assumption. With longer pre-treatment periods, the LPO assumption may be adequate: there are more opportunities for important time-varying "factors" to be visible in the pre-treatment outcomes, and more balance constraints to ensure balance on these factors. However, with fewer pre-treatment periods, there are fewer balance constraints to solve, and solutions may exist that obtain good mean balance on each pre-treatment period, but fail to obtain balance on factors that influence potential outcomes. Mean balancing alone does not ensure that higher order features of the pre-treatment trajectory—such as "volatility," "variance" or "curviness"—are balanced between treated and control groups. Relatedly, mean balance can be obtained without any guarantee that the distribution of pre-treatment trajectories looks similar in the treated and control groups, as it ensures only the means look similar periodwise. To solve these problems, we take a higher-dimensional feature expansion of the pre-treatment trajectory as well as covariates, and seek approximate balance on this feature expansion.

This corresponds to a weakening of the LPO assumption, requiring linearity of the non-treatment potential outcome in the higher-dimensional feature set rather than the original data. A natural choice for such an expansion is a kernel-based procedure, that effectively considers the similarity of each unit's trajectory to each other unit's trajectory in a multivariate space, and takes this vector

of similarities as the features to be balanced. The *kernel balancing* procedure has the desirable property of ensuring that any function of the pre-treatment outcomes in a large space of smooth functions will have equal means in the treated and control groups (Hazlett 2018). Again, due to feasibility concerns, the procedure seeks balance on the first P eigenvectors of the kernel based features. We describe the bound on the worst-case bias due to obtaining approximate balance, and this bound is minimized through the optimization procedure.

The trajectory balancing methods we propose inherit the same useful properties as the SCM and LFMs in coping with time-varying confounding through explicitly using the pre-treatment outcome data, but offers additional advantages by (1) improving feasibility and stability with reduced user discretion compared to existing approaches; (2) accommodating both short and long pre-treatment time periods, with many or few treated units; and (3) achieving balance on the high-order "trajectory" of pre-treatment outcomes rather than their simple averages at each time period. We illustrate this method with simulations and two empirical examples, and provide access to these tools in the `tjbal` package for `R`.

In what follows, Section 2 provides the framework and setup; Section 3 proposes the trajectory balancing method, details, and extensions. Section 4 provides a simulated example to fix ideas, and Section 5 provides two empirical applications (with two additional applications in the Online Appendix). We conclude with Section 6.

2. Setup

In this section, we set up the analytical framework and describe a family of existing models for causal inference with TSCS data. Though apparently diverse and flexible, these methods all require that for each unit i, it's non-treatment potential outcomes in the post-treatment period are linear transformations of i's non-treatment potential outcomes in the pre-treatment period. We define the assumption more precisely below and refer to it as the *Linearity in Prior Outcomes* (LPO) assumption. We find that many existing approaches fall in this category.For simplicity, we begin by describing these methods in a context without covariates, i.e. only pre-treatment outcomes are used for estimation purposes. We will later generalize our methods with the inclusion of covariates.

2.1. Notation, Assumptions, and Estimation Strategy

Suppose we have a time-series cross-sectional (TSCS) dataset of the generalized DID type, with N_{tr} treated units and N_{co} units; hence, the total number of units is $N = N_{tr} + N_{co}$. Denote a group indicator $G_i = 1$ if i belongs to the treatment group and $G_i = 0$ if i belongs to the control group. All units are observed for T periods from time 1 to time T. There may be a single unit, or multiple treated units all exposed to treatment for the first time at period $T_0 + 1$. Control units are never exposed to the treatment throughout the observed time period. Let Y_{it} be the outcome of interest of unit i at time t, and D_{it} be an indicator of treatment status (i.e., $D_{it} = 1$ when $G_i = 1$ and $t > T_0$ and $D_{it} = 0$ otherwise). Denote $\{Y_{it}^1, Y_{it}^0\}$ the potential outcomes for unit i at time t when $D_{it} = 1$ or $D_{it} = 0$, respectively. Define $\tau_{it} = Y_{it}^1 - Y_{it}^0$ the individual contemporaneous treatment effect for unit i at time t.

Estimand. The primary causal quantity of interest is the Average Treatment Effect on the Treated (ATT) at time t, $T_0 < t \leq T$, i.e.,

$$ATT_t = \mathbb{E}[\tau_{it}|G_i = 1], \qquad T_0 < t \leq T.$$

Identification assumptions. In order to identify the ATT_t, first, we assume the standard conditional ignorability assumption, but where the conditioning is done on the pre-treatment realizations of the non-treatment potential outcomes.

Assumption 1 (Conditional Ignorability)

$$Y_{it}^0 \perp\!\!\!\perp G_i|\mathbf{Y}_{i,pre}, \quad \forall t > T_0$$

in which $\mathbf{Y}_{i,pre} = (Y_{i1}, Y_{i2}, \cdots, Y_{iT_0})$, a $(1 \times T_0)$ vector of pre-treatment outcomes. We note that only the non-treatment potential outcome Y_{it}^0, and not the treatment potential outcome Y_{it}^1 is involved for purposes of estimating the ATT.[2] This assumption is analogous to the selection-on-observable

[2]To extend our results to the average treatment effect among the controls (ATC) or among all units (ATE) would require assumptions on Y_{it}^1 as well. However when only the ATT is required, the treatment potential outcomes observed directly, and the non-treatment potential outcomes of the treated are imputed from those of control units, requiring assumptions on the ignorability of treatment only for the non-treatment potential outcomes.

assumption in the causal inference literature (e.g. Rosenbaum and Rubin 1983) except that it is the pre-treatment outcomes being conditioned upon. Assumption 1 implies that once we condition on the pre-treatment outcome history, the treatment is "as-if" randomly assigned between the treated and control units. In other words, among units with the same pre-treatment histories, who gets the treatment is independent of (non-treatment) potential outcomes in the post-treatment periods.

Because the entire distribution of Y_{it}^0 is unchanged by knowledge of treatment status conditional on the pre-treatment history under Assumption 1, so is the expectation of Y_{it}^0, i.e.

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = \mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}, G_i].$$

In fact, it is only this "mean independence" assumption we require for the results below, but we employ the more commonly invoked Assumption 1 for consistency with the literature. It is convenient to rewrite this in terms of a generic functional form for the conditional expectation,

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = f(\mathbf{Y}_{i,pre}; \theta_t) = f(\mathbf{Y}_{i,pre}; \theta_t; G_i), \quad T_0 < t \leq T, \tag{1}$$

in which $f(\cdot)$ is a flexible function of pre-treatment outcomes indexed by parameters θ_t. Later, we will see that the different procedures we propose correspond to different choices about both $f(\cdot)$ and what is being conditioned upon. The starting point that we emphasize is that the conditional expectation of Y_{it}^0 is linear in that unit's pre-treatment outcomes. We call this the *Linearity in Prior Outcomes* (LPO) assumption:

Assumption 2 (Linearity in Pre-Treatment Outcomes)

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = (1 \ \mathbf{Y}_{i,pre})^\top \theta_t, \qquad T_0 < t \leq T,$$

in which θ_t is a $(T_0 + 1)$ vector of coefficients. We index θ by t to indicate that each post-treatment time period may have a separate θ. However, we emphasize that θ_t is fixed across units i. Note that the leading term in the vector $(1 \ \mathbf{Y}_{i,pre})$ and the corresponding first element of θ allows an intercept of time t. Thus, Y_{it}^0 is *affine* in $\mathbf{Y}_{i,pre}$, but for ease of exposition we use the term *linear*.

2.2. Examples

A surprising number of approaches either explicitly posit a model of the DGP that implies LPO, or otherwise involve an estimation procedure that results in provably unbiased ATT estimates only when LPO holds. Below we provide several examples: (1) the difference-in-differences (DID) method and, relatedly, two-way fixed effect models: (2) a general structured time-series cross-sectional model; and (3) a latent factor model (LFM), which also provides a justification for the SCM.

(1) DID and two-way fixed effects and models. We start with the DID model specified in Equation (2), in which $D_{i2} = 1$ for a subset of the units and 0 otherwise; α_i and ξ_t are unit and time fixed effects and ε_{it} represents idiosyncratic shocks. In a two-period DID model, the treatment effect τ_{it} is assumed to be heterogeneous both across unit and over time. A closely related approach is the two-way fixed effect model specified in Equation (3). Note that a crucial difference between the two models is that the two-way fixed effects model estimates a single τ, unchanging across time and units.

$$(DID) \qquad Y_{it} = \tau_{it} D_{it} + \alpha_i + \xi_t + \varepsilon_{it}, \quad t = 1, 2. \tag{2}$$

$$(Two\text{-}way) \qquad Y_{it} = \tau D_{it} + \alpha_i + \xi_t + \varepsilon_{it}, \quad t = 1, \cdots, T. \tag{3}$$

In either case, $Y_{it}^0 = \alpha_i + \xi_t + \varepsilon_{it}$. In Section A.1 in the Appendix, we show that the non-treated potential outcome in the post-treatment period $Y_{it,t>T_0}^0$ can be expressed as an average of pre-treatment outcomes plus some time-specific intercept shift and a zero-mean noise.

(2) Auto-regressive model with local trends. Next, we investigate a structural time-series cross-sectional model that covers both traditional auto-regressive models and those with more complicated local, unit-specific trends, which is considered in Brodersen et al. (2015):

$$
\begin{aligned}
Y_{it} &= \tau_{it} D_{it} + \mu_{it} + \varepsilon_{it} \\
\textit{in which} \quad \mu_{it} &= \mu_{i,t-1} + \xi_{it} + \eta^{\mu}_{it} \\
\textit{and} \quad \xi_{it} &= \rho(\xi_{i,t-1} - \kappa) + \kappa + \eta^{\xi}_{it} \quad ;
\end{aligned}
$$

where the error terms ε_{it}, η^{μ}_{it}, and η^{ξ}_{it} are independently distributed and have mean zero for all i and t; μ_{it} is a local, auto-regressive trend with a changing slope ξ_{xi}; the slope of the time trend is a variation of AR(1) with a long-term slope κ when $|\rho| < 1$. When $\rho = 0$, Y^0_{it} follows a random walk process. When both ρ and κ are equal to 0, the model is reduced to a AR(1) model. Similar to the DID setup, we allow the treatment effect τ_{it} to be heterogeneous across unit and time. In this arrangement, too, the dependency of current non-treatment potential outcomes on past ones results in the LPO assumption. For details of the proof, see Secion A.1 in the Appendix.

(3) The SCM and LFMs. Less obviously, we next consider a LFM, which also justifies the SCM. This model is also called the interactive fixed effect (IFE) models in the econometrics literature:

$$
Y^0_{it} = f_t^{\top} \lambda_i + \xi_t + \varepsilon_{it}, \quad \forall i, t \; ;^{3} \tag{4}
$$

in which $\mathbb{E}[\varepsilon_{it} | f_t, \lambda_i, \xi_t] = 0$ for all i, t. The bilinear form of $f_t^{\top} \lambda_i$ gives this model two interpretations. One interpretation is that at any time t, a vector of characteristics f_t exists, and each unit takes a linear combination of these characteristics, as given by a vector λ_i. Unit i must always take combination λ_i of the characteristics in f_t while f_t can vary freely over time. The alternative interpretation reverses the emphasis of these two components: each unit i has some unknown time-invariant characteristics, λ_i; though they are time invariant, their effects on the outcome can change freely over time, according to f_t. However, all units must share the same set of coefficients f_t at a given time. This arrangement may seem quite flexible, but it turns out that it also requires Y^0_{it} in the post-treatment period to be linear in unit i's pre-treatment Y^0_{it}. Specifically, we can rewrite Equation (4) as:

[3] We omit (time-invariant) covariates for the time-being.

$$Y_{it}^0 = \sum_{s=1}^{T_0} f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} f_s Y_{is} + \left[\xi_t - \sum_{s=1}^{T_0} f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} f_s \xi_s \right]$$
$$+ \left[\varepsilon_{it} - \sum_{s=1}^{T_0} f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} f_s \varepsilon_{is} \right] \qquad \forall i, t > T_0 \ ;$$

which reveals that Y_{it}^0 is a linear combination of the pre-treatment outcome (the first term on the right hand side (RHS)), plus some time-specific intercept shift (the second term on the RHS) and a zero-mean error term (the last term on the RHS). Again, the proof is given in Section A.1 in the Appendix.

3. Proposed Method

The LPO assumption implicated in these models suggests a more general estimation procedure that sidesteps a number of practical concerns with existing approaches. In this section, we begin describing the *trajectory balancing* method first with a *mean balancing* procedure that operates under the same LPO assumption. Then, we extend the method to a kernel based feature expansion that relaxes the LPO assumption.

