The Econometrics Journal

Econometrics Journal (2019), volume **22**, pp. 117–130.
doi: 10.1093/ectj/utz001

On the role of covariates in the synthetic control method

Irene Botosaru[†] and Bruno Ferman[‡]

[†]*Department of Economics, University of Bristol, Department of Economics, Priory Road Complex, Priory Road, Bristol BS8 1TU, UK.*
Email: irene.botosaru@bristol.ac.uk

[‡]*Sao Paulo School of Economics - FGV , Rua Itapeva 474, Sao Paulo 01332-000, Brazil.*
Email: bruno.ferman@fgv.br

First version received: 20 July 2018; final version accepted: 7 January 2019.

Summary: Abadie et al. (2010) derive bounds on the bias of the synthetic control estimator under a perfect balance assumption on both observed covariates and pre-treatment outcomes. In the absence of a perfect balance on covariates, we show that it is still possible to derive such bounds, albeit at the expense of relying on stronger assumptions about the effects of observed and unobserved covariates and of generating looser bounds. We also show that a perfect balance on pre-treatment outcomes does not generally imply an approximate balance for all covariates, even when they are all relevant. Our results have important implications for the implementation of the method.

Keywords: *Synthetic controls*, *covariates*, *perfect balance*.

JEL codes: *C21*, *C13*, *C23*.

1. INTRODUCTION

Social scientists are often interested in evaluating the effect of a policy or a treatment on an outcome of interest. To perform such an analysis, it is necessary to construct a counterfactual outcome for the treated unit for a scenario in which there were no treatment. In the absence of randomized experiments, however, it is often difficult to find a suitable comparison unit to construct such a counterfactual. The synthetic control (SC) method, developed in a series of papers by Abadie et al. (2010), Abadie et al. (2015), and Abadie and Gardeazabal (2003), allows practitioners to construct a counterfactual outcome for the treated unit from a set of potential control units. The method uses a data-driven weighted average of the selected control units to construct a synthetic control unit that is more similar to the treated unit than any of the individual control units. Since its inception, this method has been used widely in social sciences, becoming an important part of the toolbox used in the policy evaluation literature (see Athey and Imbens, 2017).

When potential outcomes follow a linear factor model, Abadie et al. (2010) showed that the existence of weights that achieve a perfect balance for the treated unit on *both* pre-treatment outcomes and observed covariates implies that it is possible to obtain bounds on the bias of the SC estimator that uses those weights to construct the SC unit.[1] Such bounds are valid when

[1] In this context, a perfect balance on pre-treatment outcomes and covariates means that there is a set of weights such that, for each of these variables, the value of the treated unit is equal to a weighted average of the control units using those weights.

the number of pre-treatment periods is fixed, and they become smaller as the number of pre-treatment periods increases. This result helped popularize the SC method. It showed that the SC method allows for substantially more flexibility in the treatment assignment mechanism relative to alternative methods, such as difference in differences, provided that a good balance in pre-treatment outcomes and observed covariates is achieved. In line with this result, Abadie et al. (2010, 2015) recommended estimating weights to construct an SC unit that provides a good balance in terms of both pre-treatment outcomes and observed covariates. Moreover, they recommended that the SC method be used only if the SC unit provides a good fit in terms of these variables.

Recent work, however, has placed more emphasis on the importance of matching on pre-treatment outcomes rather than on observed covariates.[2] This focus seems to be at odds with the recommendation of Abadie et al. (2010, 2015) to use the SC method only when covariates are also well balanced. In the present paper, we consider the role that observed covariates play in the SC method by characterizing the bias properties of the SC estimator without imposing a perfect balance assumption on observed covariates but maintaining a perfect balance on pre-treatment outcomes.

Our analysis has two main contributions. Our first contribution is to show that it is still possible to derive bounds on the bias of the SC estimator when we relax the assumption of perfect balance in terms of covariates. However, relaxing this assumption comes at some costs. In the absence of a perfect balance on covariates, we require stronger assumptions about the effects of both observed and unobserved covariates to derive bounds on the bias of the SC estimator.[3] This implies that we will generally require a larger number of pre-treatment periods to derive such bounds. Moreover, we show that the bounds obtained in this case are looser than when a perfect balance on covariates is also imposed, clarifying the role played by the observed covariates in the derivation of bounds on the bias of the SC estimator.

Our second contribution is to show that, contrary to the accepted intuition in the SC literature, a perfect balance on pre-treatment outcomes does not necessarily imply that the treated and SC units are alike in terms of observed and unobserved covariates, even when sufficient conditions to derive bounds on the bias of the SC estimator are satisfied. This can be the case even for covariates that are relevant in determining the potential outcomes. This result is somewhat surprising in light of the argument outlined by Abadie et al. (2015) that matching on pre-intervention outcomes would control for unobserved factors and for the heterogeneity of the effect of the observed and unobserved factors on the outcome of interest. The authors intuited that only units that are alike in terms of both observed and unobserved determinants of the outcome variable would also be alike in terms of trajectories of the outcome variable over extended periods of time. Although our results partially support that argument, we show that a perfect balance on lagged outcomes does not imply an approximate balance for covariates whose effects on the potential outcomes are multicollinear with the effects of the other observed and unobserved covariates.[4] We also

[2] For example, Doudchenko and Imbens (2016) suggested that, in terms of predictive power, lagged outcomes tend to be substantially more important than other covariates, which implies that covariates should play, in practice, a relatively minor role, whereas Gobillon and Magnac (2016) analysed the case in which all pre-treatment outcomes are included as predictors.

[3] Under a perfect balance only in the pre-treatment outcomes, we have to assume that the effects of both observed and unobserved covariates in the pre-treatment periods are linearly independent, so that it is possible to derive bounds on the bias of the SC estimator. In contrast, under a perfect balance on both pre-treatment outcomes and covariates, such a condition is required for the effects of only unobserved covariates.

[4] We refer to *approximate balance* for a covariate as the difference between the covariate for the treated unit and for the SC unit being bounded by a function that goes to zero with the number of pre-treatment periods.

show that a perfect balance on lagged outcomes fails to imply an approximate balance on relevant covariates if the covariates have nonlinear effects on the potential outcomes. Therefore, we show that it is still possible to derive bounds on the bias of the SC estimator, even when there is no approximate balance for observed covariates.

Our results have important implications for researchers applying the SC method. First, we show that the existence of a good balance in terms of both covariates and pre-treatment outcomes implies tighter bounds on the bias of the SC estimator relative to when a good balance is achieved in terms of only pre-treatment outcomes. However, it may not always be possible to have a good balance on both covariates and pre-treatment outcomes, either because there are no weights that provide a good fit on both or because covariates that are considered relevant in determining the potential outcomes are not observed. Our results show that, even in these cases, it may still be possible to bound the bias of the SC estimator. Therefore, a lack of a perfect or approximate balance on covariates should not necessarily be interpreted as evidence against the use of the SC method, as long as there is a good balance in terms of lagged outcomes over an extended period of time before the treatment. An important caveat, however, is that these results rely on stronger assumptions about the effects of observed and unobserved covariates than the assumptions considered by Abadie et al. (2010).

Second, our results provide new insights on the trade-offs involved in the choice of predictors in the implementation of the SC estimator. A common practice in the implementation of the SC method is to include all pre-treatment outcomes as predictors, which, as explained by Kaul et al. (2018), implies that the optimization method used to estimate the SC weights will render irrelevant all other covariates used as predictors.[5] On the one hand, our results show that the bias of the SC estimator is still bounded when we have good balance on pre-treatment outcomes, even if relevant covariates are imbalanced. Therefore, an estimation procedure that does not directly attempt to match on the covariates is not necessarily problematic, even when covariates are relevant in determining the potential outcomes. On the other hand, achieving a good balance in terms of covariates has the advantage of providing tighter bounds, so using information from covariates to construct weights that provide a better balance in terms of covariates may be warranted. In this case, given that we also show that there are instances in which balance on pre-treatment outcomes does not imply approximate balance on covariates, our results also highlight the importance of using a data-driven procedure to determine the relative importance of each predictor used in the estimation procedure. This guarantees that only covariates that should be balanced are considered in the estimation method.

