Synthetic Controls and Weighted Event Studies
with Staggered Adoption*

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Abstract

Staggered adoption of policies by different units at different times creates promising opportunities for observational causal inference. The synthetic control method (SCM) is a recent addition to the evaluation toolkit but is designed to study a single treated unit and does not easily accommodate staggered adoption. In this paper, we generalize SCM to the staggered adoption setting. Current practice involves fitting SCM separately for each treated unit and then averaging. We show that the average of separate SCM fits does not necessarily achieve good balance for the average of the treated units, leading to possible bias in the estimated effect. We propose “partially pooled” SCM weights that instead minimize both average and state-specific imbalance, and show that the resulting estimator controls bias under a linear factor model. We also combine our partially pooled SCM weights with traditional fixed effects methods to obtain an augmented estimator that improves over both SCM weighting and fixed effects estimation alone. We assess the performance of the proposed method via extensive simulations and apply our results to the question of whether teacher collective bargaining leads to higher school spending, finding minimal impacts. We implement the proposed method in the augsynth R package.

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1 Introduction

Jurisdictions often adopt policies at different times, creating promising opportunities for observational causal inference. In our motivating application, 33 states passed laws between 1964 and 1987 mandating that school districts bargain with teachers unions (Hoxby, 1996; Paglayan, 2019); our goal is to estimate the impact of these laws on student expenditures and teacher salaries.

Estimating causal effects under staggered adoption remains challenging, however. One promising approach is to use the synthetic control method (SCM; Abadie et al., 2010, 2015). Developed for the case with a single treated unit, SCM estimates the counterfactual untreated outcome via a weighted average of untreated units, with weights chosen to match the treated unit’s pre-treatment outcomes as closely as possible. Some applied researchers have used SCM in staggered adoption settings by estimating SCM weights separately for each treated unit and then averaging the estimates (see, e.g., Dube and Zipperer, 2015; Donohue et al., 2019). This approach is not well understood, however, especially in applications like ours where some treated units have poor SCM fits.

We develop SCM for the staggered adoption setting. We first consider two immediate adaptations: separate SCM, which reflects the current practice of estimating weights that separately minimize the pre-treatment imbalance for each treated unit; and pooled SCM, which instead estimates weights that minimize the average pre-treatment imbalance across all treated units. Both approaches have drawbacks. Separate SCM can lead to poor fit for the average, leading to possible bias. Pooled SCM, by contrast, can achieve nearly perfect fit for the average but can yield substantially worse unit-specific fits, making the estimator susceptible to interpolation bias from non-linearity and settings where the outcome process varies over time.

Our main proposal is partially pooled SCM, which finds intermediate weights between these two extremes. First, we show that, under a linear factor model, the error of the Average Treatment Effect on the Treated (ATT) estimate decomposes into error stemming from the pooled fit and from state-specific fits. By minimizing both quantities, partially pooled SCM thus directly controls the corresponding bias. We also motivate our proposal by examining the Lagrangian dual of the constrained optimization problem, showing that method partially pools parameters in the dual parameter space.

Partially pooled SCM in general does not perfectly balance both the unit-specific and pooled average pre-treatment outcomes. Recent proposals in settings with a single unit have demonstrated that augmenting SCM with an outcome model can correct for possible bias due to imperfect pre-treatment fit (e.g., Ben-Michael et al., 2019). We extend these augmented panel data methods to the staggered adoption setting. While the framework is general, we focus here on augmentation with an average of pre-treatment outcomes, as would arise from a fixed effects specification. We refer to the augmented estimator as a weighted event study; the (unweighted) “event study” estimator is common in econometrics but has a number of pathologies that our weighted approach avoids (e.g., Abraham and Sun, 2018; Callaway and Sant’Anna, 2018). We can also view this as adapting
intercept-shifted SCM (Doudchenko and Imbens, 2017; Ferman and Pinto, 2018) to the staggered adoption case.

We apply our methods to estimating the impact of mandatory teacher collective bargaining and show that they achieve better pre-treatment balance than existing approaches. We find no impact of teacher collective bargaining laws on either teacher salaries or student expenditures, consistent with several recent papers (Frandsen, 2016; Paglayan, 2019) but counter to earlier claims (most notably Hoxby, 1996).

Related work. Our paper contributes to several active methodological literatures. First, there is a large and active applied econometrics literature on challenges and remedies for two-way fixed effects models with multiple treated units, including event study models; see Borusyak and Jaravel (2017); Abraham and Sun (2018); Athey and Imbens (2018); Goodman-Bacon (2018); Callaway and Sant’Anna (2018); see also Xu (2017) and Athey et al. (2017) for recent generalizations of these models. SCM has also attracted a great deal of attention; see Abadie (2019) for a recent review. Several recent papers have explored SCM with multiple treated units. In the case where all units adopt treatment at the same time, some propose to first average the units and then estimate SCM weights for the average, analogous to our fully pooled SCM estimate; for discussion, see Kreif et al. (2016); Robbins et al. (2017). An alternative is Abadie and L’Hour (2018), who instead propose to estimate separate SCM weights for each treated unit. In particular, they propose a penalized SCM approach that aims to reduce interpolation bias, allowing for weights that move continuously between standard SCM and nearest-neighbor matching. Our approach complements these papers by adapting some of these ideas to the staggered adoption setting. For some other examples of SCM under staggered adoption, see also Dube and Zipperer (2015); Toulis and Shaikh (2018); Donohue et al. (2019).

We also build on recent papers that combine outcome modeling and SCM in panel data settings, which themselves adapt “doubly robust” estimators more common in the (cross sectional) observational studies literature. To date, these approaches have been limited to the case with a single treated unit or, if multiple units are treated, to a single adoption time. Ben-Michael et al. (2019); Ferman and Pinto (2018); Abadie and L’Hour (2018); Chernozhukov et al. (2018); Arkhangelsky et al. (2019) all propose versions of bias correction. See also Arkhangelsky and Imbens (2019) for a more general discussion of double robustness in panel data settings.

Motivating example: Teacher collective bargaining. The United States, like other developed countries, spends substantial resources on public education. Approximately 80% of education spending goes to teacher salaries and benefits (U.S. Department of Education, National Center for Education Statistics, 2018), and recent research points to teacher quality as a key determinant of student outcomes (Jackson et al., 2014). Over recent decades, the teacher employment relationship has changed dramatically via the introduction of unions and collective bargaining agreements
Figure 1: Staggered adoption of mandatory collective bargaining laws from 1964 to 1990.

Goldstein (2015). Critics identify these as a “harmful anachronism” and “the most daunting impediments” to education reform (Hess and West, 2006). A major 2018 Supreme Court decision, Janus v AFSCME, is expected to weaken teachers’ unions, bringing renewed attention to this area and raising interest in understanding the effects of teacher collective bargaining.

Since 1964, a number of states have passed laws mandating that school districts bargain with teachers’ unions. Given the strong criticism directed at teachers’ unions, there is surprisingly little evidence that they, or the mandatory bargaining laws, have any effect at all. In a seminal study, Hoxby (1996) uses state-level changes in collective bargaining laws to argue that teacher collective bargaining raises teacher salaries and school expenditures but reduces student outcomes. Several more recent papers have disputed Hoxby’s conclusions, however. Using a panel of school districts, Lovenheim (2009) finds little effect of unionization on teacher pay or class size. Frandsen (2016) similarly finds little effect of state unionization laws on teacher pay. Finally, Paglayan (2019) extends the historical state-level data set from Hoxby (1996). In a two-way fixed effect event study specification, she finds precisely estimated zero effects of mandatory bargaining laws on school expenditures and teacher salaries. See Appendix C.1 for additional discussion of Paglayan (2019).

Motivated in part by recent criticisms of event study models (Goodman-Bacon, 2018), we revisit the Paglayan (2019) analysis using different methods. Figure 1 shows adoption times of state mandatory bargaining laws between 1964 and 1990. Adoptions were spread across 14 separate years, though 16 states adopted laws between 1965 and 1970. Following Paglayan (2019), our main outcomes of interest are per-pupil student expenditures and teacher salaries, both measured in log 2010 dollars. We observe these outcomes back to 1959 for 49 states; we exclude Washington DC.

1 Another 10 states allow but do not require collective bargaining, while 7 prohibit it. We focus on identifying the effects of mandates.
and Wisconsin, which adopted a mandatory bargaining law in 1960 and thus has only one year of pre-intervention data. This gives between 6 and 28 years of data before the adoption of mandatory bargaining, with an average of 13 years.

Paper roadmap. Section 2 lays out the technical background. Section 3 develops SCM for the staggered adoption setting and introduces partially pooled SCM. Section 4 gives theoretical results for the generalized SCM approaches. Section 5 introduces augmented panel data methods and the weighted event study estimator. Section 6 describes a calibrated simulation study. Section 7 gives additional results for the teacher collective bargaining application. Finally, Section 8 discusses some directions for future work. The appendix includes further analyses and technical results.

2 Preliminaries

2.1 Setup and notation

We consider a panel data setting where we observe outcomes \( Y_{it} \) for \( i = 1, \ldots, N \) units over \( t = 1, \ldots, T \) time periods. In the teacher collective bargaining application, \( N = 49 \) and \( T = 39 \). Some but not all of the units, indicated by \( W_i = 1 \), adopt the treatment during the panel; once units adopt treatment, they stay treated for the remainder of the panel. See Imai and Kim (2019) for a more general discussion. Let \( T_i \) represent the date that unit \( i \) receives treatment, with \( T_i = \infty \) denoting never-treated units. Without loss of generality, we order units so that \( T_1 \leq T_2 \leq \cdots \leq T_N \).

We assume that there are non-zero number of never-treated units, \( N_0 \equiv N - \sum_i W_i > 0 \), and we let \( J = N - N_0 = \sum_i W_i \). To clearly differentiate ever treated units, we index them by \( j = 1, \ldots, J \).

For treated units, we require a sufficient number of time periods both before and after treatment: we assume that \( T_1 \gg 1 \) and \( T_J \leq T - K \) for some \( K > 0 \), representing the longest lagged treatment effect we will examine.\(^2\)

We adopt a potential outcomes framework to express causal quantities (Neyman, 1923; Rubin, 1974) and assume stable treatment and no interference between units (SUTVA; Rubin, 1980). In principle, each unit \( i \) in each time \( t \) might have a distinct potential outcome for each potential treatment time \( s \), \( Y_{it}(s) \), for \( s = 1, \ldots, T, \infty \). Following Athey and Imbens (2018), we adopt two assumptions that impose mild substantive restrictions but dramatically simplify the notation. First, we assume “no anticipation”: prior to treatment, a unit’s potential outcomes are equal to the control potential outcome, i.e. \( Y_{it}(s) = Y_{it}(0) \) for \( t < s \), with treatment time \( s \). Second, we assume “invariance to history”: following treatment, a unit’s potential outcomes are equal to the treated potential outcome, \( Y_{it}(s) = Y_{it}(1) \) for any \( 0 < s \leq t \), and do not depend on the timing of treatment.

\(^2\)This ensures that we observe both pre-treatment outcomes and the outcome measures of interest for all treated units, and that there are untreated comparisons for even the last treated units; \( N_0 = 17, T_1 = 6, \) and \( K = 10 \) in our application. See Athey and Imbens (2018) for a discussion of the setting in which all units eventually adopt treatment.
These assumptions allow us to use just two potential outcomes, \( Y_{it}(0) \) and \( Y_{it}(1) \); the observed outcome is

\[
Y_{it} = 1\{t < T_i\}Y_{it}(0) + 1\{t \geq T_i\}Y_{it}(1)
\]

for units with \( W_i = 1 \) and is \( Y_{it} = Y_{it}(0) \) for all \( t \) for units with \( W_i = 0 \). The first assumption is relatively innocuous, and is a generalization of the SUTVA assumption typically employed in cross-sectional studies (Rubin, 1980). The second is stronger, ruling out treatment effects that phase in over time. However, this is not too restrictive since we do not restrict how \( \{Y_{it}(0), Y_{it}(1)\} \) vary across units. See Imai and Kim (2019) for related discussion.