3.1. Trajectory Balancing with Mean Balancing Weights

We first present a simple method for estimation when the LPO is expected to hold. We refer to this method as the mean balancing procedure, because it simply implies choosing weights on the controls such that the weighted average of the outcome is equal to the (unweighted) average of the treated units at each time-point in the pre-treatment period. In other words, mean balancing seeks to find a set of the weights w such that:

$$\frac{1}{N_{tr}} \sum_{G_i=1} \mathbf{Y}_{i,pre} = \sum_{G_i=0} w_i \mathbf{Y}_{i,pre}$$

There may be many sets of weights satisfying these constraints. Methods for choosing them generally maximize some criterion subject to the mean balance conditions.[4] For example, maxi-

[4]It would be possible to instead use matching on the pre-treatment histories, using some form of distance over the entire pre-treatment vector in order to establish what control units are sufficiently close to each treated unit (e.g.

mum entropy weights maximize $\sum_i w_i log(w)$; and maximum empirical likelihood weights maximize $\sum_i log(w_i)$.[5] Such weights can only be obtained when it is feasible, implying an additional assumption:

ASSUMPTION 3 (FEASIBILITY OF BALANCING WEIGHTS) *There exists a set of non-negative weights* $\{w_i\}_{G_i=0}$ *for the control units such that* $\sum_{G_i=0} w_i = 1$ *and pre-treatment outcomes are balanced between the treatment and reweighted control groups:*

$$\frac{1}{N_{tr}} \sum_{G_i=1} Y_{it} = \sum_{G_i=0} w_i Y_{it}, \quad t = 1, 2, \cdots, T_0.$$

In the usual non-parametric setting for selection-on-observables, a "common support" assumption is also required, whereby at every value that the history of pre-treatment outcomes $\mathbf{Y}_{i,pre}$ could take, the probability of being a treated unit is greater than 0 and less than 1. In the current setup, this is not strictly necessary, and the feasibility assumption plays a similar role. The LPO assumption suggests that it is the means of $\mathbf{Y}_{i,pre}$ for the treated and controls that must be made equal, and doing so ensures equal expected non-treatment potential outcomes for the two groups.[6] This, as shown next, eliminates bias in the ATT estimate. Although common support is not required, Assumption 3 can still fail if the treated and control group are too starkly different, for example, if the treated units have more extreme values on pre-treatment outcomes than any controls.

Under Assumptions 2 and 3, these weights can be used to estimate the ATT:

ESTIMATOR 1 *The mean balancing estimator for* ATT_t *is given by:*

$$\widehat{ATT}_t = \frac{1}{N_{tr}} \sum_{G_i=1} Y_{it} - \sum_{G_i=0} w_i Y_{it}$$

where w_j *are chosen s.t.*

$$\frac{1}{N_{tr}} \sum_{G_i=1} Y_{it} = \sum_{G_i=0} w_i Y_{it}, \quad t = 1, 2, \cdots, T_0 \ ;$$

Imai, Kim and Wang 2018). Instead, here we take the weighting approach. Among other reasons, one benefit of doing so is that we can analyze the solution using the approach above, in which we first establish a feature space in which the outcomes are linear, and second establish equal means on all these features.

[5] We are agnostic as to what objective is maximized in seeking these weights and provide support for both maximum empirical likelihood and maximum entropy approaches.

[6] In other words, because the linearity assumption takes us out of the fully non-parametric setting, common support is no longer required. Instead, because we have written bases for the non-treatment potential outcomes in the post-treatment period, we simply require feasibility of obtaining equal means on these bases.

and $\sum_{G_i=0} w_i = 1$*;* $w_i \geq 0$*, for all* i *in the controls.*

PROPOSITION 1 (UNBIASEDNESS OF THE MEAN BALANCING ESTIMATOR) *Under Assumptions 1, 2, and 3, we have,*

$$\mathbb{E}[\widehat{ATT}_t | \mathbf{Y}_{1,pre}, \mathbf{Y}_{2,pre}, \cdots, \mathbf{Y}_{N,pre}] = ATT_t, \quad \forall t > T_0$$

in which $\mathbf{Y}_{1,pre}, \mathbf{Y}_{2,pre}, \cdots, \mathbf{Y}_{N,pre}$ *are pre-treatment outcome trajectories, and* $ATT_t = \frac{1}{N_{tr}} \sum_{G_i=1} \tau_{it}$*.*

Proof is straightforward and can be found in the Section A.1 of the Appendix.

A similar approach to what we have described thus far is taken by Robbins et al. (2017), where they do not consider identification or show unbiasedness with an assumption such as LPO, but do show that when the data are assumed to be generated by an LFM, the bound on the bias of a mean balancing estimator is diminishing in the number of pre-treatment periods. Furthermore, we are not aware of existing work that makes the LPO assumption of synth-like methods explicit, that describes an approximation approach and bounds on the resulting bias, or that allows for the feature expansion or kernel procedures, all of which we describe below.

Approximate balance through eigen-approximation. Assumption 3 can fail in practice. Exact weights may be infeasible in a given sample, particular when there are many moment constraints (due to many pre-treatment time periods and/or covariates) and smaller numbers of control units to work with.Thus, existing procedures for achieving balance often fail in both simulations and applied examples and investigators turn to other approaches.

To address this issue, we employ an approximate balancing procedure. Briefly, the algorithm seeks balance on the first P left singular vectors, or equivalently, the principal components of the $(N \times T_0)$ matrix $\mathbf{Y}_{pre} = (\mathbf{Y}_{1,pre}^\top, \mathbf{Y}_{2,pre}^\top, \cdots, \mathbf{Y}_{N,pre}^\top)$.[7] Moreover, under the linearity assumptions

[7] If the left singular vectors of $\mathbf{Y}_{pre}$ are given by $\mathbf{U}$, then the principle components are $\mathbf{UA}$, where $\mathbf{A}$ is the diagonal matrix containing the singular values of $\mathbf{Y}_{pre}$. That is, the principle component scores are simply rescalings of the left singular vectors, so balance can be achieved on either with the same consequence. In actuality we construct the "linear kernel" matrix, $\mathbf{Y}_{pre}(\mathbf{Y}_{pre})^\top$, an $(N \times N)$ matrix. This simplifies generalization to the cases of non-linear kernels discussed below and allows us to use the same routine, distance measures, etc. whether a linear kernel is used or some other one. For the linear kernel, balancing on $\mathbf{Y}_{pre}$ or on its linear kernel $\mathbf{K} = \mathbf{Y}_{pre}(\mathbf{Y}_{pre})^\top$ is numerically equivalent. To see this, note that both have the same rank (the column rank of $\mathbf{Y}_{pre}$) and the same left singular vectors, $\mathbf{U}$. Further, because $\mathbf{K}$ is symmetric positive definite, the left singular vectors are simply the eigenvectors. Hence, in practice we balance on the eigenvectors of $\mathbf{K}$ and interchangeably refer to this approximation as either an "eigen-approximation" or as the use of principal components.

we have already made, the worst-case bias in the ATT estimate that arises from the approximation can be bounded, and we choose P to minimize this potential for bias. In the next section, we will describe both the approximation approach and this bound at greater length, with specific reference to the kernelized version of the procedure introduced there. However, we note that the mean balancing approach is a special case of the kernelized approach, where the kernel is the linear kernel, which is simply $\mathbf{Y}_{pre}\mathbf{Y}_{pre}^{\top}$. Thus, the tools developed below for kernel balancing can be applied to mean balancing.

3.2. Relaxing the LPO Assumption: Feature Expansion Using Kernel

An important limitation of mean balancing is that the number of constraints solved by the weights is limited to T_0. With large T_0 this is less troubling. Intuitively, not only does large T_0 imply more constraints, but factors influencing Y^0 at post-treatment times will have a chance to appear in the pre-treatment period so that balancing on the pre-treatment outcomes in every period ensures that these factors are themselves well balanced. That said, even then this approach does nothing to guarantee that the trajectories of individual units look similar in the treated and control groups. For example, a control group that varies wildly around a flat line could be well mean balanced to a treated group that has all "flat" trajectories by giving equal weights to each control unit. Yet, the treated and control groups would look very different on features such as variance or volatility. If features like variance or volatility later come to have a large directional impact on Y^0, then this imbalance can generate bias. We provide a similar example in simulations below (Figure 2).

While the LPO corresponds to the mean balance procedure, relaxing the LPO assumption corresponds to an approach of seeking balance on additional, non-linear features of the pre-treatment outcomes. Consider a non-linear feature mapping $X \mapsto \phi(X)$ from $\mathbb{R}^D \mapsto \mathbb{R}^{D'}$. We are agnostic as to what this mapping is, but generally $D' \gg D$. Below, we propose a particular mapping implied by the use of a universal kernel. The key requirement of this mapping is that it enables us to claim that each unit's expected post-treatment Y^0 outcomes are approximately linear in $\phi(\mathbf{Y}^0_{i,pre})$. Hence, Assumption 2 is replaced by the following assumption:

Assumption 4 (Linearity of non-treatment outcome in $\phi(Y)$)

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = \phi(\mathbf{Y}_{i,pre})^\top \theta_t \; t > T_0;$$

in which $\mathbf{Y}_{i,pre} = (Y_{i1}, Y_{i2}, \cdots, Y_{iT_0})$*, a* $(1 \times T_0)$ *vector of pre-treatment outcomes.*

Note that Assumption 4 is also a special case of Equation (1), implied by Assumption 1. By the same logic as above, weights that achieve mean balance on $\phi(\mathbf{Y}_{i,pre})$ for the treated and control group will achieve equal means on $\phi(\mathbf{Y}_{i,pre})^\top \theta_t$ for the treated and control groups, regardless of θ_t. Thus, as LPO motivated mean balance in the original data ($\mathbf{Y}_{i,pre}$), Assumption 4 motivates mean balance on the features $\phi(\mathbf{Y}_{i,pre})$.

The choice of $\phi(\cdot)$ is clearly critical to the credibility of Assumption 4. We postpone this choice to the discussion of kernels below, as kernels will give us the ability to choose high- or infinite-dimensional choices of ϕ with desirable properties. For analytical purposes, we begin by again requiring a feasibility assumption,

Assumption 5 (Feasibility of ϕ-balance) *There exist a set of non-negative weights* $\{w_i\}_{G_i=0}$ *for the control units such that* $\sum_{G_i=0} w_i = 1$ *and pre-treatment outcomes are balanced between the treatment and reweighted control groups:*

$$\frac{1}{N_{tr}} \sum_{G_i=1} \phi(\mathbf{Y}_{i,pre}) = \sum_{G_i=0} w_i \phi(\mathbf{Y}_{i,pre}).$$

As noted above, this feasibility assumption supplants the more usual common support assumption. As $\phi(\cdot)$ is higher dimensional, this transformation makes feasibility easier to violate, which makes the approximation approach even more necessary. If, for example, the treated units are sufficiently different from the controls that the $\phi(\mathbf{Y})$ describing them on average lies outside the span of the $\phi(\mathbf{Y})$ of the control units, then the weights will be infeasible.

We now suggest the kernel-based variant of trajectory balancing under Assumption 4. It employs the same mean balancing idea as above, but with weights that obtain mean balance on $\phi(\mathbf{Y})$ between the treated and control units. The estimator is unbiased for ATT_t under Assumptions 4 and 5,

ESTIMATOR 2 *The kernal balancing estimator for ATT_t is given by:*

$$\widehat{ATT}_t^k = \frac{1}{N_{tr}} \sum_{G_i=1} Y_{it} - \sum_{G_i=0} w_i Y_{it}$$

where w_i are chosen s.t.

$$\frac{1}{N_{tr}} \sum_{G_i=1} \phi(\mathbf{Y}_{i,pre}) = \sum_{G_i=0} w_i \phi(\mathbf{Y}_{i,pre})$$

and $\sum_{G_i=0} w_i = 1$; $w_i > 0$, for all i in the controls.

PROPOSITION 2 (UNBIASEDNESS OF THE KERNAL BALANCING ESTIMATOR) *Under Assumptions 1, 4, and 5, we have,*

$$\mathbb{E}[\widehat{ATT}_t^k | \mathbf{Y}_{1,pre}, \mathbf{Y}_{2,pre}, \cdots, \mathbf{Y}_{N,pre}] = ATT_t, \quad \forall t > T_0$$

in which $\mathbf{Y}_{1,pre}, \mathbf{Y}_{2,pre}, \cdots, \mathbf{Y}_{N,pre}$ are pre-treatment outcome trajectories, and $ATT_t^k = \frac{1}{N_{tr}} \sum_{G_i=1} \tau_{it}$.

We omit the proof as it is very similar to that of Proposition 1. While both the mean balancing and kernel balancing procedures will attempt to make the average trajectory of control units match that of treated units in the pre-treatment period, the latter is more fully in keeping with the name "trajectory balancing." This is because the higher-order representation of the history of pre-treatment outcome given by $\phi(\mathbf{Y}_{pre})$ allows us to go beyond balancing on the average trajectory, ensuring that the control units looks more similar to the treated units in their trajectories receive higher weights. We demonstrate how such a step improves higher-order comparability of the chosen controls to the treated in simulations and applied examples below.

Kernel-based choice of ϕ. We now briefly discuss the kernel based approach that effectively chooses $\phi(\cdot)$ and determines the weights. The basis for this method is kernel balancing (`kbal`, Hazlett 2018). While a much more technical discussion of kernels is possible, for present purposes, we can pose them simply as functions that assess similarity in some sense. Let $k(Y_i, Y_j)$ be a function from $\mathbb{R}^{T_0} \times \mathbb{R}^{T_0} \mapsto \mathbb{R}$ that measures the similarity of Y_i and Y_j. With N observations, an $(N \times N)$ kernel matrix $\mathbf{K}$ can be constructed such that $K_{i,j} = k(Y_i, Y_j)$. Note that row i of $\mathbf{K}$, which we will designate K_i has the form $[k(Y_i, Y_1), k(Y_i, Y_2), \ldots, k(Y_i, Y_N)]$. The simplest view is one that regards $\phi(\mathbf{Y}_i^{pre})$ as simply K_i, or in matrix form, our feature expansion simply replaces $\mathbf{Y}^{pre}$ with $\mathbf{K}$. Thus,

for each unit i, we are proposing to replace the original T_0-dimensional sequence of pre-treatment outcomes with a new, N-dimensional feature vector, K_i. This K_i encodes how similar unit i is to unit 1, unit 2, and so on, making it a very rich representation of the data.