The remainder of this paper is organized as follows. In Section 2, we set up the model and briefly review the results of Abadie et al. (2010). In Section 3, we present the new results. In Section 4, we discuss the implications of our results for the implementation of the SC method. In Section 5, we illustrate our results by revisiting the application studied by Abadie et al. (2015). Section 6 concludes. All proofs are in the Appendix.

[5] A nonexhaustive list of papers that applied the SC method using all pre-treatment outcomes as predictors includes Bilgel and Galle (2015), Billmeier and Nannicini (2013), Bohn et al. (2014), Cavallo et al. (2013), Hinrichs (2012), Kreif et al. (2016), and Liu (2015). The result derived by Kaul et al. (2018) is valid when all pre-treatment outcomes used in the 'outer optimization problem' are also included in the 'inner optimization problem' in the estimation procedure proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). This may not necessarily be the case if we consider the use of cross-validation, as proposed by Abadie et al. (2015).

2. BASELINE SYNTHETIC CONTROL MODEL

Let $Y_{it}(1)$ and $Y_{it}(0)$ be potential outcomes in the presence and in the absence of a treatment, respectively, for unit i at time t. Consider the model:

$$\begin{cases} Y_{it}(0) = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it} \\ Y_{it}(1) = \alpha_{it} + Y_{it}(0) \end{cases}, \tag{2.1}$$

where δ_t is an unknown common factor with constant factor loadings across units; λ_t is a $(1 \times F)$ vector of common factors; μ_i is a $(F \times 1)$ vector of unknown factor loadings; θ_t is a $(1 \times r)$ vector of unknown parameter; Z_i is a $(r \times 1)$ vector of observed covariates (not affected by the intervention), and the error terms ε_{it} are unobserved transitory shocks.[6] As in Abadie et al. (2010), we treat θ_t and λ_t as parameters. We say that a covariate Z_{ki}, for $1 \leq k \leq r$, is relevant if its associated coefficient $\theta_{kt} \neq 0$ for some t, and we refer to μ_i as an unobserved covariate. The observed outcomes are given by

$$Y_{it} = D_{it} Y_{it}(1) + (1 - D_{it}) Y_{it}(0), \tag{2.2}$$

where $D_{it} = 1$ if unit i is treated at time t.

Suppose that only unit 1 is treated and that we observe the outcomes of the treated unit and of J control units for T_0 pre-intervention periods and for T_1 post-intervention periods. We label the time periods as $-T_0 + 1, ..., 0, 1, ..., T_1$. Therefore, we have that $D_{it} = 1$ if $i = 1$ and $t > 0$, and $D_{it} = 0$ otherwise.

The main goal of the SC method is to estimate the treatment effect for unit 1 at each time $t > 0$—that is, α_{1t}.[7] Because $Y_{1t}(0)$ for $t > 0$ is not observed, the main idea of the SC method is to consider a weighted average of the control units to construct a proxy for this counterfactual. That is, for a given set of weights,

$$\mathbf{w} \in \{(w_2, ..., w_{J+1}) | \sum_{j=2}^{J+1} w_j = 1 \text{ and } w_j \geq 0\}, \tag{2.3}$$

we consider an estimator of the form

$$\hat{\alpha}_{1t}(\mathbf{w}) = Y_{1t} - \sum_{j \neq 1} w_j Y_{jt}$$

for $t > 0$.

Abadie et al. (2010) assumed the existence of $\mathbf{w}^* \in \mathbb{R}^J$, which satisfies (2.3), and such that

$$Y_{1t} = \sum_{j \neq 1} w_j^* Y_{jt}, \text{ for all } t \text{ such that } -T_0 + 1 \leq t \leq 0, \tag{2.4}$$

$$Z_1 = \sum_{j \neq 1} w_j^* Z_j, \tag{2.5}$$

where (2.4) is the assumption of a perfect balance on pre-treatment outcomes, and (2.5) is the assumption of a perfect balance on observed covariates. Given (2.4) and (2.5), Abadie et al.

[6] For example, Abadie and Gardeazabal (2003) considered as covariates Z_i gross total investment / GDP (average for 1964–1969), population density (in 1969), economic sectoral shares (average for 1961–1968), and human capital distribution (average for 1964–1969).

[7] We treat α_{1t} as given once the sample is drawn, as did Abadie et al. (2010) and Xu (2017).

(2010) considered an SC estimator using $\mathbf{w}^*$ as weights—that is,

$$\hat{\alpha}^*_{1t} = Y_{1t} - \sum_{j \neq 1} w^*_j Y_{jt}.$$

Under additional assumptions, they showed that the bias of $\hat{\alpha}^*_{1t}$ is bounded by a function that depends on T_0 and on the scale of the transitory shocks (defined in (3.1) below), such that this function goes to zero as T_0 increases.[8] As discussed by Abadie et al. (2010), the interpretation of this result is that the bias of the SC estimator is small when T_0 is large relative to the scale of the transitory shocks. The main intuition for this result is that conditions (2.4) and (2.5) can be satisfied for a long set of pre-treatment periods only if we also have that $\mu_1 \approx \sum_{j \neq 1} w^*_j \mu_j$. This result relies on the assumption of perfect balance in terms of lagged outcomes and covariates, so it should be thought of as a property of the SC estimator using $\mathbf{w}^*$, conditional on a realization of the data satisfying the conditions for existence of $\mathbf{w}^*$. This is an important result because it justifies, in a linear factor model setting, the procedure suggested in the original SC papers to estimate the SC weights by choosing weights that minimize the distance between the treated and the SC units in terms of pre-treatment outcomes and observed covariates. Although conditions (2.4) and (2.5) are generally not exactly satisfied for a given realization of the data, these results should be considered a good approximation when these conditions are approximately valid.[9] Indeed, Abadie et al. (2010, 2015) argued that the method should not be used if there is a poor balance in terms of pre-treatment outcomes and/or covariates.

3. THE ROLE OF COVARIATES IN THE SYNTHETIC CONTROL METHOD

We first derive conditions under which it is still possible to derive bounds on the bias of the SC estimator when (2.4) is assumed but (2.5) is not. We also derive conditions under which assuming (2.4) implies that (2.5) holds approximately. The main idea of our proof is to treat observed covariates (Z_i) as factor loadings and their associated time-varying effects (θ_t) as common factors.

Consider the following assumptions with regard to the transitory shocks, which are similar to those considered by Abadie et al. (2010).

ASSUMPTION 3.1. (a) ε_{it} are i.n.i.d; (b) $\mathbb{E}[\varepsilon_{it} | Z_i, \mu_i] = 0$; (c) for some even integer $p \geq 2$, $\mathbb{E}\left[|\varepsilon_{it}|^p\right] < \infty$, for all $t = -T_0 + 1, ..., 0$ and $i = 2, ..., J+1$.

We define the $1 \times (r + F)$ row vector $\gamma_t \equiv (\theta_t, \lambda_t)$, which is the vector of the effects of observed and unobserved covariates on the potential outcomes. Additionally, we denote by $\xi(T_0)$ the smallest eigenvalue of $\frac{1}{T_0} \sum_{t=-T_0+1}^{0} \gamma_t' \gamma_t$.

ASSUMPTION 3.2. $\xi(T_0) > 0$.

$\xi(T_0)$ is a measure of the degree of linear independence of the effects of observed and unobserved covariates in the T_0 pre-treatment periods. If, for example, $T_0 < r + F$, then the effects of observed and unobserved covariates in the T_0 pre-treatment periods must be multicollinear, and $\xi(T_0) = 0$. Assumption 3.2 implies that the effects of observed and unobserved covariates in the T_0 pre-treatment periods are linearly independent.