2.2 Estimands

As is common in event studies, we focus on effects a specified period after treatment onset. For treated unit \( j \), we index “event time” relative to treatment time \( T_j \) by \( k = t - T_j \). The unit-level treatment effect for treated unit \( j \) at event time \( k \geq 0 \) is:

\[
\tau_{jk} = Y_{j,T_j+k}(1) - Y_{j,T_j+k}(0).
\]

Our primary estimand of interest is the Average Treatment Effect on the Treated \( k \) periods after treatment onset, sometimes called the “event study” or “dynamic” ATT (Abraham and Sun, 2018):

\[
\text{ATT}_k \equiv \frac{1}{J} \sum_{j=1}^{J} \tau_{jk} = \frac{1}{J} \sum_{j=1}^{J} Y_{j,T_j+k}(1) - Y_{j,T_j+k}(0).
\]

We are also interested in the average post-treatment effect, averaging across \( k \): \( \text{ATT} = \frac{1}{K} \sum_{k=1}^{K} \text{ATT}_k \).

Our methods generalize to many other estimands; see Callaway and Sant’Anna (2018) for examples in this setting.

We observe all treated potential outcomes for treated units post-treatment adoption; that is, \( Y_{j,T_j+k}(1) \) is observed for \( k \leq K \leq T - T_j \) for all \( W_j = 1 \). The key challenge is therefore to impute the average of the missing un-treated potential outcomes:

\[
\mu_k \equiv \frac{1}{J} \sum_{j=1}^{J} Y_{j,T_j+k}(0).
\]

It is useful to define the set of not yet treated units, which are the potential “donor units” for SCM. For fixed event time \( k \), the possible donor units for treated unit \( j \) are those units that are either never treated or are not yet treated by time \( T_j + k \). We denote these as \( D_{jk} = \{i : W_i = 0 \text{ or } T_i > T_j + k\} \). For a given treated unit \( j \), \( D_{jk} \) can differ across event time \( k \) since units adopt treatment over time; however we use a fixed set of donors for each treated unit. Following Paglayan (2019), we set the maximum value of leads to \( K = 10 \), and restrict our attention to the set \( D_j \equiv D_{jK} \). This reduces the number of available donor units but simplifies both estimation and
exposition.

Finally, auxiliary covariates play an important role in many panel data settings. Consistent with the analysis in Paglayan (2019), we do not include such covariates here. However, it is straightforward to extend our results to consider auxiliary covariates in parallel to the lagged outcomes. See Ben-Michael et al. (2019) for additional discussion.

### 2.3 SCM for a single treated unit

In the synthetic control method, the counterfactual outcome under control is estimated from a weighted average, known as a synthetic control of untreated units, where weights are chosen to minimize the squared imbalance between the lagged outcomes for the treated unit and the weighted control (“donor”) units.

For a fixed treated unit $j$, we consider a modified version of the original SCM estimator of Abadie et al. (2010, 2015). In this version, the SCM weights $\hat{\gamma}_j$ for treated unit $j$ are the solution to a constrained optimization problem:

$$
\min_{\gamma_j \in \Delta_{scm}^j} \frac{1}{2(T_j - 1)} \sum_{t=1}^{T_j-1} \left( Y_{j,T_j-t} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-t} \right)^2 + \lambda \sum_{i=1}^{N} f(\gamma_{ij}),
$$

(1)

where $\gamma_j \in \Delta_{scm}^j$ is an $N$-vector that represents SCM donor unit weights, with elements $\{\gamma_{ij}\}$ that satisfy $\gamma_{ij} \geq 0$ for all $i$, $\sum_i \gamma_{ij} = 1$, and $\gamma_{ij} = 0$ whenever $i$ is not a possible donor, $i \notin D_j$.3

Equation (1) modifies the original SCM proposal in two key ways. First, where Abadie et al. (2010, 2015) balance auxiliary covariates, we focus exclusively on lagged outcomes. Second, following a suggestion in Abadie et al. (2015), we include a term that penalizes the weights toward uniformity, with hyperparameter $\lambda$; see Doudchenko and Imbens (2017); Abadie and L’Hour (2018). In settings with perfect pre-treatment fit, the choice of penalty can be important as Equation (1) may not have a unique solution for $\lambda = 0$. This is not the case in our setting, however, and so we largely view this term as a technical convenience.

The SCM estimate of the missing potential outcome for treated unit $j$ at event time $k$, $Y_{j,T_j+k}(0)$, is then:

$$
\hat{Y}_{j,T_j+k}(0) = \sum_{i=1}^{N} \hat{\gamma}_{ij} Y_{i,T_j+k},
$$

with estimated treatment effect $\hat{\tau}_{jk}^{scm} = Y_{j,T_j+k} - \hat{Y}_{j,T_j+k}(0)$. Thus, the optimization problem (1) minimizes the $L_2$ norm of the imbalance between the treated unit and the synthetic control over the pre-treatment period. Alternatively, it can be seen as minimizing the sum of squared placebo

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3Recall that the possible donor units for unit $j$, $D_j$, are the not yet treated units, defined as either never-treated units or units that have not yet been treated at time $T_j + K$ where we set $K = 10$. Thus, the set of possible donor states for Michigan, which adopted mandatory collective bargaining in 1965, includes the never-treated states as well as Nebraska, which also adopted mandatory collective bargaining in 1987.
treatment effects on pre-treatment outcomes.

A central question for SCM is assessing whether \( \hat{Y}_{j,T_j+k}(0) \) is a reasonable estimate for \( Y_{j,T_j+k}(0) \). Abadie et al. (2010) argue that, in addition to other model checks, SCM will be a compelling estimator when the placebo estimates are close to zero, i.e. \( Y_{j,T_j-\ell} - \hat{Y}_{j,T_j-\ell} \approx 0 \) for all lags \( \ell \). Accordingly, Abadie et al. (2010, 2015) recommend only proceeding with an SCM analysis if the pre-treatment fit is excellent. Figure 2a shows SCM “gap plots,” of \( Y_{j,T_j-\ell} - \hat{Y}_{j,T_j-\ell} \) against \( \ell \) for three illustrative treated states taken one at a time. Ohio shows relatively good pre-treatment fit; however, the SCM estimates for Illinois and New York fail to closely track those states’ pre-treatment outcomes, suggesting SCM is likely to give misleading estimates for these states.

3 Generalizing to staggered adoption: Partially Pooled SCM

We now extend SCM to the staggered adoption setting. The literature provides little guidance about how to do this, and applied researchers have used a range of ad hoc approaches. We start by outlining two immediate generalizations: separate SCM, which estimates weights that separately minimize the pre-treatment imbalance for each treated unit; and pooled SCM, which estimates weights that minimize the average pre-treatment imbalance across all treated units. Both approaches have drawbacks, however. Separate SCM can lead to poor fit for the average of the treated units, leading to bias in the estimated ATT. Pooled SCM, by contrast, can achieve nearly perfect fit for the average but yields substantially worse state-specific fits, making the estimator more susceptible to interpolation bias from non-linearity and when both treatment adoption and the outcome process vary across time. Recognizing this, we then propose partially pooled SCM, which finds intermediate weights between these two extremes. We turn to the theoretical properties of this approach in the next section.

3.1 Separate SCM for each treated unit

A number of applied researchers have confronted the problem of using SCM when there are multiple treated units with staggered adoption. These studies have taken each treated unit one at a time, forming a separate synthetic control for each, and then estimating the ATT by averaging the unit-specific SCM estimates (see, for example, Dube and Zipperer, 2015; Donohue et al., 2019). We can re-write this separate SCM strategy as solving a single joint optimization problem over a matrix
of weights $\Gamma = [\gamma_1, \ldots, \gamma_J] \in \mathbb{R}^{N \times J}$:

$$\min_{\gamma_1, \ldots, \gamma_J \in \Delta_{scm}} \frac{1}{2J} \sum_{j=1}^{J} \left[ \frac{1}{T_j - 1} \sum_{\ell=1}^{T_j - 1} \left( Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \right)^2 + \lambda \sum_{j=1}^{J} \sum_{i=1}^{N} f(\gamma_{ij}) \right],$$

where $q^{sep}$ is the average pre-intervention mean square error across the $J$ treated units. The estimated ATT is then:

$$\hat{\text{ATT}}_k = \frac{1}{J} \sum_{j=1}^{J} \left[ Y_{j,T_j+k} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j+k} \right] = \frac{1}{J} \sum_{j=1}^{J} Y_{j,T_j+k} - \frac{1}{J} \sum_{j=1}^{J} \hat{Y}_{j,T_j+k}(0),$$

where the last term imputes the missing (average) potential outcome by averaging over the unit-specific SCM estimates.

As with SCM for a single treated unit, an important question is when this separate SCM strategy will yield a reasonable estimate of $\hat{\text{ATT}}_k$. One possible justification is that $\hat{\text{ATT}}_k$ will be an unbiased estimate of $\text{ATT}_k$ if the set of the state-specific SCM estimates $\{\hat{Y}_{j,T_j+k(0)}\}_j$ are all unbiased for the corresponding potential outcomes $\{Y_{j,T_j+k(0)}\}_j$. However, Figure 2a, which plots the placebo gaps for three example states, shows this is not the case in our application. This suggests that the separate SCM strategy will not yield convincing estimates in our setting. We expect that in many applications there will be several treated units with poor pre-treatment fit (see e.g. Dube and Zipperer, 2015; Donohue et al., 2019). This motivates the search for other approaches.

### 3.2 Pooled SCM

An alternative strategy is to estimate weights that balance the average across treated units directly. We call this pooled SCM. Specifically, we modify the separate SCM problem in Equation (2) to:

$$\min_{\gamma_1, \ldots, \gamma_J \in \Delta_{scm}} \frac{1}{L} \sum_{\ell=1}^{L} \sum_{T_j > \ell} \left[ Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \right]^2 + \lambda \sum_{j=1}^{J} \sum_{i=1}^{N} f(\gamma_{ij}),$$

where $L = T_J - 1$ is the maximum number of observed lags, $q^{pool}$ is the imbalance between the average synthetic control and the average treated unit at each lag $\ell$, summed over the possible $\ell$, \footnote{Recall that we adopt the convention that each $\gamma_j$ is an $N$-vector, with entries corresponding to inadmissible donor units fixed at zero. We fix the same value of the hyper-parameter $\lambda$ across all problems. It is straightforward to generalize this to a separate $\lambda_j$ for each treated unit, but complicates the exposition. In principle, we could also give different weights to different units in the ATT, for example, prioritizing larger states over smaller states or weighting units by dose (e.g., Dube and Zipperer, 2015).}
(a) SCM “gap plots” for three illustrative states  
(b) SCM pre-treatment fits by state  

Figure 2: (a) The SCM pre-treatment fit for Ohio is good overall. The pre-treatment fit for Illinois is poor: the SCM estimate fails to match an important pre-treatment trend. The pre-treatment fit for New York is quite bad: the pre-treatment imbalance for New York is roughly an order of magnitude larger than typical estimates for the impact of teacher mandatory bargaining. (b) SCM fits by state show that Separate SCM gives better pre-treatment fit for all treated states.

(a) Separate SCM  
(b) Pooled SCM  

Figure 3: Estimated ATT on per-pupil expenditure (log, 2010 $) using (a) separate SCM, and (b) pooled SCM.
and the weights are again constrained to be non-negative, to sum to one for each \( j \), and to be zero for any \( i \) not in the set of donors \( D_j \). Intuitively, by minimizing \( q_{\text{pool}} \) the pooled SCM approach finds weights that minimize the placebo estimates for the ATT, rather than weights that minimize the average unit-specific placebo estimates, as in \( q_{\text{sep}} \). We can see this in Figure 3, which shows the implied placebo estimates for the ATT using the two approaches: The placebo ATT estimates are consistently positive for separate SCM weights and are all nearly identical to zero for pooled SCM weights.

At the same time, the pooled SCM weights generally yield worse state-specific fits, which do not enter the objective function in Equation (4). Figure 2b plots the state-level pre-treatment imbalances for separate SCM vs pooled SCM, showing that the separate SCM fit is better for all treated states. The resulting estimator is therefore more susceptible to interpolation biases due to non-linearity (see e.g. Abadie and L’Hour, 2018, for a setting with abundant micro data). Furthermore, as we show through simulation in Appendix B, even under linearity the pooled SCM estimator can be poor when both treatment adoption and the outcome process vary over time.