In theory, we would like to find weights that make the weighted average of K_i among the controls equal to the unweighted average of T_i among the treated,

$$\frac{1}{N_{tr}} \sum_{G_i=1} K_i = \sum_{G_i=0} w_i K_i \ s.t. \ \sum_{G_i=0} w_i = 1, \ w_i > 0, \ \forall i, G_i = 0 \tag{5}$$

though we will relax this requirement with an approximation momentarily. While many choices of the kernel function $k(\cdot, \cdot)$ are possible, here we use the Gaussian kernel, the workhorse kernel in machine learning,

$$k(Y_i, Y_j) = exp(-||Y_i - Y_j||^2/h)$$

where $||Y_i - Y_j||$ is the Euclidean distance. It can be shown that all the functions that are linear in K_i are also linear in an infinite-dimensional feature expansion, one for which is $\langle \phi(Y_i), \phi(Y_j) \rangle = k(Y_i, Y_j)$, and that as $N \to \infty$, this space of functions contains all continuous functions.

Approximate balance on K. As exact balance on all N dimensions of $\mathbf{K}$ is typically infeasible, we instead seek approximate balance. The approach is identical to that briefly described regarding the linear kernel and mean balancing above, but we describe it at greater length here now that the kernel matrix has been introduced. The basic idea is to achieve approximate balance while minimizing the (worst-case) bias due to this approximation: (1) take the eigenvectors of $\mathbf{K}$ based on singular value decomposition (SVD), and (2) achieve balance on the first P eigenvectors, leaving those whose eigenvalues rank $P+1$ to N unbalanced, where (3) the value of P is chosen to minimize the "worst-case" bias that could arise due to remaining imbalances.

The needed bound on the bias is derived as follows. Recalling Assumption 4 and replacing $\phi(\mathbf{Y}_i^{pre})$ with K_i, we have:

$$\mathbb{E}[\mathbf{Y}_i | \mathbf{Y}_i^{pre}] = \mathbf{K}c = \mathbf{V}\mathbf{A}\mathbf{V}^\top c = \mathbf{V}d$$

where c is a set of coefficient on $\mathbf{K}$ that exist due to the linearity assumption; $\mathbf{V}$ is the matrix of eigenvectors of $\mathbf{K}$, $\mathbf{A}$ is the matrix whose diagonal contains the eigenvalues of $\mathbf{K}$, and $d = \mathbf{A}\mathbf{V}^\top c$.

Also note that the (reproducing kernel Hilbert space) norm for the chosen function is given by $c^\top \mathbf{K} c = d^\top \mathbf{A}^{-1}\mathbf{V} d$. In conceptual terms, this norm describes how complicated or "wiggly" this function is, which will be a quantity that we control in constructing the actual bias bound, but which will remain constant in a given analysis and need not be estimated in order to choose the number of dimensions P to balance on.

Let $\mathbf{V}_1$ be rows of $\mathbf{V}$ corresponding to treated units, and $\mathbf{V}_0$ the rows of $\mathbf{V}$ corresponding to control units. Suppose then we choose the vector of weights w_0 on the control units, and w_1 on treated units. Because we target the ATT, every element of w_1 is set equal to 1 over the number of treated units. The bias of the ATT due to approximation, denoted $bias_w$, is then

$$\begin{aligned} bias_w &= \mathbb{E}[Y^0|G_i = 1] - \mathbb{E}[Y^0|G_i = 0] \\ &= (w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)d \\ &= (w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)\mathbf{A}\mathbf{V}^\top c \end{aligned}$$

Note that the first term, $(w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)$, is the imbalance on the eigenvectors under a given weighting scheme, and the second term, $\mathbf{A}\mathbf{V}^\top c$, determines how much each imbalance matters towards producing bias. To obtain a worst-case bound on this bias when we do not know c (or d), we must instead control some related quantity. We impose control over only the Hilbert norm of the regression function, $c^\top \mathbf{K} c$, as this controls how wildly the regression function is allowed to vary. Suppose we restrict the function to those with norm $c^\top \mathbf{K} c \leq \gamma$. It is only if one desires an actualy numerical estimate of the worst-case bias that a choice of γ would be required.[8] We are then interested in the worst-case bias due to the approximation, *bias bound*, given by

$$bias\ bound = \sup_{c^\top \mathbf{K} c \leq \gamma} |(w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)\mathbf{A}\mathbf{V}^\top c|$$

Letting $z = c^\top \mathbf{K}^{1/2}\gamma^{-1}$, the above can be rewritten as

$$\sqrt{\gamma} \sup_{z^\top z \leq 1} |(w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)\mathbf{A}\mathbf{V}^\top \mathbf{K}^{-1/2} z|$$

[8] A reasonable choice of γ can be made, for example by actually regressing the outcome among control units on their pre-treatment outcomes, using the same kernelized specification (e.g. using Kernel Regularized Least Squares, Hainmueller and Hazlett 2014).

which by Cauchy-Schwarz gives

$$\begin{aligned} \textit{bias bound} &\leq \sqrt{\gamma}||(w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)\mathbf{A}\mathbf{V}^\top \mathbf{K}^{-1/2}||_2 \\ &\leq \sqrt{\gamma}||(w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)\mathbf{A}^{1/2}||_2 \end{aligned}$$

The form of this worst-case bound is informative. First, the L_2 norm of the regression function ($\sqrt{\gamma}$) controls the overall scale of potential bias. Second, the imbalance on the eigenvectors of $\mathbf{K}$ after weighting, $(w_1^\top \mathbf{V}_1 - w_0^\top \mathbf{V}_0)$, enters directly. Third and most illuminating, the impact of imbalance on each eigenvector is scaled by the square root of the corresponding eigenvalue. This suggests that our approach of achieving balance on the first P eigenvectors is a reasonable strategy for quickly minimizing worst case bias. Because the matrix $\mathbf{K}$ typically has a few large eigenvalues, then many very small ones, it is usually possible to achieve fine balance on enough eigenvectors such that the remaining eigenvalues carry a tiny fraction of the total variation in $\mathbf{K}$.

This bound is effectively a measure of remaining imbalance on $\mathbf{K}$ as it can impact ATT estimates, and we use it as the target of optimization when choosing how many eigenvectors to seek balance on. For optimization purposes, γ does not vary, hence, it can be ignored. The procedure thus chooses P so as to minimize the worst-case bias in the ATT that remains due to the approximate nature of balancing, regardless of the true norm of the regression function or the actual coefficient values.

3.3. Extensions

We discuss two extensions to the trajectory balancing approach. First, we incorporate time-varying pre-treatment covariates in a balancing procedure. Second, we allow an intercept shift prior to balancing.

Pre-treatment covariates. Consistent with other work (e.g., ADH 2010), we consider the possibility that users would like to explicitly ensure balance on pre-treatment, time-invariant covariates as well. The argument for including covariates should typically be that they account for potential confounders; hence, the identifying assumption is more credible after their inclusion. Denote X_i

as a vector of time-invariant covariates for unit i. Assumption 1 (conditional ignorability) then becomes:

$$Y_{it}^0 \perp\!\!\!\perp G_i | X_i, \mathbf{Y}_{i,pre}, \quad \forall t > T_0,$$

which implies $\mathbb{E}[Y_{it}^0 | X_i, \mathbf{Y}_{i,pre}] = f(X_i, \mathbf{Y}_{i,pre}; \theta_t)$, for all $t > T_0$. Therefore, if we add X_i to the $\mathbf{Y}_{i,pre}$ being conditioned upon in Assumptions 2–5 and in both the mean balancing and kernel balancing estimators, the unbiasedness property follows.

With the kernel estimator, by including both the covariates and the pre-treatment outcomes in $\phi(\cdot)$, this feature expansion includes the full range of interactions between the two. In practice, it means that the kernel matrix $\mathbf{K}$ is formulated using vectors $[\tilde{X}_i, Y_i]$ as arguments to the Gaussian kernel in Equation (3.2), where $\tilde{X}_i$ is a rescaled version of the covariates with mean zero and variance one. Again, weights are chosen to achieve mean balance on the rows, K_i, as in Equation (5). Approximate mean balance on this form of K_i can be thought of as ensuring that the joint-distribution of covariates and pre-treatment outcomes is approximately balanced.

In general, adding covariates will increase dimensionality, making balance more difficult to achieve. Moreover, if any treated units take values on the covariates more extreme than that of control units, it can make even the best available weights perform poorly. In the first empirical example below (Truex 2014), this is not an issue in the observed data, though through bootstrap repetitions we notice that it could easily become an issue had key control units not been sampled. The addition of covariates makes effective weights more difficult to find in the second example (ADH 2010), which we use to motivate a demeaning procedure to which we now turn.

Intercept shift and demeaning. When the number of control units is small or the outcome trajectories of the treated units do not lie in the convex hull of those of the control units, finding a set of weights that significantly reduce the imbalance between the treatment and control groups can be difficult. Differences in the "intercepts" or overall level of units contribute heavily to such problems. Feasibility (Assumption 5) can thus fail.

One option to alleviate this problem is to focus on the changes or dynamics only, and remove considerations owing to level – i.e. allow an "intercept shift" for each unit. Specifically, before

reweighting, the average outcome from period 1 to period T_0 is subtracted from the original outcome for each unit, ensuring mean zero outcomes in the pre-treatment period for each unit.

While making feasible weights easier to find, this comes with at the cost of an invariance assumptino that may or may not be palatable. First, the conditional ignorability assumption (Assumption 1) needs to be replaced by the following "parallel trends" assumption:

Assumption 6 (Parallel Trends)

$$\mathbb{E}[Y_{it}^0 - Y_{is}^0 | \dot{\mathbf{Y}}_{i,pre}] = \mathbb{E}[Y_{it}^0 - Y_{is}^0 | \dot{\mathbf{Y}}_{i,pre}, G_i], \quad \forall\ t, s \in \{1, 2, \cdots, T\}$$

where $\dot{\mathbf{Y}}_{i,pre} = (\dot{Y}_{i1}, \dot{Y}_{i2}, \cdots, \dot{Y}_{iT_0})$ *is a* $(1 \times T_0)$ *vector demeaned pre-treatment outcomes, in which* $\dot{Y}_{it} = Y_{it} - \bar{Y}_{i,pre}$ *and* $\bar{Y}_{i,pre} = \sum_{t=1}^{T_0} Y_{it}/T_0$.

Assumption 6 says that, once the demeaned pre-treatment dynamics are being conditioned on, the average Y_0 of the treated units would follow a parallel path of that of control units. This assumption is analogous to the "parallel trends" assumption in the two-period DID framework (Abadie 2005). Assumption 6 implies that:

$$\mathbb{E}[\dot{Y}_{it}^0 | \dot{\mathbf{Y}}_{i,pre}] = f(\dot{\mathbf{Y}}_{i,pre}; \theta_t), \text{ for all } t > T_0.$$

Hence, we replace Assumption 4 with: $\mathbb{E}[\dot{Y}_{it}^0 | \dot{\mathbf{Y}}_{i,pre}] = \phi(\dot{\mathbf{Y}}_{i,pre})^\top \theta_t$.

This is effectively an invariance assumption: any two pre-treatment trajectories that vary only by a vertical translation (an intercept shift) are assumed to carry the same information. This may be defensible only in some circumstances. [9] However, commonly used methods such as DID and fixed effects models, do impose such an invariance to vertical shifts (when no non-linear transformation of the outcome is attempted). With DID, for example, only within-unit changes matter; with unit fixed effects, each unit receives it's own independent intercept shift, with the same consequence. Doudchenko and Imbens (2016) considers forbidding intercept shifts an unnecessary restriction under many circumstances.

[9] For example, a country's annual GDP growth rate falls from 7% to 6% (an absolute change of 1 percentage point) is likely very different in kind from one that drops from 2% to 1%, with the same absolute percentage point change. To treat them as similar by allowing vertical translations thus may create inappropriate counterfactuals. Therefore, whenever it is possible, we prefer not to impose this additional assumption.

3.4. Quantifying Uncertainty

The question of variance estimation under this procedure remains open to further research. When the number of treated units is very small or one as in many applications of the SCM, then the treated outcome has no well defined variance. While it may be possible to construct standard errors on the (weighted) average outcome among the controls (see below), the treated unit's outcome has no standard error or confidence interval, and thus the uncertainty of the ATT is not defined in the conventional sense. In such cases investigators may avoid classical inference altogether, and instead seek to estimate a large number of placebo tests in which effects are estimated for units that are known not to have a treatment effect (because they did not receive treatment), generateing a null distribution for the estimates obtained when there is no effect. The estimated effect can then be compared to this null. For example, treatment can be reassigned to different units, much as in Fisherian permutation inference.[10] Such approaches can be pursued with this method as well, though it can be computationally expensive.

The trajectory balancing method introduced in this paper tolerates many more treated units than the original SCM, opening the door to inference on the ATT estimates when numerous treated units are available. One approach would be to regard the weights as fixed and apply them as a pre-processing step. Any average taken with these weights can then be given the usual weighted standard error. However, the user may also reasonably worry about the uncertainty due to the choice of weights. This is similar to the challenges faced by users of inverse propensity score weighted estimators: the weights are themselves uncertain and would vary if one contemplates a re-sampling experiments. This makes the standard (weighted) variance estimates problematic, along with sandwich-type robust estimators, as neither account for this uncertainty. As found in Austin (2016), naive and robust standard error estimates are thus biased, whereas the bootstrap correctly accounts for uncertainty in the weights and provides reasonable results. We similarly propose a bootstrap procedure in order to incoroporate uncertainty due to estimation of the weights. While a closed-form solution to this problem would be highly desirable, but we are not yet aware of such an option. Specifically we propose,

[10] See, for example, ADH2010 and more recently Robbins et al. (2017).

