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11 March 2018

Cherry Picking with Synthetic Controls*

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First Draft: June 2016
This Draft: March 2018

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Abstract

We evaluate whether a lack of guidance on how to choose the matching variables used in the Synthetic Control (SC) estimator creates specification-searching opportunities. We first provide theoretical results showing that specification-searching opportunities would be asymptotically irrelevant when the number of pre-treatment periods goes to infinity when we restrict to a subset of SC specifications. However, based on Monte Carlo simulations and simulations with real datasets, we show significant room for specification searching when the number of pre-treatment periods is finite and when alternative specifications commonly used in SC applications are also considered. This undermines one of the potential advantages of the method, which is providing a transparent way of choosing comparison units and, therefore, being less susceptible to specification searching than alternative methods. To address this problem, we provide recommendations to limit the possibilities for specification searching in the SC method. Finally, we analyze the possibilities for specification searching and our recommendations in two empirical applications.

Keywords: inference; synthetic control; p-hacking; specification searching

JEL Codes: C12; C21; C33

*We would like to thank Sergio Firpo, Ricardo Masini, Masayuki Sawada, and participants at the Sao School of Economics seminar, Yale Econometrics Lunch, African Meeting of the Econometric Society, and the 2016 Meeting of the Brazilian Econometric Society for the excellent comments and suggestions. Deivis Angeli provided outstanding research assistance.

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1 Introduction

The synthetic control (SC) method has been recently proposed in a series of seminal papers by Abadie & Gardeazabal (2003), Abadie et al. (2010), and Abadie et al. (2015) as an alternative method to estimate treatment effects in comparative case studies. Despite being relatively new, this method has been used in a wide range of applications, including the evaluation of the impact of terrorism, civil wars and political risk, natural resources and disasters, international finance, education and research policy, health policy, economic and trade liberalization, political reforms, labor, taxation, crime, social connections, and local development.[1] Athey & Imbens (2017) describe the SC method as arguably the most important innovation in the policy evaluation literature in the last fifteen years.

Abadie et al. (2010) and Abadie et al. (2015) describe many advantages of the SC estimator over techniques traditionally used in comparative studies. Among them, one important feature of the SC method is that it provides a transparent way to choose comparison units. In the SC method, a data-driven process is used to choose the weights that will build the weighted-average of the controls' outcomes that will represent the counterfactual for the treated unit. Also, since the estimation of the SC weights does not require access to post-intervention outcomes, researchers could decide on the study design without knowing how those decisions would affect the conclusions of their studies. Taken together, these features potentially make the SC method less susceptible to specification searching relative to alternative methods for comparative case studies. This could be an important advantage of the SC method given the growing debate about transparency in social science research (e.g., Miguel et al. (2014)).[2]

An important limitation of the SC method, however, is that it does not provide clear guidance

[1] SC has been used in the evaluation of the impact of terrorism, civil wars and political risk (Abadie & Gardeazabal (2003), Bove et al. (2014), Li (2012), Montalvo (2011), Yu & Wang (2013)), natural resources and disasters (Barone & Mocetti (2014), Cavallo et al. (2013), Coffman & Noy (2011), DuPont & Noy (2012), Mideksa (2013), Sills et al. (2015), Smith (2015)), international finance (Jinjarak et al. (2013), Sanso-Navarro (2011)), education and research policy (Belot & Vandenberghe (2014), Chan et al. (2014), Hinrichs (2012)), health policy (Bauhoff (2014), Kreif et al. (2015)), economic and trade liberalization (Billmeier & Nannicini (2013), Gathani et al. (2013), Hosny (2012)), political reforms (Billmeier & Nannicini (2009), Carrasco et al. (2014), Dhungana (2011) Ribeiro et al. (2013)), labor (Bohn et al. (2014), Calderon (2014)), taxation (Kleven et al. (2013), de Souza (2014)), crime (Pinotti (2012*b*), Pinotti (2012*a*), Robbins et al. (2017), Saunders et al. (2014)), social connections (Acemoglu et al. (2013)), and local development (Ando (2015), Gobillon & Magnac (2016), Kirkpatrick & Bennear (2014), Liu (2015), Possebom (2017), Severnini (2014)).

[2] See Christensen & Miguel (2016) for an extensive literature review on research transparency and reproducibility both in economics and other fields.

on the choice of predictor variables and covariates that should be used to estimate the SC weights.[3] Although Abadie et al. (2010) define vectors of linear combinations of pre-intervention outcomes that could be used as predictors, there is no specific recommendation about which variables should be used. Such lack of guidance on how to choose the predictors when implementing the synthetic control method translates into a wide variety of different specifications in empirical applications of this method. For example, some applied papers use all pre-treatment outcome lags as economic predictors, other papers select a subset of the pre-treatment outcome lags as economic predictors, while other papers use the mean of all pre-treatment outcome lags and other covariates as economic predictors.[4] If different specifications result in widely different choices of the SC unit, then a researcher would have relevant opportunities to select "statistically significant" specifications even when there is no effect.[5] Since a researcher would usually not be able to commit to a particular specification before knowing how these decisions would affect the conclusion of her study, this flexibility may undermine one of the main advantages of the SC method.[6]

In this paper, we evaluate the extent to which this variety of options in the synthetic control method creates opportunities for specification searching considering only one particular step of the method: the choice of predictors used in the estimation of the SC weights.[7] We first provide

[3]To the best of our knowledge, Dube & Zipperer (2015) and Kaul et al. (2015) are the only other authors to point out that there is little explicit guidance in the SC literature to determine the choice of predictors. However, they do not explore the implications of such lack of specific guidance on the possibilities for specification searching in SC applications.

[4]For example, Abadie & Gardeazabal (2003), Abadie et al. (2015) and Kleven et al. (2013) use the mean of all pre-treatment outcome values and other covariates as predictors; Billmeier & Nannicini (2013), Bohn et al. (2014), Gobillon & Magnac (2016), Hinrichs (2012) use all the pre-treatment outcome values; Smith (2015) selects 4 out of 10 pre-treatment periods; Abadie et al. (2010) select 3 out of 19 pre-treatment periods; and Montalvo (2011) uses only the last two pre-treatment outcome values.

[5]We consider for inference the placebo test suggested in Abadie et al. (2010). Although this is not a formal randomization test if treatment is not randomly assigned, we focus on this test because it is the most commonly used test in SC application. Following Abadie et al. (2010), we can think of this test as the probability of having a test statistic on the top 5% of the distribution of test statistics in the placebo runs. In practice, this is how most applied researchers evaluate whether the SC estimator is significant in their applications. Moreover, the randomization inference assumptions are valid in the data generating processes in our simulations. Therefore, the placebo test is statistically valid in our simulations. See Firpo & Possebom (2017), Ferman & Pinto (2017) and Hahn & Shi (2016) for details on the statistical properties of this test. As a robustness check, we also consider inference with an *infeasible* test in our MC simulations where we take advantage that the data generating process is known to calculate the distribution of the test statistics, instead of using the distribution of placebo runs as in the test proposed in Abadie et al. (2010). All results are qualitatively the same.

[6]Olken (2015) and Coffman & Niederle (2015) evaluate the use of pre-analysis plans in social sciences. For randomized control trials (RCT), the American Economic Association (AEA) launched a site to register experimental designs. However, there is no site where one would be able to register a prospective synthetic control study. Moreover, in many synthetic control applications both pre- and post-intervention information would be available to the researcher before the possibility of registering the study. In this case, it would be unfeasible to commit to a particular specification.

[7]There may be other dimensions in the implementation of the SC method that provide discretionary choices for

conditions under which different SC specifications lead to asymptotically equivalent estimators when the number of pre-treatment periods (T_0) goes to infinity, as long as we restrict to specifications such that the number of pre-treatment outcome lags used as predictors goes to infinity with T_0.[8] Under these conditions, we also show that the placebo test suggested in Abadie et al. (2010) will asymptotically lead to the same conclusion, as long as we restrict to this subset of SC specifications. However, these results leave open the possibility for specification searching in SC applications for at least two reasons. First, many SC applications do not have a large number of pre-treatment periods to justify large-T_0 asymptotics, as argued in Doudchenko & Imbens (2016), which may still leave room to specification searching even if we restrict to this class of SC specifications. Moreover, there are common SC specifications that do not satisfy the condition on the number of pre-treatment periods used as predictors going to infinity, which might lead to specification-searching opportunities even when the number of pre-treatment periods is large.

Guided by our theoretical results, we then evaluate the extent to which specification searching may be a problem in SC applications using Monte Carlo (MC) simulations and placebo simulations with the Current Population Survey (CPS). We calculate the probability that a researcher would find at least one specification such that the ratio of post/pre mean squared prediction error (MSPE) of the actual intervention is in the top 5% distribution of the post/pre MSPE estimated for units not exposed to the intervention when the actual effect of the intervention is zero, which would lead him/her to interpret the results as "significant". If different SC specifications lead to similar SC estimators, then this probability would be close to 5%, while it may be much higher than 5% if different SC specifications lead to wildly different estimates, implying that there is room for specification searching. We consider six different specifications commonly used in SC applications: (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome, (6) the mean of all pre-treatment outcome values, and (7)

the researcher. For example, Klöbner et al. (2016) show that different SC estimators are obtained depending on the software used or on how the dataset is sorted.

[8] This happens because all such specification will be asymptotically equivalent when the number of pre-treatment periods goes to infinity.

three outcome values (the first, the middle and the last ones).[9,10] We focus on the placebo test suggested in Abadie et al. (2010) to assess the "statistical significance" of the estimates because this is how researchers usually assess whether their results are significant in SC applications. While, as noticed in Abadie et al. (2010), this is not generally a formal statistical test when treatment is not randomly assigned, it is still informative about whether or not the estimated effect of the actual intervention is large relative to the distribution of the effects estimated for units not exposed to the intervention. Importantly, note that the conditions such that this test is actually valid are satisfied in our simulations. Moreover, we also consider as a robustness check an *infeasible* test based on actual distribution of the test statistic in our MC simulations to assess the statistical significance of the results, and we find similar results. Therefore, our results are not driven by potential size distortions of the placebo test. For brevity, we refer to this probability of having an extreme test statistic in the placebo test as the probability of rejecting the null throughout the paper.

We find that the probability of detecting a false positive in at least one specification for a 5% significance test can be as high as 14% when there are 12 pre-treatment periods (25% if we consider a 10% significance test). The possibilities for specification searching remain high even when the number of pre-treatment periods is large. For example, with 400 pre-treatment periods, we still find a probability of around 13% that at least one specification is significant at 5% (24% if we consider a 10% significance test). These results suggest that, even with a large number of pre-treatment periods, different specifications can still lead to significantly different synthetic control units, generating substantial opportunities for specification searching. This is true both in data generating processes with stationary and non-stationary common factors. We also find similar results in placebo simulations using the CPS. Importantly, we still find that the probability of rejecting the null in at least one specification can be significantly higher than the nominal test size even when we restrict the set of choices to specifications with a good pre-treatment fit.[11]

[9] In order to simplify the presentation of our results, we do not consider in our simulations the use of time-invariant covariates, as is commonly used in specifications that rely on the pre-treatment outcome mean. In Appendix B we show that our results remain valid if we consider specifications that use time-invariant covariates as economic predictors in addition to functions of the pre-treatment outcomes. Note also that these seven specifications do not exhaust all specification options that have been considered in SC applications.

[10] Note that specifications (1)-(5) satisfy the condition in our theoretical results that the number of pre-intervention periods used as predictor variables increase with T_0.

[11] There are at least two possible explanations for still finding over-rejection even when we condition on specifications with a good pre-treatment fit. First, in many SC applications, including those in Abadie & Gardeazabal (2003), Abadie et al. (2010), and Abadie et al. (2015), the outcome variable is non-stationary. In this case, most SC specifications will provide a good pre-treatment fit, as it will provide a good approximation to the non-stationary

Given our theoretical results, it is expected that the significant specification-searching possibilities with a large T_0 are driven by specifications that do not increase the number of pre-treatment lags used as predictors when the number of pre-treatment periods goes to infinity. Indeed, we find that excluding the specifications whose number of pre-intervention periods used as predictor variables do not increase with T_0 from the set of options strongly attenuates the specification-searching problem when T_0 is large, although we still find room for specification searching when T_0 is not so large. Note that the data-generating process (DGP) in our MC simulations also provides a way to measure the extent to which different specifications assign positive weight to control units that should not be considered in the synthetic control unit. Consistent with the intuition that specifications that use more pre-treatment outcome lags as predictors would better control for unobserved confounders, we find that the specifications that limit the number of pre-treatment outcome lags misallocate substantially more weights, suggesting that such specifications should not be considered in SC applications.