[8] See Abadie et al. (2010), page 504.

[9] See Ferman and Pinto (2018) for the implications of relaxing the perfect balance assumption on pre-treatment outcomes.

We also define

$$\overline{m}_p(T_0) \equiv \max_{i=2,...,J+1} \left(\frac{1}{T_0} \sum_{s=-T_0+1}^{0} E\left[|\varepsilon_{is}|^p\right] \right), \tag{3.1}$$

for the even integer p defined in Assumption 3.1 (c). Following Abadie et al. (2010), we refer to $\overline{m}_p(T_0)$ as a measure of the scale of the transitory shocks, ϵ_{jt}. Finally, let

$$\bar{\gamma}(T_0) \equiv \max_{t=-T_0+1,...,0,1,...,T_1; s=1,...,r+F} |\gamma_{ts}| .$$

Proposition 3.1. *Suppose that Y_{it}, $i = 1, ..., J+1$, $t = -T_0+1, ..., 0, 1, ..., T_1$, are observed and given by (2.1) and (2.2). Let there be weights $\mathbf{w}^* \in \mathbb{R}^J$ such that (2.3) and (2.4) hold, and let Assumptions 3.1 and 3.2 hold. Then, for any $t > 0$, $k = 1, ..., r$, and $l = 1, ..., F$,*

$$\left|\mathbb{E}\left[\hat{\alpha}^*_{1t}\right] - \alpha_{1t}\right| \leq \frac{C_\alpha \bar{\gamma}(T_0)^2}{\xi(T_0)} \max\left\{\frac{\overline{m}_p(T_0)^{1/p}}{T_0^{1-1/p}}, \frac{\overline{m}_2(T_0)^{1/2}}{T_0^{1/2}}\right\}, \tag{3.2}$$

$$\left|\mathbb{E}\left[Z_{k1} - \sum_{j=2}^{J+1} w^*_j Z_{kj}\right]\right| \leq \frac{C_{Z,k} \max\{\bar{\gamma}(T_0)^2, 1\}}{\xi(T_0)} \max\left\{\frac{\overline{m}_p(T_0)^{1/p}}{T_0^{1-1/p}}, \frac{\overline{m}_2(T_0)^{1/2}}{T_0^{1/2}}\right\}, \tag{3.3}$$

$$\left|\mathbb{E}\left[\mu_{l1} - \sum_{j=2}^{J+1} w^*_j \mu_{lj}\right]\right| \leq \frac{C_{\mu,l} \max\{\bar{\gamma}(T_0)^2, 1\}}{\xi(T_0)} \max\left\{\frac{\overline{m}_p(T_0)^{1/p}}{T_0^{1-1/p}}, \frac{\overline{m}_2(T_0)^{1/2}}{T_0^{1/2}}\right\}.$$

where C_α, $C_{Z,k}$, and $C_{\mu,l}$ are constants that do not depend on T_0 and that are defined in the proof.

Proof 1. We provide the proof of Proposition 3.1 in Appendix A.1. □

Proposition 3.1 shows that, when the effects of observed and unobserved covariates in the T_0 pre-treatment periods are linearly independent (Assumption 3.2), then a perfect balance in terms of pre-treatment outcomes implies that we can bound the degree of imbalance in terms of observed and unobserved covariates, which also makes it possible to derive bounds on the bias of the SC estimator. The bound on the bias of the SC estimator is increasing with the scale of the transitory shocks and decreasing with T_0 and with $\xi(T_0)$. In particular, when $\xi(T_0) = 0$, the effects of the covariates in the pre-treatment periods would be multicollinear, and it would not be possible to derive bounds on the imbalance of observed and unobserved covariates under a perfect balance on pre-treatment outcomes. Therefore, a necessary condition for Assumption 3.2 is that $T_0 \geq r + F$. We show below that it is still possible to derive bounds on the bias of the SC estimator when we relax Assumption 3.2.

If $\bar{m}_p(T_0)$ and $\bar{\gamma}(T_0)$ are bounded, and there are positive constants $\underline{\xi}$ and $\bar{T}$, such that $\xi(T_0) \geq \underline{\xi}$ for all $T_0 > \bar{T}$, then the function that bounds the bias of the SC estimator goes to zero as T_0 increases.[10] The intuition of this result is that, when observed and unobserved covariates have linearly independent effects on the potential outcomes, then it would be possible to have a good balance in terms of pre-treatment outcomes for a long set of pre-treatment periods only if we also have a good balance in terms of observed and unobserved covariates. Although, under these

[10] Those are sufficient, but not necessary, conditions so that the bounds asymptote to zero with $T_0 \to \infty$. If, for example, $\bar{m}_p(T_0)$ is unbounded, but it increases at a rate lower than $T_0^{1/2}$, then the bounds would still asymptote to zero.

conditions, the bias of the SC estimator would go to zero when $T_0 \to \infty$, note that the bounds derived in Proposition 3.1 are valid for finite T_0. As in Abadie et al. (2010), result (3.2)hould be understood as the bias of the SC estimator being small when T_0 is large relative to the scale of the transitory shocks.

Although Proposition 3.1 shows that it is still possible to derive bounds on the bias of the SC estimator even when (2.5) is not assumed, it comes at a cost of requiring stronger conditions on the effects of observed and unobserved covariates than those required for the bounds derived by Abadie et al. (2010), who assumed both (2.4) and (2.5). When both (2.4) and (2.5) are assumed, then Assumption 3.2 can be replaced by $\dot{\xi}(T_0) > 0$, where $\dot{\xi}(T_0)$ is the smallest eigenvalue of $\frac{1}{T_0} \sum_{t=-T_0+1}^{0} \lambda_t' \lambda_t$. Because λ_t is a subvector of γ_t, $\xi(T_0) > 0$ implies that $\dot{\xi}(T_0) > 0$. However, the converse may not be true. Therefore, in some settings, it may be possible to construct bounds on the basis of (2.4) and (2.5), whereas it would not be possible to construct bounds in the absence of (2.5). In particular, although a necessary condition for $\xi(T_0) > 0$ is that $T_0 \geq r + F$, a necessary condition for $\dot{\xi}(T_0) > 0$ is that $T_0 \geq F$. Therefore, in general, we require a larger T_0 to derive bounds on the bias of the SC estimator when (2.5) is relaxed.

For a given T_0, we also have that the bounds assuming both (2.4) and (2.5) are tighter than those assuming only (2.4).[11] The intuition is that assuming (2.5) eliminates the bias due to imbalance in observed covariates. Also, assuming both (2.4) and (2.5) provides tighter bounds on the degree of imbalance in terms of unobserved covariates relative to when only (2.4) is assumed, providing another reason why the bound on the bias of the SC estimator is tighter in this case. Therefore, although Proposition 3.1 shows that perfect balance in terms of covariates is not a necessary condition to derive bounds on the bias of the SC estimator, our results also highlight potential advantages of using covariates, one of which is that the bounds on the bias are tighter. Given this, we do not intend to argue that practitioners should not directly attempt to match on covariates. In fact, weights that satisfy both (2.4) and (2.5) should be preferred to weights that satisfy only (2.4), not only because this obtains tighter bounds on the bias, but also because it circumvents imposing stronger assumptions for the existence of bounds on the bias of the SC estimator.

The importance of considering the properties of the SC estimator when a perfect balance in terms of covariates is not assumed comes from the fact that, in empirical applications, covariates that are thought to be relevant in determining the potential outcomes may not all be observed. Moreover, even if all relevant covariates are observed, there may not be weights that provide a good balance in terms of both pre-treatment outcomes and such covariates. Proposition 3.1 shows that, when potential outcomes are given by (2.1) and observed and unobserved covariates have linearly independent effects on the potential outcomes, then an approximate balance on observed covariates follows when there is a perfect balance on pre-treatment outcomes. However, we now show different settings in which it is possible to derive bounds on the basis of a perfect balance in pre-treatment outcomes, even when there is no approximate balance on observed covariates. This can be the case when covariates are irrelevant in determining the potential outcomes, when the effects of observed and unobserved covariates are multicollinear, or when observed covariates have nonlinear effects on the potential outcomes. We discuss each case below.