1. Re-sample both the treatment and control group with replacement, preserving the time-series of each unit. N_{tr} and N_{co} are kept as fixed.[11]
2. Estimate the weights, allowing all the same sources of variability involved in the initial estimate (e.g. allowing the number of dimensions chosen by the approximation routine to vary as it is data-driven)
3. Estimate the desired target quantity, such as the ATT at a given time period or an average of ATTs over the post-treatment periods
4. Use the variation in the estimated quantities across trials to construct confidence intervals, either by directly examining percentiles or taking the standard deviation (error) and applying normal theory.

Note that it is easiest to think of the data in "wide" format, where each unit takes a row and the outcomes at different times are spread across columns, as are any covariates. In this format, the bootstrap is a simple one: each row in the treatment group will have the same probability of being sampled with replacement; so is each row in the control group. If one thinks instead of a "long" format, in which a given unit takes multiple rows representing each time point, then this would amount to a "block bootstrap" on the unit level. The validity of the above procedure requires further investigation, and ideally can be replaced by a faster, potentially closed-form alternative.

4. A Simulated Example

In this section, we provide a simulated example to illustrate situations in which the kernel balancing method may outperform mean balancing. We focus on the advantage of using the kernel method in terms of ensuring balance on higher-order features with fewer pre-treatment periods.

The main shortcoming of mean balancing, in intuitive terms, is that there may be many ways to average together the trajectories of control units to produce a weighted average that "looks like" that of the treated at each time point, but these solutions can give substantial weight to units whose trajectories do not look like those of the treated. Because $\phi(\mathbf{Y}_i^{pre})$ encodes higher-order features such as "curvature" and "low frequency oscillation" in the history of pre-treatment outcome, obtaining balance on $\phi(\mathbf{Y}_i^{pre})$ ensures similarity in these qualities.

Consider a dataset of $N = 200$ countries of 24 years $T \in \{1, 2, ..., 24\}$, and a simulated outcome

we label as GDP. Further, we imagine two "types" of countries in this stylized example. Suppose that 100 of these countries have an outcome that is volatile, with cyclical noise, and no long-term growth, i.e.,

$$GDP_{it} = 5 + a_i \sin(.2\pi t) + b_i \cos(.2\pi t) + .1\varepsilon_{it}$$
$$\varepsilon_{it} \sim N(0,1), \quad a_i, b_i \sim U(-1,1)$$

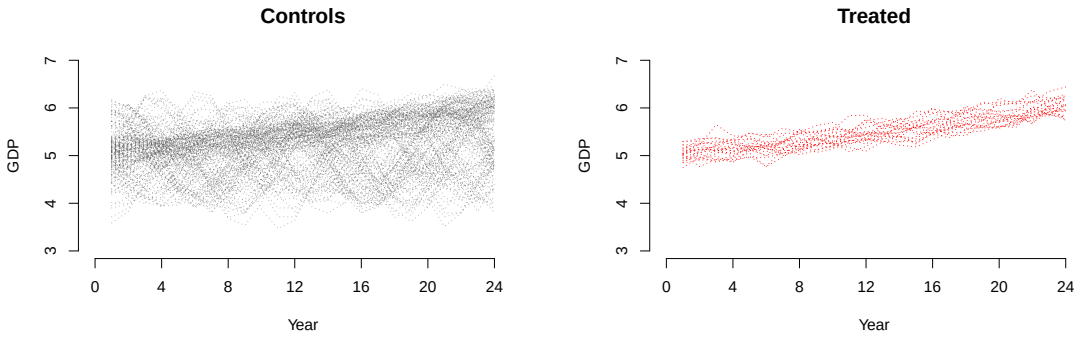
These outcomes are thus periodic, but with a phase that differs between units depending on a_i and b_i. Some countries may have low values of both a_i and b_i, producing more stable outcomes, but not steady growth. The remaining 100 countries have slow-but-steady growth at a rate of 4% per year:

$$GDP_{it} = 4 + c_i 1.04^t + .1\varepsilon_{it}$$
$$\varepsilon_{it} \sim N(0,1), \quad c_i \sim U(0.9, 1.1)$$

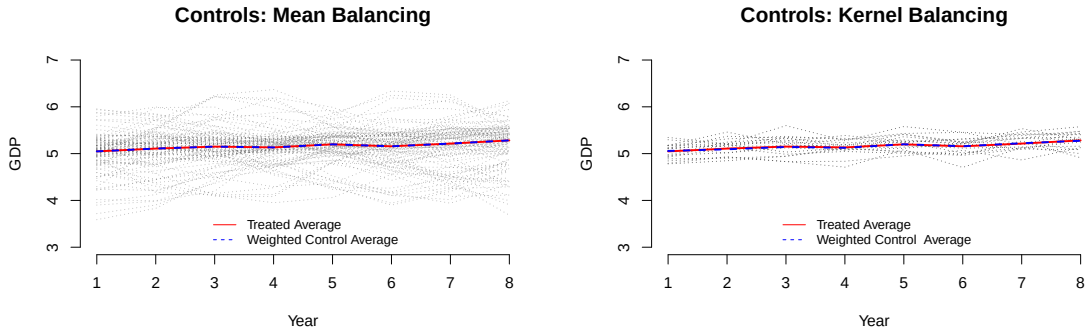
We note that this is a case of an IFE with three factors: each country takes some linear combination of the sine, cosine, and steady growth functions. Finally, assume that a treatment (e.g. the onset of some public policy) begins at a certain time. It is assigned only to stable countries, with one quarter of them assigned to treatment at random. For simplicity, let us assume the treatment effect is exactly zero, and so the values of GDP represent both the treatment and non-treatment potential outcomes. As a result, if we find a good counterfactual for the treated by weighting the control units properly, it should match exactly the post-treatment outcomes of the treated units. We will know our solution generates bias if we see an apparent effect in the post-treatment period. Figure 1(a) plots the raw outcome trajectories of the control and treated units.

We use both mean balancing and kernel balancing to find weights for the controls. To illustrate the difference of the two methods, let us assume that the treatment start in Year 9. Hence, we take the first 8 periods as the pre-treatment period and use the outcomes from Year 1 to Year 8 as inputs for both methods. Figure 1(b) shows the trajectories of the most heavily-weighted control units (those accounting for 90% of the total weight), as selected by mean balancing and kernel balancing in the pre-treatment period. Mean balancing finds a good match on the pre-treatment

FIGURE 1. RAW DATA AND HEAVILY WEIGHTED CONTROLS
SIMULATED EXAMPLE



(a) Raw data



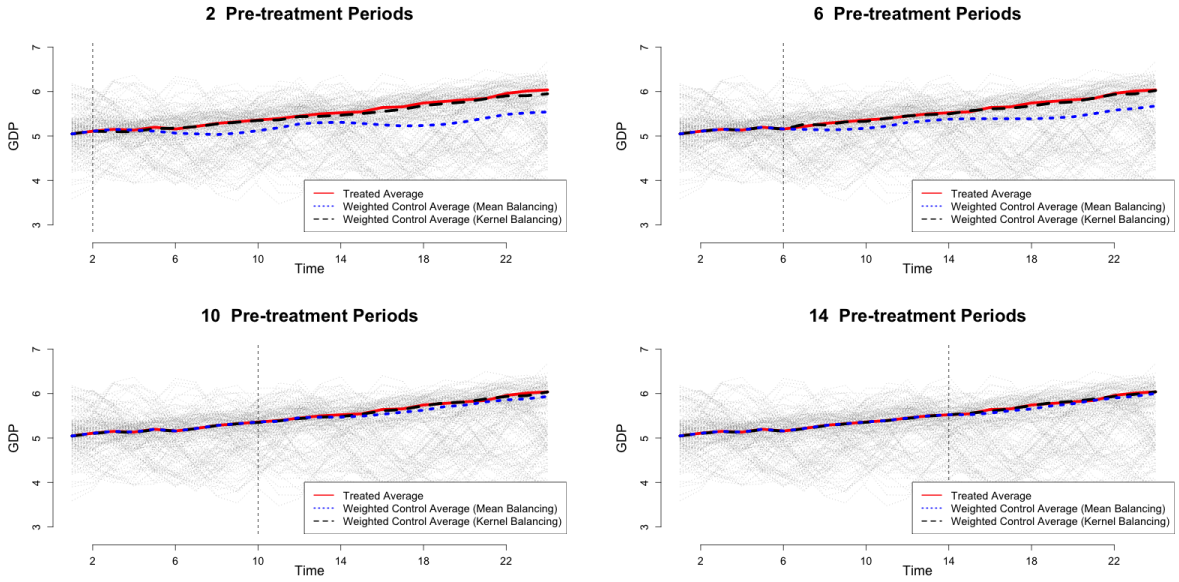
(b) Weighted controls in the pre-treatment period

Note: The upper panel shows the raw outcome data of the control and treated units. The lower panel shows heavily weighted controls chosen by mean balancing and kernel balancing. The *bottom-left* panel shows the pre-treatment time trends for the control units that receive the top 90% of the total weight, under *mean balancing*. Many high-volatility type units are included, in addition to some more stable-growing units. The *bottom-right* panel shows the pre-treatment time trends for the most heavily weighted controls chosen by *kernel balancing*, also accounting for 90% of the total weight. This approach weights almost exclusively the stable-growing units, which are more similar to the treated units on features such as variance or frequency content.

period, but does so by spreading its weight among a combination of volatile and stable-growing types of countries. By contrast, kernel balancing emphasizes almost entirely stable-growing types of countries exactly because it takes into account higher order features.

Next, we investigate the consequences of these weighting choices. To illustrate the benefits of using the kernel method, we consider three scenarios that differ only in the amount of available pre-treatment data: 2 years, 6 years, 10 years, or 14 years. Figure 2 shows balance on pre- and post-treatment periods for each. In all 4 cases, as expected, both methods can achieve good mean

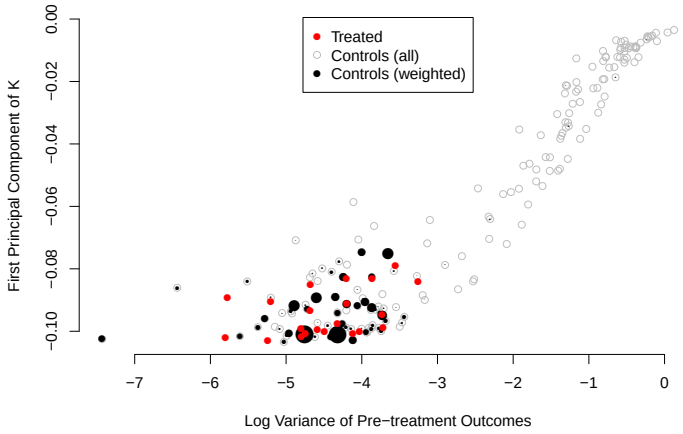
FIGURE 2. MEAN BALANCING VS. KERNEL BALANCING: A SIMULATED EXAMPLE



Note: Pre- and post-treatment trends as determined by mean balancing and kernel balancing, varying the number of pre-treatment periods available. The true treatment effect is zero in every post-treatment period, hence an unbiased estimator should show no "gap" between the treatment and constructed control outcomes. In each panel, the solid red line shows the average GDP of the treated units. Under mean balancing, the weighted average of the controls (dashed blue line) is an excellent match on the pre-treatment trends of that of the treated. However, when the number of pre-treatment periods is small, this does not imply a good prediction for the treated counterfactual average in the post-treatment period. By contrast, using kernel balancing, the weighted control average (dashed black line) follows the treated average closely in the post-treatment period even when the number of pre-treatment periods is as small as 2. Only when the number of pre-treatment periods grows to approximately 14 (*bottom right*) does the mean balancing approach almost fully catch up.

balance on the pre-treatment trends. However, when there are few pre-treatment periods, mean balancing fails to predict the average trajectory of the treated units because it chooses relatively equal weights on control units, giving too much weight to the high volatility units. As a result, the growth experienced by the treated units is lost in the noise. The number of constraints solved by the balancing procedure is too small to produce an average counterfactual that captures that growth of the treated. However, the kernel based approach correctly selects the low-volatility units that are more similar to the treated units. This is because it solves for more constraints and ensure balance on higher order features of the pre-treatment outcome. As a result, the predicted counterfactual remains well matched to the average of treated units in the post-treatment period. As the number of periods grows, kernel balancing maintains its performance while mean balancing continues to select control units poorly until approximately 14 time periods are available (bottom-right panel).

FIGURE 3. PRE-TREATMENT VARIANCE AND THE FIRST COMPONENT OF **K**



Note: Analysis of first component of **K**. Empty circles show control units, whose pattern reveals a strong relationship between the log of variance of each country's pre-treatment outcomes and its value on the first eigenvector or component of **K**. Filled red circles show the location of the treated units. The filled black circles again show the control units, but sized according to the weight that trajectory balance assigns them. The choice of weights focuses heavily on control units similar to the treated on variance, and thus on the first component of **K**.

Finally, for insight into what is being balanced upon, we examine the principal components of the kernel matrix when there are 8 pre-treatment periods. An important feature of the outcome variable in each unit's pre-treatment period is its volatility. Figure 3 shows that the first principal component of **K** is highly informative as to the variance in each country's pre-treatment history. The horizontal axis gives each country's log of variance in pre-treatment outcomes, and the vertical axis is each country's value on the first principal component of **K**. The empty gray circles show these values for each control unit and the red filled circles represent the treated. Furthermore, the filled black circles of varying size show the location of control units again, but scaled by the weights found by kernel balancing. Figure 3 shows that these weights are largest in the low-variance region occupied by the treated units, indicating that the method properly upweights those units whose variance are similar to that of the treated and that almost no weight is given to units further afield. Note that variance is a non-linear function of the pre-treatment history, and hence, not automatically balanced on by simply achieving mean balance.