It is important to note that our results by no means imply that researchers that have implemented the SC method did engage in specification searching. Given that this is a relatively new method, there would not be enough papers to formally test for specification searching.[12] However, given the mounting evidence that there is a high return for reporting "significant" results and that scientists tend to engage in p-hacking, our findings raise important concerns about the synthetic control method.[13] Also, while we find room for specification searching in the SC method, it does not imply that this problem is more relevant for the SC method when compared to alternatives

trend, as shown in Ferman & Pinto (2016). Our results suggest that, in this scenario, different SC specifications can still yield substantially different estimators even if most specifications provide a good approximation to the non-stationary trend. Second, as shown in Ferman & Pinto (2017), the SC permutation test can lead to over-rejection if we consider the SC estimator conditional on a good pre-treatment fit. This explains why we may still have significant over-rejection even when the researcher has only a few (or even just one) specifications with a good pre-intervention fit to choose from.

[12] Brodeur et al. (2016) analyzes 641 articles (providing more than 50,000 tests) published in the *American Economic Review*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics*. They identify a residual in the distribution of tests that cannot be explained solely by journals favoring rejection of the null hypothesis. Simonsohn et al. (2014) suggest the use of the p-curve as a way to distinguish between selective reporting findings and true effects. One of the requirements to the inference from p-curve to be valid is that we have a great pool of studies from which we can select studies and p-values that test similar hypothesis. Given that the synthetic control estimator is a relatively recent method, there would not be enough published papers that used this method even if we consider a wide range of journals. Therefore, it would be unfeasible to replicate these methodologies for synthetic control applications.

[13] See Rosenthal (1979), Lovell (1983), De Long & Lang (1992), Simmons et al. (2011), Simonsohn et al. (2014), and Brodeur et al. (2016).

methods.[14] The main conclusion of our paper is that, despite providing a data-driven method to construct the counterfactual unit, the SC method does not completely solve the specification-searching problem due to a lack of consensus on how the SC weights should be estimated.

If there were a consensus on how the SC specification should be selected, then the risk of p-hacking (at least in this dimension) would be limited. Our results suggest that restricting the set of options for researchers can strongly attenuate this problem, particularly if we restrict to specifications that use many pre-treatment outcome lags as predictors. Another possible solution would be to require researchers applying the SC method to report results for different specifications. However, it is important to note that testing all the possible SC specifications separately would not provide a valid hypothesis test since there would not be a defined decision rule (see White (2000)). One alternative is to consider a test statistic for the permutation test that combines the test statistics for all individual specifications, as suggested in Imbens & Rubin (2015).

Finally, we also consider the possibilities for specification searching and the implementability of the above recommendations in two empirical applications, based on Smith (2015) and Abadie et al. (2010). In our empirical examples, we analyze three cases: one whose conclusion is robust to specification searching, one where different specifications can reach either significant and non-significant results (clearly showing the potential for specification searching in the synthetic control framework), and one where all results are significant, but at different significance levels. Moreover, after applying our recommendations, we show that one can reach a clear conclusion about the significance of the results in all three examples.

The remainder of this paper proceeds as follows. In Section 2, we provide a brief overview of the SC estimation, and then we derive conditions under which the SC estimators using different specifications will be asymptotically equivalent when the number of pre-treatment periods goes to infinity. Then, we provide Monte Carlo simulations in Section 3 and simulations with real data in Section 4. We present our main recommendations in Section 5, and we discuss three empirical examples in Section 6. We conclude in Section 7.

[14] For example, Gardeazabal & Vega-Bayo (2016) compare the synthetic control method with a panel data approach developed in Hsiao et al. (2012), and conclude that the SC estimator is more robust to changes in the donor pool.

2 Synthetic Control Method and Specification Searching

Abadie & Gardeazabal (2003), Abadie et al. (2010) and Abadie et al. (2015) have developed the Synthetic Control Method in order to address counterfactual questions involving only one treated unit and a few control units. Intuitively, this method estimates the potential outcome of the treated unit if there were no treatment by constructing a weighted average of control units that is as similar as possible to the treated unit regarding the pre-treatment outcome variables and covariates. For this reason, this weighted average of control units is known as the synthetic control unit and treatment effects can be flexibly estimated for each post-treatment period. Below, we follow Abadie et al. (2010), explaining their estimator.

Suppose that we observe data for $(J+1) \in \mathbb{N}$ units during $T \in \mathbb{N}$ time periods. Additionally, assume that there is a treatment that affects only unit 1 from period $T_0 + 1$ to period T uninterruptedly, where $T_0 \in (1, T) \cap \mathbb{N}$. Let the scalar $Y^0_{j,t}$ be the potential outcome that would be observed for unit j in period t if there were no treatment for $j \in \{1, ..., J+1\}$ and $t \in \{1, ..., T\}$. Let the scalar $Y^1_{j,t}$ be the potential outcome that would be observed for unit j in period t if unit j received the treatment from period $T_0 + 1$ to T. Define:

$$\alpha_{j,t} := Y^1_{j,t} - Y^0_{j,t} \tag{1}$$

as the treatment effect for unit j in period t and $D_{j,t}$ as a dummy variable that assumes value 1 if unit j is treated in period t and value 0 otherwise. With this notation, we have that the observed outcome for unit j in period t is given by

$$Y_{j,t} := Y^0_{j,t}\left(1 - D_{j,t}\right) + Y^1_{j,t} D_{j,t}.$$

Since only the first unit receives the treatment from period $T_0 + 1$ to T, we have that:

$$D_{j,t} := \begin{cases} 1 & \text{if } j = 1 \text{ and } t > T_0 \\ 0 & \text{otherwise.} \end{cases}$$

We aim to identify $(\alpha_{1,T_0+1}, ..., \alpha_{1,T})$. Since $Y^1_{1,t}$ is observable for $t > T_0$, equation (1) guarantees that we only need to estimate the counterfactual $Y^0_{1,t}$ to accomplish this goal.

Let $\mathbf{Y_j} := [Y_{j,1}...Y_{j,T_0}]'$ be the vector of observed outcomes for unit $j \in \{1,...,J+1\}$ in the pre-treatment period and $\mathbf{X_j}$ a $(F \times 1)$-vector of predictors of $\mathbf{Y_j}$. Those predictors can be not only covariates that explain the outcome variable, but also linear combinations of the variables in $\mathbf{Y_j}$.[15] Let also $\mathbf{Y_0} = [\mathbf{Y_2}...\mathbf{Y_{J+1}}]$ be a $(T_0 \times J)$-matrix and $\mathbf{X_0} = [\mathbf{X_2}...\mathbf{X_{J+1}}]$ be a $(F \times J)$-matrix.

Given the choice of predictors in matrix $\mathbf{X_j}$, the idea of the SC method is to construct the counterfactual for the treated unit using a weighted average of the control units:

$$\widehat{Y}^0_{1,t} := \sum_{j=2}^{J+1} \widehat{w}_j Y_{j,t} \tag{2}$$

The weights $\widehat{\mathbf{W}} = [\widehat{w}_2...\widehat{w}_{j+1}]' := \widehat{\mathbf{W}}(\widehat{\mathbf{V}}) \in \mathbb{R}^J$ are given by the solution to a nested minimization problem:

$$\widehat{\mathbf{W}}(\mathbf{V}) := \arg\min_{\mathbf{W} \in \mathcal{W}} (\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W}) \tag{3}$$

where $\mathcal{W} := \left\{ \mathbf{W} = [w_2...w_{J+1}]' \in \mathbb{R}^J : w_j \geq 0 \text{ for each } j \in \{2,...,J+1\} \text{ and } \sum_{j=2}^{J+1} w_j = 1 \right\}$ and $\mathbf{V}$ is a diagonal positive semidefinite matrix of dimension $(F \times F)$ whose trace equals one. Moreover,

$$\widehat{\mathbf{V}} := \arg\min_{\mathbf{V} \in \mathcal{V}} (\mathbf{Y_1} - \mathbf{Y_0}\widehat{\mathbf{W}}(\mathbf{V}))'(\mathbf{Y_1} - \mathbf{Y_0}\widehat{\mathbf{W}}(\mathbf{V})) \tag{4}$$

where $\mathcal{V}$ is the set of diagonal positive semidefinite matrix of dimension $(F \times F)$ whose trace equals one.

Finally, we define the Synthetic Control Estimator of $\alpha_{1,t}$ (or the estimated gap) as

$$\widehat{\alpha}_{1,t} := Y_{1,t} - \widehat{Y}^N_{1,t} \tag{5}$$

for each $t \in \{1,...,T\}$.

Intuitively, $\widehat{\mathbf{W}}$ is a weighting vector that measures the relative importance of each unit in the synthetic control of unit 1 and $\widehat{\mathbf{V}}$ measures the relative importance of each one of the F predictors. Abadie et al. (2010) discuss alternative ways to choose the matrix $\widehat{\mathbf{V}}$. We focus our attention on

[15] For example, if the outcome variable is a country's per capita GDP and $T_0 = 12$, $\mathbf{X_j}$ may contain the investment rate, some measures of human capital and institutional quality, population, and the average per capita GDP from 1 to 4, from 5 to 8 and from 9 to 12.

the most common method of choosing $\widehat{\mathbf{V}}$, which involves solving the nested minimization problem given by equations (3) and (4).

Even though a crucial part in the implementation of the SC method is the choice of economic predictors, there is little guidance about which variables should be included in matrix $\mathbf{X_j}$. This lack of guidance can create an opportunity for the researcher to look for specifications that yield "better" results by including or excluding some pre-treatment outcome values from its specification. This risk is even greater when we consider that there is no consensus about which functions of the outcome values should be included in $\mathbf{X_j}$: Abadie & Gardeazabal (2003), Abadie et al. (2015) and Kleven et al. (2013) use the mean of all pre-treatment outcome values and additional covariates; Smith (2015) uses Y_{j,T_0}, Y_{j,T_0-2}, Y_{j,T_0-4} and Y_{j,T_0-6}; Abadie et al. (2010) picks Y_{j,T_0}, Y_{j,T_0-8} and Y_{j,T_0-13}; Billmeier & Nannicini (2013), Bohn et al. (2014), Gobillon & Magnac (2016), Hinrichs (2012) use all pre-treatment outcome values; and Montalvo (2011) uses only the last two pre-treatment outcome values.[16]

A key question, therefore, is whether different specifications may lead to substantially different SC estimators. We consider the asymptotic behavior of different SC specifications when $T_0 \to \infty$. We define a specification s by the set of predictors $\mathbf{X}_j(s, T_0)$ that are used when there are T_0 pre-treatment periods. Let $\mathcal{I}(s, T_0)$ be the set of pre-treatment periods t such that $Y_{j,t}$ is included as a predictor when there are T_0 pre-treatment periods, and let $L(s, T_0) = \#\mathcal{I}(s, T_0)$.[17] Let $\mathbf{y}^0_{-j,t}$ be the $J \times 1$ vector of potential outcomes for all units except unit j at time t. We consider a sufficient assumption to guarantee that a broad set of SC specifications will be asymptotically equivalent when $T_0 \to \infty$.

Assumption 1 For any sequence of integers $\{t_k\}_{k \in \mathbb{N}}$ with $t_k > t_{k-1}$, and for any $j \in \{1, ..., J+1\}$, we have that:

$$\sup_{\mathbf{W} \in \mathcal{W}} \left| \frac{1}{K} \sum_{k=1}^{K} \left(Y^0_{j,t_k} - \mathbf{y}^0_{-j,t_k}{}' \mathbf{W} \right)^2 - Q_j(\mathbf{W}) \right| \xrightarrow{p} 0 \text{ when } K \to \infty \tag{6}$$

where $Q_j(\mathbf{W})$ is a continuous and strictly convex function.

[16] By no means we imply that those authors have engaged in specification searching. We have only listed them as prominent examples of different choices regarding predictor variables.

[17] For example, let a specification s be such that R covariates and the first half of the pre-treatment outcome lags are used as predictors. Then $\mathcal{I}(s, T_0) = \{1, 2, ..., \frac{T_0}{2}\}$ and $L(s, T_0) = \frac{T_0}{2}$. Note that, in this case, the dimension of $\mathbf{X}_j$ would be $R + \frac{T_0}{2}$.