We consider first the case in which some covariates are irrelevant or have effects that are multicollinear with the effects of other observed and unobserved covariates. That is, we allow for $\gamma_t \mathbf{b} = 0$ for all t for some $\mathbf{b} \in \mathbb{R}^{r+F} \backslash \{0\}$.[12] If we were considering a setting with only unobserved covariates, then we would always be able to redefine the unobserved covariates so that we had

[11] See details in Appendix A.2.

[12] For example, this allows for irrelevant covariates or for two or more covariates with time-invariant effects.

an observationally equivalent model with no irrelevant covariates, and such that the effects of the covariates were linearly independent. However, this is not the case if we have observed covariates. Let $1 \leq d \leq r + F$ be the dimension of the space $\{\mathbf{b} \in \mathbb{R}^{r+F}\setminus\{0\} | \gamma_t \mathbf{b} = 0 \; \forall \, t\}$. Therefore, for any T_0, $\frac{1}{T_0}\sum_{t=-T_0+1}^{0} \gamma_t' \gamma_t$ has at least d eigenvalues equal to zero, so $\xi(T_0) = 0$, and it is not possible to directly apply Proposition 3.1.

Without loss of generality, suppose that the first $\tilde{r}$ observed covariates are relevant and have effects that are not multicollinear with the effects of other observed and unobserved covariates. Let $\tilde{\theta}_t$ be a $1 \times \tilde{r}$ vector with the first $\tilde{r}$ components of θ_t, and let $\tilde{Z}_i$ be an $\tilde{r} \times 1$ vector with the first $\tilde{r}$ components of Z_i. Also, let $\tilde{a}$ be the dimension of the complement of the space $\{\mathbf{b} \in \mathbb{R}^{r+F}\setminus\{0\} | \gamma_t \mathbf{b} = 0 \; \forall \, t\}$. Then we can always find a $1 \times \tilde{a}$ vector $\tilde{\gamma}_t$ with first $\tilde{r}$ components equal to $\tilde{\theta}_t$, such that, for any $b \in \mathbb{R}^{r+F}$, there will be a $\tilde{b} \in \mathbb{R}^{\tilde{a}}$, such that $\gamma_t b = \tilde{\gamma}_t \tilde{b}$ for all t. Moreover, the first $\tilde{r}$ components of b are the same as the first $\tilde{r}$ components of $\tilde{b}$. Therefore, for any $X_i = (Z_i, \mu_i)'$, we can find an $\tilde{a} \times 1$ vector $\tilde{X}_i$, such that model (2.1) can be rewritten as

$$Y_{it}(0) = \delta_t + \tilde{\gamma}_t \tilde{X}_i + \varepsilon_{it}, \tag{3.4}$$

where the first $\tilde{r}$ components of $\tilde{X}_i$ are equal to $\tilde{Z}_i$.

Assuming that the smallest eigenvalue of $\frac{1}{T_0}\sum_{t=-T_0+1}^{0} \tilde{\gamma}_t' \tilde{\gamma}_t$ is greater than zero allows us to apply Proposition 3.1 to the rewritten model (3.4). Therefore, bounds on the bias of the SC estimator can be achieved under weaker conditions than those stated in Proposition 3.1. However, in this case, Proposition 3.1 guarantees an approximate balance only for the components of $\tilde{X}_i$, so it is not possible to guarantee an approximate balance for all observed covariates if $r > \tilde{r}$. There are two reasons for this. First, some covariates may be irrelevant in determining the potential outcomes. In that case, it is clear that a perfect balance on pre-treatment outcomes may be achieved even in the presence of imbalance in such covariates. More interestingly, there may be imbalance even for covariates that are relevant. For example, imagine that there is a time-invariant common factor $\lambda_{1t} = 1$, with associated factor loading μ_{1i}, and a covariate Z_{1i} with time-invariant effects $\theta_{1t} = \theta_1$. In this case, we would guarantee an approximate balance for $(\mu_{1i} + Z_{1i}\theta_1)$, but we would not be able to guarantee an approximate balance for μ_{1i} and for Z_{1i} separately. Intuitively, this multicollinearity implies that there would be weighted averages of the control units that may provide a good balance for the treated unit in terms of pre-treatment outcomes *even if there is imbalance in relevant observed covariates.* In other words, if we consider model (2.1), then we are able to guarantee approximate balance only for covariates whose effects are linearly independent of the effects of other observed and unobserved covariates. In this setting, our previous results indicating that we need stronger assumptions to derive bounds on the bias of the SC estimator, and that the bounds are looser when a perfect balance on covariates is not assumed, are still valid.[13]

Another setting in which perfect balance in terms of pre-treatment outcomes may not imply approximate balance in terms of covariates is when Z_i enters nonlinearly into the potential outcomes equation. In this case, it may be possible to derive bounds on the bias of the SC estimator without an approximate balance on covariates, even when the effects of covariates are not multicollinear. Suppose, for example, that $Y_{it}(0) = \delta_t + \theta_t g(Z_i) + \lambda_t \mu_i + \varepsilon_{it}$. Then, if $\xi(T_0) > 0$, a perfect balance on pre-treatment outcomes would imply bounds on the bias of

[13] If the smallest eigenvalue of $\frac{1}{T_0}\sum_{t=-T_0+1}^{0} \tilde{\gamma}_t' \tilde{\gamma}_t$ is greater than zero, then $\dot{\xi}(T_0) > 0$. Therefore, in this case, the sufficient conditions that allow us to derive the bounds on the bias of the SC estimator when (2.5) is not assumed are still stronger than the sufficient conditions when (2.5) is assumed. In this case, a necessary condition for the smallest eigenvalue of $\frac{1}{T_0}\sum_{t=-T_0+1}^{0} \tilde{\gamma}_t' \tilde{\gamma}_t$ being greater than zero is that $T_0 \geq \tilde{a} \geq F$. We also still have that the bounds derived when (2.5) is assumed are tighter than bounds derived when (2.5) is not assumed.

the SC estimator and approximate balance for $g(Z_1)$. However, in general, we would not have approximate balance for Z_1 unless g (.) were linear. The same reasoning would apply if we consider a more general model in which potential outcomes follow a linear factor model, where factor loadings are functions of observed and unobserved covariates, i.e., $Y_{it}(0) = \omega_t h(Z_i, \mu_i) + \varepsilon_{it}$. Interestingly, when we relax the functional form of model (2.1) in this way, we should *not* expect to find weights that provide a good balance in terms of both pre-treatment outcomes and covariates. However, it is still possible to provide bounds on the bias of the SC estimator when we have perfect balance only in terms of pre-treatment outcomes.

Taken together, these results show that there are advantages to considering weights that provide a good balance in terms of both pre-treatment outcomes and covariates. However, there may be empirical applications in which covariates thought to be relevant are not observed and/or there are no weights that provide a good balance in terms of observed covariates. In such cases, we show that it is still possible to derive bounds on the bias of the SC estimator, provided that there are weights that provide a perfect balance in terms of pre-treatment outcomes.