### 3.3 Partially pooled SCM

We can now define our main proposal, *partially pooled SCM*, which finds weights that minimize a convex combination of state-level imbalance, \( q_{\text{sep}} \), and pooled imbalance, \( q_{\text{pool}} \):

\[
\min_{\gamma_1, \ldots, \gamma_J \in \Delta_{\text{scm}}} \frac{\nu}{2} q_{\text{pool}}(\Gamma) + \frac{1 - \nu}{2} q_{\text{sep}}(\Gamma) + \lambda \sum_{j=1}^{J} \sum_{i=1}^{N} f(\gamma_{ij}),
\]

with hyperparameter \( \nu \in [0, 1] \). This optimization problem nests both the separate SCM approach (2) with \( \nu = 0 \) and the pooled SCM approach (4) with \( \nu = 1 \). In the next section, we show that intermediate values of \( \nu \) correspond to a *partial pooling* solution for the \( \gamma \) weights in the dual parameter space, and discuss the specific choice of \( \nu \).

Figure 4a shows the *balance possibility frontier* for all \( \nu \): the impact of changing \( \nu \) on the pooled imbalance \( q_{\text{pool}} \) (the y-axis) and on the average state-level imbalance \( q_{\text{sep}} \) (the x-axis) for the teacher collective bargaining application.\(^5\) The end points, \( \nu = 0 \) and \( \nu = 1 \) correspond to separate SCM and pooled SCM, respectively. As \( \nu \) rises, pooled imbalance falls while state-level imbalance rises, though at different rates. Moving from the separate SCM estimate of \( \nu = 0 \) to a partially pooled SCM estimate of \( \nu = 0.5 \) reduces the pooled imbalance by over 90 percent, with more modest further reductions as \( \nu \to 1 \). This is consistent with Figure 2, which shows poor fit for \( \nu = 0 \) and nearly perfect pre-treatment fit for \( \nu = 1 \).

Meanwhile, average state-level imbalance increases relatively slowly as \( \nu \) rises from 0 to 0.5, increasing more rapidly as \( \nu \) nears its upper limit. Even a very small deviation from the pooled

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\(^5\)See King et al. (2017) and Pimentel and Kelz (2019) for other examples of balance frontiers in observational settings.
Figure 4: (a) Partially pooled SCM estimates for per-pupil current expenditures (log, 2010 $), with the heuristic \( \hat{\nu} = \sqrt{q_{\text{pool}}} / \sqrt{q_{\text{sep}}} \approx 0.44 \); see Section 4.1. (b) The trade-off between pooled imbalance and state-specific imbalance, where \( \nu = 0 \) is the separate SCM solution and \( \nu = 1 \) is the pooled SCM solution. The large distance in average state-level imbalance between \( \nu = 0.99 \) and \( \nu = 1 \) suggest meaningful gains in balance from deviating from the complete pooling estimate even by a small amount.

SCM solution, such as from \( \nu = 1 \) to \( \nu = 0.99 \), cuts the average state-level imbalance by roughly one-fifth with essentially no change in the overall imbalance. Due to the number of degrees of freedom involved, in many cases the pooled imbalance will be near zero for \( \nu = 1 \), and the objective function \( q_{\text{pool}} \) will be relatively flat in the neighborhood of the pooled solution. Therefore we expect that in many cases it will be possible to trade off a small increase in pooled imbalance for a large decrease in the state-level imbalance, adding robustness to the estimator at relatively little cost.

Based on Figure 4a, the intermediate estimate with \( \nu = 0.5 \) has very similar global pre-treatment imbalance to the fully pooled estimator, \( \nu = 1 \), with only a modest increase in state-level imbalance relative to the separate SCM estimate, \( \nu = 0 \). This is reflected in Figure 4b, which shows the placebo ATT estimates for partially pooled SCM. While the imbalance for the ATT is slightly larger than for pooled SCM, it is substantially better than for separate SCM.

We can now turn to the ATT estimates themselves. Figure 4b shows \( \hat{\text{ATT}}_k \) for \( k \in [0, 10] \) for partially pooled SCM. Following Arkhangelsky et al. (2019), we quantify uncertainty using the (leave-one-unit-out) jackknife to estimate standard errors and (asymptotic) Normality to compute 95% confidence intervals.\(^6\) Similar to the estimates from separate SCM and pooled SCM — and

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\(^6\)The theoretical results in Arkhangelsky et al. (2019) focus on the setting with simultaneous adoption; we leave the formal extensions to this setting for future work. While we do not pursue it here, we anticipate that we could also apply the influence function-based inference method in Callaway and Sant’Anna (2018), especially for the weighted...
consistent with Paglayan (2019) — we find no effect of mandatory teacher collective bargaining laws on student expenditures. We explore additional analyses in Section 7.

4 Theoretical results for partially pooled SCM

This section explores some of the theoretical properties of SCM under staggered adoption, especially partially pooled SCM. First, we show that, under a linear factor model, the error of the ATT estimate decomposes into error stemming from imperfection of the pooled fit and from imperfections in the state-specific fits. By minimizing a weighted average of these quantities, partially pooled SCM, with appropriately chosen \( \nu \), can thus directly control the corresponding bias. We also motivate partially pooled SCM by examining the Lagrangian dual of the constrained optimization problem, showing that method partially pools parameters in the dual parameter space.

4.1 Pre-treatment fit and bias under a linear factor model

We consider the bias of a generic weighting estimator under staggered adoption. Following the recent literature on panel data methods, we consider data generated by a linear factor model; in Appendix D.2.1 we show analogous results for a time-varying autoregressive process. For ease of exposition we consider the case where we balance the first \( t = 1, \ldots, L \) time periods for each unit, and we focus on the (absolute) error in estimating the ATT at event time \( k \), \( \left| \hat{\text{ATT}}_k - \text{ATT}_k \right| \).

We assume that there are \( F \) latent time-varying factors, where \( F \) is typically small relative to both \( N \) and \( T \). We let \( \mu_t \in \mathbb{R}^F \) represent the vector of factor values at time \( t \), and assume it is bounded: \( \max_t \| \mu_t \|_\infty \leq M \). Each unit has a vector of time-invariant factor loadings \( \phi_i \in \mathbb{R}^F \), and the control potential outcomes are generated as:

\[
Y_{it}(0) = \phi_i^T \mu_t + \varepsilon_{it},
\]

where \( \varepsilon_{it} \) is independent, sub-Gaussian additive noise with scale parameter \( \sigma \).

Two summaries of the factor values at the various treatment times will be important to our analysis: \( \bar{\mu}_k = \frac{1}{J} \sum_{j=1}^J \mu_{T_j+k} \in \mathbb{R}^F \), the average factor value across the \( J \) treatment times, and \( S_k^2 = \frac{1}{J} \sum_{j=1}^J \| \mu_{T_j+k} - \bar{\mu}_k \|_2^2 \), the corresponding variance. Together, the relative values of the magnitude of the mean \( \| \bar{\mu}_k \|_2 \) and the standard deviation \( S_k \) measure the amount of heterogeneity in the factors over treatment times.\(^7\) Our first result is that under the standard linear factor model the error bound for the ATT depends on both the pooled pre-treatment fit and the state-specific pre-treatment fits, where \( \| \bar{\mu}_k \|_2 \) and \( S_k \) control their relative importance for the bound.

\(^7\)For example, if all treatment times are the same, \( T_1 = \ldots = T_J \), then the standard deviation \( S_k = 0 \). Similarly, in the special case of a unit fixed effects model (e.g. a single, time constant factor) the standard deviation is also zero. Conversely, if the factor values vary widely over time, the standard deviation \( S_k \) will be large relative to the magnitude of the average factor value \( \| \bar{\mu}_k \|_2 \).
**Theorem 1.** For \( \hat{\gamma}_1, \ldots, \hat{\gamma}_J \in \Delta_{scm} \) where \( \hat{\gamma}_j \) is independent of \( \varepsilon \cdot T_{j,+} \) and \( \delta > 0 \), if \( Y_{it}(0) \) follows a linear factor model (6) the error for \( \widehat{ATT}_k \) is

\[
\left| \widehat{ATT}_k - ATT_k \right| \leq \frac{M\sqrt{F}}{\sqrt{L}} \left( \left\| \hat{\mu}_k \right\|_2 \sum_{t=1}^L \left( \frac{1}{J} \sum_{j=1}^J Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2 \right) + S_k \left( \frac{1}{J} \sum_{j=1}^J \sum_{t=1}^L \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2 \right)
\]

\[
+ \frac{\sigma M^2 F}{\sqrt{L}} \left( 3\delta + 2\sqrt{\log NJ} \right) + \frac{\delta \sigma}{\sqrt{J}} \left( 1 + \|\hat{\Gamma}\|_F \right)
\]

with probability at least \( 1 - 6e^{-\frac{\delta^2}{2}} \), where \( \max_t \|\mu_t\|_{\infty} \leq M \) and \( \|\Gamma\|_F^2 = \sum_{j=1}^J \sum_{i=1}^N \gamma_{ij}^2 \) is the Frobenius norm of the weights.

Theorem 1 shows that the error for the ATT is bounded by several distinct terms, each of which can be controlled by the partially pooled optimization problem (5). First, we can directly control the variance term due to noise by penalizing the dispersion of the weights, e.g., \( \|\hat{\mu}_k\|_2 \). Second, there is an approximation error that arises due to balancing — and possibly over-fitting to — noisy outcomes, rather than to the true underlying factor loadings. In the worst case, the \( J \) synthetic controls put maximal weight on the control units with the largest noise. Constraining the weights to lie in the simplex reduces the impact of this worst case, however, and the error decreases as more lagged outcomes are balanced; see Abadie et al. (2010); Ben-Michael et al. (2019); Arkhangelsky et al. (2019) for further discussion.

We are most interested in the bias arising from the pooled and state-specific fits. Theorem 1 shows that the bias in a weighting estimator for the ATT is controlled by choosing weights that optimize a weighted sum of these two pre-treatment fits. The relative importance of these fits is governed by the ratio of the average factor value \( \|\bar{\mu}_k\|_2 \) and the factor standard deviation \( S_k \). When the average factor value is large relative to the standard deviation, then the level of pooled fit is more important than the state-specific fits. Conversely, when the factors vary widely over treatment times then the state-specific fits outweigh the pooled fit. In the special case where \( S_k = 0 \), such as when all treatment times are the same or under a unit (one-way) fixed effects model, Theorem 1 shows that only the pooled level of fit is important for bias, at least under a linear factor model. In Appendix D.2.1 we show that a time-varying auto-regressive model also exhibits this behavior.

Following Theorem 1, the (infeasible) optimization problem would choose hyperparameter \( \nu = \frac{\|\bar{\mu}_k\|_2}{\|\mu_k\|_2 + S_k} \) for some \( k \), minimizing the first two terms in Equation (7). However, in general both \( \|\bar{\mu}_k\|_2 \) and \( S_k \) are unknown, so we propose a heuristic to set \( \nu \) based on the ratio of \( q_{pool} \) and \( q_{sep} \). First we solve the partially pooled SCM problem (5) with \( \nu = 0 \) (i.e. the separate SCM problem),
then set $\nu$ to be the ratio of $q_{\text{pool}}$ and $q_{\text{sep}}$: $\hat{\nu} = \sqrt{q_{\text{pool}}} / \sqrt{q_{\text{sep}}} \in [0, 1]$.\footnote{Note that by the triangle inequality, $\sqrt{q_{\text{pool}}} \leq \sqrt{q_{\text{sep}}}$. Thus their ratio is bounded above by 1. If the SCM fits are perfect for each state, $q_{\text{sep}} = 0$, then the overall fit will also be perfect, $q_{\text{pool}} = 0$, and we define $\nu = 0$. This is not a common situation.} If the separate SCM problem (2) achieves good pooled balance on its own, this approach will set a small $\nu$. Conversely, if the pooled balance is poor, $\nu$ will be large in order to account for this discrepancy. In the teacher bargaining example, we choose $\hat{\nu} \approx 0.44$ for the per-pupil expenditure outcome, which is close to the $\nu = 0.5$ plotted in Figure 4a.

4.2 Partially pooled SCM: Dual shrinkage

We now inspect the Lagrangian dual problem to the partially pooled SCM problem (5), showing that the optimization problem partially pools a set of state-specific dual variables toward global dual variables. We focus on balancing the first $L = T_1 - 1$ lagged outcomes, which are observed for each treated unit; see Appendix D.1.1 for the general case.