Note that assumption 1 implies that pre-treatment averages of the second moments of every subsequence of $(Y^0_{1,t}, ..., Y^0_{J+1,t})$ converge to the same value. We show in Appendix A that this assumption is satisfied if, for example, we assume that $\{\mathbf{y}^0_t \mathbf{y}^{0\prime}_t\}_{t=1}^{T_0}$ is weakly stationarity, each element of $\{\mathbf{y}^0_t \mathbf{y}^{0\prime}_t\}$ has absolutely summable covariances, and $\mathbb{E}\left[\mathbf{y}^0_t \mathbf{y}^{0\prime}_t\right]$ is non-singular, where $\mathbf{y}^0_t = (Y^0_{1,t}, ..., Y^0_{J+1,t})'$.

Given these assumptions, we have the following results:[18]

Proposition 1 Let $\widehat{\mathbf{W}}(s, T_0)$ be the SC weights using specification s when there are T_0 pre-intervention periods. If $L(s, T_0) \to \infty$ when $T_0 \to \infty$, then, under assumption 1, $\widehat{\mathbf{W}}(s, T_0) \overset{p}{\to} \overline{\mathbf{W}} = argmin_{\mathbf{W} \in \mathcal{W}} Q_1(\mathbf{W})$.

Corollary 2 Let $\hat{\alpha}_{1t}(s, T_0)$ and $\hat{\alpha}_{1t}(s', T_0)$ be two SC estimators for the treatment effect at time $t > T_0$ using specifications s and s' such that $L(s, T_0) \to \infty$ and $L(s', T_0) \to \infty$ when $T_0 \to \infty$. Then, under assumption 1, $|\hat{\alpha}_{1t}(s, T_0) - \hat{\alpha}_{1t}(s', T_0)| = o_p(1)$.

Therefore, while different SC specifications may generate different SC estimates, our result from Proposition 1 and Corollary 2 show that, under some conditions, different specifications will lead to asymptotically equivalent SC estimators, as long as the number of pre-treatment lags used as predictors goes to infinity with T_0. Note, however, that our results do not guarantee that different SC specifications would lead to similar SC estimates when T_0 is finite. Moreover, there are common specifications used in SC applications that do not satisfy the condition on the number of pre-treatment lags used as economic predictors going to infinity with T_0. For example, many authors consider the use of the mean of all pre-treatment outcome values in addition to other covariates as economic predictors, while other authors consider the use of only a few pre-treatment outcome lags as economic predictors. These alternative specifications would generally lead to SC weights that will not converge to $\overline{\mathbf{W}}$, so there may still be significant variation in the SC estimates even when T_0 is large.

Note that our results are valid irrespectively of whether the SC estimator is unbiased, as we are only comparing the asymptotic behavior of the SC estimator under different specifications. For a thorough analysis on the asymptotic bias of the SC estimator when $T_0 \to \infty$, see Ferman & Pinto

[18] All proofs are presented in Appendix A.

(2016). In our simulations in Sections 3 and 4, the condition in which the SC estimator is unbiased are satisfied. Also, note that our results are related to the results from Kaul et al. (2015), who show that covariates would become irrelevant in the minimization problem 3 if all pre-treatment lags are included as predictors. Since our result from Proposition 1 holds whether or not we include other covariates as predictors, this implies that covariates would also become asymptotically irrelevant in the minimization problem 3 whenever we consider specifications such that $L(s, T_0) \to \infty$ when $T_0 \to \infty$, even if we do not include all pre-treatment outcome lags. Note, however, that this does not necessarily imply that the SC weights will not attempt to match the covariates of the treated unit nor that the SC estimator will be asymptotically biased, as explained in Botosaru & Ferman (2017).

Conditional on a given SC specification, Abadie et al. (2015) propose an inference procedure that consists in a straightforward placebo test. They permute which unit is assumed to be treated and estimate, for each $j \in \{2, ..., J+1\}$ and $t \in \{1, ..., T\}$, $\widehat{\alpha}_{j,t}$ as described above. Then, they compute the test statistic

$$RMSPE_j := \frac{\sum_{t=T_0+1}^{T} \left(Y_{j,t} - \widehat{Y_{j,t}^N}\right)^2 / (T - T_0)}{\sum_{t=1}^{T_0} \left(Y_{j,t} - \widehat{Y_{j,t}^N}\right)^2 / T_0}$$

where the acronym RMSPE stands for *ratio of the mean squared prediction errors*. Moreover, they propose to calculate a p-value

$$p := \frac{\sum_{j=1}^{J+1} \mathbb{1}\left[RMSPE_j \geq RMSPE_1\right]}{J+1}, \tag{7}$$

where $\mathbb{1}[\diamond]$ is the indicator function of event $\diamond$, and reject the null hypothesis of no effect if p is less than some pre-specified significance level, such as the traditional value of 0.05. Abadie et al. (2010) recognize that the randomization inference assumptions are very restrictive for the SC set-up, as treatment is not, in general, randomly assigned.[19] In the absence of random assignment, they interpret the p-value as the probability of obtaining an estimate value for the test statistics at least as large as the value obtained using the treated case as if the intervention was randomly assigned

[19] Firpo & Possebom (2017) discuss a sensitivity mechanism analysis for this test, while Ferman & Pinto (2017) analyze the statistical properties of this placebo test when treatment is not randomly assigned. Hahn & Shi (2016) also consider the properties of placebo test in the SC setting. For our purposes in this paper, we consider Abadie et al. (2010) interpretation of the placebo test p-value.

among the data. Although the p-value from this placebo test lacks a clear statistical interpretation, this test is commonly used in SC application. Therefore, our simulation exercises can be seen as the probability that a researcher applying the SC method would find a test statistic that is in the top 5% of the distribution of test statistics in the placebo runs, which is how researchers applying the SC method usually assess whether their estimates are significant. Moreover, note that, in our simulations, the placebo test considering a single SC specification would have a rejection rate under the null of 5% by construction.

As a corollary from Proposition 1, we show that the ranking of $RMSPE_j$ will remain asymptotically invariant to changes in the SC specification when $T_0 \to \infty$ as long as we consider only specifications such that the number of pre-treatment outcome lags goes to infinity with T_0.

Corollary 3 Under assumption 1 and assuming that Y_{jt} is continuous, the ordering of $\{RMSPE_1, ..., RMSPE_{J+1}\}$ is asymptotically invariant to SC specifications such that $L(s, T_0) \to \infty$ when $T_0 \to \infty$ and $T - T_0$ is finite.

The result from corollary 3 shows that, if we restrain to SC specifications such that number of pre-treatment outcome lags goes to infinity with T_0, then the possibilities for specification searching would be limited, as a test based on different SC specifications would lead to the same conclusion with probability approaching to one when $T_0 \to \infty$. It is important to emphasize, however, that we may still have room for specification searching if T_0 is finite. Moreover, this result is not valid if we consider alternative SC specifications such that the number of pre-treatment outcome lags used as economic predictors remain fixed when $T_0 \to \infty$.

3 Monte Carlo Simulations

In the previous section, we provide theoretical results showing that possibilities for specifications searching should be limited if a researcher restraints herself to specifications that uses an infinitely large number of pre-intervention outcome values as $T_0 \to \infty$. Therefore, proposition 1 and corollaries 2 and 3 provide guidance on the conditions in which specification searching could be a relevant problem in SC applications: (i) when T_0 is not large and/or (ii) when one considers specifications with few pre-treatment outcomes as predictors. In this section, we design a MC simulation based on such guidance.

In the Monte Carlo exercise, we generate 10,000 data sets and, for each one of them, test the null hypothesis of no effect whatsoever adopting several different specifications. Conditional on a given specification, in our simulations this placebo test should provide a rejection rate of $\alpha\%$ under the null for a $\alpha\%$ significance test by construction. We are interested, however, in the probability of rejecting the null hypothesis at the 5%-significance level for at least one specification. If different specifications result in wildly different SC estimators, then the probability of finding one specification that rejects the null at $\alpha\%$ can be significantly higher than $\alpha\%$. In the extreme case in which we have K different specifications and these specifications lead to independent estimators, this probability would be given by $1-(1-\alpha)^K$, where K is the number of different specifications.[20] In this case, such lack of guidance about which specifications should be used could generate substantial opportunities for specification searching. In contrast, if different SC specifications lead to similar SC weights, then this rejection rate will be close to $\alpha\%$ and the risk of specification searching would be very low. We consider two data generating processes. In Section 4, we consider placebo simulations with the CPS.

In the first data generating process (DGP), we consider a linear factor model in which all units are divided into groups that follow different stationary time trends.

$$Y_{j,t}^0 = \delta_t + \lambda_t^k + \epsilon_{j,t} \tag{8}$$

for some $k = 1, ..., K$. We consider the case in which $J+1 = 20$ and $K = 10$. Therefore, units 1 and 2 follow the trend λ_t^1, units 3 and 4 follow the trend λ_t^2, and so on. We consider that λ_t^k is normally distributed following an AR(1) process with 0.5 serial correlation parameter, $\delta_t \sim N(0,1)$ and $\epsilon_{j,t} \sim N(0, 0.1)$.

In our second DGP, we modify the linear factor model such that a subset of the common factors are non-stationary. In this case, we consider DGP which includes a non-stationary trend ϕ_t^r that follows a random walk:

$$Y_{j,t}^0 = \delta_t + \lambda_t^k + \phi_t^r + \epsilon_{jt} \tag{9}$$

[20] Lovell (1983) provides a similar formula, but considering the decision on which variables to include in a regression model.

for some $k = 1, ..., K$ and $r = 1, ..., R$. We consider in our simulations $K = 10$ and $R = 2$. Therefore, units $j = 2, ..., 10$ follow the same non-stationary path ϕ_t^1 as the treated unit, although only unit $j = 2$ also follows the same stationary path λ_t^1 as the treated unit.

In both models, we impose that there is no treatment effect, i.e., $Y_{j,t} = Y_{j,t}^0 = Y_{j,t}^1$ for each time period $t \in \{1, ..., T_0\}$. We fix the number of post-treatment periods $T - T_0 = 10$ and we vary the number of pre-intervention periods in the DGPs, $T_0 \in \{12, 32, 100, 400\}$. In the Appendix, we consider variations in our stationary model (8) by setting (i) $\epsilon_{j,t} \sim N(0,1)$, (ii) $K = 2$, or (iii) including time-invariant covariates. We find similar results as the ones presented in the main text.

We calculate the SC estimator using the following seven specifications that differ only in the linear combinations of pre-treatment outcome values used as predictors:[21]

1. All pre-treatment outcome values: $\mathbf{X}_j = [Y_{j,1} \cdots Y_{j,T_0}]'$
2. The first three fourths of the pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,1} \cdots Y_{j,3T_0/4}\right]'$
3. The first half of the pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,1} \cdots Y_{j,T_0/2}\right]'$
4. Odd pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,1} \;\; Y_{j,3} \cdots Y_{j,(T_0-3)} \;\; Y_{j,(T_0-1)}\right]'$
5. Even pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,2} \;\; Y_{j,4} \cdots Y_{j,(T_0-2)} \;\; Y_{j,T_0}\right]'$
6. Pre-treatment outcome mean: $\mathbf{X}_j = \left[\sum_{t=1}^{T_0} Y_{j,t} / T_0\right]$
7. Three outcome values (the first one, the middle one, and the last one): $\mathbf{X}_j = \left[Y_{j,1} \;\; Y_{j,T_0/2} \;\; Y_{j,T_0}\right]'$

Observe that specifications 1-5 satisfy the conditions of Proposition 1 and Corollaries 2 and 3, while specifications 6 and 7 do not. We stress that, in order to simplify the presentation of our results, we do not consider in our MC simulations the use of time-invariant covariates, as is commonly used in specifications that rely on the pre-treatment outcome mean. In Appendix B we show that our results remain valid if we consider specifications that use time-invariant covariates as economic predictors in addition to functions of the pre-treatment outcomes.

[21] In order to compute the SC estimator, we use the *Synth* package in *R*. (See Abadie et al. (2011) for details.) This package solves the nested minimization problem described by equations (3) and (4). We specify the optimization method to be *BFGS* only and use optimization routine *Low Rank Quadratic Programming* when *Interior Point* optimization routine does not converge.