5. Empirical Examples

In this section, we demonstrate the applicability of trajectory balancing to several empirical examples. We explore two examples that have previously been analyzed using entropy balancing and the synthetic control method, Truex (2014) and ADH (2010). They not only employ different estimation strategies but also vary in important features such as the number of pre-treatment periods and the numbers of treated and control units.

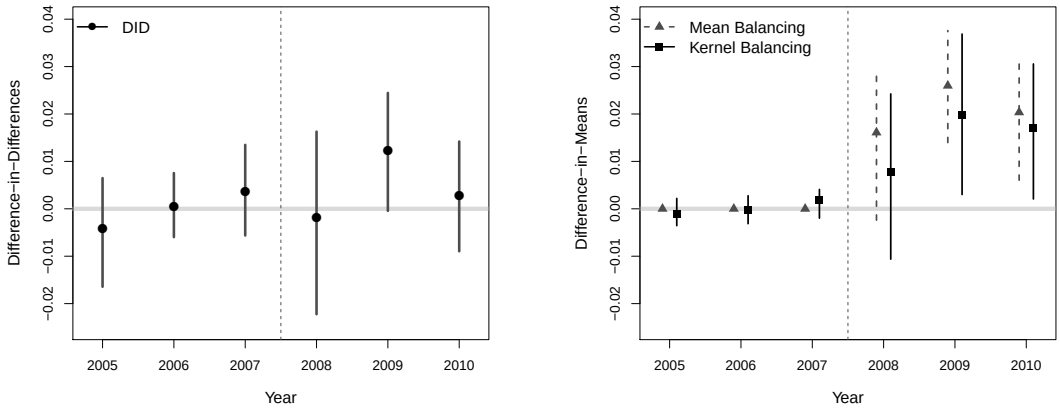
5.1. Return to Office in China's National People's Congress (Truex 2014)

First, we investigate the data from Truex (2014), who studied the return to office of China's "rubber-stamp" parliament, the National People's Congress (NPC), using a firm-level annual dataset spanning from 2005 to 2010. There are 48 treated firms, whose CEOs started to hold seats in the NPC in 2008, and 948 untreated firms, whose CEOs never held seats in the NPC during this time window. Hence, the first three periods (2005–2007) are pre-treatment. The author uses entropy balancing to achieve equal means on the three pre-treatment outcomes and two time-invariant covariates – share of state ownership and each firm's total revenue in 2007. This is effectively the mean balancing approach as described here, though Truex (2014) then runs a fixed effects model on the weighted sample with weights taken as fixed to estimate the standard errors. He finds that a seat in the NPC occupied by a company's CEO will lead to an increase in return to assets (ROA) of 3 to 4 percentage points.

We re-examine the data in three ways, with results visualized in Figure 4. First, we employ a DID approach, as this is often the expected procedure in such circumstances. We do so by taking the difference in mean outcomes between the treated and untreated groups in the pre-treatment period, and subtract this value from the mean treated group outcomes in every period. Plotting these differences in the left panel of Figure 4 reveals the implied imbalances in outcomes in the pre-treatment periods (which average to zero by construction), and the DID treatment effect estimate that would be obtained in each post-treatment period.

Next, we employ the mean balancing approach and kernel balancing approaches.[12] As in the original paper, we include all three pre-treatment outcome measures and the two covariates. The right panel of Figure 4 shows the ATT estimates for the effect of an NPC seat on ROA, together with confidence intervals constructed by resampling the data with replacement and re-estimation of the entire procedure. The ATT estimates based on the two reweighting methods turn out to be very similar to each other. Using mean balancing, we find an estimated 2.1 (95% CI: 0.9, 3.2) percentage point increase in ROA averaged over the three years after a firm's CEO takes a seat in the NPC. The estimate is 1.6 percentage points (95% CI: 0.3, 2.6) using kernel balancing, suggesting that the original finding is robust to adjusting for higher-order features in the outcome trajectories.

FIGURE 4. RETURN TO OFFICE OF CHINA'S NATIONAL PEOPLE'S CONGRESS



(a) DID (b) Trajectory Balancing

Note: Results for re-analysis of Truex (2014). *Left:* Simple DID estimates. *Right:* results using both the mean balancing and kernel balancing, which seek balance on the three pre-treatment outcomes (from 2005 to 2007) and two covariates, state ownership and revenue in 2007 (`rev2007`). Note that a single treated firm, whose `rev2007` is 81105 million RMB (while the maximum of `rev2007` in the control group is 76180 million RMB) is dropped from the sample due to incomparability in some resamples; see Appendix A1. The DID estimates show poor balance on pre-treatment outcomes in separate periods, and produce results that are more erratic and largely indistinguishable from zero. By contrast, the kernel balancing and mean balancing produce similar results to each other, show good balance on pre-treatment outcomes in each period, and suggest a statistically significant positive treatment effect at two years, three years, and when averageing over all three post-treatment years .

We also observe from Figure 4 that, with mean balancing, the confidence intervals in the pre-treatment period are reduced to the three point estimates. This is because with each bootstrap run, the algorithm is able to achieve exact mean balance on the included variables. In contrast,

[12] We do not compare the result to SCM, because it is not an ideal option for two reasons: (1) the algorithm will have to be repeatedly run for each treated unit; (2) the small number of pre-treatment periods prevents the algorithm from measuring the quality of matches.

with kernel balancing, the confidence intervals on the pre-treatment estimates signal that some proportion of resampled estimates do not find close balance on the pre-treatment outcomes. In general it is the case that mean balance will not be *exact* under kernel balancing because the moment conditions solved are different and must also attain balance on higher-order functions. Critically, the results shown for kernel balancing here are those after we removed a single treated unit. This firm had revenue in 2007 (`rev2007`) greater than that of any other unit at 81105 million RMB, and only one control unit came close (76180 million RMB). Thus, even though good balance and performance could be achieved on the kernel in the original full sample, on bootstrap resamples that lacked this one control unit, feasible balance could not be found, leaving wide imbalances. We show results with all units in Appendix A1. When such imbalances are found, they are informative and should lead investigators to determine what treated units are difficult to balance. See our proposed workflow and guidelines in Section 2.

For insights into how these two reweighting schemes differ, we present Table 1, a balance table using both methods. It shows the differences between the treatment and control groups in the pre-treatment outcomes and covariates before and after reweighting using both mean balancing and kernel balancing. We see that the mean balancing approach is able to achieve exact balance in this case on the pre-treatment outcomes and covariates, while a small amount of mean imbalance remains with kernel balancing. However, kernel balancing also achieves approximate balance on other functions of the pre-treatment data. Graphically, in Figure 5, we plot outcome trajectories

Table 1. Mean Balancing vs. Kernel Balancing: Balance Table

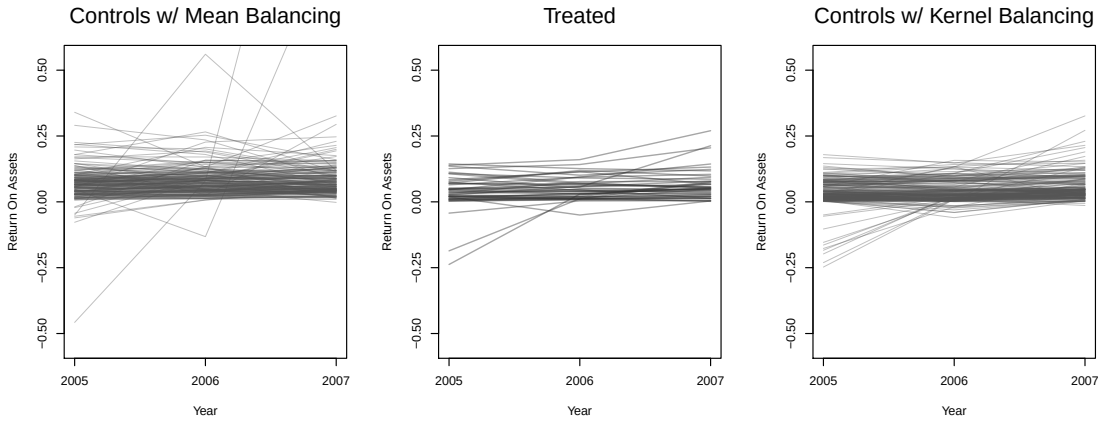
	Treated		Unweighted Controls			Reweighted Controls w/ Mean Balancing			Reweighted Controls w/ Kernel Balancing		
	Mean	SD	Mean	SD	δ	Mean	SD	δ	Mean	SD	δ
ROA in 2005	0.038	0.067	0.022	0.080	0.151	0.038	0.078	0.000	0.040	0.067	-0.016
ROA in 2006	0.051	0.042	0.030	0.076	0.231	0.051	0.066	0.000	0.052	0.046	-0.025
ROA in 2007	0.064	0.056	0.041	0.103	0.196	0.064	0.160	0.000	0.064	0.056	0.004
State Ownership	0.325	0.244	0.249	0.222	0.231	0.325	0.237	0.000	0.323	0.240	0.007
Revenue in 2007	7.976	14.333	2.647	5.531	0.000	7.976	16.881	0.000	7.890	13.609	0.000

Note: The above table shows the means, standard deviations (SD) of the treated units, the unweighted control units, the reweighted control units using both mean balancing and kernel balancing, as well as the standardized differences (δ) between the treated units and unweighted/reweighed control units.

of the control units that receives heavy weights from each method and compare them with those of the treated units. The left and right panels of Figure 5 are the outcome trajectories of the

top 25% most highly weighted control units using mean balancing (left) and trajectory balancing (right), respectively, while the panel in the middle shows the outcome trajectories of the 48 treated units. Figure 5 shows that, as in the simulation example, trajectory balancing utilizes information of higher order transformations of the pre-treatment outcomes such that the outcome trajectory in the treated and reweighted controls are similar in distribution while mean balancing only ensures balance in means of the outcome measures at each time point. Though this has little impact on the final ATT estimate in this example, the greater similarity of the reweighted controls to the treated units in the pre-treatment period lends greater credibility to the belief that the constructed counterfactual will correctly predict the non-treatment potential outcomes of the treated unit in the post-treatment period—even if the post-treatment outcomes do not happen to be linear in the pre-treatment outcomes.

FIGURE 5. PRE-TREATMENT TRAJECTORIES OF TREATED AND HEAVILY WEIGHTED CONTROLS



Note: The above plots show the pre-treatment outcome trajectories for the treated units (middle) and top 25% most highly weighted controls using mean balancing (left) and kernel balancing (right). Data are from Truex (2014).

5.2. Tobacco Control Program and Cigarette Sales (ADH 2010)

Finally, we apply the trajectory balancing approach to a classic example of the SCM (ADH 2010). The authors use U.S. state level data to examine the effect of Proposition 99, a tobacco control program the state of California implemented in 1988, including a 25-cent per tax on cigarettes, taxes on other tobacco products, and a ban on cigarette vending machines in certain areas. We are

interested in this case for two reasons. First, we intend to compare the performance of trajectory balancing, including both mean balancing and kernel balancing, and the SCM, since it has become increasingly popular in comparative case studies. Second, in the previous case, the number of pre-treatment periods T_0 is small and the number of control untis N_{co} is relatively large; by contrast, this example has $T_0 = 19$, but with only 39 control units and a single treated unit.

In the original paper, the authors match on only three periods of the pre-treatment outcome (1975, 1980, and 1988), plus four pre-treatment covariates,[13] reserving the remaining pre-treatment outcomes to select a weighting matrix required by the algorithm. However, in trajectory balancing we include all 19 periods of the pre-treatment outcome (from 1970 to 1988) as well as the four covariates for three reasons: (1) we want to minimize user discretion when choosing pre-treatment variables; (2) no validation period is required to be set aside by our procedure; and (3) by allowing as many periods as possible to enter the kernel matrix, we hope that trajectory balancing can take full advantage of information in the dynamics of each unit.

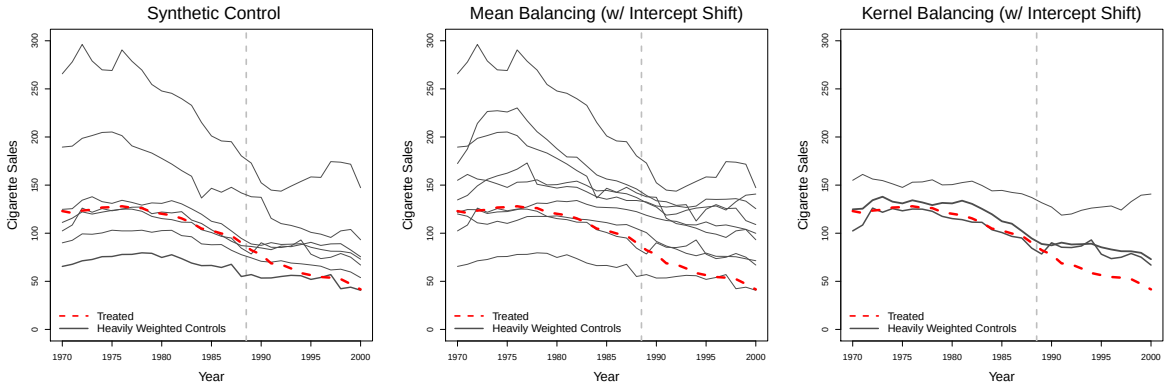
We found that when only balancing on the pre-treatment outcomes, both mean balancing and kernel balancing are able to obtain an excellent improvement in the imbalance and a pre-treatment trajectory for the weighted controls that almost exactly matches that of the treated unit, California; see Figure A2 in the Appendix. However, once covariates are added, neither balancing method is able to find substantial improvements in the pre-treatment trends without an intercept shift.This is because only a handful of states share a similar pre-treatment trajectory in terms on both the *dynamics* and the *level* with California. We thus invoke the demeaning procedure as described in Section 3, removing the mean of each unit's pre-treatment outcome.