For each treated unit $j$, the sum-to-one constraint induces a Lagrange multiplier $\alpha_j \in \mathbb{R}$, and the state-level balance measure induces a set of Lagrange multipliers $\beta_j \in \mathbb{R}^L$, with elements $\beta_{\ell j}$. We combine these dual parameters into a vector $\alpha = [\alpha_1, \ldots, \alpha_J] \in \mathbb{R}^J$ and a matrix $\beta = [\beta_1, \ldots, \beta_J] \in \mathbb{R}^{L \times J}$. In addition to the $J$ sets of Lagrange multipliers — one for each treated unit — the pooled balance measure in the partially pooled SCM problem Equation (5) induces a set of global Lagrange multipliers $\mu_\beta \in \mathbb{R}^L$. As we see in the following proposition, in the dual problem the parameters $\beta_1, \ldots, \beta_J$ are regularized toward this set of pooled Lagrange multipliers, $\mu_\beta$.

**Proposition 1.** The Lagrangian dual to Equation (5) is:

$$\min_{\alpha, \mu_\beta, \beta} \mathcal{L}(\alpha, \beta) + \frac{\lambda L}{2(1 - \nu)} \sum_{j=1}^J \left\| \beta_j - \mu_\beta \right\|_2^2 + \frac{\lambda L}{2\nu} \left\| \mu_\beta \right\|_2^2. \quad (8)$$

Where the dual objective function is

$$\mathcal{L}(\alpha, \beta) \equiv \sum_{j=1}^J \left[ \sum_{W_i = 0} f^* \left( \alpha_j + \sum_{\ell=1}^L \beta_{\ell j} Y_{i,T_j-\ell} \right) - \left( \alpha_j + \sum_{\ell=1}^L \beta_{\ell j} Y_{j,T_j-\ell} \right) \right], \quad (9)$$

and $f^* (y) = \sup_{x} x'y - f(x)$ is the convex conjugate of $f$.\footnote{For example, if $f(x) = x \log x$ is an entropy penalty, then $f^* (y) = \exp(y - 1)$ is an exponential. If $f(x) = \frac{1}{2} x^2$, then $f^* (y) = \frac{1}{2} y^2$.} For treated unit $j$, the synthetic control weight on unit $i$ is $\hat{\gamma}_{ij} = f^{**} \left( \hat{\alpha}_j + \sum_{\ell=1}^L \hat{\beta}_{\ell j} Y_{j,T_j-\ell} \right)$.

The dual objective function (9) is an example of a calibrated loss function for propensity score parameters (see e.g. Zhao et al., 2019; Wang and Zubizarreta, 2019); we develop this propensity score connection in Appendix A. More relevant for our purposes is the form of the regularization
terms in (8). Proposition 1 highlights that the estimator partially pools the individual synthetic controls to the pooled synthetic control in the dual parameter space, with \( \nu \) controlling the level of pooling. When \( \nu = 0 \) in the separate SCM problem, the parameters \( \beta_1, \ldots, \beta_J \) are shrunk towards zero rather than a set of global parameters. By contrast, when \( \nu = 1 \), \( \beta_1, \ldots, \beta_J \) are constrained to be equal to \( \mu_\beta \), fitting a single pooled synthetic control in the dual parameter space. By choosing \( \nu \in (0, 1) \), we move continuously between the two extremes of \( J \) separate Lagrangian dual problems and a single dual problem, regularizing the individual \( \beta_j \)'s toward the pooled \( \mu_\beta \), allowing for some limited differences between the \( J \) dual parameters.

5 Combining SCM and outcome modeling

We have established that the partially pooled SCM estimator achieves nearly as good overall balance as the fully pooled estimator, while achieving much better balance for each state. Nevertheless, balance will typically be imperfect, especially at the state level. We now combine partially pooled SCM with outcome modeling, which can correct for imperfect pre-treatment balance in the SCM estimator (e.g., Ben-Michael et al., 2019). We first describe the general framework to combine SCM with an arbitrary panel data imputation method. We then focus on augmentation with a (possibly weighted) average of pre-treatment outcomes, which we refer to as a weighted event study.

5.1 Augmentation with generic panel data methods

Constructing the augmented estimator proceeds in three steps. First, we consider a working model for the potential outcome under control, \( k \) periods after treatment time \( T_j \); \( Y_{i,T_j+k} (0) = m_{ijk} + \varepsilon_{i,T_j+k} \); we give specific examples below. We estimate \( m_{ijk} \) with a pilot estimate \( \hat{m}_{ijk} \), and define the corresponding residuals, \( \hat{Y}_{i,T_j+k} \equiv Y_{i,T_j+k} - \hat{m}_{ijk} \) for event time \( k \). Second, we estimate SCM weights \( \hat{\gamma}^*_{ij} \) using these residuals, i.e., by modifying the balance criteria \( q_{pool} \) and \( q_{sep} \) in Equation (5) to depend on the residuals \( \{\hat{Y}_{i,T_j+k}\} \) rather than “raw” \( \{Y_{i,T_j+k}\} \). Finally, we impute the counterfactual for treated unit \( j \), \( k \) periods after treatment as:

\[
\hat{Y}^{aug}_{j,T_j+k} = \sum_{i=1}^{n} \hat{\gamma}^*_{ij} Y_{i,T_j+k} + \left( \hat{m}_{jjk} - \sum_{i=1}^{n} \hat{\gamma}^*_{ij} \hat{m}_{ijk} \right) = \hat{m}_{jjk} + \sum_{i=1}^{n} \hat{\gamma}^*_{ij} (Y_{i,T_j+k} - \hat{m}_{ijk}) .
\]

Following Ben-Michael et al. (2019), we can view this approach as analogous to bias correction for matching (Rubin, 1973; Abadie and Imbens, 2011), where \( \hat{m}_{jjk} - \sum_{i=1}^{n} \hat{\gamma}^*_{ij} \hat{m}_{ijk} \) is an estimate of the bias. As with partially pooled SCM, we then estimate \( \hat{\text{ATT}}_k^{aug} = \frac{1}{J} \sum_{j=1}^{J} \hat{\tau}^{aug}_j \).

This formulation is quite general and can accommodate any panel data imputation method for the pilot estimate \( \hat{m}_{ijk} \). We focus next on simple outcome models, especially unit fixed effects.
More broadly, however, we can estimate the factor model (6) directly, as in the generalized SCM approach of Xu (2017) and set the pilot estimate to be the imputed counterfactual \( \hat{m}_{ijk} = \hat{\phi}_i \hat{\mu}_{T_j + k} \). Alternatively, we can estimate \( \hat{m}_{ijk} \) using a direct matrix completion approach (Hastie et al., 2015; Athey et al., 2017). We inspect the performance of augmenting SCM with a factor model through simulation in Section 6 and apply it to the teacher collective bargaining example in Section 7.

### 5.2 Weighted event studies

Our primary recommendation is to augment partially pooled SCM with a (possibly weighted) average of pre-treatment outcomes, which we refer to as a weighted event study; see Abraham and Sun (2018); Callaway and Sant’Anna (2018) for further discussion of event study models. Here, the pilot estimate for unit \( i \), \( k \) periods after treatment time \( T_j \) is a weighted average of the pre-treatment outcomes:

\[
\hat{m}_{ijk} = \hat{\eta}_j Y_{i,T_j}^\text{pre} \equiv \sum_{\ell=1}^{T_j-1} \hat{\eta}_{j\ell} Y_{i,T_j - \ell},
\]

where the weights \( \eta_j \) need not be on the simplex. The treatment effect estimates for \( \tau_{jk} \), the impact for treated unit \( j \) at event time \( k \), have a particularly useful form:

\[
\hat{\tau}_{jk}^\text{aug} = \left( Y_{j,T_j + k} - \sum_{\ell=1}^{T_j-1} \hat{\eta}_{j\ell} Y_{j,T_j - \ell} \right) - \sum_{i=1}^{N} \hat{\gamma}_{ij} \left( Y_{i,T_j + k} - \sum_{\ell=1}^{T_j-1} \hat{\eta}_{j\ell} Y_{i,T_j - \ell} \right),
\]

where \( \hat{\text{ATT}}_{jk}^\text{aug} \) is the simple average of \( \hat{\tau}_{jk}^\text{aug} \) over treated units \( j \). We can view this approach as a weighted average over all possible two-period, two-group difference-in-differences estimates. Specifically, the base difference-in-differences estimate compares the “single difference” in outcomes for treated unit \( j \) at two time points, \( Y_{j,T_j + k} - Y_{j,T_j - \ell} \), to the “single difference” in outcomes for donor unit \( i \) at the same time points, \( Y_{i,T_j + k} - Y_{i,T_j - \ell} \). Equation (12) fixes treated unit \( j \) and event time \( k \), but then takes a double-weighted average, first over pre-treatment periods to form a “synthetic pre treatment time period”, then over donor units to track pre-intervention trends (see Arkhangelsky et al., 2019, for additional discussion).

Our default approach is to set uniform weights over time periods, \( \hat{\eta}_{j\ell} = \frac{1}{T_j - 1} \):

\[
\hat{\tau}_{jk}^\text{aug} = \frac{1}{T_j - 1} \sum_{\ell=1}^{T_j-1} \left( Y_{j,T_j + k} - Y_{j,T_j - \ell} \right) - \sum_{i=1}^{N} \hat{\gamma}_{ij} \left( Y_{i,T_j + k} - Y_{i,T_j - \ell} \right),
\]

which is equivalent to augmenting SCM with a unit fixed effects model, \( m_{ijk} = \frac{1}{T_j - 1} \sum_{\ell=1}^{T_j-1} Y_{i,T_j - \ell} \). This approach extends the intercept-shifted or de-meaned SCM estimator, which has attractive robustness properties (Doudchenko and Imbens, 2017; Ferman and Pinto, 2018), to the staggered adoption setting.
Figure 5: (a) The balance possibility frontier for SCM alone and for the weighted event study model, which combines SCM and fixed effects, as well as the implied imbalance for fixed effects alone. Incorporating unit-level fixed effects leads to substantial improvements in balance. We use Equation (12) to estimate the event study estimator and compute the implied balance as $\sqrt{\sum_{\ell=2}^L \hat{\delta}_\ell^2}$, the RMSE of the placebo estimates. (b) Weighted event study estimates for per-pupil current expenditure (log, 2010 $).

A second special case is the unweighted event study model that imposes uniform weights over units, $\hat{\gamma}_{ij}^* = 1/\|D_j\|$, as well as over time periods. In this form, Equation (12) is the simple average over all two-period, two-group DID estimates averaged over all pre-treatment lags $\ell$ and donor units $i$.\footnote{This parallels recent proposals from, among others, Abraham and Sun (2018) and Callaway and Sant’Anna (2018). We can also consider estimating $\eta_t$ rather than restricting them to be uniform. Following Ben-Michael et al. (2019), we could estimate these weights via ridge regression, which would allow for negative weights; we could also restrict these weights to be on the simplex as in Arkhangelsky et al. (2019). We leave a thorough analysis of these estimators to future work.}

Figure 5 shows the weighted event study estimates for the teacher collective bargaining application, with $\hat{\nu}$ chosen by applying the procedure in Section 3.3 to the residuals $\hat{Y}$. Figure 5a shows the balance possibility frontier for SCM alone and for the weighted event study estimator, as well as the implied imbalance for the event study estimator alone. The frontier for the weighted event study estimator is a clear improvement over either the FE or SCM estimates alone, regardless of the level of tuning parameter $\nu$. We see similar results when examining the state-specific fits; see, for example, Appendix Figure C.4. The left of Figure 5b shows the placebo estimates from Equation (12), where $k < 0$.\footnote{These placebo checks differ from those typically performed in traditional event studies, which test for the parallel
either the event study model or SCM alone. As with the estimates for partially pooled SCM alone, the weighted event study estimates show no impact of mandatory teacher collective bargaining on student expenditures.

6 Simulation study

We now consider the performance of different approaches in a simulation study calibrated to the collective bargaining dataset; we turn to the impacts of mandatory teacher collective bargaining laws in the actual data in the next section. We use the Generalized Synthetic Control Method, implemented in the R package `gsynth` (Xu, 2017) to estimate the parameters of simple data generating processes that best fit these data. Specifically, we estimate an interactive two-way fixed effects model with a 2-dimensional latent time-varying factor \( \mu_t \in \mathbb{R}^2 \) and unit-specific coefficients \( \phi_i \in \mathbb{R}^2 \):

\[
Y_{it} = \text{int} + \text{unit}_i + \text{time}_t + \phi_i' \mu_t + \varepsilon_{it}.
\] (14)

We estimate (14) using untreated units and time periods, then estimate the variance-covariance matrix of the unit fixed effects and factor loadings, \( \hat{\Sigma} \), and the variance of the error term \( \hat{\sigma}^2 \). We then generate simulated data sets with the same dimensions as the data, \( N = 49 \) and \( T = 39 \), using the estimated \( \{\hat{\text{time}}_t, \hat{\mu}_t\} \), and drawing \( \{\text{unit}_i, \phi_i\} \sim \text{iid MVN}(0, \hat{\Sigma}) \) and \( \varepsilon_{it} \sim \text{iid N}(0, \hat{\sigma}^2) \). We impose a sharp null of no treatment effect, \( Y_{it}(1) = Y_{it}(0) = Y_{it} \).