For each specification, we run a placebo test using the RMSPE test statistic proposed in Abadie et al. (2010) and reject the null at 5%-significance level if the treated unit has the largest RMSPE among the 20 units. By construction, this leads to a 5% rejection rate when we look at each specification separately. We are interested, however, in the probability that we would reject the null at the 5%-significance level in at least one specification. This is the probability that a researcher would be able to report a significant result even when there is no effect if she were to engage in specification searching. If all different specifications result in the same synthetic control unit, then we would find that the probability of rejecting the null in at least one specification would be equal to 5% as well. However, this probability may be higher if the synthetic control weights depend on specification choices, which may be the case in finite samples or for specifications 6 and 7.

We present in columns 1 and 2 of Table 1 the probability of rejecting the null at 5% and at 10% significance levels in at least one of our seven specifications for the stationary model. Columns 3 and 4 present the same results for the non-stationary model.[22] With $T_0 = 12$, a researcher considering these seven different specifications would be able to report a specification with statistically significant results at the 5% level with probability 14.3% for the stationary model and 14.2% for the non-stationary. If we consider 10% significance tests, then the probability of rejecting the null in at least one specification would be up to 25.0% and 25.4%, respectively for the stationary and the non-stationary models. Therefore, with few pre-treatment periods, a researcher would have substantial opportunities to select statistically significant specifications even when the null hypothesis is true. Importantly, note that it is not unusual to have SC applications with as few as 12 pre-intervention periods.[23]

If the variation in the SC weights across different specifications vanishes when the number of pre-treatment periods goes to infinity even for the specifications that do not satisfy the assumption of Proposition 1 and Corollaries 2 and 3, then we would expect the rejection rate to get closer to 5% once the number of pre-treatment periods gets large. In this case, all different specifications would provide roughly the same SC unit and, therefore, the same treatment effect estimate. The results in Table 1 show that the probabilities of rejecting the null are still significantly higher than

[22] See table A.1 for results using different data generating processes.

[23] See, for example, Abadie & Gardeazabal (2003), Kleven et al. (2013), Kreif et al. (2015), Smith (2015), Ando (2015), Liu (2015), Sills et al. (2015), Billmeier & Nannicini (2013), Bohn et al. (2014), Cavallo et al. (2013), Hinrichs (2012), Montalvo (2011), Li (2012) and Hosny (2012).

the test size even when the number of pre-intervention periods is large. In a scenario with 400 pre-intervention periods, in the non-stationary model it would be possible to reject the null in at least one specification 14.5% (25.5%) of the time for a 5% (10%) significance test.[24] These results suggest that, when we include specifications that violate the conditions of Proposition 1 and Corollaries 2 and 3, specification searching remains a problem for the SC method even when the number of pre-intervention periods is remarkably large for empirical applications.

In the previous exercise, we assumed that the researcher would be able to choose any of the 7 specifications we considered in our MC simulations. However, Abadie et al. (2010) and Abadie et al. (2015) emphasize that the SC control estimator should only be used in the situations with good pre-treatment fit, i.e., in situations in which the weighted average of the controls' pre-treatment outcomes is a good approximation for the treated pre-treatment outcome. It is important, therefore, to check whether the specification-searching problem we identified in the SC method arises because we allow the researcher to choose specifications that provide a poor pre-treatment fit. We consider a pre-treatment normalized mean squared error index to determine whether a specification provides a good pre-treatment fit:[25]

$$\tilde{R}^2 = 1 - \frac{\sum_{t=1}^{T_0} \left(Y_{1,t} - \widehat{Y}_{1,t}^N\right)^2}{\sum_{t=1}^{T_0} \left(Y_{1,t} - \overline{Y}_1\right)^2} \tag{10}$$

where $\overline{Y}_1 = \frac{\sum_{t=1}^{T_0} Y_{1,t}}{T_0}$. If this measure is one, then we have a perfect fit.[26]

In order to capture a good fit, we consider two thresholds for $\tilde{R}^2$, $\tilde{R}^2 > 0.80$ and $\tilde{R}^2 > 0.95$. Considering these two thresholds, panel A of Table 2 shows the probability of finding a good pre-treatment fit for at least one of the seven specifications. The probability of finding specifications

[24]Note that the probability of specification searching is not monotonic in T_0. This happens because, with a very small T_0, the chance that a pre-treatment MSPE is close to zero is very high. Since there is a high correlation of pre-treatment MSPE across specifications, it is likely that one unit will have a pre-treatment MSPE close to zero for many specifications. This implies that this unit will have a large test statistic for all these specifications, so the placebo test will reject the null for these specifications most of the time. As T_0 increases, the probability of having a pre-treatment MSPE close to zero will be small.

[25]This measure is very similar to the "pre-treatment fit index" proposed by Adhikari & Alm (2016). These authors propose a measure that is the ratio between the squared root of the mean squared predicted error (the numerator of $1 - \tilde{R}^2$) and $\sqrt{\frac{\sum_{t=1}^{T_0} Y_{1t}^2}{T_0}}$. The advantage of our measure relative to the one proposed by Adhikari & Alm (2016) is that our measure is invariant to linearly additive changes. Dube & Zipperer (2015) also propose a pre-treatment fit criterion that is equal to the numerator of our measure, the root of the mean squared error predictor between the synthetic and the actual outcomes in the pre-treatment period. However, differently from our suggestion, their measure is not scale invariant.

[26]Note that, differently from the standard R^2 measure, $\tilde{R}^2$ can be negative.

with a good pre-treatment fit depends crucially on how we define whether a specification provided a good fit and on whether we consider a stationary or a non-stationary model. We present in columns 1 and 2 the results for the stationary model. With a moderate T_0, the probability of finding at least one specification with good fit is close to one when we consider the weaker definition of good fit, and close to zero when we consider the more stringent definition. We highlight that, according to panels B and C, the specifications that do not satisfy the conditions of Proposition 1 and Corollaries 2 and 3 have a relatively small chance of providing a good pre-intervention even under the weaker definition of good fit, illustrating that this kind of specification behaves poorly in small samples.

We present, in columns 3 and 4, the results for the non-stationary model. In this case, the probability of having at least one specification with a good fit is close to one even when we consider the more stringent definition of good fit. Also, there is a high probability that all specifications (including specifications 6 and 7) provide a good fit, especially when T_0 is large. This happens because, with large T_0, the non-stationary factors dominate the variance of $Y_{1,t}$. Since the SC estimator is extremely efficient in controlling for the non-stationary factors (see Ferman & Pinto (2016)), it will usually provide a good pre-treatment fit.

Given these definitions of good fit, we present in Table 3 the probabilities of rejecting the null in at least one specification when we restrict the researcher to consider only specifications that provide a good pre-treatment fit. Note that the possibilities for specification searching in the non-stationary model (columns 3 and 4) are virtually the same as when we do not restrict for specifications with a good pre-treatment fit, especially when T_0 is large (columns 3 and 4 of Table 1). This is not surprising, given that all specifications will usually provide a good pre-treatment fit in this model. For the stationary model (columns 1 and 2 of Table 3), the specification-search problem is attenuated when we restrict to specifications with a good fit if we use the more lenient definition of good fit (panel A). In practice, in this case the restriction of considering only specifications with a good fit prevents the researcher from choosing specifications 6 and 7, whose weights, as we show below, are very different from the ones chosen by the other specifications, that satisfy the conditions of the conditions of Proposition 1 and Corollaries 2 and 3. If we consider the more stringent definition of good fit, however, then the probability of rejecting the null in at least one specifications is substantially higher (panel B). This happens because, if we consider that the SC method should only be used when the pre-treatment fit is good (as suggested in Abadie et al.

(2010) and Abadie et al. (2015)), then there is a low probability of finding a good fit for at least one specification and we would only consider specifications such that the denominator of the test statistic for the treated unit is close to zero. Since the test statistic for the placebo units are not conditional on a good pre-treatment, this leads to over-rejection, as shown in Ferman & Pinto (2017).

Overall, these results suggest that restricting the researcher to consider only specifications with a good fit does not necessarily attenuate the specification-searching problem. On the one hand, if conditioning on a good fit does not actually restrict the set of options a researcher has (as happens with our non-stationary model), then we have the same results as in the unconditional case. On the other hand, if conditioning severely restricts the set of options, then we have over-rejection because the test statistic for the treated unit is conditional on a denominator that is close to zero, while the test statistics for the placebo units are unconditional.

The results so far indicate that different specifications can provide substantially different SC estimators in finite samples. However, based on our theoretical results from section 2, specifications 1-5 should provide similar SC weights, while specifications 6-7, that lie outside the scope of Proposition 1, could potentially provide SC weights that differ wildly. To analyze this possibility, we calculate a measure of variability of weights in comparison to specification 1. For each specification $x \in \{2, ..., 7\}$, we compute the difference between the weight allocated by specification 1 and specification x for each unit in the donor pool. Then, we take the maximum value of this difference across units in the donor pool. We present this measure for specifications 2-7 on table 4. On the one hand, analyzing specifications 2-5, we find that the variability of weights between specifications is small and, most importantly, decreasing when the pre-intervention period gets large, as expected given our theoretical results. On the other hand, for specifications 6 and 7, we find strikingly different results: their weights differ substantially from the weights of specification 1 and this difference does not decrease when the pre-intervention period gets large.

Beyond the variability of weights between specifications, an interesting feature of our MC simulations is that the SC estimator should assigned positive weights only for unit 2 (which has the same factor loadings of unit 1), so we can actually calculate the proportion of weights that is misallocated for each specification. We present in columns 1 to 7 of Table 5 the proportion of misallocated weights for each specification using both of our DGPs. Interestingly, specifications 6 and 7, that do not satisfy the conditions of proposition 1, misallocate substantially more weights relative to the other specifications. In particularly, specification 6, which uses the pre-treatment mean as economic predictor, does a particularly poor job. For the stationary model (panel A), with $T_0 = 12$, specifications 6 and 7 misallocates more than 80% and 45% of the weights, while the misallocations for other specifications ranges from 23% to 32%. Most importantly, the misallocation of weights decreases with T_0 for all specifications, except for specifications 6 and 7. Results are qualitatively the same for the non-stationary model (panel B). These results suggests that specifications outside the scope of proposition 1, such as specification 6 and 7, behave poorly because they do not capture the time-series dynamics of the units, which is the main goal of the SC method.[27,28]

Given that specifications 6 and 7 stand out by misallocating significantly more weights and by picking weights that are very different from the ones chosen by specification 1, we consider, in Table 6, the specification-searching possibilities excluding specifications 6 and 7. As expected based on corollary 3, excluding specifications 6 and 7 significantly attenuates the specification-searching problem, especially when the number of pre-treatment periods is large.[29] However, it does not completely solve the problem even when T_0 is moderate. Importantly, although corollary 3 suggests that specification-searching possibilities within a well defined class of specifications should be very small asymptotically, we still find room for specification-searching even when T_0 is relatively large in comparison to usual dataset sizes in common SC applications.

We also stress that the attenuation in the specification-searching problem after excluding the specifications outside the scope of corollary 3 is not simply because we are considering five specifications instead of seven. If we exclude, for example, specifications 2 and 3 instead of specifications 6 and 7, then there is virtually no change in the specification-search problem relative to the case that we consider all seven specifications (Appendix Table A.2).

[27] Although any specification could potentially take into account the time series dynamics of the outcome variable because the matrix V is chosen to minimize the pre-treatment MSPE in the second step of the optimization process, this process is very limited because the first minimization problem can severely restrict the set of possible weights $\mathbf{W}^*(V)$ that may be chosen in the second step, as suggested in Ferman & Pinto (2016).

[28] In Appendix B, we show that specifications 6 and 7 can fail to properly exploit the time-series dynamics of the data even if we also include time-invariant covariates as economic predictors. In this case, they will still remain different from the specifications that use many pre-treatment outcome lags as economic predictors. Therefore, our result that the possibilities of specification searching may not diminish with the number of pre-treatment periods when we consider specifications outside the scope of proposition 1 remains valid even if we consider the addition of time-invariant variables as economic predictors.

[29] The only exception is when we consider the stationary model conditional on a good fit with $\tilde{R}^2 > 0.95$. This happens because, in this case, there is a low probability that we find at least one specification with a good fit.

Finally, as a robustness check, we take advantage of the fact that the DGP is known in our MC simulations, and we replicate our results using an infeasible test based on the actual distributions of the test statistics to determine whether the SC estimator for a given specification is statistically significant. The results based on this infeasible test, presented in Appendix Table A.4, corroborate the results presented in this section, showing that our results are not driven by distortions of the placebo test used in the SC inference.