4. IMPLICATIONS FOR ESTIMATION OF THE SC WEIGHTS

Given a realization of the data, in general, there will not be weights that perfectly satisfy conditions (2.3), (2.4), and (2.5). Therefore, the original SC papers suggested an optimization procedure to estimate the SC weights, aiming at satisfying these conditions as closely as possible. They defined a set of K predictors, where X_1 is a $(K \times 1)$ vector containing the predictors for the treated unit, and X_0 is a $(K \times J)$ matrix of predictors for the control units. Predictors can be, for example, linear combinations of pre-intervention values of the outcome variable and observed covariates. The SC weights are estimated by minimizing $||X_1 - X_0\mathbf{w}||_V$ subject to $\sum_{i=2}^{J+1} w_j = 1$ and $w_j \geq 0$, where V is a $(K \times K)$ positive semidefinite matrix. Abadie and Gardeazabal (2003) and Abadie et al. (2010) discussed different possibilities for choosing the matrix V, including a data-driven process in which V is chosen such that the solution to the $||X_1 - X_0\mathbf{w}||_V$ optimization problem minimizes the pre-intervention squared prediction error.

If there is a $\mathbf{w}^*$ such that conditions (2.3), (2.4), and (2.5) are satisfied, then $\mathbf{w}^*$ would trivially be a solution to this optimization problem.[14] Therefore, because the theoretical results presented by Abadie et al. (2010) rely on the existence of such a $\mathbf{w}^*$, these results are not directly derived from the optimization procedure suggested to estimate the SC weights. Rather, these results provide a theoretical justification for estimation methods that aim to achieve a good balance in terms of pre-treatment outcomes and covariates. The idea is that, if a close-to-perfect balance is achieved (that is, conditions (2.3) and (2.4) are approximately satisfied), then these theoretical results should be approximately valid. In fact, Abadie et al. (2010, 2015) recommended that the method not be used if the pre-treatment fit is bad. In light of that, our results have important implications for the implementation of the SC method.

First, our results provide new insights on the trade-offs involved in the choice of predictors in the implementation of the SC estimator. For example, it is not clear whether one should include all pre-treatment outcome lags as predictors. Although this is a common procedure in SC applications, Kaul et al. (2018) showed that including all pre-treatment outcome lags as predictors renders all other covariates irrelevant in the optimization procedure suggested by Abadie and Gardeazabal (2003) and Abadie et al. (2010). Although this may appear to be a problem at first sight, inasmuch as the optimization procedure will not directly attempt to match on observed covariates, we show

[14] This is true regardless of how the matrix V is chosen.

that the bias of the SC estimator can still be bounded even if we have good balance only for pre-treatment outcomes. This is true even if those covariates are relevant. On the one hand, on the basis of our results, focusing on weights that also provide a good balance in covariates may impose unnecessary constraints in the search for weights that provide a good balance in terms of pre-treatment outcomes. For example, if X_1 includes only covariates Z_1, then the solution to the optimization problem suggested by Abadie and Gardeazabal (2003) and Abadie et al. (2010) may be a $\hat{\mathbf{w}}$ such that $Y_{1t} \neq \sum_{j \neq 1} \hat{w}_j Y_{jt}$ for some $t \leq 0$, even when condition (2.4) is satisfied. This suggests that at least some pre-treatment outcome lags should be included as predictors. On the other hand, achieving a good balance in terms of covariates may provide a better control for the effects of those *observed* covariates if the number of pre-treatment periods is small, as suggested by Kaul et al. (2018).

Interestingly, note that the inclusion of covariates as predictors when the outcome equation is nonlinear in covariates should not necessarily be a problem if one uses the data-driven procedure suggested by Abadie and Gardeazabal (2003) and Abadie et al. (2010) to determine the matrix V. In this case, if pre-treatment outcome lags are also included as predictors, then the importance given to such covariates would be relatively small, and the estimation procedure would not attempt to match on covariates that should not be matched on. In contrast, if a researcher uses an ad hoc matrix V (e.g., the identity matrix), then weights would be chosen to provide balance on covariates that should not be balanced. Therefore, our results also highlight the importance of using a procedure to choose the matrix V, such as the one suggested by Abadie and Gardeazabal (2003) and Abadie et al. (2010).

We also show that it is still possible to derive bounds on the bias of the SC estimator when observed covariates are imbalanced, even if such covariates are relevant in determining potential outcomes. Therefore, imbalance in terms of observed covariates should not necessarily be taken as evidence that the SC method should not be used, as long as we have a good balance in terms of pre-treatment outcomes. This expands the possibilities of empirical applications in which the SC method may be considered, inasmuch as observed covariates of the treated unit may not be in the convex hull of the observed covariates of the control units, while it may still be possible to find weights that provide a good balance in terms of pre-treatment outcomes. Our results also show that the SC method can be used even when covariates that are thought to be crucial determinants of the potential outcomes are not observed.

We have focused so far on the implementation of the SC method as suggested in the original SC papers (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015). In particular, we focused on the setting considered by Abadie et al. (2010), in which J is fixed, and we relied on perfect balance assumptions to derive bounds on the bias of the SC estimator. In this setting, we analysed the importance of assuming balance on covariates to derive such bounds and the role of considering balance on covariates as a diagnostic for the method. Recent papers, such as Xu (2017) and Athey et al. (2018), have proposed generalizations of the SC method. Xu (2017) separately estimated λ_t and μ_i and then constructed the counterfactual for the treated unit in the post-treatment periods by using these estimators, whereas Athey et al. (2018) considered matrix completion methods to construct the counterfactuals. These papers relied on both J and T_0 going to infinity so that the bias of their estimators goes to zero. By doing so, they were able to relax the assumptions of perfect balance on pre-treatment outcomes and covariates, so, in contrast to the original SC papers, they did not consider balance on covariates as a diagnostic for whether their proposed methods should be used. Therefore, our result that imbalance on covariates should not be taken as evidence against the use of the SC method is more relevant when J is not large, which is the

Table 1. Balance in observed covariates: Abadie et al. (2015) application.

	OECD (1)	West Germany (2)	Synthetic West Germany (Abadie et al, 2015) (3)	Synthetic West Germany (no covariates) (4)
GDP per capita	8021.1	15808.9	15802.2	15841.9
Trade openness	31.9	56.8	56.9	56.9
Inflation rate	7.4	2.6	3.5	4.8
Industry share	34.2	34.5	34.4	34.0
Schooling	44.1	55.5	55.2	53.2
Investment rate	25.9	27.0	27.0	25.0

Note: This table presents covariate values for a population-weighted average for the 16 OECD countries in the donor pool (column 1), West Germany (column 2), the synthetic West Germany obtained by using the specification considered by Abadie et al. (2015) (column 3), and the synthetic West Germany obtained by using all pre-treatment outcomes as predictors, with no covariates (column 4). GDP per capita, inflation rate, trade openness, and industry share are averaged for the 1981–1990 period. Investment rate and schooling are averaged for the 1980–1985 period.

setting considered in the original SC papers. Further analysis of the role of covariates in these generalizations of the SC method would be an interesting area for future research.

5. EMPIRICAL ILLUSTRATION: THE ECONOMIC COST OF THE 1990 GERMAN REUNIFICATION

Abadie et al. (2015) used the SC method to estimate the economic impact of the 1990 German reunification on West Germany. They constructed the SC weights by using average GDP per capita, trade openness, inflation rate, industry share, schooling, and investment rate as predictor variables. Table 1 shows that the synthetic West Germany is much closer to the actual West Germany in terms of these covariates than is a population-weighted average for the 16 Organisation for Economic Co-operation and Development (OECD) countries in the donor pool. This is not particularly striking, given that the optimization procedure to estimate the SC weights they considered included these variables as predictors.

We take advantage of the fact that there are a sizable number of pre-treatment periods in this application, and we consider an alternative SC specification in which we include all pre-treatment outcome lags (from 1960 to 1990) as predictor variables. When this alternative procedure is used to estimate the SC weights, the synthetic West Germany is still, in general, much closer to the actual West Germany in terms of the observed covariates than is the average of the OECD countries (Table 1, column 4). Although the balance in terms of these covariates is not as good as in the specification considered by Abadie et al. (2015), we emphasize that the optimization procedure we used to estimate these SC weights did not directly use information on these variables, whereas the procedure used by Abadie et al. (2015) did. In light of Proposition 3.1, this suggests that some of these covariates are relevant and have independent effects on the potential outcomes, so that the optimization procedure provides an approximate balance for them even though it does not directly attempt to provide such balance. Finally, Figure 1 shows that the two alternative specifications to estimate the SC weights provide very similar counterfactuals for West Germany after the reunification.