A key component of the simulation model is selection into treatment. We fix the treatment times to be the same as in the teacher unionization application, and set the probability that unit \( i \) is treated at each treatment time to be \( \pi_i = \text{logit}(\theta_0 + \theta_1 (\text{unit}_i + \phi_{i1} + \phi_{i2})) \). For each treatment time, we assign treatment to those units not already treated with probability \( \pi_i \), sweeping through the fixed set of treatment times. We set \( \theta_0 = -2.7 \) and \( \theta_1 = -1 \) to ensure that around 32 units are eventually treated in each simulation draw, following the distribution of the data. We provide additional simulation results under a two-way fixed effects model and a random-effects autoregressive model in Appendix B.

We consider several estimators for the average post-treatment effect ATT. Figure 6 shows five: (1) A simple difference-in-differences estimator (i.e., an unweighted event study), (2) the partially pooled SCM estimator, with a “bias frontier” as we vary \( \nu \) between 0 and 1, (3) the weighted event study estimator that combines fixed effects and partially pooled SCM, again presented as a bias frontier, (4) directly estimating the factor model with `gsynth`, and (5) augmenting partially pooled SCM with `gsynth` using the heuristic value of \( \hat{\nu} \). The vertical axis of each panel shows the absolute trends assumption by comparing pre-treatment outcomes between treated and control units. These tests generally have low power, however; see, e.g., Roth (2018); Bilinski and Hatfield (2018); Kahn-Lang and Lang (2019). In contrast, the weighted event study estimator uses pre-treatment outcomes to select donor units that best balance the treated units, in effect optimizing for the placebo test. It is still possible to inspect pre-treatment fit, as in standard SCM, but this is best seen as an assessment of the quality of the match rather than as a formal placebo test.

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bias for the ATT, $|\mathbb{E}[\text{ATT} - \hat{\text{ATT}}]|$, while the horizontal axis shows the Mean Absolute Deviation (MAD) of the individual average post-treatment effect estimates, $\mathbb{E}\left[\frac{1}{J} \sum_{j=1}^{J} |\tau_j - \hat{\tau}_j|\right]$. Appendix Figure B.1 additionally plots the the MAD and RMSE for both estimates.

There are several key takeaways from Figure 6. First, the unweighted event study model is misspecified here, and does not do particularly well at controlling either ATT or unit-level bias. Second, partially pooled SCM significantly reduces the bias for the overall ATT relative to separate SCM, and a small amount of pooling also leads to slightly better individual ATT estimates. The gains to pooling, however, diminish for $\nu$ close to 1, with the fully pooled SCM yielding poor individual ATT estimates and slightly worse overall ATT estimates than partially pooled SCM. Third, the weighted event study estimator dominates either of the alternatives in terms of both pooled and state-level imbalance. Here again there are gains to partially pooling SCM, although the gains are limited together with the fixed effects augmentation. Finally, as expected, directly estimating the oracle gsynth model has lower bias for the overall ATT. However, the MAD for the individual ATT estimates is similar to partially pooled SCM, due to the low number of pre-intervention periods for many of the treated units. Combining gsynth and SCM leads to very similar estimates in this case.

Appendix Figure B.2 shows the results for a two-way fixed effects model, where the unweighted event study model is the oracle estimator. In this setting we see that the pooled SCM estimate has half the bias of the separate SCM estimate, with no ill effects from pooling. Additionally, all forms of augmentation lead to nearly unbiased estimators. Appendix Figure B.3 shows the results for the random effects AR model. In this setting it is possible to over pool, with both the separate and
fully pooled SCM estimators performing worse than partially pooled SCM. In addition, although
the fixed effects model is misspecified, the partially pooled weighted event study performs better
than either partially pooled SCM or the event study alone.

7 Impacts of mandatory teacher collective bargaining laws

We now return to our primary application of the impact of mandatory teacher collective bargaining.
We first consider additional analyses on per-pupil expenditures and then turn to the effects on
teacher salary.

7.1 Effects on per-pupil expenditures

As we show in Figure 5, we find no meaningful effects of mandatory teacher collective bargain-
ing on per-pupil expenditures. Pooled across the ten years after treatment adoption, the overall
estimate from the combined event study and SCM model is essentially zero: \( \hat{\text{ATT}} = -0.01 \), or
a 1 percent reduction in per-pupil expenditures, with an approximate 95% confidence interval of
\([-0.049, +0.029]\). Supplementing our main analyses, Appendix Figure C.5 presents corresponding
estimates from the generalized synthetic control method (Xu, 2017), both alone and combined with
partially pooled SCM, showing similar null results overall. Taken together, these estimates are
in stark contrast to the results from Hoxby (1996), who argues for a 12 percent positive effect,
although she gives a range of estimates.

We can assess the strength of evidence by conducting robustness and placebo checks. First,
following Abadie et al. (2015), we begin by assessing out-of-sample validity via in time placebo
checks. These checks re-index treatment time to be earlier in order to hold out some pre-treatment
time periods (i.e. setting \( T_j' = T_j - x \) for some \( x \)), then estimate placebo effects for the held-out
pre-intervention time periods. Figure 7 shows the placebo estimates for the weighted event study
estimator with placebo treatment time two and four periods before the true treatment time. Both
estimators achieve excellent pre-treatment fit and estimate small negative placebo effects that are
indistinguishable from zero.

Next, we consider the result of trimming states with poor pre-treatment fit, following common
practice in the matching and SCM literatures. Figure 8a shows the state-level fit for both partially
pooled SCM and the weighted event study; two states, New York and Alaska, have especially
bad pre-treatment fits without augmentation, though interestingly the augmented model fits these
states much better. Figure 8b shows the overall ATT estimates and 95% confidence intervals when
removing an increasing number of treated units with poor fits, in the order of state-level fit shown
in Figure 8a. We see that the substantive conclusions remain the same.

Appendix C includes several additional analyses of the impact on per-pupil expenditures. First,
an important feature of SCM-based methods is that we can directly inspect the weights. Appendix
(a) Two year in-time placebo estimates. $\widehat{\text{ATT}} = -0.015$, approximate 95% confidence interval $[-0.076, 0.046]$.

(b) Four year in-time placebo estimates. $\widehat{\text{ATT}} = -0.018$, approximate 95% confidence interval $[-0.067, 0.031]$.

Figure 7: Placebo estimates for per-pupil expenditures re-indexing treatment time to (a) two and (b) four years before the true treatment time. The placebo effects are very close to zero and are indistinguishable from zero at this level of precision.

Figures C.6 and C.7 show the state-specific weights over donor states for each treated unit for partially pooled SCM and the weighted event study, respectively. Appendix Figure C.8 shows the number of times each potential donor state is part of a treated state’s synthetic control. Taken together, these figures highlight the role of augmentation in constructing more plausible estimators. For the weights from SCM alone, both Illinois and Wyoming are consistently important donor states; after removing the unit fixed effects, the weights are much more evenly distributed across the donor pool, suggesting that estimates are not overly reliant on a single control unit. Finally, we can assess the sensitivity of our estimates to the particular choice of pooling parameter $\nu$. Appendix Figure C.9b shows the overall ATT estimates for partially pooled SCM and the weighted event study estimator varying $\nu$ from separate SCM $\nu = 0$ to pooled SCM $\nu = 1$. We see that the partially pooled SCM estimates are more sensitive to the choice of $\nu$, but no choice of $\nu$ substantively changes the conclusions for either estimator.

7.2 Effects on average teacher salary

Thus far, we have focused on the impacts of teacher collective bargaining agreements on expenditures, finding no effect overall. One possible explanation is that school districts are able to divert funds from other purposes to fund higher teacher salaries with no net effect on total expenditures. In Figure 9, we therefore repeat the analysis focusing on average teacher salaries (in log, 2010 $)
Figure 8: (a) The distribution of state-level fits (in terms of RMSE) with and without augmentation; Alaska and New York are clear outliers on the original scale, but have similar pre-treatment fits to other states after removing pre-treatment averages. (b) Estimates are not especially sensitive to dropping an increasing number of units (ranked by pre-treatment imbalance), although the uncertainty intervals are wider with fewer units in the analysis.

as the outcome of interest. Figure 9a shows the separate SCM estimate, which, similar to our discussion in Section 3.1, shows poor balance for average pre-treatment outcomes. By contrast, Figure 9b shows the weighted event study estimate, which has excellent pre-treatment balance. Estimates with partially pooled SCM alone are similar.

Consistent with the estimates on expenditures, the estimates from Figure 9 do not show any meaningful impact of mandatory teacher collective bargaining on teacher salaries. Specifically, impacts larger than around 0.04 are outside the 95% intervals, even nine years after implementation of the laws. When we average over all post-treatment years, as in Paglayan (2019, Table 2), the estimate is again essentially zero: $\hat{ATT} = -0.006$ with an approximate 95% confidence interval of $[-0.051, +0.039]$. While not as severe as for per-pupil expenditures, Hoxby (1996)'s estimate that unions raise teacher salaries by 5 percent is also outside this interval.

8 Discussion

In this paper, we develop a new framework for estimating the impact of a treatment adopted gradually by units over time. In our motivating example, 33 states have enacted laws mandating school districts to bargain with teachers unions (Paglayan, 2019), and we seek to estimate the effects of these laws on educational expenditures and teacher pay. To do so, we adapt SCM to the staggered
adoption setting. We argue that current practice of estimating separate SCM weights for each treated unit is unlikely to yield good results, but also that fully pooled SCM may over-correct; our preferred approach, partially pooled SCM, finds weights that balance both state-specific and overall pre-treatment fit. We then augment SCM with a simple average of pre-treatment outcomes, which yields a weighted event study estimator that has advantages over either the event study or SCM estimator alone. We apply this approach to the teacher bargaining example and, consistent with recent analyses, find precisely estimated null effects on teacher salaries and student expenditures.

We briefly note some directions for future work. First, we could extend these ideas to settings with multiple treated units but where treatment can “shut off” for some units, deviating from the staggered adoption structure. This would necessarily require additional assumptions; see, for example, Imai and Kim (2019). We could similarly incorporate other structure from our application. For example, in staggered adoption settings where multiple units adopt treatment at the same time, we could add a layer in the hierarchy and more closely pool units treated at the same time while still partially pooling different treatment cohorts.

Second, many SCM analyses explore multiple outcomes. As in other SCM studies, we treat each outcome separately, choosing different synthetic control weights for each. In many settings, however, lagged values from one outcome may predict future values of another, suggesting that balancing multiple outcome variables would be useful. This seems especially important in settings like ours with relatively few units.

Finally, we focus on relatively simple outcome models, and in particular a simple pre-treatment average. More complex models are possible and may be desirable. For example, Fesler and Pender

Figure 9: (a) Separate SCM and (b) weighted event study estimates for the impact of mandatory collective bargaining laws on average teacher salary (log, 2010 $).
(2019) apply the Ridge Augmented SCM proposal in Ben-Michael et al. (2019) to a staggered adoption setting, modeling each treated unit separately. Partial pooling may be helpful here. In another direction, we might consider an outcome model that incorporates the time weights used in Arkhangelsky et al. (2019). We anticipate that, unlike in the simple case with unit fixed effects, these augmented approaches likely require more elaborate shrinkage estimation, such as via matrix penalties.
References


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A  The dual perspective: generalized propensity score weighting and conditional parallel trends

As we discuss in the main text, we can view partially pooled SCM as a form of balancing weights. By exploiting the duality between balancing weights and inverse propensity weighting, we can then interpret these weights as a form of (generalized) propensity score weighting (Imbens, 2000; Imai and Van Dyk, 2004). We further use this dual interpretation to show identification under a particular conditional parallel trends assumption (Abadie, 2005). For this section we consider combining units into treatment time cohorts indexed by $Z_i$ where $Z_i = j$ if $T_i = T_j < \infty$ and $Z_i = 0$ if $T_i = \infty$ for control units. This is equivalent to fully pooling the synthetic control for units that share a treatment time.