4 Simulations with Real Data

The results presented in Section 3 suggest that different specifications of the SC method can generate significant specification-searching opportunities in finite samples. In particular, we also find that using only specifications that satisfy the conditions of proposition 1 and corollaries 2 and 3 alleviate this problem even though it does not solve it completely. We now check whether the results we find in our MC simulations are also relevant when we consider real datasets by conducting simulations of placebo interventions with the Current Population Survey (CPS). We use the CPS Merged Outgoing Rotation Groups for the years 1979 to 2014. Following Bertrand et al. (2004), we extract information on employment status and earnings for women between ages 25 and 50. We also consider in a separate set of simulations information on men in the same age range.

Before we proceed to the placebo simulations, we briefly discuss the raw data for these outcome variables. There are important distinctions in the time series characteristics when we consider information for men versus women and when we consider log wages versus employment. Figures 1a and 1b present the time series of log wages for all US states, respectively for men and women. As expected, the time series of log wages is non-stationary and increasing for both men and women. These graphs suggest that there is a strong non-stationary factor that affects all states in the same way. Figures 1c and 1d present the time series of employment for all US states, respectively for men and women. In this case, the time series for men should be closer to our stationary model from Section 3, while the time series for women has an increasing trend in the 80s and 90s.

We first consider simulations with 12 pre-intervention periods, 4 post-intervention periods, and 20 states. In each simulation, we randomly select one treated and 19 control states out of the 51 states (including Washington, D.C.) and then we randomly select the first period between 1979 and

1999. Then we consider simulations with 32 pre-intervention periods, 4 post-intervention periods, and 20 states. In this case, we randomly select 20 states and use the entire 36 years of data. In each scenario, we run 5,000 simulations using either employment or log wages as the dependent variable and test the null hypothesis using the same seven specifications of Section 3.[30]

We start presenting the probability of finding specifications with a good fit in Table 7. When the outcome variable is log wages, the probability of having at least one specification with a good fit is close to one, especially when we consider $T_0 = 32$ (columns 1 to 4, panel A). Most importantly, when we consider $T_0 = 32$, specifications 6 and 7 have a high probability of fitting the data closely. These results are consistent with our MC simulations considering that the log wages series appear to have important non-stationary common factors. The probability of finding specifications with a good fit is lower when we consider employment instead of log wages as outcome variable, and even lower when we consider men relative to women. This is consistent with the employment time series for men being closer to a stationary process.

We present in Table 8 the probabilities of rejecting the null in at least one specification.[31] In panel A, we present the specification-search probabilities including any of the seven specification that provide a good fit, i.e., $\tilde{R}^2 > 0.80$. The results are very similar to our findings in the MC simulations. With $T_0 = 12$, depending on the sample and outcome variable, there is 13-26% probability of finding a specification with statistically significant results at 5% and a 21-41% probability of finding a specification with statistically significant results at 10%. With $T_0 = 32$ these probabilities are slightly lower, but still significantly higher than the test nominal size for all cases but men employment rates. In panel B, we present the results searching only specifications that satisfy the conditions of corollary 3, i.e., we exclude specifications 6 and 7. As in our MC simulations, restricting to specifications 1-5 reduce the specification-searching problem but do not solve it entirely. In particularly, for $T_0 = 32$, we cannot reject the null hypothesis that the rejection rate is equal to the nominal level for all but one case. We stress that this reduction is not a mechanical consequence of searching five instead of seven specification. If we exclude specifications 2 and 3, we find rejection rates that are very similar to the ones including all seven specifications.[32] In general,

[30]Standard errors are clustered at the level of the treated state when we calculate the probability of having a good fit and when we calculate rejection rates.

[31]Standard errors for these simulation results are clustered at the treated-state level, in order to take into account that the simulations are not independent.

[32]Detailed results are available upon request.

these results suggest that specification-searching possibilities in SC applications can be relevant in real applications of the SC method even when we restrict ourselves to specifications that have satisfy the conditions of proposition 1 and corollaries 2 and 3.

5 Recommendations

The specification-searching problem we identify arises from a lack of consensus about which specifications should be used in SC applications. Our first recommendation is that researchers should only consider specifications that satisfy the conditions of proposition 1 and corollaries 2 and 3 — i.e., specifications that uses an infinitely large number of pre-intervention outcome values when the pre-intervention period gets large — because our results suggest that the specification-searching problem is magnified by specifications with undesirable properties, such as the specification that uses only the mean pre-treatment outcome as economic predictor or the one that uses only the initial, middle and final pre-intervention outcome values. If we discard these specifications, then the specification-searching problem is attenuated, especially if we have a large number of pre-treatment periods, even though it does not solve the problem completely.

We also recommend that researchers applying the SC should report results for different specifications. However, even if a researcher present results for all possible SC specifications with an hypothesis test for each specification, this would not provide a valid hypothesis test. If the decision rule is to reject the null if the test rejects in all specifications, then we could end up with a very conservative test (Romano & Wolf (2005)).[33] If the decision rule is to reject the null if the test rejects in at least one specification, then we would be back in the situation where we over-reject the null. One possible solution is to base the inference procedure on a new test statistic that is a function that combines all the test statistics for the individual specifications, as suggested by Imbens & Rubin (2015).The drawback of this solution is that it does not provide an obvious point-estimator. There are two possible ways to handle this disadvantage. First, if the test function is simply a weighted average of the test statistics for individual specifications, then Christensen & Miguel (2016) and Cohen-Cole et al. (2009) suggest using the same weights to compute a weighted

[33] When we adopt this decision rule in our MC simulations, then probability of rejecting the null at 5% for all specifications is lower than 1% in all scenarios. If we discard specifications 6 and 7, then this rejection rate ranges from 1% when $T_0 = 12$ to 2.8% when $T_0 = 400$.

average of the point-estimator of each specification and using this weighted average as an estimate that incorporates model uncertainty. As another alternative, we can focus on set identification, as suggested by Firpo & Possebom (2017). In this case, we would invert this combination of test statistics to compute confidence sets that contain all treatment effects functions within a pre-specified class that are not rejected by the inference procedure that uses the chosen combination of test statistics.

6 Empirical Applications

We analyze the possibilities for specification searching and the implementability of our recommendations in two empirical examples.

6.1 The resource curse exorcised: Evidence from a panel of countries (Smith (2015))

Smith (2015) evaluates the impact of major natural resource discoveries since 1950 on GDP per capita using different methods, including the synthetic control method.[34] Major oil and gas discoveries happened in Equatorial Guine and Ecuador in 1992 and 1972 respectively, implying that pre and post-treatment periods are 1950-1991 and 1992-2008 for the first country and 1950-1971 and 1972-2008 for the second one. While the donor pool for Equatorial Guine consists of Sub-Saharan African Countries (Benin, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chad, Cote d'Ivoire, Gambia, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mauritius, Mozambique, Namibia, Niger, Rwanda, Senegal, Somalia, Sudan, Swaziland, Tanzania, Togo, Uganda, Zambia, Zimbabwe), the donor pool for Ecuador consists of Latin American and Caribbean countries (Costa Rica, Cuba, Dominican Republic, El Salvador, Guatemala, Honduras, Jamaica, Nicaragua, Panama, Paraguay, Puerto Rico, Uruguay).

We estimate the impact of major oil and gas discoveries on GDP per capita using the synthetic control method with fourteen different specifications. Specifically, we test seven different specifications that differ in which functions of the pre-treatment periods are included and, for each one of

[34] Following the best practices in terms of transparency and replicability, he made his dataset and replication files available online (http://www.brockdsmith.com/research.html).

them, we either include two covariates[35] or not. Our seven basic specifications are:[36]

1. All pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,(T_0-6)} \cdots Y_{j,T_0}\right]'$
2. The first three fourths of the pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,(T_0-6)} \cdots Y_{j,(T_0-2)}\right]'$
3. The first half of the pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,(T_0-6)} \cdots Y_{j,(T_0-4)}\right]'$
4. Odd pre-treatment outcome values: $\mathbf{X}_j = \left[Y_{j,(T_0-5)} \;\; Y_{j,(T_0-3)} \;\; Y_{j,(T_0-1)}\right]'$
5. Even pre-treatment outcome values (Original Specification): $\mathbf{X}_j = \left[Y_{j,(T_0-6)} \;\; Y_{j,(T_0-4)} \;\; Y_{j,(T_0-2)} \;\; Y_{j,T_0}\right]'$
6. Pre-treatment outcome mean: $\mathbf{X}_j = \left[\sum_{t=T_0-6}^{T_0} Y_{j,t}/7\right]$
7. Three outcome values: $\mathbf{X}_j = \left[Y_{j,(T_0-6)} \;\; Y_{j,(T_0-3)} \;\; Y_{j,(T_0)}\right]'$

where $T_0 = 1991$ for Equatorial Guine and $T_0 = 1971$ for Ecuador.

Table 9 shows the p-value and our goodness of fit measure for each specification and each country. On the one hand, the results for Equatorial Guinea are robust to specification searching, since all specifications provide treatment effect estimates that are significant at the 5%-level. On the other hand, the results for Ecuador show that the researcher could try different specifications and pick one whose result is significant. In particular, all fourteen specifications have a good fit ($\tilde{R}^2 > 0.80$), but only two of them are significant (specifications 4b and 6b), implying that the researcher could, potentially, report a false-positive result.[37]

We now test our recommendations in these particular applications. First of all, by presenting results for more than one specification as we do in Table 9, a sensible conclusion would be that major oil and gas discoveries had a significant effect on Equatorial Guinea's GDP per capita even though there is no evidence of such effect on Ecuador's GDP per capital. Figure 2 shows that

[35] The included covariates are ethnic fragmentation and population size one year before the discovery.

[36] Although the number of pre-treatment years is larger than seven, we followed Smith (2015) and considered for this exercise different specifications using only seven years of pre-treatment data in the first minimization problem (equation (3)) while accounting for the entire pre-treatment period in the second minimization problem (equation (4)). Had we considered only seven years of pre-treatment data in the second step, we would reach similar conclusions to the ones in the main text. Had we considered the same specifications using the full pre-treatment data in the first step, then we would fail to reject the null for all specifications. This is consistent with our result that the variation between specifications that use pre-treatment outcome lags as economic predictor diminishes when the number of pre-treatment periods increases. Results are available upon request.

[37] We stress that the specification considered by Smith (2015) is not one of these three that would have led him to conclude that there is a significant effect.

this conclusion is reasonable since, in the case of Equatorial Guinea, we find that all specifications with a good fit have estimates of similar magnitude while, in the case of Ecuador, our results vary widely across specifications. The next step is to test the null hypothesis using a test statistics that combine the test statistics of all specifications. Restricting ourselves to specifications with good fit ($\tilde{R}^2 > 0.80$), we find that the p-value of a test that uses the mean of the RMSPE statistic across specifications, as suggested by Imbens & Rubin (2015), is equal to 0.031 and 0.308 for Equatorial Guinea and Ecuador, corroborating our conclusion that the treatment effect is positive in the first case and zero in the second one. Now, following the suggestion of Christensen & Miguel (2016) and Cohen-Cole et al. (2009), figure 3 shows the average treatment effect across specifications with good fit as a black line and the associated placebo effects as gray lines. Clearly, the effects for Equatorial Guinea and Ecuador are, respectively, large and small when compared to their empirical distributions. Finally, in line with the suggestions of Firpo & Possebom (2017), we invert tests based on the mean of the RMSPE statistic across specifications to compute confidence sets for the treatment effect over time. Our confidence sets (see figure 4) include all treatment effect functions that we fail to reject using this combined test statistic, considering functions that are deviations from the average treatment effect across specifications by an additive and constant factor. Analyzing sub-figure 4a, we see that, although we cannot reject treatment effect functions that are initially negative, all treatment effect functions in our confidence sets increase very fast, becoming positive after a few years of treatment. For Ecuador (see sub-figure 4b), we find that our confidence set include a zero effect for almost all years after the beginning of treatment, suggesting that the discovery of oil and gas in Ecuador had almost no impact on per-capita GDP.

For Equatorial Guinea and Ecuador, the results based on the recommendations by Imbens & Rubin (2015), Christensen & Miguel (2016), Cohen-Cole et al. (2009) and Firpo & Possebom (2017) point all to the same direction. Therefore, a reasonable conclusion would be that the treatment effect is significant for Equatorial Guinea and not significant for Ecuador.