In Figure 6, we plot the outcomes of the treated unit and heavily weighted control units based the SCM (left), mean balancing (middel), and kernel balancing (right). In all three plots, the horizontal axis is time, and vertical axis is the outcome variable, *per capita* number of cigarette packs sold during that year. The red dashed line represents the outcome trajectory of California while the gray lines represent the control states that receive weights bigger than 0.03 using each of

[13] They include average values of beer consumption, log income, tobacco retail price, and the share of population aged from 15 to 24 during the period of 1980 to 1988.

the three methods.[14] We see that the three methods differ in significant ways. In particular, kernel balancing puts heavy weights on units that share similar pre-treatment trajectories (e.g. Colorado, Idaho, and Delaware) while both mean balancing and the SCM put heavy weights on units that help ensure mean balance is achieved. Weights generated by mean balancing are much more evenly distributed than those produced by kernel balancing, a consequence of the fact that balancing on trajectories (encoded by higher-order features) is much more selective than balancing on means.

FIGURE 6. CALIFORNIA TOBACCO CONTROL PROGRAM
TREATED AND MOST HEAVILY WEIGHTED CONTROLS



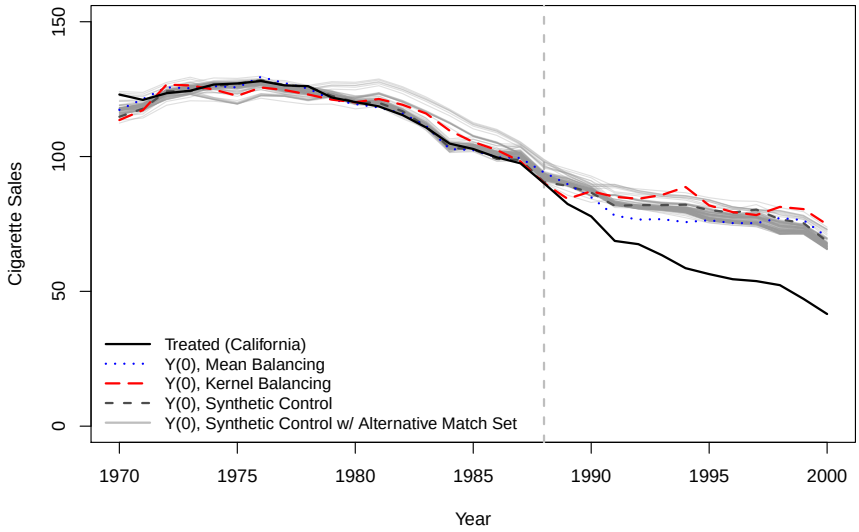
Note: The above plots show the outcome trajectories of the treated unit (California, red dashed line) and the the control states that have weights bigger than 0.03 (gray) using the SCM (left), mean balancing (middle), and kernel balancing (right). The SCM matches on four time-invariant covariates as well as the outcome (packs of cigarettes) measured in 1975, 1980, and 1988. Both mean balancing and kernel balancing use the same four covariates and the outcome variable for *all* pre-treatment periods (1970–1988). Line widths of the controls correspond to the assigned weights. The data are from ADH (2010).

Figure 7 plots results from both the SCM and trajectory (kernel) balancing methods, showing the actual treated outcome (red solid line), the treated counterfactual constructed by SCM (dark gray, dashed line), and the counterfactual constructed by trajectory balancing (black, solid line). We see that predicted Y_0 from both methods matches the pre-treatment outcome well except for the very early 1970s, and differ only slightly in the post-treatment period. If anything, trajectory balancing provides slightly bigger estimates of the effects of Proposition 99 on tobacco consumption.

One challenge of employing the SCM is that it asks users to choose the set of pre-treatment variables—and what periods of the pre-treatment outcomes—to be matched on. Recent research

[14]The weights are shown in Table A1 in the Appendix.

FIGURE 7. CALIFORNIA TOBACCO CONTROL PROGRAM
TREATED AND PREDICTED Y(0)



Note: Ooutcome of treated unit (California) and estimated counterfactual using the SCM and trajectory balancing with both the mean balancing and kernel balancing estimators. For the SCM, besides using the default match set (the outcome variable in 1975, 1980, and 1988 plus covariates), we also repeatedly select alternative pre-treatment periods to be matched on—each time, we randomly select 3 periods from 1970 to 1988—resulting in slightly different predicted counterfactual; in contrast, trajectory balancing uses all pre-treatment periods. The data are from ADH (2010).

has shown that cherry picking pre-treatment variables in the SCM may lead to over-rejection of the null hypothesis (Ferman et al. 2017). To examine how severe this problem may be, we randomly select three periods of the pre-treatment to be matched on and conduct synthetic control analyses repeatedly for 100 such choices. The results are also shown in Figure 7. On the left, each gray line represent the trajectory of a synthetic control unit using an alternative combination of pre-treatment outcome measures as matching targets; on the right, each gray line is a gap plot for each combination. We see that, depending on the set of pre-treatment periods to be matched on, the estimated effect can vary by as much as 10 packs at a given time. In contrast, such discretion is completely removed with trajectory balancing since all pre-treatment periods have been taken into account.

In summary, the above empirical examples illustrate the versatility and robustness of trajectory balancing under different circumstances. It can accommodate TSCS/panel data structure with either short T_0 or long T_0, and allows for relatively small N if needed. Beyond the theoretical motivation for relaxing the LPO assumption, these examples show that trajectory balancing has

advantages over the SCM in two respects: (1) it give heavy weights to control units that share almost the exact same trajectories of that the treated unit and (2) it minimizes user discretion in choosing the variables to be matched/balanced on.

6. Conclusion

In this paper, we propose trajectory balancing, a general reweighting approach to causal inference with TSCS data, and specifically "generalized DID" scenarios. We introduce two trajectory balancing estimators: (1) mean balancing and (2) kernel balancing.

The mean balancing estimator reweights the control units such that the treatment group and reweighted control group will have equal means on pre-treatment outcomes and covariates. It directly exploits the LPO assumption built into a variety of existing procedures for causal inference with TSCS and generalized DID data, including those that seek to deal with time varying confounders such as synthetic control method (SCM) and latent factor models (LFMs). However, it avoids a number of practical concerns of the SCM and LFMs. For example, it allows few or many pre-treatment periods, few or many treated units, and ensures convergence utilizing an approximation.

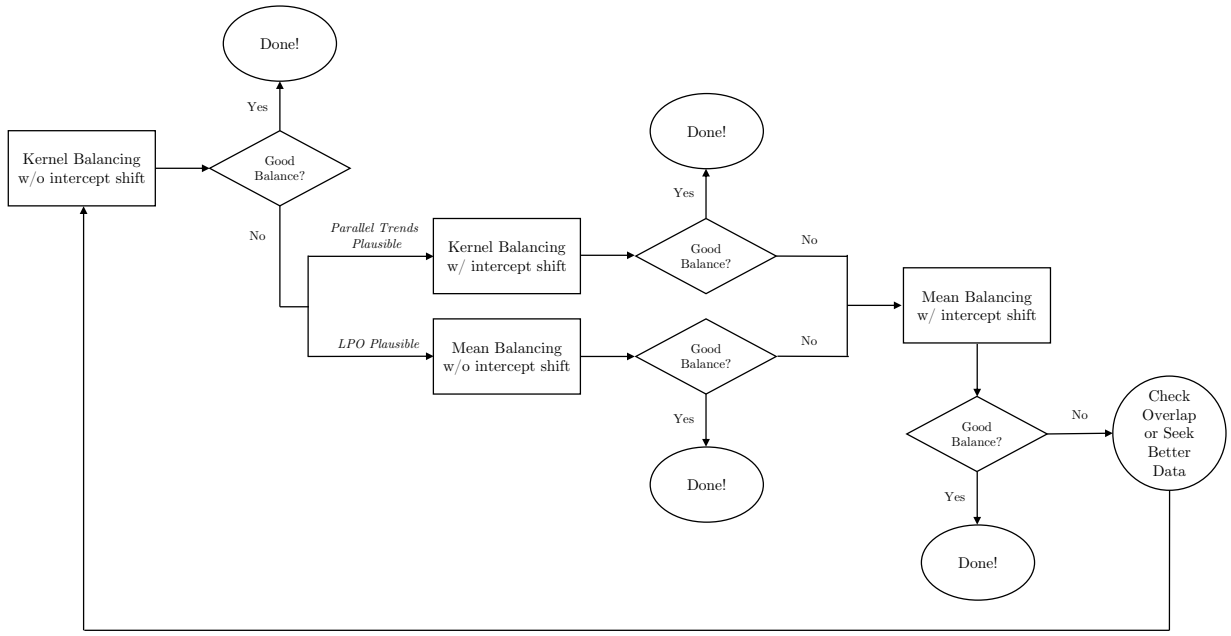
The kernel balancing estimator relaxes the LPO assumption underlying mean balancing procedure by seeking balance on a high-dimensional (kernel) description of the existing features. As a result, the weighted control group is similar to the treated not only on the period-wise means prior to treatment, but on high-order features of the trajectories. Intuitively, kernel balancing ensures the overall distributions of trajectories are similar between the treatment group and reweighted control group, which is not necessarily the case when mean balancing is applied.

Prior to employing these methods, users may wish to examine their data to assess whether there are treated units that fall outside the range of the control units, at least separately on each margin. The results may suggest particular *a priori* strategies: (1) If treated units occupy minimum or maximum values of the pre-treatment outcomes, the demeaning procedure may become necessary to achieve good balance. (2) If treated units occupy minimum or maximum values of the *covariates*, then the user might (a) reconsider the importance of including those covariates; or (b) trimming of

the offending "non-comparable" unit in the treatment group.[15]

As a general guideline, one workflow we propose is as follows (illustrated in Figure 8). Before applying trajectory balancing, the user would often benefit from examining potential issues of non-overlap, at least determining if any treated unit occupies the maximum or minimum value of an pre-treatment outcome or covariate. After trimming decisions are made, if any, to begin employing trajectory balancing users can start with the weakest set of assumption: the kernel balancing approach, without demeaning. The quality of match in the pre-treatment outcomes reveal whether weights could be found that perform well. One should also check the weights to see how heavily the result relies upon a small group of controls. If the balance is poor or the result is heavily

FIGURE 8. RECOMMENDED WORKFLOW FOR IMPLEMENTING TRAJECTORY BALANCING



weighted towards a few units, the user may wish to impose additional assumptions that can relieve these problems. One option is to allow for the intercept-shift/demeaning. This is most reasonable when one believes that a time-invariant difference in the *level* of the outcomes for a given unit is not important to the expected trend or trajectory of changes. Alternatively, one could employ the mean balancing option rather than use the Gaussian kernel to see if the resulting weights are

[15] We use the term "non-comparable" to refer to (treated) units with values more extreme than control units and which thus create an inability to find comparable control units.

more satisfactory. This implies the stricter LPO assumption, but may remain reasonable especially when there are many pre-treatment time points. Recall that with many pre-treatment time points, the mean balancing approach consequently has many moment conditions to satisfy, and solutions that achieve good balance on the pre-treatment period more plausibly maintain balance on Y^0 thereafter. By contrast, when there are very few pre-treatment periods, mean balance implies fewer constraints and is "too easy" to achieve, thus the kernel balancing approach imposes stronger condition by achieving balance on high-order functions of the pre-treatment trajectories. While we recommend these general guidelines, further theoretical and applied work will be useful in better understanding the conditions that recommend one method over the other.

Compared with the latent factor approach and other model-based methods, our approach requires minimum modeling assumptions. Though we require that the non-treatment potential outcomes fall in a particular function space determined by the kernel, this is a very general function space. The procedure never directly fits a model, hence, chances of erroneous extrapolation based on estimated model parameters is minimized. Second, because we assign an explicit non-negative weight to each control unit, the method is transparent and easy to understand in terms of the average counterfactual it constructs.

Compared with the SCM, our approach, and kernel balancing in particular, has several additional advantages. First and foremost it achieves balance on (an approximation of) the full trajectory of pre-treatment outcomes rather than their average levels. Thus, the reweighted control units are much more similar in their joint distribution of pre-treatment variables to the treated group than can be ensured when using the SCM. Second, our method accommodates various types of data—it works whether the number of the pre-treatment periods is small or big, or the number of treated unit is one or many—thus providing a unified framework. By contrast, the SCM usually requires a long pre-treatment period and is often difficult to extend to cases with multiple treated units. Third, our method is easy to implement and requires minimum user input. Unlike the SCM, it does not require users to specify which pre-treatment outcomes or their higher order interactions to be matched on, thus minimizing the negative effects of research degrees of freedom.

There are several limitations of our approach. First, kernel balancing potentially requires more

data than existing methods simply because it attempts to achieve balance on a higher order description of the pre-treatment variables instead of the means only. One way to relieve this is by relaxing assumptions, such as allowing intercept shifts. A related limitation is that, in practice, it is not possible to achieve balance on all the dimensions of $\mathbf{K}$, which form the bases of the assumed function space. Rather, as described, we must instead obtain balance on the best rank P approximation of $\mathbf{K}$, where P is selected to minimize an overall imbalance metric. That said, even existing approaches that involve only balance on averages can run into a similar feasibility limitation, which we also handle through the same approximation under mean balancing. Third, the inferential method for trajectory balancing is not fully developed. Future work is needed to address this issue.