A.1 Interpretation: inverse generalized propensity score weighting

In the case of a single treated unit, Ben-Michael et al. (2019) relate the synthetic control problem to propensity score estimation through the Lagrangian dual; see also Zhao and Percival (2017), Zhao et al. (2019), and Wang and Zubizarreta (2019). A simple extension of that result shows that the loss function $L(\alpha, \beta)$ in the dual problem (8) estimates the parameters of $J$ separate propensity score models that are linear in the lagged outcomes, with link function $f^\ast$; i.e. the propensity score model follows

$$f^\prime \left( \frac{P(Z_i = j \mid Y_{i,T_j-L}, \ldots, Y_{i,T_j-1})}{P(Z_i = 0 \mid Y_{i,T_j-L}, \ldots, Y_{i,T_j-1})} \right) = \alpha_j + \sum_{\ell=1}^L \beta_{\ell j} Y_{i,T_j-\ell}, \quad j = 1, \ldots, J. \quad (15)$$

This dual problem has the form of a propensity score model for the probability of assignment to treatment level $j$, $Z_i = j$, relative to control, $Z_i = 0$, conditioned on the previous $T_1$ outcomes. The loss function $L(\alpha, \beta)$ is a so-called calibrated loss, rather than the more standard likelihood approach. Using a calibrated loss generally leads to better finite sample properties for the resulting weights than more traditional methods; see Tan (2017); Ben-Michael et al. (2019). Jointly, this series of separate logistic models has the form of a multinomial regression for the generalized propensity score (Imbens, 2000).

Armed with this IPW interpretation of the loss function, we can shed more light on the regularization in Equation (8) by comparing it to the corresponding regularized maximum likelihood estimate. This estimate is a maximum a priori estimate of the following hierarchical propensity score model (Li et al., 2013):

$$f^\prime \left( \frac{P(Z_i = j \mid Y_{i,T_j-L}, \ldots, Y_{i,T_j-1})}{P(Z_i = 0 \mid Y_{i,T_j-L}, \ldots, Y_{i,T_j-1})} \right) = \alpha_j + \sum_{\ell=1}^L \beta_{\ell j} Y_{i,T_j-\ell}, \quad j = 1, \ldots, J$$

$$\beta_{\ell j} \mid \mu_{\beta \ell} \sim N \left( \mu_{\beta \ell}, \frac{(1 - \nu)}{\lambda} \right)$$

$$\mu_{\beta \ell} \sim N \left( 0, \frac{\nu}{\lambda} \right). \quad (16)$$

Written in a Bayesian form, we see that the dual problem (8) shrinks cohort specific parameters $\beta_j$ towards a global model of treatment $\mu_{\beta}$.

Finally, we note that while the form of Equation (15) holds whenever $\lambda > 0$, interpreting this
model as a generalized propensity score requires additional considerations. First, treatment levels must be well defined. This is most natural when we consider cohorts with multiple treated units, and so can imagine another unit adopting treatment at the same time. Second, all control units must have well-defined treated potential outcomes. This is plausible in our collective bargaining example — we can conceive of never treated states adopting such laws — but is not always appropriate, such as in classic SCM examples (Abadie et al., 2015).

A.2 Connection to semiparametric DID and identification under conditional parallel trends

With this IPW interpretation, we consider the oracle estimator that uses the true (unpenalized) propensity score weights to estimate the ATT and show that it identifies causal treatment effects under a conditional parallel trends assumption. To do so, we show that this approach is a version of semiparametric DID (Abadie, 2005; Callaway and Sant’Anna, 2018) and then apply existing results. Unlike existing methods, however, the weighted event study approach instead conditions on pre-treatment dynamics, specifically the residuals after subtracting off the pre-treatment average.

To formalize these results, we make some additional assumptions that are standard in the event studies literature (see Callaway and Sant’Anna, 2018) but which might not necessarily hold in all settings. First, we assume that the observed units are sampled from an underlying population.

Assumption 1 (Sampling). \( \{Y_{i1}, \ldots, Y_{iT_j}, T_i\}_{i=1}^N \overset{iid}{\sim} \mathcal{P}(\cdot) \) for some joint distribution \( \mathcal{P}(\cdot) \)

Second, we assume that every unit has a non-zero probability of adopting treatment at any time, both overall and conditional on lagged residuals.

Assumption 2 (Overlap). \( \mathbb{P}(Z = j) > 0 \) for all \( j = 0, \ldots, J. \)

Finally, we relax the parallel trends assumption to only hold conditionally. Following Hazlett and Xu (2018), we assume that parallel trends holds given the vector of residuals, \( \hat{Y}_{it} = Y_{it} - \bar{Y}_{i,j}^{\text{pre}}. \)

Assumption 3 (Conditional parallel trends). For \( t' < t \) and \( j = 1, \ldots, J, \)

\[
\mathbb{E}[Y_{it}(0) - Y_{it'}(0) \mid \hat{Y}_{i1}, \ldots, \hat{Y}_{iT_j-1}, Z = j] = \mathbb{E}[Y_{it}(0) - Y_{it'}(0) \mid \hat{Y}_{i1}, \ldots, \hat{Y}_{iT_j-1}, Z = 0]. \tag{17}
\]

Assumption 3 loosens the usual parallel trends assumption by allowing trends to differ depending on how the lagged outcomes deviate from their baseline value. Thus, we are essentially conditioning on pre-treatment “dynamics,” rather than pre-treatment levels. For instance, even if two states have very different levels of student expenditures, under conditional parallel trends we can compare them so long as they have similar pre-treatment trends and shocks. See Hazlett and Xu (2018) for further discussion.

Given these assumptions, we now return to the SCM-weighted event study estimator in Equation (12). Let \( \hat{p}_{ij} = \frac{\gamma_{ij}}{1 - \gamma_{ij}} \) be the implied propensity score for unit \( i \) for treatment \( Z_j = j \) from Equation (15). Then we can re-write Equation (12) as:

\[
\hat{\tau}_{jk}^{\text{aug}} = \frac{1}{T_j - 1} \sum_{t=1}^{T_j-1} \left( \sum_{i=1}^{N} \hat{p}_{ij} \left( Y_{i,T_j+k} - Y_{i,t} \right) - \sum_{i=1}^{N} \frac{\hat{p}_{ij}}{1 - \hat{p}_{ij}} \left( Y_{i,T_j+k} - Y_{i,t} \right) \right). \tag{18}
\]
This is identical in form to the semiparametric DID estimators proposed by Abadie (2005) and Callaway and Sant’Anna (2018). We can then immediately apply Theorem 1 in Callaway and Sant’Anna (2018). Specifically, the population analog of $\tau_{jk}^{aug}$ identifies $\tau_{jk}$ under Assumptions 1-3:

$$\frac{1}{T_j - 1} \sum_{t=1}^{T_j - 1} \left\{ \frac{\mathbb{E}[Y_{T_j+k}(1) - Y_{t}(0) | Z = j]}{\mathbb{P}(Z = j | \hat{Y}_1, \ldots, \hat{Y}_{T_j-1})} - \frac{\mathbb{P}(Z = j | \hat{Y}_1, \ldots, \hat{Y}_{T_j-1})}{\mathbb{P}(Z = 0 | \hat{Y}_1, \ldots, \hat{Y}_{T_j-1})} \mathbb{E} [Y_{T_j+k}(0) - Y_{t}(0) | Z = 0] \right\}$$

$$= \frac{1}{T_j - 1} \sum_{t=1}^{T_j - 1} \mathbb{E}[Y_{T_j+k}(1) - Y_{T_j+k}(0) | Z = j]$$

$$= \mathbb{E}[Y_{T_j+k}(1) - Y_{T_j+k}(0) | Z = j].$$

This shows that the weighted event study approach estimates causal effects under weaker assumptions than are needed for traditional event studies. This is not the only proposal to generalize DID: existing semiparametric approaches condition on auxiliary or time-invariant covariates, rather than on lagged outcomes. We anticipate that blended strategies that condition both on auxiliary covariates and pre-treatment outcome dynamics may be attractive in many applications.

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12The estimator in Callaway and Sant’Anna (2018) also standardizes by the sum of $\frac{\hat{\beta}_{ij}}{\hat{\beta}_{ij}}$. This sum equals 1 by construction when $\hat{\Gamma}$ is estimated via the calibrated approach above. The estimator in Callaway and Sant’Anna (2018) also restricts estimation to $t = T_j - 1$. We slightly modify their Theorem 1 under the stronger assumption that conditional parallel trends hold for all pre-treatment times, $t = 1, \ldots, T_j$.  

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B Additional simulation results

We now describe the two-way fixed effects model and the autoregressive model that we use in our simulation studies. First, the two-way fixed effects model is:

\[ Y_{it} = \text{int} + \text{unit}_i + \text{time}_t + \varepsilon_{it}. \]  

(20)

Here, both unit and time effects are normalized to have mean zero. We estimate (20) using just the un-treated observations, and extract the estimated variance of the unit effects, \( \hat{\Sigma} \), and of the error term, \( \hat{\sigma}^2_{\varepsilon} \). We then generate simulated data sets with the same \( N = 49 \) and \( T = 38 \) where \( \text{unit}_i \overset{iid}{\sim} N(0, \hat{\Sigma}) \) and \( \varepsilon_{it} \overset{iid}{\sim} N(0, \hat{\sigma}^2_{\varepsilon}) \). We impose a sharp null of no treatment effect, \( Y_{it}(1) = Y_{it}(0) = Y_{it} \). This model satisfies the parallel trends assumption needed for traditional event studies and DID models. We set the probability that unit \( i \) is treated at each treatment time to be \( \pi_i = \text{logit}(\theta_0 + \theta_1 \cdot \text{unit}_i) \), with \( \theta_0 = -2.7 \) and \( \theta_1 = -1 \) to ensure that around 30 units are eventually treated in each simulation draw.

We also consider a random effects autoregressive model:

\[ Y_{it} = \sum_{\ell=1}^{3} \rho_{\ell} Y_{i,t-\ell} + \varepsilon_{it} \]

(21)

\[ \rho \sim N(\mu_\rho, \sigma^2_\rho). \]

We fit this random effects model using \texttt{lme4} (Bates et al., 2015) to get estimates \( \hat{\mu}_\rho \) and \( \hat{\sigma}_\rho \). In order to increase the level of heterogeneity across time, we simulate from this hierarchical model with 8 times the standard deviation \( 8\hat{\sigma}_\rho \). We allow selection to depend on the three lagged outcomes \( \pi_i = \text{logit} \left( \theta_0 + \theta_1 \sum_{\ell=1}^{L} Y_{i,t-\ell} \right) \), where \( \theta_0 = \log 0.04 \) and \( \theta_1 = -2 \).
Figure B.1: Monte Carlo estimates of the MAD and RMSE for the overall ATT and the individual ATT estimates under a linear factor model (14).
Figure B.2: Monte Carlo estimates of the MAD and RMSE for the overall ATT and the individual ATT estimates under a two-way fixed effects model (20).
Figure B.3: Monte Carlo estimates of the MAD and RMSE for the overall ATT and the individual ATT estimates under a random effects AR model (21).
C Additional results and figures for mandatory collective bargain-
ing

C.1 Event study estimates for the teacher collective bargaining application

A common approach for estimating causal effects under staggered adoption is by fitting a variant of the two-way fixed effect model, known as the event study, or dynamic treatment effect, specification:

\[ Y_{it} = \text{unit}_i + \text{time}_t + \sum_{\ell=2}^{L} \delta_{\ell} \mathbb{1}\{T_i = t - \ell\} + \sum_{k=0}^{K} \tau_{k} \mathbb{1}\{T_i = t + k\} + \varepsilon_{it}, \]  

(22)

for outcome \( Y_{it} \) for state \( i \) at time \( t \), where \( L = T_J \) and \( K = T - T_1 \) are the maximum number of pre-treatment outcomes (lags) and post-treatment outcomes (leads) observed for all treated units in the sample. Following standard practice, the coefficient for \( \delta_1 \) is normalized to zero. This is arbitrary, and researchers sometimes impose a different normalization (e.g., \( \delta_L = 0 \)). If all units are eventually treated, a second normalization is required. Paglayan (2019) uses a slightly different normalization to the equation shown here, though the differences are immaterial. See Abraham and Sun (2018); Callaway and Sant’Anna (2018) for further discussion of this workhorse model.