6.2 Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program (Abadie et al. (2010))

Abadie et al. (2010) evalute the effect of Proposition 99, a large-scale tobacco control program that California implemented in 1988, on annual per-capita cigarette sales.[38] The pre and post-treatment periods are 1970-1988 and 1989-2000. The donor pool includes thirty-eight American states (Alabama, Arkansas, Colorado, Connecticut, Delaware, Georgia, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, West Virginia, Wisconsin, Wyoming).

We estimate the impact of Proposition 99 on California's annual per-capita cigarette sales using the synthetic control method with fourteen different specifications. Specifically, we test seven different specifications that differ in which functions of the pre-treatment periods are included and, for each one of them, we either include four covariates [39] or not. The seven basic specification are (1) all pre-treatment outcome values, (2) the first three fourths of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) pre-treatment outcome mean, and (7) three outcome values (original specification by Abadie et al. (2010) — years 1975, 1980 and 1988). .

Table 10 shows the p-value and our goodness of fit measure for each of the 14 specifications we considered. Note that quality of the fit varies widely across specifications: eight of them fit the data very closely ($\tilde{R}^2 \geq 0.975$), five of them have an intermediate value for our measure of goodness of fit ($0.80 < \tilde{R}^2 < 0.975$) and one of them fit the data very poorly ($\tilde{R}^2 \leq 0.80$). Most importantly, all specifications with good fit have significant estimates whose magnitude is similar according to figure 5, although p-values vary from 0.026 (the p-value in the specification considered in Abadie et al. (2010)) to 0.077 depending on the specification.

Now, we test the null hypothesis using a test statistic that combine the test statistics of all specifications. Restricting ourselves to specifications with a fit as good as the original specification ($\tilde{R}^2 > 0.975$), we find that the p-value of a test that uses the mean of the RMSPE statistic across specifications, as suggested by Imbens & Rubin (2015), is equal to 0.077, which is larger than the p-value of the original specification (0.026). Hence, the treatment effect is still significant even

[38] Following the best practices in terms of transparency and replicability, they made their dataset and replication files available through the command *synth* in the software *Stata*.

[39] The included covariates are average retail price of cigarettes, per capita state personal income (logged), percentage of the population age 15–24, and per capita beer consumption

though the test statistic for California does not stands out as the largest one among all placebo runs as it does when we consider the original specification. Additionally, figure 3 shows the average treatment effect across specifications with good fit as a black line and the associated placebo effects as gray lines following the suggestion of Christensen & Miguel (2016) and Cohen-Cole et al. (2009). Note that the treatment effects for California seem to be larger (or, at least, more stable) than the placebo effects. Finally, in line with the suggestions of Firpo & Possebom (2017), we invert tests based on the mean of the RMSPE statistic across specifications to compute confidence sets for the treatment effect over time. Our confidence set include all treatment effect functions that we fail to reject using this test, considering functions that are deviations from the average treatment effect across specifications by an additive and constant factor. Analyzing figure 7, we see that, although we cannot reject treatment effect functions that are initially positive, all treatment effect functions in our confidence sets become negative after a few years of treatment, suggesting Proposition 99 eventually reduced tobacco consumption in California.

Henceforth, our results suggest that the effect of the California's tobacco control program is significantly different from zero, although the test statistic for California is not always the largest one among all placebo runs when we consider different specifications, even if we consider only specifications that provide a good pre-treatment fit.

7 Conclusion

We analyze whether a lack of specific guidance on how to choose among different SC specifications creates the potential for specification searching with synthetic controls. We first provide theoretical results showing that the possibility for specification searching become asymptotically irrelevant when the number of pre-treatment periods goes to infinity if we restrict ourselves to SC specifications such that the number of pre-treatment outcome lags used as predictors goes to infinity when the number of pre-treatment periods goes to infinity. However, guided by our theoretical results, we provide evidence from MC simulations and from simulations with real datasets showing that specification searching may be a relevant problem in real SC applications for at least two reasons: first, many SC applications do not have a large number of pre-treatment periods to guarantee that our asymptotic results are approximately valid. Second, many SC applications rely on specifications that do not satisfy the conditions in our theoretical results.

We also provide evidence that restricting the set of options a researcher has when applying the SC method can substantially attenuate this specification-searching problem. We move in this direction by showing that the specification that uses the average of the pre-treatment outcome or a limited number of pre-treatment outcomes as predictors may fail to exploit the dynamics of the time series, which is the main goal of the SC method. Discarding these specifications significantly reduces the room for specification searching when the number of pre-treatment periods is large, even though it does not completely solve the problem. However, further research is necessary to determine in which circumstances one should use, for example, all pre-treatment lags as economic predictors or only a subset of the pre-treatment outcome lags (and, in this case, which subset should be used). Consequently, additional restrictions on the set of specifications applied researchers can use when employing the SC method in a given application can further reduce the scope for specification searching with synthetic controls. Furthermore, we also recommend that researchers report results using different specifications, and we suggest alternatives to take into account the fact that the treatment effect can be estimated using different specifications. Finally, we show that these recommendations can easily be implemented in practice, providing clear conclusions about the significance of an estimate.

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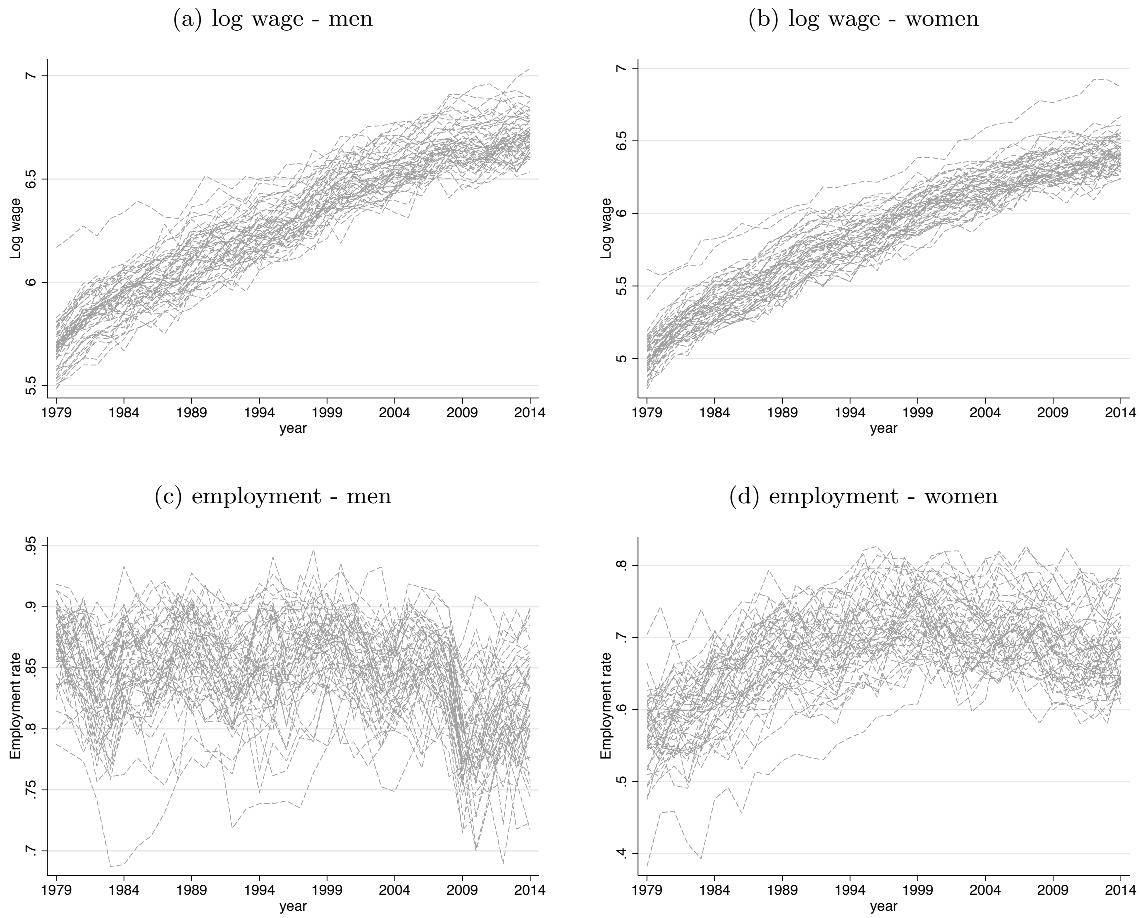
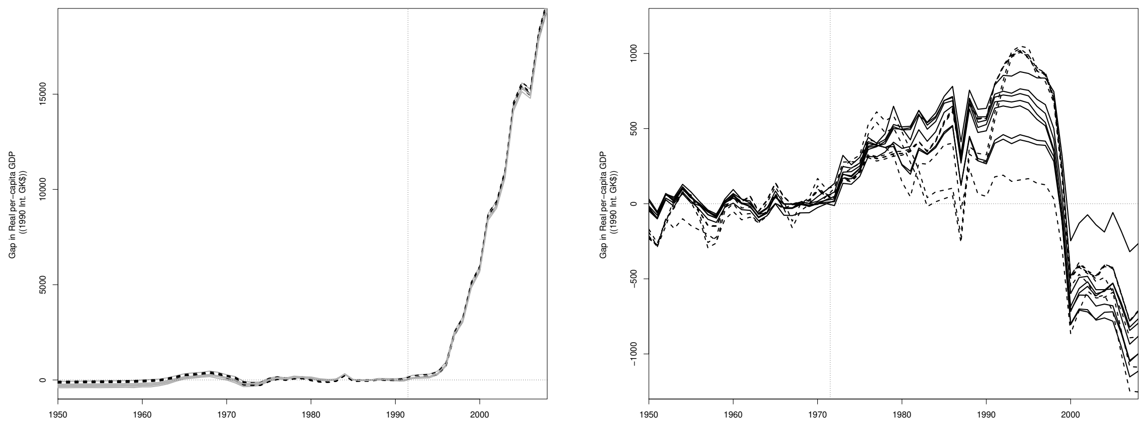
Figure 1: **Outcome trajectories in the CPS data**



Notes: We present the time series of log wages and employment rates for all US states separately by men and women.

Figure 2: **Treatment Effects for All Specifications - Database from Smith (2015)**

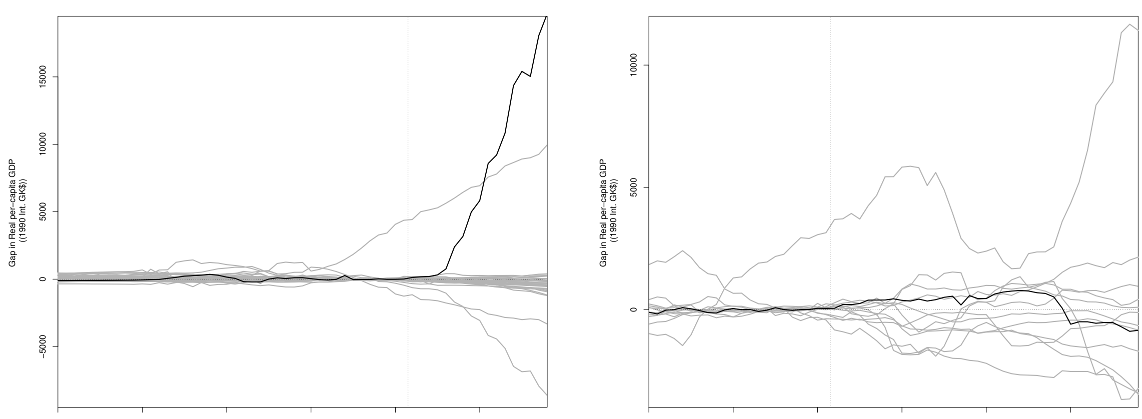
(a) Equatorial Guinea (b) Ecuador



Notes: Gray lines have $\tilde{R}^2 \leq 0.80$, dashed lines have $0.80 < \tilde{R}^2 \leq 0.95$ and solid black lines have $\tilde{R}^2 > 0.95$, where $\tilde{R}^2$ is defined by equation (10). The vertical lines denote the beginning of the post-treatment period.