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A. Supplementary Information

A.1. Proofs

A.1.1. DID and two-way fixed effects models imply the LPO assumption

Proof: In DID and two-way fixed effects models, $Y_{it}^0 = \alpha_i + \xi_t + \varepsilon_{it'},\ \forall i, t$. Therefore,

$$\begin{aligned} Y_{it}^0 &= (Y_{it}^0 - T_0^{-1}\sum_{s=1}^{T_0} Y_{is}^0) + T_0^{-1}\sum_{s=1}^{T_0} Y_{is}^0 \\ &= \left[(\alpha_i + \xi_t + \varepsilon_{it}) - T_0^{-1}\sum_{s=1}^{T_0}(\alpha_i + \xi_s + \varepsilon_{is})\right] + T_0^{-1}\sum_{s=1}^{T_0} Y_{is}^0 \\ &= (\xi_t - T_0^{-1}\sum_{s=1}^{T_0}\xi_s) + (\varepsilon_{it} - T_0^{-1}\sum_{s=1}^{T_0}\varepsilon_{is}) + T_0^{-1}\sum_{s=1}^{T_0} Y_{is}^0 \quad ; \end{aligned}$$

in which the first term on the right-hand-side (RHS), $\xi_t - \sum_{s=1}^{T_0}\xi_s$ is a time-specific but unit-independent deviation; the second term on the RHS, $\varepsilon_{it} - T_0^{-1}\sum_{s=1}^{T_0}\varepsilon_{is}$, has expectation of zero, when we assume that $\mathbb{E}[\varepsilon_{it}] = 0$ for all i, t; and the third term on the RHS is an average of pre-treatment outcomes. Q.E.D.

A.1.2. The structure time-series model considered in Section 2 implies the LPO assumption

Proof: The non-treatment outcome of the model can be written recursively as:

$$\begin{aligned} Y_{it}^0 &= Y_{i,t-1}^0 + (\varepsilon_{it} - \varepsilon_{i,t-1}) + \xi_{it} + \eta_t^\mu \\ &= Y_{i,t-2}^0 + (\xi_{it} + \xi_{i,t-1}) + (\eta_{it}^\mu + \eta_{i,t-1}^\mu) + (\varepsilon_{it} - \varepsilon_{i,t-2}) \\ &= \cdots \\ &= Y_{iT_0}^0 + \sum_{s=T_0+1}^{t}\xi_{is} + \sum_{s=T_0+1}^{t}\eta_{is}^\mu + (\varepsilon_{it} - \varepsilon_{iT_0}), \quad \forall t > T_0,\ \forall i \quad ; \end{aligned}$$

in which the first term on the RHS, $Y_{iT_0}^0$, is in $\mathbf{Y}_{i,pre}^0$ and the last term on the RHS, $\sum_{s=T_0+1}^{t}\eta_{is}^\mu + (\varepsilon_{it} - \varepsilon_{iT_0})$, has expectation of zero when $\mathbb{E}[\eta_{it}^\mu] = 0$ and $\mathbb{E}[\varepsilon_{it}] = 0$ for all i, t. Hence, we only need to consider the second term, $\sum_{s=T_0+1}^{t}\xi_{is}$. We know that:

$$\xi_{is} = \rho^{(s-T_0)}\xi_{iT_0} + (s - T_0)(1-\rho)\kappa + \sum_{p=T_0+1}^{s}\eta_{ip}^\xi$$

in which the second term on the RHS, $(s-T_0)(1-\rho)\kappa$, is a constant; the last term, $\sum_{p=T_0+1}^{s}\eta_{ip}^{\xi}$, is of zero mean; and the first term can be expressed as:

$$\xi_{iT_0} = (Y_{iT_0}^0 - Y_{i,T_0-1}^0) - (\varepsilon_{iT_0} - \varepsilon_{i,T_0-1}) + \eta_{iT_0}^{\mu}$$

which is a linear combination of two pre-treatment outcomes plus a zero-mean noise. Hence, Y_{it}^0 can be written as a combination of the linear transformation of pre-treatment outcomes, a time-specific but unit-independent coefficient, and a zero-mean noise. Q.E.D.

A.1.3. IFE models imply the LPO assumption

Proof: Consider for each unit i, the projection Y_{it}^0 in the pre-treatment period onto the span of f_p, obtaining a set of coefficients:

$$\begin{aligned}
\hat{\lambda}_i = &\ (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{s=1}^{T_0} f_p Y_{ip} \\
= &\ (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{p=1}^{T_0} f_p (f_p^\top \lambda_i + \xi_p + \varepsilon_{ip}) \\
= &\ \lambda_i + (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{p=1}^{T_0} f_p (\xi_p + \varepsilon_{ip})
\end{aligned}$$

Note that, because there is no unit fixed effect to act as an intercept in the model, the prediction for Y_{it}^0 is simply $f_t^\top \hat{\lambda}$. Finally, at some post-treatment time t, we could think of Y_{it}^0 as a combination of the predicted value $\hat{Y}_{it}^0$ and a residual $Y_{it}^0 - \hat{Y}_{it}^0$, yielding:

$$\begin{aligned}
Y_{it}^0 &= \hat{Y}_{it}^0 + (Y_{it}^0 - \hat{Y}_{it}^0) \\
&= f_t^\top \hat{\lambda}_i + (f_t^\top \lambda_i + \xi_t + \varepsilon_{it}) - f_t^\top \left[\lambda_i + (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{p=1}^{T_0} f_p (\xi_p + \varepsilon_{ip}) \right] \\
&= f_t^\top \hat{\lambda}_i + \xi_t + \varepsilon_{it} - f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{p=1}^{T_0} f_p (\xi_p + \varepsilon_{ip}) \\
&= f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{s=1}^{T_0} f_s Y_{is} + (\xi_t + \varepsilon_{it}) - f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} \sum_{s=1}^{T_0} f_p (\xi_p + \varepsilon_{ip}) \\
&= \sum_{s=1}^{T_0} f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} f_s Y_{is}^0 + \left[\xi_t - \sum_{s=1}^{T_0} f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} f_s \xi_s \right] + \left[\varepsilon_{it} - \sum_{s=1}^{T_0} f_t^\top (\sum_{p=1}^{T_0} f_p f_p^\top)^{-1} f_s \varepsilon_{is} \right]
\end{aligned}$$

which reveals that Y_{it}^0 is a linear combination of the pre-treatment outcomes (the first term on the RHS), plus some time-specific intercept shift (the second term on the RHS) and a zero-mean error term (the last term on the RHS). Q.E.D.

A.1.4. Unbiasedness of the mean balancing estimator

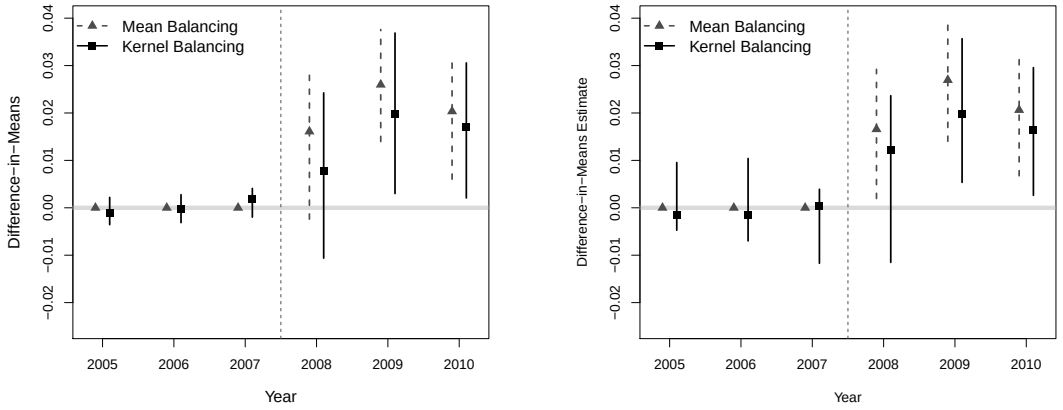
Proof: Under Assumptions 1, 2, and 3:

$$
\begin{aligned}
\mathbb{E}[\widehat{ATT}_t|\mathbf{Y}_{pre}] &= \frac{1}{N_{tr}}\sum_{G_i=1}\mathbb{E}[Y_{it}|G_i=1,\mathbf{Y}_{pre}] - \sum_{G_i=0}\mathbb{E}[w_i Y_{it}|G_i=0,\mathbf{Y}_{pre}] \\
&= \frac{1}{N_{tr}}\sum_{G_i=1}\mathbb{E}[\tau_{it}+Y^0_{it}|G_i=1,\mathbf{Y}_{pre}] - \sum_{G_i=0}\mathbb{E}[w_i Y^0_{it}|G_i=0,\mathbf{Y}_{pre}] \\
&= ATT_t + \frac{1}{N_{tr}}\sum_{G_i=1}\mathbb{E}[Y^0_{it}|G_i=1,\mathbf{Y}_{i,pre}] - \sum_{G_i=0} w_i\mathbb{E}[Y^0_{it}|G_i=0,\mathbf{Y}_{i,pre}] \\
&\overset{\text{CI}}{=} ATT_t + \frac{1}{N_{tr}}\sum_{G_i=1}\mathbb{E}[Y^0_{it}|\mathbf{Y}_{i,pre}] - \sum_{G_i=0} w_i\mathbb{E}[Y^0_{it}|\mathbf{Y}_{i,pre}] \\
&\overset{\text{LPO}}{=} ATT_t + \frac{1}{N_{tr}}\sum_{G_i=1}(1\ \mathbf{Y}_{i,pre})^\top\theta_t - \sum_{G_i=0} w_i(1\ \mathbf{Y}_{i,pre})^\top\theta_t \\
&\overset{\text{feasibility}}{=} ATT_t.
\end{aligned}
$$

Q.E.D.

A.2. Additional Results for the Empirical Examples

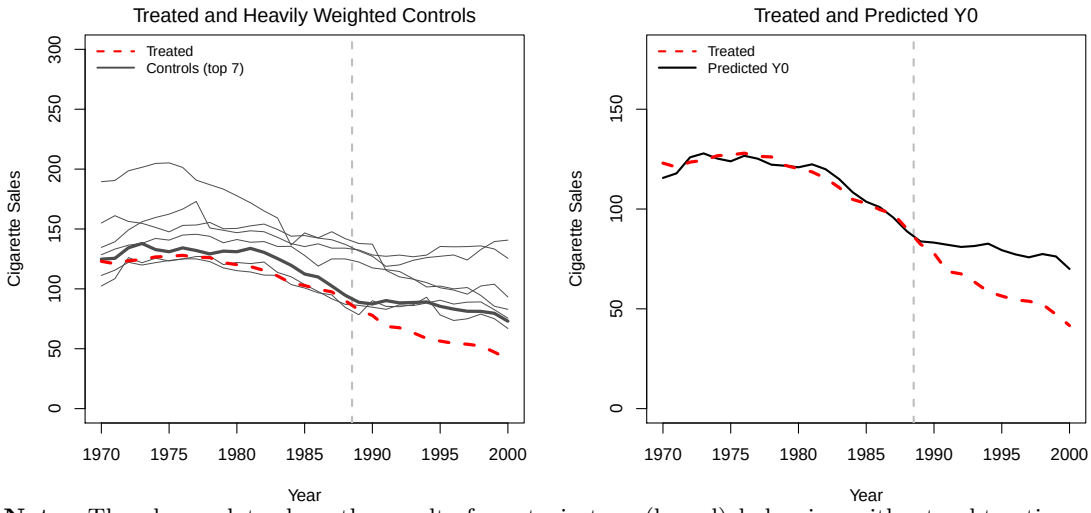
FIGURE A1. RETURN TO OFFICE OF CHINA'S NATIONAL PEOPLE'S CONGRESS



(a) One treated unit dropped (b) Original sample

Note: The above figures compare the results in the main text (Figure 4), where one treated firm is dropped, and the results using the original sample. The substantive findings are similar, but when the unit whose value is too extreme to be supported by the controls (the "non-comparable" unit) is kept in the sample, it is harder to achieve balance in the pre-treatment period, resulting in wider confidence intervals for kernel balancing.

Figure A2. Trajectory Balancing without Demeaning



Note: The above plots show the results from trajectory (kernel) balancing without subtracting each unit's pre-treatment outcome mean. The outcome variable of all pre-treatment periods are included in the balancing scheme, but not the four time-invariant covariates. **(a)**: the outcome trajectories of the treated unit (red, dashed line) and the top 7 control units that receive the largest weights (gray, solid lines). The width of each gray solid line corresponds to the weight of the control unit. We see that the control unit that resembles the treated unit the most in outcome trajectory in the pre-treatment period (Montana) receives the largest weight. **(b)**: the outcome trajectories of the treated unit and the average of reweighted control outcomes (predicted Y_0). We see that control units that receive large weights are those sharing a similar trajectory (in terms of both the dynamics and the level) to that of the control unit. Data are from ADH (2010).

Table A1. Weights for Control Units from Synthetic Control and Trajectory Balancing

State	Synthetic Control	Mean Balancing	Kernel Balancing
Utah	0.34	0.04	0.00
Montana	0.25	0.01	0.00
Nevada	0.19	0.17	0.00
Colorado	0.05	0.02	0.45
New Mexico	0.05	0.02	0.00
Idaho	0.03	0.03	0.36
New Hampshire	0.03	0.08	0.00
Alabama	0.00	0.01	0.00
Arkansas	0.00	0.01	0.00
Connecticut	0.00	0.10	0.00
Delaware	0.00	0.18	0.19
Georgia	0.00	0.02	0.00
Illinois	0.00	0.01	0.00
Indiana	0.00	0.03	0.00
Iowa	0.00	0.01	0.00
Kansas	0.00	0.00	0.00
Kentucky	0.00	0.00	0.00
Louisiana	0.00	0.00	0.00
Maine	0.00	0.03	0.00
Minnesota	0.00	0.01	0.00
Mississippi	0.00	0.01	0.00
Missouri	0.00	0.01	0.00
Nebraska	0.00	0.02	0.00
North Carolina	0.00	0.04	0.00
North Dakota	0.00	0.00	0.00
Ohio	0.00	0.04	0.00
Oklahoma	0.00	0.00	0.00
Pennsylvania	0.00	0.01	0.00
Rhode Island	0.00	0.01	0.00
South Carolina	0.00	0.00	0.00
South Dakota	0.00	0.01	0.00
Tennessee	0.00	0.01	0.00
Texas	0.00	0.00	0.00
Vermont	0.00	0.00	0.00
Virginia	0.00	0.00	0.00
West Virginia	0.00	0.03	0.00
Wisconsin	0.00	0.02	0.00
Wyoming	0.00	0.00	0.00

A.3. Two Additional Empirical Examples

We apply trajectory balancing to two additional examples. The first one is a class example on the effect of a job training program from LaLonde (1986). The second one comes from Xu (2017), who studies the effect of Election-Day Registration (EDR) on voter turnout using a linear factor model (LFM).