Figures C.10 and C.11a shows the results from estimating Equation (22) on the full data in Paglayan (2019) for per-pupil expenditures and average teacher salary, respectively; we show standard errors clustered by state (Pustejovsky and Tipton, 2018). The placebo estimates to the left of treatment adoption time, the coefficients \( \hat{\delta}_{\ell} \) from Equation (22), show that states that pass laws have declining expenditures — and, to a lesser degree, declining salaries — in the several years prior to adoption, relative to other states at the same time. These “pre-trends” suggest that the critical parallel trends assumption is likely violated in this setting, raising doubts about the estimates to the right of the treatment adoption time, the coefficients \( \hat{\tau}_{k} \) from Equation (22). Paglayan (2019) estimates Equation (22) using only the ever-treated states, for which there is no evidence against the parallel trends assumption. Figures C.10 and C.11a are nearly identical to the corresponding figures from the supplementary materials in Paglayan (2019).

C.2 Additional figures
Figure C.4: Illustrative fits for the weighted event study

Figure C.5: \texttt{gsynth} and augmented estimates for per-pupil student expenditures (log, 2010 $).
Figure C.6: Partially pooled SCM weights. White cells indicate zero weight, black cells indicate a weight of 1.
Figure C.7: Weighted event study weights. White cells indicate zero weight, black cells indicate a weight of 1.
Figure C.8: Partially pooled and weighted event study weights. The number of times that each state is part of a treated state’s synthetic control, normalized by the number of times it is a possible donor state. Note that California is an eligible donor state in only two cases. Colors on a log scale.
(a) Removing NY and AK ($\hat{v} = 0.27$)  

(b) Varying $\nu$ from 0 to 1.

Figure C.9: (a) Partially pooled SCM estimates removing the two worst fit states, (b) $\hat{\text{ATT}}$ as $\nu$ varies between 0 and 1 (plotted against $q^{sep}$), estimates and approximate 95% confidence intervals using $\hat{\nu}$ shown.

Figure C.10: Event study estimates for per pupil expenditures (log 2010 $).
Figure C.11: (a) Event study and (b) partially pooled SCM estimates for average teacher salary (log, 2010 $).

(a) Event study regression

(b) Partially Pooled SCM, $\hat{\nu} = 0.50$
D Additional results and proofs

D.1 Partial pooling in dual parameters

Lemma 1. The Lagrangian dual to Equation (2) is

\[
\min_{\alpha, \beta} \sum_{j=1}^{J} \left[ \sum_{W_i=0} \left( f^* \left( \alpha_j + \sum_{\ell=1}^{L} \beta_{\ell j} Y_{i,T_j-\ell} \right) - \left( \alpha_j + \sum_{\ell=1}^{L} \beta_{\ell j} Y_{j,T_j-\ell} \right) \right) \right] + \sum_{j=1}^{J} \frac{\lambda J L}{2} \| \beta_j \|_2^2, \tag{23}
\]

where \( f^*(y) = \sup_x x^t y - f(x) \) is the convex conjugate of \( f \). The resulting donor weights are \( \hat{\gamma}_{ij} = f^* \left( \hat{\alpha}_j - \sum_{\ell=1}^{L} \hat{\beta}_{\ell j} Y_{i,T_j-\ell} \right) \).

Proof of Lemma 1. Notice that the separate synth problem (2) separates into \( J \) optimization problems:

\[
\min_{\gamma_1, \ldots, \gamma_J} q^{sep}(\Gamma) + \lambda \sum_{j=1}^{J} \sum_{i=1}^{N} f(\gamma_{ij})
\]

\[
= \frac{1}{2J} \sum_{j=1}^{J} \min_{\gamma_i} \left[ \left( \frac{1}{T_j-1} \sum_{\ell=1}^{T_j-1} \left( Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \right)^2 \right) + \lambda \sum_{i=1}^{N} f(\gamma_{ij}) \right] \tag{24}
\]

Thus the Lagrangian dual objective is the sum of the Langrangian dual objectives of the individual objectives in Equation (24). Inserting the dual objectives derived by Ben-Michael et al. (2019) yields the result.

Proof of Proposition 1. We start be defining auxiliary variables, \( \mathcal{E}_0, \mathcal{E}_1, \ldots, \mathcal{E}_J \in \mathbb{R}^L \) where \( \mathcal{E}_{j\ell} = Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \) for \( j \geq 1 \) and \( \mathcal{E}_{0\ell} = \sum_{T_j>\ell} \left( Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \right) \). Additionally we rescale by \( \frac{1}{\lambda} \). Then we can write the partially pooled SCM problem (5) as

\[
\min_{\gamma_1, \ldots, \gamma_J, \mathcal{E}_0, \ldots, \mathcal{E}_J} \frac{\nu}{2L\lambda} \sum_{\ell=1}^{L} \mathcal{E}_{0\ell}^2 + \frac{1-\nu}{2J\lambda} \sum_{j=1}^{J} \frac{1}{L} \mathcal{E}_{j\ell}^2 + \sum_{j=1}^{J} \sum_{i=1}^{N} f(\gamma_{ij})
\]

subject to \( \mathcal{E}_{j\ell} = Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \)

\[
\mathcal{E}_{0\ell} = \sum_{T_j>\ell} \left( Y_{j,T_j-\ell} - \sum_{i=1}^{N} \gamma_{ij} Y_{i,T_j-\ell} \right)
\]

\( \gamma_j \in \Delta_{j}^{scm} \)

With Lagrange multipliers \( \mu_\beta, \zeta_1, \ldots, \zeta_J \in \mathbb{R}^L \) and \( \alpha_1, \ldots, \alpha_J \in \mathbb{R} \), the Lagrangian to Equation (25) is
\[ \mathcal{L}(\Gamma, E_0, \ldots, E_J, \alpha, \ldots, \alpha, \mu, \beta, \beta, \ldots, \beta) = \]
\[ \sum_{\ell=1}^{L} \left[ \frac{\nu}{2L\lambda} E_{0,\ell}^2 - \mu,\beta,\ell \left( \sum_{j=1}^{J} Y_{j,T_j-\ell} - \sum_{i \in D_j} \gamma_{ij} Y_{i,T_j-\ell} \right) - E_{0,\ell} \mu,\beta,\ell \right] \]
\[ + \sum_{j=1}^{J} \sum_{\ell=1}^{L} \left[ \frac{1-\nu}{2JL\lambda} E_{j,\ell}^2 - \zeta,\ell \left( Y_{j,T_j-\ell} - \sum_{i \in D_j} \gamma_{ij} Y_{i,T_j-\ell} \right) - \zeta,\ell E_{j,\ell} \right] \]
\[ + \sum_{j=1}^{J} \sum_{i \in D_j} f(\gamma_{ij}) - \alpha_j \gamma_{ij} - \alpha \]

Defining \( \beta_j = \mu + \zeta_j \), the dual problem is:

\[ -\min_{\Gamma, E_0, \ldots, E_J} L(\cdot) = -\sum_{j=1}^{J} \sum_{i \in D_j} \min_{\gamma_{ij}} \left\{ f(\gamma_{ij}) - \left( \alpha_j - \sum_{\ell=1}^{L} \beta_{\ell j} Y_{i,T_j-\ell} \right) \gamma_{ij} \right\} + \sum_{j=1}^{J} \alpha_j + \sum_{\ell=1}^{L} \beta_{\ell j} Y_{j,T_j-\ell} \]
\[ - \sum_{\ell=1}^{L} \min_{E_{j,\ell}} \left\{ \frac{1-\nu}{2JL\lambda} E_{j,\ell}^2 - E_{j,\ell}(\beta_{\ell j} - \mu,\beta,\ell) \right\} \]
\[ - \sum_{\ell=1}^{L} \min_{E_{0,\ell}} \left\{ \frac{\nu}{2L\lambda} E_{0,\ell}^2 - E_{0,\ell} \mu,\beta,\ell \right\} \]

From Lemma 1, we see that the first term in (27) is \( \mathcal{L}(\alpha, \beta) \) and we have the same form for the implied weights. The next two terms are the convex conjugates of a scaled \( L^2 \) norm. Using the computation that the convex conjugate of \( \frac{a}{2} \| x \|_2^2 \) is \( \frac{1}{2a} \| x \|_2^2 \). Finally, the primal problem (5) is still convex and a primal feasible point exists, so by Slater’s condition strong duality holds.

**D.1.1 Duality when balancing a general number of lagged outcomes**

When balancing a different number of lagged outcomes for each treated unit, the lagrangian dual problem only changes slightly. First, we immediately see that the dual to separate SCM (2) is nearly identical to Equation (23):

\[ \min_{\alpha,\beta} \mathcal{L}(\alpha, \beta) + \sum_{j=1}^{J} \lambda(T_j - 1) \| \beta_j \|_2^2 = \min_{\alpha,\beta} \mathcal{L}(\alpha, \beta) + \sum_{j=1}^{J} \sum_{\ell=1}^{T_j-1} \frac{\lambda(T_j - 1)}{2} \beta_{j,\ell}^2 \]  

An important distinction is that for each treatment level \( j \) there is a different number of lagged outcomes \( T_j \); therefore the dual parameters \( \beta_j \) have different lengths. Scaling the ridge penalty by \( T_j \) ensures that the dual parameters are shrunk equally towards zero, and balancing a different number of lagged outcomes for each treatment level changes the implied selection model.

As before, the average balance term induces an additional set of Lagrange multipliers \( \mu,\beta, \in \mathbb{R}^{T_j} \),
where the varying-length $\beta_j$'s are pooled towards $\mu_\beta$ for each $1 \leq \ell \leq T_j - 1$.

$$\min_{\alpha, \beta, \mu_\beta} \mathcal{L}(\alpha, \beta) + \sum_{j=1}^{J} \frac{\lambda (T_j - 1)}{2(1 - \nu)} \sum_{\ell=1}^{T_j - 1} (\beta_{\ell j} - \mu_\beta)^2 + \frac{\lambda L}{2\nu} \|\mu_\beta\|_2^2. \tag{29}$$

D.2 Error bounds

D.2.1 Time-varying AR

**Theorem 2.** Let $Y_{it}(0)$ follow a time-varying AR($L$) process

$$Y_{it}(0) = \sum_{\ell=1}^{L} (\bar{\rho}_\ell + \xi_{t\ell}) \rho_{t\ell} Y_{i,t-\ell} + \varepsilon_{it}, \tag{30}$$

where $\varepsilon_{it}$ are independent sub-Gaussian random variables with scale parameter $\sigma_i$, and $S^2 \equiv \frac{1}{J} \sum_{j=1}^{J} \sum_{\ell=1}^{L} \xi_{T_j \ell}^2$. For $\hat{\gamma}_1, \ldots, \hat{\gamma}_J \in \mathcal{D}_{scm}$, the error at time $T_j$ for treated unit $j$ is

$$|\hat{\tau}_{j0} - \tau_{j0}| \leq \|\bar{\rho} + \xi_{T_j}\|_2 \sqrt{\sum_{\ell=1}^{L} \left( Y_{j,T_j-\ell} - \sum_{i \in \mathcal{D}_j} \hat{\gamma}_{ij} Y_{i,T_j-\ell} \right)^2} + \delta \sigma (1 + \|\hat{\gamma}_j\|_2) \tag{31}$$

with probability at least $1 - 2e^{-\frac{\delta^2}{2}}$. Furthermore, the error for $\hat{\Delta T}_0$ is

$$\hat{\Delta T}_0 - \Delta T_0 = \frac{1}{J} \sum_{j=1}^{J} |\hat{\tau}_{j0} - \tau_{j0}| = \|\bar{\rho}\|_2 \sqrt{\sum_{\ell=1}^{L} \left( \frac{1}{J} \sum_{j=1}^{J} Y_{j,T_j-\ell} - \sum_{i \in \mathcal{D}_j} \hat{\gamma}_{ij} Y_{i,T_j-\ell} \right)^2} \tag{32}$$

$$+ S \sqrt{\frac{1}{J} \sum_{j=1}^{J} \sum_{\ell=1}^{L} \left( Y_{j,T_j-\ell} - \sum_{i \in \mathcal{D}_j} \hat{\gamma}_{ij} Y_{i,T_j-\ell} \right)^2} \tag{32}$$

$$+ \frac{\delta \sigma}{\sqrt{J}} (1 + \|\Gamma\|_F)$$

with probability at least $1 - 2e^{-\frac{\delta^2}{2}}$.