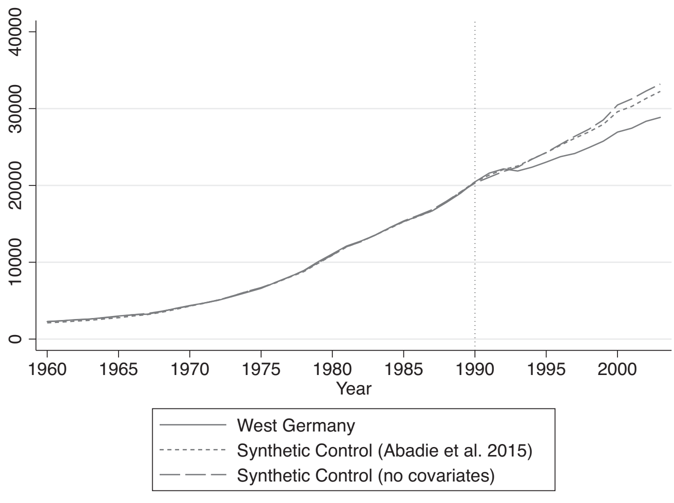

Figure 1. Trends in per capita GDP: West Germany versus synthetic West Germany. *Note:* This figure presents the per capita GDP for West Germany and for two alternative SC units: one computed by using the SC specification outlined in Abadie et al. (2015), and another computed by using all pre-treatment outcome lags as predictors, with no covariates.

Overall, the results from this empirical illustration corroborate one of the main messages of our paper. In this application, the SC method focusing on matching only on the pre-treatment outcomes leads to an SC unit that also approximates the treated unit in terms of many of the covariates. As a consequence, the counterfactual estimated by using only pre-treatment outcomes is very close to the original one that directly attempted to match on these covariates. Given that we show in Section 3 that the bounds when both pre-treatment outcomes and covariates are perfectly balanced are tighter than the bounds when we have perfect balance only on pre-treatment outcomes, it is not clear that ignoring the covariates would be a better alternative in this case because information on covariates can be important to reduce the bias of the estimator (for a given T_0). Still, these results suggest that, in this application, it would be possible to construct a counterfactual for the treated unit even if there were no information on relevant covariates.

6. CONCLUSION

We have revisited the role of observed covariates in the SC method. We formally derived two sets of results. First, we provided conditions under which the result derived by Abadie et al. (2010) with regard to the bias of the SC estimator remains valid when we relax the assumption of perfect balance on covariates and assume a perfect balance only on pre-treatment outcomes. We showed that it remains possible to derive bounds on the bias of the SC estimator when a perfect balance on covariates is not assumed, but that this relies on stronger assumptions and generates looser bounds. Second, we provided conditions under which a perfect balance on pre-treatment outcomes implies an approximate balance for the covariates. We showed that an approximate balance for covariates may not be achieved even when the bias of the SC estimator is bounded. This may be the case even for relevant covariates. Taken together, our results show that, although there may be advantages to balancing on covariates to construct the SC estimator, a perfect balance on covariates should not be required for the SC method as long as there is a perfect balance on a long set of pre-treatment outcomes. Our results have direct implications on how applied researchers should implement the SC method and on how they should assess the applicability of the method.

ACKNOWLEDGEMENT

Bruno Ferman gratefully acknowledges financial support from FAPESP.

REFERENCES

Abadie, A., A. Diamond and J. Hainmueller (2010). Synthetic control methods for comparative case studies: estimating the effect of California's tobacco control program. *Journal of the American Statiscal Association 105*, 493–505.

Abadie, A., A. Diamond and J. Hainmueller (2015). Comparative politics and the synthetic control method. *American Journal of Political Science 59*, 495–510.

Abadie, A. and J. Gardeazabal (2003). The economic costs of conflict: a case study of the Basque country. *American Economic Review 93*, 1, 113–32.

Athey, S., M. Bayati, N. Doudchenko, G. Imbens and K. Khosravi (2018). Matrix completion methods for causal panel data models. Working Paper, Stanford University, Stanford, CA, USA, arXiv:1710.10251.

Athey, S. and G. W. Imbens (2017). The state of applied econometrics: causality and policy evaluation. *Journal of Economic Perspectives 31*, 2, 3–32.

Bilgel, F. and B. Galle (2015). Financial incentives for kidney donation: a comparative case study using synthetic controls. *Journal of Health Economics 43*, 103–17.

Billmeier, A. and T. Nannicini (2013). Assessing economic liberalization episodes: a synthetic control approach. *Review of Economics and Statistics 95*, 983–1001.

Bohn, S., M. Lofstrom and S. Raphael (2014). Did the 2007 Legal Arizona Workers Act reduce the state's unauthorized immigrant population? *Review of Economics and Statistics 96*, 258–69.

Cavallo, E., S. Galiani, I. Noy and J. Pantano (2013). Catastrophic natural disasters and economic growth. *Review of Economics and Statistics 95*, 1549–61.

Doudchenko, N. and G. Imbens (2016). Balancing, regression, difference-in-differences and synthetic control methods: a synthesis. Working Paper, Stanford University, Stanford, CA, USA, arXiv:1610.07748.

Ferman, B. and C. Pinto (2018). Synthetic controls with imperfect pre-treatment fit. Working Paper, Sao Paulo School of Economics - FGV, Sao Paulo, Brazil.

Gobillon, L. and T. Magnac (2016). Regional policy evaluation: interactive fixed effects and synthetic controls. *Review of Economics and Statistics 98*, 535–51.

Hinrichs, P. (2012). The effects of affirmative action bans on college enrollment, educational attainment, and the demographic composition of universities. *Review of Economics and Statistics 94*, 712–22.

Kaul, A., S. Klöbner, G. Pfeifer and M. Schieler (2018). Synthetic control methods: never use all pre-intervention outcomes together with covariates. Working Paper, Saarland University, Saarbrücken, Saarland, Germany.

Kreif, N., R. Grieve, D. Hangartner, A. J. Turner, S. Nikolova and M. Sutton (2016). Examination of the synthetic control method for evaluating health policies with multiple treated units. *Health Economics 25*, 1514–28.

Liu, S. (2015). Spillovers from universities: evidence from the land-grant program. *Journal of Urban Economics 87*, 25–41.
Xu, Y. (2017). Generalized synthetic control method: causal inference with interactive fixed effects models. *Political Analysis 25*, 57–76.

SUPPORTING INFORMATION

Additional Supporting Information may be found in the online appendix of this article at the publisher's website:

appendix
Replication Package

Co-editor John Rust handled this manuscript.

APPENDIX A: PROOFS OF RESULTS

A.1. Proof of Proposition 3.1

The proof closely follows Abadie et al. (2010). We first prove result (3.2) of Proposition 3.1. First, notice that

$$Y_{1t}(0) - \sum_{i=2}^{J+1} w_i Y_{it}(0) = \gamma_t \left(X_1 - \sum_{i=1}^{J+1} w_i X_i \right) + \sum_{i=2}^{J+1} w_i (\varepsilon_{1t} - \varepsilon_{it}), \text{ for any } t, \tag{A.1}$$

where $X_i = (Z_i, \mu_i)'$ is an $(r + F) \times 1$ vector.

Stacking pre-treatment variables, i.e., $Y_i^P \equiv (Y_{i1}, ..., Y_{iT_0})'$, we have that:

$$Y_1^P - \sum_{i=2}^{J+1} w_i Y_i^P = \Gamma^P \left(X_1 - \sum_{i=1}^{J+1} w_i X_i \right) + \sum_{i=2}^{J+1} w_i \left(\varepsilon_1^P - \varepsilon_i^P \right), \tag{A.2}$$

where Y_i^P and ε_i^P are $T_0 \times 1$ vectors, and $\Gamma^P = [\gamma_1', ..., \gamma_{T_0}']'$ is a $T_0 \times (r + F)$ matrix.