A.3.1. Effect of Job Training (LaLonde 1986)

It is useful to know whether this approach can recover an average treatment effect estimate in an observational setting where we nevertheless know the "true" answer. To do so, we consider the National Supported Work (NSW) program which was first used as a benchmark in obserational studies by LaLonde (1986) and Dehejia and Wahba (1999), and which has become a routine benchmark for matching and weighting approaches (e.g. Diamond and Sekhon 2005; Iacus et al. 2011; Hainmueller 2012).

Following LaLonde (1986), the treated sample from the NSW experiment is compared to a control sample drawn from a separate, observational sample. Methods of adjustment are tested to see if they accurately recover the treatment effect despite large observable differences between the control sample and the treated sample. Usually this is done on a range of pre-treatment covariates: age, years of education, real earnings in 1974, real earnings in 1975 and a series of indicator variables: Black, Hispanic, and married, and often indicators for being unemployed (having income of $0) in 1974 and 1975, and an indicator for having no highschool degree (fewer than 12 years of education).

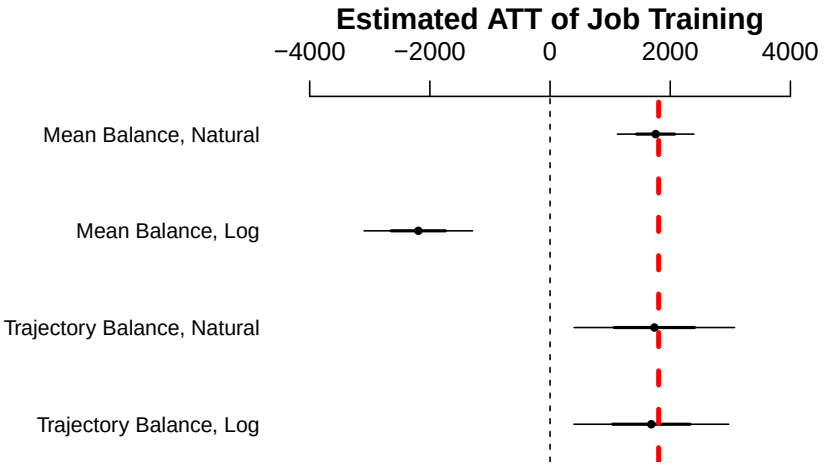
Here we instead use this approach in a TSCS setting by considering only the prior outcome measures, earnings in 1974 and 1975. As far as we know, we are the first to show how using only pre-treatment outcome measures in a TSCS allows for estimates in this example that very closely match the experimental benchmark, without need of covariates or addition of an indicator for zero income.[16]

We use the 185 treated units from NSW, originally selected by Dehejia and Wahba (1999) for the treated sample. The experimental benchmark for this group of treated units is $1794, which is computed by difference-in-means in the original experimental data with these 185 treated units. The control sample is drawn from the Panel Study of Income Dynamics (PSID-1), containing 2490 individuals.

[16] As found by Dehejia and Wahba (1999), propensity score matching can effectively recover reasonable estimates of the ATT, but are highly sensitive to specification choices in constructing the propensity score model (Smith and Todd 2001). Diamond and Sekhon (2005) use genetic matching to estimate treatment effects with the same treated sample. While matching solutions with the highest degree of balance produced estimates very close to the experimental benchmark, these models included the addition of squared terms and two-way interactions, not to mention the constructed indicators for zero income in 1974 and 1975. Similarly, entropy balancing (Hainmueller 2012) has also been shown to recover good estimates using a similar setup, using a control dataset based on the Current Population Survey (CPS-1), employing all pairwise interactions and squared terms for continuous variables, amounting to 52 covariates.

Figure A3 shows that the ATT of job training on earning in 1978 with different combinations of the reweighting scheme (mean balancing versus kernel balancing) and transformation of the outcome measure or the lack of thereof (logarithmic versus real dollar value). For both mean balance and kernel balance, we use the pre-treatment outcomes (1974 and 1975 income) as the input.

FIGURE A3. LALONDE DATA WITH MEAN BALANCING AND TRAJECTORY BALANCING



Note: The above plots show our replication using the Lalonde data. We use entropy balancing to achieve mean balance on earnings in 1974 and 1975 with dollar values (first estimate) or their logarithmic transformation (second estimate). We apply kernel balancing using dollar values of 1974 and 1975 earnings (third estimate) or their logarithmic transformations (fourth estimate) as input.

Some may argue that investigators should have sufficient substantive knowledge to know what transformations of the variables to work in, such that LPO becomes reasonable. Our general position is not only that authors cannot be expected to know what transformations of the outcome to use, but that giving them this responsibility instead opens the door to abuse through additional researcher degrees of freedom. For example, studying the effect of a job training program on income, it would seem a defensible choice to log those incomes (after adding one to avoid the log of zero) for purposes of estimating weights. And yet, doing so radically alters the results. Figure A3 shows that on the natural scale, mean balance actually does an excellent job and recovers approximately the correct (benchmark) ATT estimate. However, if one takes the reasonable step of logging incomes, the result changes by thousands of dollars. By contrast, trajectory balancing is relatively indifferent to the choice of transformation used, recovering an approximately correct answer in both cases. Because this approach ensures a very rich representation of the pre-treatment trajectory is similar in the treated and in the counterfactual control group, we need not rely on investigators to guess transformations, nor allow them to.

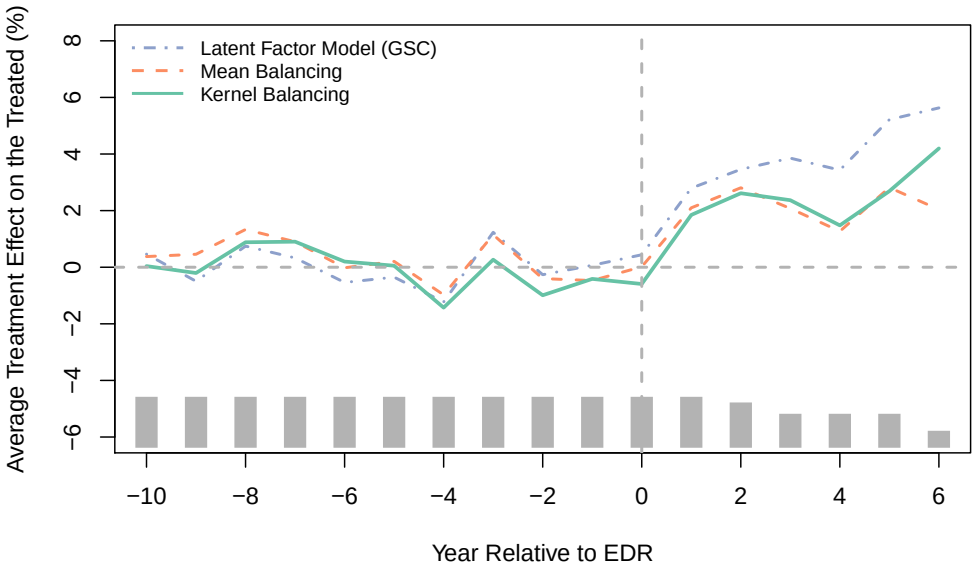
A.3.2. Election Day Registration on Voter Turnout (Xu 2017)

Next, we apply trajectory balancing to the example of EDR and voter turnout in the United States from 1920 to 2012. This example is different from the ones appearing in main text in that the treatment (EDR laws) took effect at different times. Nine states passed EDR laws during this time period. Three states, Maine, Minnesota, and Wisconsin enacted the EDR from 1973 to 1975, which took effect in 1976's presidential election; three states, Wyoming, Idaho, and New Hampshire, followed in 1994–1995 and the EDR took effect in 1996's presidential election. In addition, the EDR took effect in Connecticut in 2008 and in Iowa and Montana in 2012. Thirty-eight states that had never passed EDR laws during this period serve as controls.

We apply trajectory balancing (using both mean balancing and kernel balancing allowing intercept shift) for each of four different timings, 1976, 1996, 2008, and 2012. The results are shown in Figure A5. We find that the results from trajectory balancing in general agree with the results from the generalized synthetic control (GSC) method, which is based on a latent factor model (Xu 2017). We then re-align the ATT estimates based on the time passed since the beginning of the treatment and obtain the ATT estimates for all 9 treated states. The results are shown in Figure A4. It is worth noting that the SCM fails to find a solution in 5 of 9 cases.

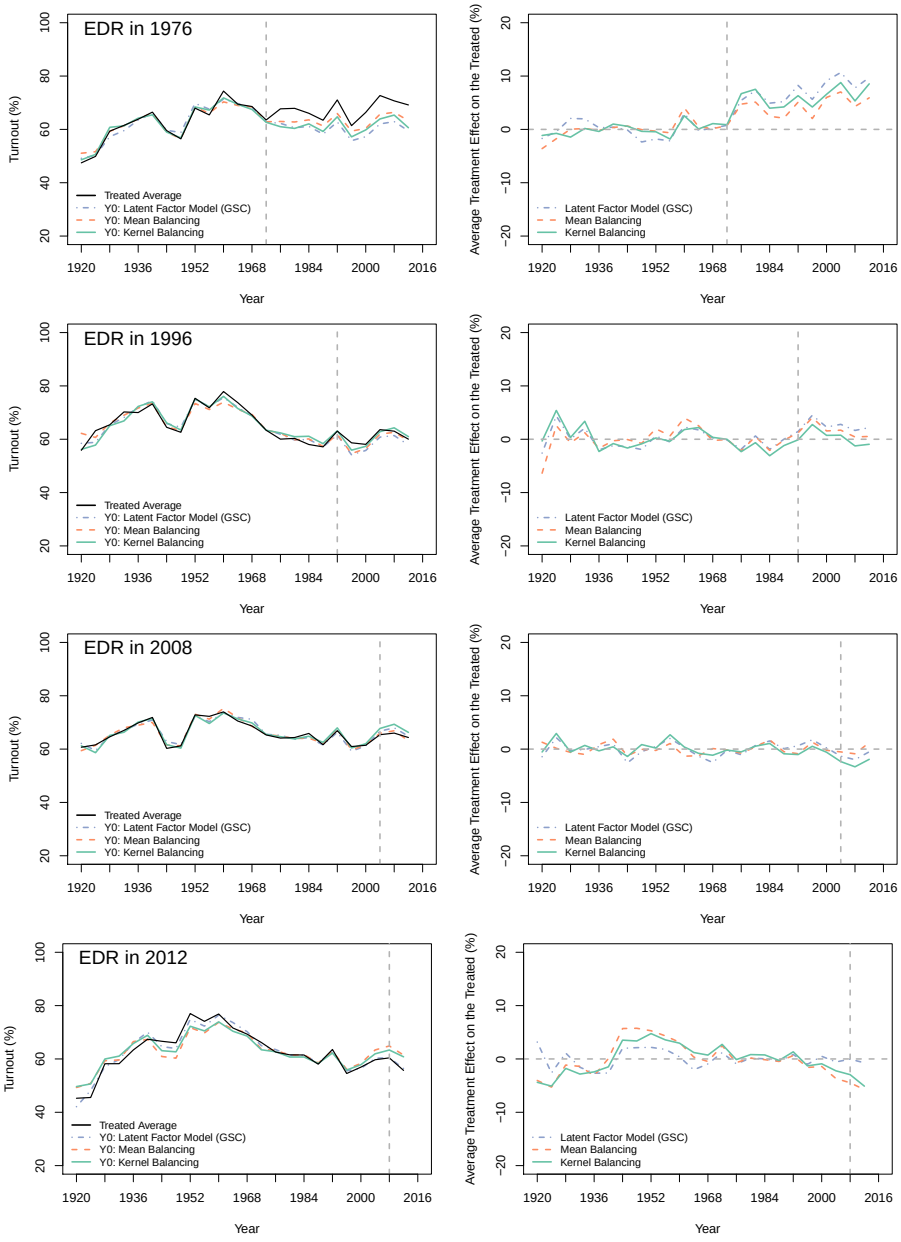
The above procedures can be easily implemented with the `tjbal` package.

FIGURE A4. ELECTION DAY REGISTRATION ON VOTER TURNOUT: ALL EDR STATES



Note: The above figure shows the ATT of EDR laws on voter turnout using three different methods: (1) the general synthetic control, which is based on a linear factor model, (2) mean balancing, and (3) kernel balancing. The gray bars at the bottom demonstrate the number of treated states at each time period.

FIGURE A5. ELECTION DAY REGISTRATION ON VOTER TURNOUT: BY EDR YEAR



Note: The panels on the left show that the treated average and counterfactual average using three different methods: (1) the general synthetic control, which is based on a linear factor model, (2) mean balancing, and (3) kernel balancing. The panels on the right show the ATT estimates. The data are from Xu (2017).