Figure 3: **Placebo Effects Using the Average Across Specifications - Database from Smith (2015)**

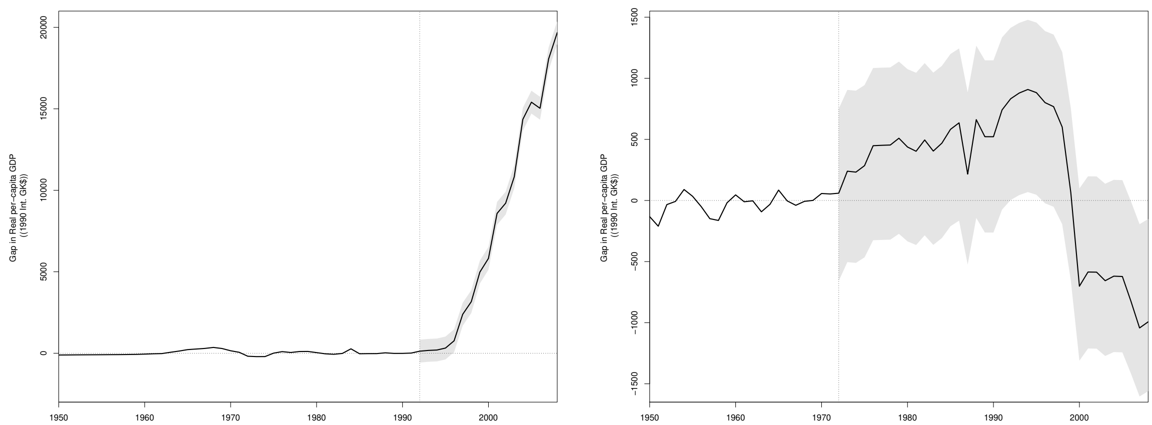
(a) Equatorial Guinea (b) Ecuador



Notes: We only consider specifications that satisfy $\tilde{R}^2 > 0.80$ to compute the average treatment effect across specifications, where $\tilde{R}^2$ is defined by equation (10). Gray lines are the placebo effects of the control countries and the black line is the average treatment effect of the treated country. The vertical lines denote the beginning of the post-treatment period.

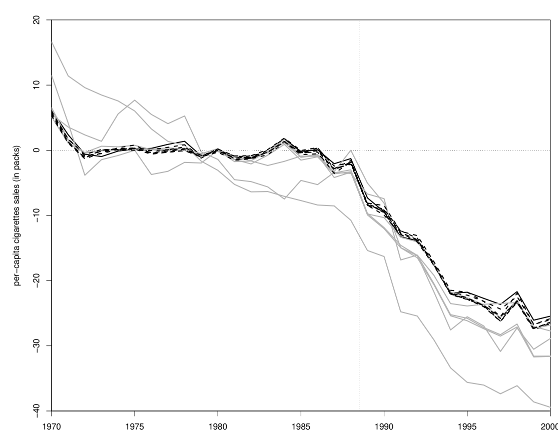
Figure 4: **90%-Confidence Sets Around the Average Across Specifications - Database from Smith (2015)**

(a) Equatorial Guinea (b) Ecuador



Notes: We compute confidence sets by inverting the average test statistic across specifications that satisfy $\tilde{R}^2 > 0.80$, where $\tilde{R}^2$ is defined by equation (10). Our confidence sets (see figure 4) include all treatment effect functions that we fail to reject using this combined test statistic, considering functions that are deviations from the average treatment effect across specifications by an additive and constant factor. The black line is the average treatment effect of the treated country and the gray area is the confidence set. The vertical lines denote the beginning of the post-treatment period.

Figure 5: **Treatment Effects for All Specifications - Database from Abadie et al. (2010)**

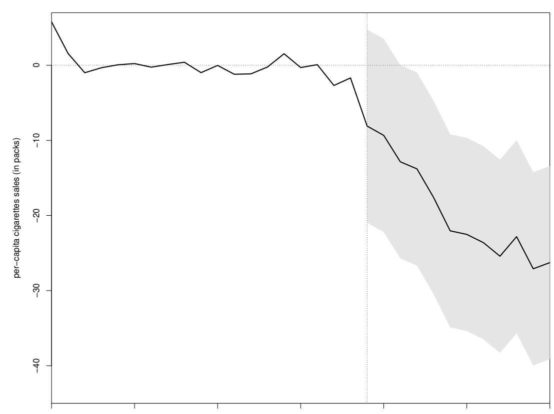


Notes: The solid black line is the original specification by Abadie et al. (2010), whose measure of goodness of fit is $\tilde{R}^2 = 0.0975$, where $\tilde{R}^2$ is defined by equation (10). Gray lines have $\tilde{R}^2 \leq 0.975$ and dashed lines have $\tilde{R}^2 > 0.975$. The vertical line denotes the beginning of the post-treatment period.

Figure 6: **Placebo Effects Using the Average Across Specifications - Database from Abadie et al. (2010)**

Notes: We only consider specifications that satisfy $\tilde{R}^2 > 0.0975$ to compute the average treatment effect across specifications, where $\tilde{R}^2$ is defined by equation (10) Gray lines are the placebo effects of the control state and the black line is the average treatment effect of California. The vertical line denotes the beginning of the post-treatment period.

Figure 7: **90%-Confidence Sets Around the Average Across Specifications - Database from Abadie et al. (2010)**



Notes: We compute confidence sets by inverting the average test statistic across specifications that satisfy $\tilde{R}^2 > 0.80$, where $\tilde{R}^2$ is defined by equation (10). Our confidence sets (see figure 4) include all treatment effect functions that we fail to reject using this combined test statistic, considering functions that are deviations from the average treatment effect across specifications by an additive and constant factor. The black line is the average treatment effect of California and the gray area is the confidence set. The vertical lines denote the beginning of the post-treatment period.

Table 1: **Specification searching**

	Stationary Model		Non-stationary Model	
	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)
$T_0 = 12$	0.143	0.250	0.142	0.254
	(0.003)	(0.004)	(0.004)	(0.004)
$T_0 = 32$	0.146	0.255	0.158	0.275
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.143	0.254	0.152	0.264
	(0.003)	(0.004)	(0.004)	(0.004)
$T_0 = 400$	0.134	0.241	0.145	0.255
	(0.003)	(0.004)	(0.004)	(0.005)

Note: Rejection rates are estimated based on 10,000 observations and on seven specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values,, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. *z% test* indicates that the nominal size of the analyzed test is z% and T_0 is the number of pre-treatment periods.

Table 2: **Probability of good pre-treatment fit**

	Stationary model		Non-stationary model	
	$\tilde{R}^2 > 0.80$	$\tilde{R}^2 > 0.95$	$\tilde{R}^2 > 0.80$	$\tilde{R}^2 > 0.95$
	(1)	(2)	(3)	(4)
Panel A: At least one specification with good fit				
$T_0 = 12$	0.947	0.271	0.990	0.642
	(0.001)	(0.003)	(0.001)	(0.003)
$T_0 = 32$	0.993	0.085	1.000	0.857
	(0.001)	(0.003)	(0.001)	(0.004)
$T_0 = 100$	1.000	0.002	1.000	0.993
	(0.001)	(0.003)	(0.001)	(0.003)
$T_0 = 400$	1.000	0.000	1.000	1.000
	(0.001)	(0.003)	(0.001)	(0.004)
Panel B: Specifications 6 has a good fit				
$T_0 = 12$	0.163	0.015	0.323	0.082
	(0.004)	(0.001)	(0.004)	(0.004)
$T_0 = 32$	0.164	0.004	0.456	0.145
	(0.004)	(0.001)	(0.005)	(0.005)
$T_0 = 100$	0.170	0.000	0.757	0.242
	(0.004)	(0.001)	(0.004)	(0.004)
$T_0 = 400$	0.168	0.000	0.994	0.667
	(0.004)	(0.001)	(0.005)	(0.005)
Panel C: Specification 7 has a good fit				
$T_0 = 12$	0.579	0.092	0.779	0.350
	(0.005)	(0.002)	(0.003)	(0.004)
$T_0 = 32$	0.576	0.024	0.837	0.525
	(0.005)	(0.002)	(0.004)	(0.005)
$T_0 = 100$	0.590	0.001	0.931	0.718
	(0.005)	(0.002)	(0.003)	(0.004)
$T_0 = 400$	0.585	0.000	0.994	0.898
	(0.005)	(0.002)	(0.004)	(0.005)

Note: Descriptive statistics are estimated based on 10,000 observations and on seven specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values,, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. T_0 is the number of pre-treatment periods. Our measure of goodness of fit is defined by equation (10). We consider two definitions of good fit; $\tilde{R}^2 > 0.80$ and $\tilde{R}^2 > 0.95$.

Table 3: **Specification searching conditional on a good pre-treatment fit**

	Stationary Model		Non-stationary Model	
	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)
	Panel A: $\tilde{R}^2 > 0.80$			
$T_0 = 12$	0.119	0.205	0.124	0.218
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.110	0.193	0.138	0.240
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.101	0.174	0.141	0.243
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.093	0.163	0.145	0.255
	(0.003)	(0.004)	(0.004)	(0.005)
	Panel B: $\tilde{R}^2 > 0.95$			
$T_0 = 12$	0.199	0.323	0.129	0.222
	(0.008)	(0.009)	(0.004)	(0.005)
$T_0 = 32$	0.218	0.348	0.123	0.210
	(0.014)	(0.016)	(0.004)	(0.005)
$T_0 = 100$	0.130	0.217	0.114	0.193
	(0.084)	(0.098)	(0.003)	(0.004)
$T_0 = 400$	-	-	0.130	0.227
	-	-	(0.004)	(0.005)

Note: Rejection rates are estimated based on 10,000 observations and on seven specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. *z% test* indicates that the nominal size of the analyzed test is z% and T_0 is the number of pre-treatment periods. Our measure of goodness of fit is defined by equation (10). We consider two definitions of good fit; $\tilde{R}^2 > 0.80$ and $\tilde{R}^2 > 0.95$.

Table 4: **Variability of weights**

	Distance between weights of specification 1 vs. specification x:					
	2	3	4	5	6	7
			Panel A: Stationary Model			
$T_0 = 12$	0.156	0.210	0.137	0.137	0.631	0.337
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)
$T_0 = 32$	0.085	0.134	0.073	0.074	0.693	0.370
	(0.001)	(0.001)	(0.000)	(0.000)	(0.003)	(0.003)
$T_0 = 100$	0.055	0.080	0.051	0.051	0.724	0.381
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.004)
$T_0 = 400$	0.032	0.048	0.032	0.032	0.740	0.391
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.004)
			Panel B: Non-stationary Model			
$T_0 = 12$	0.137	0.185	0.114	0.115	0.661	0.295
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)
$T_0 = 32$	0.071	0.115	0.067	0.066	0.723	0.312
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)
$T_0 = 100$	0.049	0.070	0.049	0.049	0.756	0.313
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)
$T_0 = 400$	0.034	0.046	0.036	0.036	0.769	0.318
	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.004)

Note: The average variability of weights is based on 10,000 observations and captures the average maximum difference of allocated weights between specifications x and 1. Specification x is one of the specifications used to compute the synthetic control unit: (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. T_0 is the number of pre-treatment periods.

Table 5: **Misallocation of weights**

	Specification:						
	1	2	3	4	5	6	7
			Panel A: Stationary Model				
$T_0 = 12$	0.225	0.278	0.315	0.249	0.248	0.813	0.474
	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
$T_0 = 32$	0.148	0.163	0.193	0.143	0.143	0.811	0.459
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
$T_0 = 100$	0.110	0.115	0.119	0.099	0.099	0.811	0.450
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
$T_0 = 400$	0.091	0.092	0.094	0.086	0.085	0.812	0.451
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.004)
			Panel B: Non-stationary Model				
$T_0 = 12$	0.187	0.233	0.267	0.204	0.203	0.805	0.401
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
$T_0 = 32$	0.116	0.125	0.159	0.119	0.120	0.807	0.373
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.004)	(0.005)
$T_0 = 100$	0.085	0.087	0.097	0.080	0.080	0.815	0.357
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)
$T_0 = 400$	0.072	0.072	0.075	0.070	0.069	0.819	0.355
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)	(0.005)

Note: The average of misallocated weights is based on 10,000 observations. The reasoning behind this variable is the following: since, in our DGP, we divide units into groups whose trends are parallel only when compared to units in the same group, the sum of the weights allocated to the units in the other groups is a measure of the relevance given by the synthetic control method to units whose true potential outcome follows a different trajectory than the one followed by the unit chosen to be the treated one. Specification x is one of the specifications used to compute the synthetic control unit: (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. T_0 is the number of pre-treatment periods.