Under Assumption 3.2, $\Gamma^{P'}\Gamma^P$ is invertible, so we can solve (A.2) for $\left(X_1 - \sum_{i=1}^{J+1} w_i X_i\right)$ to obtain

$$\left(X_1 - \sum_{i=1}^{J+1} w_i X_i \right) = \left(\Gamma^{P'}\Gamma^P \right)^{-1} \Gamma^{P'} \left(Y_1^P - \sum_{i=2}^{J+1} w_i Y_i^P \right)$$
$$- \left(\Gamma^{P'}\Gamma^P \right)^{-1} \Gamma^{P'} \sum_{i=2}^{J+1} w_i \left(\varepsilon_1^P - \varepsilon_i^P \right) \tag{A.3}$$

Plugging this into (A.1) obtains, for any $t > 0$,

$$Y_{1t}(0) - \sum_{i=2}^{J+1} w_i Y_{it}(0) = \gamma_t \left(\Gamma^{P'}\Gamma^P \right)^{-1} \Gamma^{P'} \left(Y_1^P - \sum_{i=2}^{J+1} w_i Y_i^P \right)$$
$$-\gamma_t \left(\Gamma^{P'}\Gamma^P \right)^{-1} \Gamma^{P'} \sum_{i=2}^{J+1} w_i \left(\varepsilon_1^P - \varepsilon_i^P \right)$$
$$+ \sum_{i=2}^{J+1} w_i (\varepsilon_{1t} - \varepsilon_{it}).$$

Using (2.3) and (2.4) obtains, for any $t > 0$,

$$Y_{1t}(0) - \sum_{i=2}^{J+1} w_i^* Y_{it}(0) \tag{A.4}$$

$$= \gamma_t \left(\Gamma^{P'}\Gamma^P \right)^{-1} \Gamma^{P'} \sum_{i=2}^{J+1} w_i^* \varepsilon_i^P \tag{A.5}$$

$$-\gamma_t \left(\Gamma^{P'}\Gamma^P \right)^{-1} \Gamma^{P'} \varepsilon_1^P$$

$$+\sum_{i=2}^{J+1} w_i^* \left(\varepsilon_{1t} - \varepsilon_{it}\right).$$

Importantly, note that w_i^* is a function of ϵ_{it} for $t \leq 0$, but it is independent of ϵ_{it} for $t > 0$ because of Assumption 3.1 (a). Therefore, because of Assumption 3.1 (b),

$$\mathbb{E}\left[\gamma_t \left(\Gamma^{P\prime}\Gamma^P\right)^{-1} \Gamma^{P\prime}\varepsilon_1^P\right] = 0$$

and $\mathbb{E}\left[\sum_{i=2}^{J+1} w_i^* \left(\varepsilon_{1t} - \varepsilon_{it}\right)\right] = 0$. However, we cannot guarantee that

$$\mathbb{E}\left[\gamma_t \left(\Gamma^{P\prime}\Gamma^P\right)^{-1} \Gamma^{P\prime}\sum_{i=2}^{J+1} w_i^* \varepsilon_i^P\right] = 0,$$

because w_i^* is a random variable that is a function of ϵ_i^P.

Noting that the $(r+F) \times (r+F)$ matrix $\Gamma^{P\prime}\Gamma^P = \sum_{j=-T_0+1}^{0} \gamma_j'\gamma_j$, we write the right-hand side of (A.5) as:

$$\gamma_t \left(\Gamma^{P\prime}\Gamma^P\right)^{-1} \Gamma^{P\prime}\sum_{i=2}^{J+1} w_i^* \varepsilon_i^P = \sum_{i=2}^{J+1} w_i^* \gamma_t \left(\sum_{j=-T_0+1}^{0} \gamma_j'\gamma_j\right)^{-1} \sum_{s=-T_0+1}^{0} \gamma_s'\varepsilon_{is}$$

$$= \sum_{i=2}^{J+1} w_i^* \sum_{s=-T_0+1}^{0} \psi_{ts}\varepsilon_{is} \tag{A.6}$$

where

$$\psi_{ts} \equiv \gamma_t \left(\sum_{j=-T_0+1}^{0} \gamma_j'\gamma_j\right)^{-1} \gamma_s'.$$

Therefore, taking expectations on both sides of (A.4) and using expression (A.6) obtains for any $t > 0$:

$$\mathbb{E}\left[Y_{1t}(0) - \sum_{i=2}^{J+1} w_i^* Y_{it}(0)\right] = \mathbb{E}\left[\sum_{i=2}^{J+1} w_i^* \sum_{s=-T_0+1}^{0} \psi_{ts}\varepsilon_{is}\right]. \tag{A.7}$$

We now derive bounds for expression (A.7). First, consider the following string of inequalities:

$$\psi_{ts}^2 \leq \psi_{tt}\psi_{ss} \leq \left(\frac{(r+F)\,\overline{\gamma}(T_0)^2}{T_0\xi(T_0)}\right)^2,$$

where the first inequality follows by the Cauchy-Schwarz inequality and by the fact that $\sum_{j=1}^{T_0} \gamma_j'\gamma_j$ is positive definite and symmetric, whereas the second inequality follows because $\left(\frac{1}{T_0}\sum_{j=-T_0+1}^{0} \gamma_j'\gamma_j\right)^{-1}$ is symmetric positive definite, with its largest eigenvalue given by $\xi(T_0)^{-1}$. Then

$$\psi_{tt} \leq \frac{\gamma_t\gamma_t'}{T_0\xi(T_0)} = \frac{\sum_{m=1}^{r+F} \gamma_{tm}^2}{T_0\xi} \leq \frac{(r+F)\,\overline{\gamma}(T_0)^2}{T_0\xi(T_0)},$$

and, similarly,

$$\psi_{ss} \leq \frac{(r+F)\,\overline{\gamma}(T_0)^2}{T_0\xi(T_0)}.$$

Define

$$\bar{\varepsilon}_{it} \equiv \sum_{s=-T_0+1}^{0} \psi_{ts}\varepsilon_{is},\ i = 2, ..., J+1,$$

and consider

$$\left|\sum_{i=2}^{J+1} w_i^* \bar{\varepsilon}_{it}\right| \leq \sum_{i=2}^{J+1} w_i^* \left|\bar{\varepsilon}_{it}\right|$$

$$\leq \left(\sum_{i=2}^{J+1} \left(w_i^*\right)^q\right)^{1/q} \left(\sum_{i=2}^{J+1} \left|\bar{\varepsilon}_{it}\right|^p\right)^{1/p} \tag{A.8}$$

$$\leq \left(\sum_{i=2}^{J+1} \left|\bar{\varepsilon}_{it}\right|^p\right)^{1/p}, \tag{A.9}$$

where (A.8) follows by Hölder's inequality, with $p, q > 1$ and $\frac{1}{p} + \frac{1}{q} = 1$, and (A.9) follows by norm monotonicity and (2.3). Hence, applying Hölder's again obtains:

$$\mathbb{E}\left[\sum_{i=2}^{J+1} w_i^* \left|\bar{\varepsilon}_{it}\right|\right] \leq \left[\mathbb{E}\left[\sum_{i=2}^{J+1} \left|\bar{\varepsilon}_{it}\right|^p\right]\right]^{1/p}.$$