**Proof of Theorem 2.** Notice that

$$\hat{\tau}_{j0} - \tau_{j0} = \sum_{\ell=1}^{L} (\bar{\rho}_\ell + \xi_{T_j \ell}) \left( Y_{j,T_j-\ell} - \sum_{i \in \mathcal{D}_j} \hat{\gamma}_{ij} Y_{i,T_j-\ell} \right) + \left( \varepsilon_{jT_j} - \sum_{i \in \mathcal{D}_j} \hat{\gamma}_{ij} \varepsilon_{iT_j} \right)$$

So by the triangle and Cauchy-Schwarz inequalities,
\[ |\hat{\tau}_j - \tau_j| \leq \|\hat{\rho} + \xi_{Tj}\|_2 \sum_{\ell=1}^{L} \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \gamma_{ij} Y_{i, Tj-\ell} \right)^2 + |\varepsilon_{jTj} - \sum_{i \in D_j} \gamma_{ij} \varepsilon_{iTj}|. \]

Since \( \hat{\gamma}_j \) is fit on pre-\( T_j \) outcomes, the weights are not dependent on \( \varepsilon_{Tj} \), and so the second term above is sub-Gaussian with scale parameter \( \sigma \sqrt{1 + \|\hat{\gamma}_j\|_2} \leq \sigma (1 + \|\gamma_j\|_2) \). This implies that

\[
P\left( \left| \varepsilon_{jTj} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iTj} \right| \geq \delta \sigma (1 + \|\gamma_j\|_2) \right) \leq 2 \exp\left( -\frac{\delta^2}{2} \right)
\]

This completes the proof of the first inequality. For the bound on \( \hat{\text{ATT}}_0 \), notice that

\[
\hat{\text{ATT}}_0 - \text{ATT}_0 = \frac{1}{J} \sum_{j=1}^{J} \hat{\tau}_j - \tau_j = \frac{1}{J} \sum_{j=1}^{J} \left[ \sum_{\ell=1}^{L} (\hat{\rho}_\ell + \xi_{Tj, \ell}) \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{i, Tj-\ell} \right) + \left( \varepsilon_{jTj} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iTj} \right) \right]
\]

By Cauchy-Schwarz the absolute value of the first term is

\[
\left| \sum_{\ell=1}^{L} \hat{\rho}_\ell \sum_{j=1}^{J} \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{i, Tj-\ell} \right) \right| \leq \|\hat{\rho}\|_2 \sum_{\ell=1}^{L} \left( \frac{1}{J} \sum_{j=1}^{J} \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{i, Tj-\ell} \right) \right)^2
\]

Similarly, the absolute value of the second term is

\[
\left| \frac{1}{J} \sum_{j=1}^{J} \sum_{\ell=1}^{L} \xi_{Tj, \ell} \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{i, Tj-\ell} \right) \right| \leq \frac{1}{J} \sum_{j=1}^{J} \|\xi_{Tj}\|_2 \left( \sum_{\ell=1}^{L} \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{i, Tj-\ell} \right) \right)^2
\]

\[
\leq S \left( \frac{1}{J} \sum_{j=1}^{J} \sum_{\ell=1}^{L} \left( Y_{j, Tj-\ell} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{i, Tj-\ell} \right) \right)^2
\]

Finally, notice that \( \frac{1}{J} \sum_{j=1}^{J} \varepsilon_{jTj} \) is the average of \( J \) independent sub-Gaussian random variables and so is itself sub-Gaussian with scale parameter \( \sigma / \sqrt{J} \). However, \( \frac{1}{J} \sum_{j=1}^{J} \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iTj} \) is the weighted
average of sub-Gaussian variables that are independent over $i$ but not necessarily independent over $j$, and so the weighted average is sub-Gaussian with scale parameter $\sigma \sqrt{J \| \Gamma \|_F}$. The two averages are independent of each other, so

$$P \left( \frac{1}{J} \sum_{j=1}^{J} \left( \varepsilon_{jT_j} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iT_j} \right) \right) \geq \frac{\delta \sigma}{\sqrt{J}} \left( 1 + \| \hat{\Gamma} \|_F \right) \leq 2 \exp \left( -\frac{\delta^2}{2} \right).$$

Putting together the pieces completes the proof. \qed

D.2.2 Linear factor model

We begin by bounding the error for the treatment effect for a single treated unit. This slightly extends Theorem 1 in Ben-Michael et al. (2019) to include the error due to variance as well as the error due to bias. Then we prove Theorem 1.

**Proposition 2.** Let $Y_{it}(0)$ follow a linear factor model

$$Y_{it}(0) = \mu'_t \phi_i + \varepsilon_{it}, \quad (34)$$

where $\mu_t, \phi_i \in \mathbb{R}^F$ with $\max_t \| \mu_t \|_\infty \leq M$, and $\varepsilon_{it}$ is independent sub-Gaussian with scale parameter $\sigma$. For $\hat{\gamma}_1, \ldots, \hat{\gamma}_J \in \Delta_{scm}$, the error at time $T_j$ for treated unit $j$ is

$$|\hat{\tau}_j - \tau_j| \leq \frac{M^2 F}{\sqrt{L}} \left( \sum_{t=1}^{L} \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2 + \sigma \frac{M^2 F}{\sqrt{L}} \left( 2\delta + \sqrt{\log \| D_j \|} \right) \right) \delta \sigma \left( 1 + \| \hat{\gamma}_j \|_2 \right) \quad (35)$$

with probability at least $1 - 6e^{-\delta^2/2}$.

**Proof of Proposition 2.** First notice that

$$\hat{\tau}_j - \tau_j = \mu'_T \left( \phi_j - \sum_{i \in D_j} \hat{\gamma}_{ij} \phi_i \right) + \left( \varepsilon_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iT_j} \right)$$

From the proof of Theorem 2, we know that

$$P \left( \left| \varepsilon_{jT_j} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iT_j} \right| \geq \delta \sigma \left( 1 + \| \hat{\gamma}_j \|_2 \right) \right) \leq 2 \exp \left( -\frac{\delta^2}{2} \right).$$

Following Abadie et al. (2010), we can re-write $\phi_i$ in terms of the lagged outcomes as

$$\phi_i = (\mu'_{1:L} \mu_{1:L})^{-1} \sum_{t=1}^{L} \mu_t (Y_{it} - \varepsilon_{it}) = \frac{1}{L} \sum_{t=1}^{L} \mu_t (Y_{it} - \varepsilon_{it}) \quad (36)$$

where $\mu_{1:L} \in \mathbb{R}^{L \times F}$ is the matrix of factors from time $t = 1, \ldots, L$. With the Cauchy-Schwarz
Next, since $\varepsilon_{jT_j}$ are independent sub-Gaussian,

$$P \left( \frac{1}{L} \max_{i \in D_j} \left| \sum_{t=1}^{L} \mu_{T_j}^t \mu_{jt} \varepsilon_{jt} \right| \geq \frac{\delta \sigma}{L} \| \mu_{T_j}^t \mu_{1:L} \|_2 \right) \leq 2 \exp \left( -\frac{\delta^2}{2} \right)$$

and by the standard tail bound on maxima of sub-Gaussian random variables,

$$P \left( \frac{1}{L} \max_{i \in D_j} \left| \sum_{t=1}^{L} \mu_{T_j}^t \mu_{jt} \varepsilon_{jt} \right| \geq \frac{\sigma}{L} \| \mu_{T_j}^t \mu_{1:L} \|_2 \left( \sqrt{\log \| D_j \|} + \delta \right) \right) \leq 2 \exp \left( -\frac{\delta^2}{2} \right)$$

Now notice that $\frac{1}{L} \| \mu_{T_j}^t \mu_{1:L} \|_2 \leq \frac{M^2}{\sqrt{L}}$. Combining these bounds gives the result.

\textbf{Proof of Theorem 1.} Using Equation (36), we can write the error for the ATT as

$$\bar{A}TT_k - ATT_k = \frac{1}{J} \sum_{j=1}^{J} \hat{\tau}_{jk} - \tau_{jk} = \frac{1}{J L} \sum_{j=1}^{J} \sum_{t=1}^{L} \mu_{T_j+k}^t \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)$$

$$- \frac{1}{J L} \sum_{j=1}^{J} \sum_{t=1}^{L} \mu_{T_j+k}^t \left( \varepsilon_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{it} \right)$$

$$+ \frac{1}{J} \sum_{j=1}^{J} \left( \varepsilon_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{iT_j} \right).$$

From the proof of Theorem 2, we can bound the final term in Equation (37). We now bound the first two terms. First, we decompose the first term into a time constant, and a time varying component:

$$\frac{1}{J L} \sum_{j=1}^{J} \sum_{t=1}^{L} \mu_{T_j+k}^t \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right) = \frac{1}{J L} \mu_k \sum_{t=1}^{L} \mu_t \sum_{j=1}^{J} \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)$$

$$+ \frac{1}{J L} \sum_{j=1}^{J} \sum_{t=1}^{L} \xi_{T_j+k}^t \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right),$$

$$\text{(s)}$$
where $\xi_{T+j+k} \equiv \mu_{T+j+k} - \bar{\mu}_k$. Now by Cauchy-Schwarz and using that $\frac{1}{L} \| \mu_{1:L} \|_2 \leq \frac{M \sqrt{F}}{\sqrt{L}}$ we get that

$$
|(*)| \leq M \sqrt{\frac{F}{L}} \| \bar{\mu}_k \|_2 \sqrt{\sum_{t=1}^{L} \left( \frac{1}{J} \sum_{j=1}^{J} Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2} + M \sqrt{\frac{F}{L}} \| \xi_{T+j+k} \|_2 \sqrt{\sum_{t=1}^{L} \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2}
$$

$$
\leq M \sqrt{\frac{F}{L}} \| \bar{\mu}_k \|_2 \sqrt{\sum_{t=1}^{L} \left( \frac{1}{J} \sum_{j=1}^{J} Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2} + M \sqrt{\frac{F}{L}} S_k \left( \sum_{j=1}^{J} \sum_{t=1}^{L} \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2 \right)
$$

We now turn to the second term in Equation (37). Since $\varepsilon_{it}$ are independent sub-Gaussian random variables and $\frac{1}{L} \| \mu'_{T+j+k} \mu_{1:L} \|_2 \leq \frac{M \sqrt{F}}{\sqrt{L}}$,

$$
P \left( \frac{1}{L} \left( \frac{1}{J} \sum_{j=1}^{J} \sum_{t=1}^{L} \mu'_{T+j+k} \mu_t \varepsilon_{jt} \right) \geq \delta \sigma \frac{M^2 F}{\sqrt{JL}} \right) \leq 2 \exp \left( -\frac{\delta^2}{2} \right)
$$

Next, since $\hat{\gamma}_1, \ldots, \hat{\gamma}_J \in \Delta^{scm}$, $\frac{1}{J} \sum_{j=1}^{J} \| \hat{\gamma}_j \|_1 = 1$, so by Hölder’s inequality

$$
\left| \frac{1}{J} \sum_{j=1}^{J} \sum_{t=1}^{L} \mu'_{T+j+k} \mu_t \sum_{i \in D_j} \hat{\gamma}_{ij} \varepsilon_{it} \right| \leq \max_{j \in \{1, \ldots, J\}, i \in D_j} \left\| \sum_{t=1}^{L} \mu'_{T+j+k} \mu_t \varepsilon_{it} \right\| \leq 2 \sigma \frac{M^2 F}{\sqrt{L}} \left( \sqrt{\log NJ} + \delta \right)
$$

where the final inequality holds with probability at least $1 - 2 \exp \left( -\frac{\delta^2}{2} \right)$ by the standard tail bound on the maximum of sub-Gaussian random variables. Putting together the pieces with a union bound gives that

$$
\left| \hat{T}_{T+k} - \hat{T}_k \right| \leq M \frac{\sqrt{F}}{\sqrt{L}} \left( \| \bar{\mu}_k \|_2 \sqrt{\sum_{t=1}^{L} \left( \frac{1}{J} \sum_{j=1}^{J} Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2} + S_k \sqrt{\sum_{j=1}^{J} \sum_{t=1}^{L} \left( Y_{jt} - \sum_{i \in D_j} \hat{\gamma}_{ij} Y_{it} \right)^2} \right)
$$

$$
+ \sigma \frac{M^2 F}{\sqrt{L}} \left( 2 + \frac{1}{\sqrt{J}} \right) \delta + 2 \sqrt{\log NJ} + \frac{\delta \sigma}{\sqrt{J}} \left( 1 + \| \hat{\Gamma} \|_F \right)
$$

with probability at least $1 - 6e^{-\frac{\delta^2}{2}}$. \(\square\)