Applying Rosenthal's inequality to

$$\mathbb{E}\left[\left|\bar{\varepsilon}_{it}\right|^p\right] = \mathbb{E}\left[\left|\sum_{s=-T_0+1}^{0} \psi_{ts}\varepsilon_{is}\right|^p\right]$$

obtains

$$\mathbb{E}\left[\left|\sum_{s=-T_0+1}^{0} \psi_{ts}\varepsilon_{is}\right|^p\right] \leq \tag{A.10}$$

$$\leq C\left(p\right) \max \left\{ \begin{array}{c} \left(\frac{(r+F)\overline{\gamma}(T_0)^2}{T_0\xi(T_0)}\right)^p \sum_{s=-T_0+1}^{0} \mathbb{E}\left|\varepsilon_{is}\right|^p, \\ \left(\frac{(r+F)\overline{\gamma}(T_0)^2}{T_0\xi(T_0)}\right)^p \left(\sum_{s=-T_0+1}^{0} \mathbb{E}\left(\varepsilon_{is}\right)^2\right)^{p/2} \end{array} \right\}$$

$$= C\left(p\right) \left(\frac{(r+F)\,\overline{\gamma}(T_0)^2}{\xi(T_0)}\right)^p \max \left\{ \begin{array}{c} \frac{1}{T_0^p} \sum_{s=-T_0+1}^{0} \mathbb{E}\left|\varepsilon_{is}\right|^p, \\ \left(\frac{1}{T_0^2} \sum_{s=-T_0+1}^{0} \mathbb{E}\left(\varepsilon_{is}\right)^2\right)^{p/2} \end{array} \right\},$$

where the constant $C\left(p\right) = \mathbb{E}[\theta - 1]^p$ and where θ is a Poisson random variable with parameter 1, which is the best constant as shown by Ibragimov and Sharakhmetov (). The inequality in A.10 follows because

$$\sum_{s=-T_0+1}^{0} \mathbb{E}\left[\left|\psi_{ts}\varepsilon_{is}\right|^p\right] = \sum_{s=-T_0+1}^{0} \left|\psi_{ts}\right|^p \mathbb{E}\left(\left|\varepsilon_{is}\right|^p\right)$$

$$\leq \left(\frac{(r+F)\,\overline{\gamma}(T_0)^2}{T_0\xi(T_0)}\right)^p \sum_{s=-T_0+1}^{0} \mathbb{E}\left|\varepsilon_{is}\right|^p$$

$$\sum_{s=-T_0+1}^{0} \mathbb{E}\left[|\psi_{ts}\varepsilon_{is}|^2\right] = \sum_{s=-T_0+1}^{0} (\psi_{ts})^2 \, \mathbb{E}\,(\varepsilon_{is})^2$$

$$\leq \left(\frac{(r+F)\,\overline{\gamma}(T_0)^2}{T_0\xi(T_0)}\right)^2 \sum_{s=-T_0+1}^{0} \mathbb{E}\,(\varepsilon_{is})^2 .$$

Finally, for any $t > 0$,

$$\left|\mathbb{E}\left[\widehat{\alpha}^*_{1t}\right] - \alpha_{1t}\right| \leq$$

$$\leq \mathbb{E}\left[\sum_{i=2}^{J+1} w_i^* \,|\overline{\varepsilon}_{it}|\right]$$

$$\leq \left[\mathbb{E}\left[\sum_{i=2}^{J+1} |\overline{\varepsilon}_{it}|^p\right]\right]^{1/p}$$

$$\leq \left[\sum_{i=2}^{J+1} \mathbb{E}\left[\left|\sum_{s=-T_0+1}^{0} \psi_{ts}\varepsilon_{is}\right|^p\right]\right]^{1/p}$$

$$\leq C^{1/p}(p)\left(\frac{(r+F)\,\overline{\gamma}(T_0)^2}{\xi(T_0)}\right)\left[\sum_{i=2}^{J+1} \max\left\{\begin{array}{c} \frac{1}{T_0^p}\sum_{s=-T_0+1}^{0} \mathbb{E}\,|\varepsilon_{is}|^p\,, \\ \left(\frac{1}{T_0^2}\sum_{s=-T_0+1}^{0} \mathbb{E}\,(\varepsilon_{is})^2\right)^{p/2} \end{array}\right\}\right]^{1/p}$$

$$\leq (J \times C\,(p))^{1/p}\left(\frac{(r+F)\,\overline{\gamma}(T_0)^2}{\xi(T_0)}\right)\max\left\{\frac{\overline{m}_p(T_0)^{1/p}}{T_0^{1-1/p}}, \frac{\overline{m}_2(T_0)^{1/2}}{T_0^{1/2}}\right\}. \qquad \text{(A.11)}$$

The proof for results (3.3) and () follows by similar arguments. First, define the $1 \times (r+F)$ vector $\rho_k \equiv [0, 0, ..., 1, ..., 0]$, where only the k^{th} element equals to 1. Consider k such that $1 \leq k \leq r$. From equation (A.3),e have that:

$$\left(Z_{k,1} - \sum_{i=1}^{J+1} w_i Z_{k,i}\right) = \rho_k\left(X_1 - \sum_{i=1}^{J+1} w_i X_i\right)$$

$$= \rho_k\left(\Gamma^{P\prime}\Gamma^P\right)^{-1}\Gamma^{P\prime}\left(Y_1^P - \sum_{i=2}^{J+1} w_i Y_i^P\right)$$

$$-\rho_k\left(\Gamma^{P\prime}\Gamma^P\right)^{-1}\Gamma^{P\prime}\sum_{i=2}^{J+1} w_i\left(\varepsilon_1^P - \varepsilon_i^P\right).$$

If we define $\bar{\gamma}'(T_0) = \max\{\bar{\gamma}(T_0), 1\} > 0$, then the proof of result (3.3) follows exactly the same steps as the proof of result (3.2) if we use $\bar{\gamma}'(T_0)$ instead of $\bar{\gamma}(T_0)$, so we have bounds for $\left|\mathbb{E}\left[Z_{k1} - \sum_{i=2}^{J+1} w_i^* Z_{ki}\right]\right|$. Similarly, if we consider $l > r$, we have bounds for $\left|\mathbb{E}\left[\mu_{l1} - \sum_{i=2}^{J+1} w_i^* \mu_{li}\right]\right|$.

A.2. Contrasting the bounds with and without condition (2.5)

Denote by $\dot{\xi}(T_0)$ the smallest eigenvalue of $\frac{1}{T_0}\sum_{t=-T_0+1}^{0}\lambda_t'\lambda_t$. Because λ_t is a subvector of γ_t, then $\dot{\xi}(T_0) \geq \xi(T_0)$ for all T_0. Also, define

$$\bar{\lambda}(T_0) \equiv \max_{t=-T_0+1,\ldots,0,1,\ldots,T_1;s=1,\ldots,F} |\lambda_{ts}|,$$

so that $\bar{\lambda}(T_0) \leq \bar{\gamma}(T_0)$.

Under Assumption 3.1, and assuming that $\dot{\xi}(T_0) > 0$ and that conditions (2.4) and (2.5) hold, it follows that

$$\left|\mathbb{E}\left[\widehat{\alpha}_{1t}^{*}\right] - \alpha_{1t}\right| \leq (J \times C(p))^{1/p}\left(\frac{F\bar{\lambda}(T_0)^2}{\dot{\xi}(T_0)}\right)\max\left\{\frac{\overline{m}_p(T_0)^{1/p}}{T_0^{1-1/p}}, \frac{\overline{m}_2(T_0)^{1/2}}{T_0^{1/2}}\right\}. \quad \text{(A.12)}$$

Because $r > 0$, $\bar{\lambda}(T_0) \leq \bar{\gamma}(T_0)$, and $\dot{\xi}(T_0) \geq \xi(T_0)$, it follows that the bounds derived under conditions (2.4) and (2.5) (equation A.12) are tighter than the bounds derived under condition (2.4) only (equation A.11).

REFERENCE

Ibragimov R., S. Sharakhmetov (2002). The exact constant in the Rosenthal inequality for random variables with mean zero. Theory of Probability and Its Applications 46, 127–32.