Table 6: **Specification searching - Excluding specifications 6 and 7**

	Stationary Model		Non-stationary Model	
	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)
	Panel A: Unconditional			
$T_0 = 12$	0.106	0.190	0.110	0.198
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.100	0.179	0.109	0.191
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.090	0.157	0.094	0.162
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.077	0.138	0.081	0.142
	(0.003)	(0.004)	(0.004)	(0.005)
	Panel B: Conditional on $\tilde{R}^2 > 0.80$			
$T_0 = 12$	0.104	0.184	0.107	0.192
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.099	0.177	0.108	0.191
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.090	0.157	0.094	0.162
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.077	0.138	0.081	0.142
	(0.003)	(0.004)	(0.004)	(0.005)
	Panel C: Conditional on $\tilde{R}^2 > 0.95$			
$T_0 = 12$	0.183	0.183	0.120	0.120
	(0.008)	(0.008)	(0.004)	(0.004)
$T_0 = 32$	0.208	0.208	0.113	0.113
	(0.013)	(0.013)	(0.004)	(0.004)
$T_0 = 100$	0.130	0.130	0.094	0.094
	(0.082)	(0.082)	(0.003)	(0.003)
$T_0 = 400$	-	-	0.081	0.081
	-	-	(0.004)	(0.004)

Note: Rejection rates are estimated based on 10,000 observations and on five specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, and (5) even pre-treatment outcome values. *z% test* indicates that the nominal size of the analyzed test is z% and T_0 is the number of pre-treatment periods. Our measure of goodness of fit is defined by equation (10). We consider two definitions of good fit; $\tilde{R}^2 > 0.80$ and $\tilde{R}^2 > 0.95$.

Table 7: **Probability of good pre-treatment fit - CPS**

	Log wages				Employment			
	Women		Men		Women		Men	
	$\tilde{R}^2 > 0.80$	$\tilde{R}^2 > 0.95$	$\tilde{R}^2 > 0.80$	$\tilde{R}^2 > 0.95$	$\tilde{R}^2 > 0.80$	$\tilde{R}^2 > 0.95$	$\tilde{R}^2 > 0.80$	$\tilde{R}^2 > 0.95$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: At least one specification								
$T_0 = 12$	0.914	0.573	0.876	0.413	0.276	0.031	0.153	0.017
	(0.028)	(0.043)	(0.031)	(0.044)	(0.03)	(0.011)	(0.031)	(0.008)
$T_0 = 32$	0.963	0.949	0.983	0.906	0.653	0.042	0.066	0.000
	(0.026)	(0.029)	(0.017)	(0.032)	(0.057)	(0.023)	(0.03)	-
Panel B: Specification 6 has a good fit								
$T_0 = 12$	0.846	0.224	0.719	0.087	0.069	0.000	0.008	0.000
	(0.033)	(0.035)	(0.038)	(0.023)	(0.015)	-	(0.003)	-
$T_0 = 32$	0.959	0.914	0.981	0.777	0.343	0.000	0.002	0.000
	(0.029)	(0.03)	(0.017)	(0.043)	(0.056)	-	(0.001)	-
Panel C: Specification 7 has a good fit								
$T_0 = 12$	0.874	0.317	0.790	0.168	0.107	0.001	0.020	0.001
	(0.031)	(0.036)	(0.033)	(0.031)	(0.015)	(0.001)	(0.007)	(0.001)
$T_0 = 32$	0.963	0.934	0.983	0.860	0.359	0.008	0.003	0.000
	(0.026)	(0.031)	(0.017)	(0.037)	(0.053)	(0.007)	(0.002)	-

Note: Descriptive statistics are estimated based on seven specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values — and on 5,000 observations for each outcome variable (employment and log wages), for each sample (men and women) and number of pre-treatment periods ($T_0 \in \{12, 32\}$).Our measure of goodness of fit is defined by equation (10). We consider two definitions of good fit; $\tilde{R}^2 > 0.80$ and $\tilde{R}^2 > 0.95$.

Table 8: **Specification searching - CPS simulations**

	Log wages				Employment			
	Women		Men		Women		Men	
	5% test	10% test	5% test	10% test	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Conditional on $\tilde{R}^2 > 0.80$ - All Specifications								
$T_0 = 12$	0.137***	0.234***	0.130***	0.218***	0.217***	0.351***	0.262***	0.415***
	(0.013)	(0.019)	(0.013)	(0.018)	(0.025)	(0.026)	(0.027)	(0.029)
$T_0 = 32$	0.123**	0.215***	0.117**	0.203**	0.141**	0.228**	0.151	0.242
	(0.029)	(0.039)	(0.029)	(0.04)	(0.045)	(0.056)	(0.08)	(0.108)
Panel B: Conditional on $\tilde{R}^2 > 0.80$ - Excluding Specifications 6 and 7								
$T_0 = 12$	0.108***	0.192***	0.106***	0.183***	0.201***	0.325***	0.253***	0.405***
	(0.012)	(0.018)	(0.011)	(0.016)	(0.024)	(0.027)	(0.027)	(0.029)
$T_0 = 32$	0.082	0.149	0.071	0.138	0.105	0.186*	0.151	0.242
	(0.023)	(0.033)	(0.021)	(0.033)	(0.036)	(0.049)	(0.08)	(0.108)

Note: Rejection rates are estimated based on seven specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values — and on 5,000 observations for each outcome variable (employment and log wages), for each sample (men and women) and number of pre-treatment periods ($T_0 \in \{12, 32\}$). *z% test* indicates that the nominal size of the analyzed test is z%. Our measure of goodness of fit is defined by equation (10). Here, we consider one definition of good fit; $\tilde{R}^2 > 0.80$. Unconditional results and conditional results imposing $\tilde{R}^2 > 0.95$ are available upon request. * means that we reject at 10% the null that the probability of rejecting at least one specification at *z%* is equal to *z%*. ** means that we reject at 5%, while *** means that we reject at 1%.

Table 9: **Specification Searching - Database from Smith (2015)**

	Equatorial Guinea		Ecuador	
	p-value	$\tilde{R}^2$	p-value	$\tilde{R}^2$
	(1)	(2)	(3)	(4)
(1a)	0.031	0.866	0.538	0.881
(1b)	0.031	0.797	0.385	0.975
(2a)	0.031	0.777	0.538	0.881
(2b)	0.031	0.832	0.308	0.975
(3a)	0.031	0.809	0.615	0.880
(3b)	0.031	0.790	0.231	0.972
(4a)	0.031	0.891	0.308	0.969
(4b)	0.031	0.536	0.077	0.970
(5a)	0.031	0.828	0.538	0.881
(5b)	0.031	0.744	0.769	0.804
(6a)	0.031	0.848	0.538	0.804
(6b)	0.031	0.657	0.077	0.972
(7a)	0.031	0.849	0.692	0.838
(7b)	0.031	0.671	0.231	0.955
# of Permutations	33		13	

Note: We analyze twelve different specifications. The number of the specifications refer to: (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values (original specification by Smith (2015)), (6) the mean of all pre-treatment outcome values, and (7) three outcome values. Specifications that end with an *a* include the covariates ethnic fragmentation and population size one year before the discovery, while specifications that end with an *b* do not include covariates. Our measure of goodness of fit is defined by equation (10).

Table 10: **Specification Searching - Database from Abadie et al. (2010)**

Specification	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
p-value	0.077	0.077	0.077	0.077	0.026	0.051	0.026	0.051
$\tilde{R}^2$	0.979	0.979	0.976	0.974	0.968	0.969	0.978	0.978

Specification	(5a)	(5b)	(6a)	(6b)	(7a)	(7b)
p-value	0.077	0.077	0.077	0.077	0.026	0.077
$\tilde{R}^2$	0.979	0.979	0.828	0.525	0.975	0.909

Note: We analyze fourteen different specifications. The number of the specifications refer to: (1) all pre-treatment outcome values, (2) the first three fourths of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) pre-treatment outcome mean, and (7) three outcome values (original specification by Abadie et al. (2010)). Specifications that end with an a include the covariates average retail price of cigarettes, per capita state personal income (logged), percentage of the population age 15–24, and per capita beer consumption, while specifications that end with an b do not include covariates. Our measure of goodness of fit is defined by equation (10).

ONLINE APPENDIX (NOT FOR PUBLICATION)

A Proofs of the main results

Proof of Proposition 1

Let $\widetilde{\mathcal{W}} = \{\widehat{\mathbf{W}} \in \mathcal{W} | \widehat{\mathbf{W}} \in \arg\min_{\mathbf{W} \in \mathcal{W}} (\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})$ for some $\mathbf{V} \in \mathcal{V}\}$, and $\widehat{Q}_{T_0}(\mathbf{W}) = \frac{1}{T_0}(\mathbf{Y_1} - \mathbf{Y_0}\mathbf{W})'(\mathbf{Y_1} - \mathbf{Y_0}\mathbf{W})$. Also, let $\hat{f}^s_{T_0}(\mathbf{W}, \mathbf{V}) = (\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})$, where $\mathbf{X_j}$ includes the predictors used in specification s when there are T_0 pre-treatment periods.

The SC weights computed from the nested optimization problem proposed in Abadie et al. (2010) can be defined by:

$$\widehat{\mathbf{W}}(s, T_0) = \arg \min_{\mathbf{W} \in \widetilde{\mathcal{W}}} \widehat{Q}_{T_0}(\mathbf{W}) \tag{11}$$

We want to show that $\widehat{\mathbf{W}}(s, T_0) \xrightarrow{p} \overline{\mathbf{W}}$. First, let $\mathbf{V}^*(s, T_0)$ be a diagonal matrix with diagonal entries equal to 1 for pre-treatment outcome lags and 0 for other predictors when we consider the predictors used in specification s with T_0 pre-treatment periods. Then we have that $\frac{1}{L(s,T_0)} \hat{f}^s_{T_0}(\mathbf{W}, \mathbf{V}^*(s, T_0)) = \frac{1}{L(s,T_0)} \sum_{t \in \mathcal{I}(s,T_0)} \left(Y^0_{1,t} - \mathbf{y}^0_{-1,t}{}'\mathbf{W} \right)^2$. By assumption 1 and by the fact that $L(s, T_0) \to \infty$ when $T_0 \to \infty$, $\frac{1}{L(s,T_0)} \hat{f}^s_{T_0}(\mathbf{W}, \mathbf{V}^*(s, T_0))$ converges uniformly in probability to $Q_1(\mathbf{W})$, which is uniquely minimized at $\overline{\mathbf{W}}$. Let $\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0) = \arg\min_{\mathbf{W} \in \mathcal{W}} \frac{1}{L(s,T_0)} \hat{f}^s_{T_0}(\mathbf{W}, \mathbf{V}^*(s, T_0))$. Since $\mathcal{W}$ is compact, we have that $\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0) \xrightarrow{p} \overline{\mathbf{W}}$ when $T_0 \to \infty$ (Theorem 2.1 of Newey & McFadden (1994)).

We now show that the solution to the nested problem proposed in Abadie et al. (2010) will also converge in probability to $\overline{\mathbf{W}}$. First, note that $\widehat{\mathbf{W}}(s, T_0)$ always exist. According to Berge's Maximum Theorem (Ok 2007, p. 306), $\widehat{\mathbf{W}}(\mathbf{V})$ is a compact-value, upper hemicontinuous and closed correspondence. As a consequence, $\widetilde{\mathcal{W}}$ is a compact set. To see that, take any sequence $\left\{ \widetilde{\mathbf{W}}_n \right\}_{n \in \mathbb{N}}$ such that $\widetilde{\mathbf{W}}_n \in \widetilde{\mathcal{W}}$ for any $n \in \mathbb{N}$. Since $\widetilde{\mathcal{W}} = \cup_{V \in \mathcal{V}} \widehat{W}(\mathbf{V})$ by its definition, there exists $\mathbf{V}_n \in \mathcal{V}$ for each $n \in \mathbb{N}$ such that $\widetilde{\mathbf{W}}_n \in \widehat{\mathbf{W}}(\mathbf{V}_n)$. We also know that there exists a convergent subsequence $\{\mathbf{V}_{n_m}\}_{m \in \mathbb{N}}$ such that $\lim_{m \to +\infty} \mathbf{V}_{n_m} =: \overline{\overline{\mathbf{V}}} \in \mathcal{V}$ because $\mathcal{V}$ is a compact set. By the definition of upper hemicontinuity (Stokey & Lucas 1989, p. 56), there exists a convergent subsequence $\left\{ \widetilde{\mathbf{W}}_{n_{m_l}} \right\}_{l \in \mathbb{N}}$ such that $\lim_{l \to +\infty} \widetilde{\mathbf{W}}_{n_{m_l}} =: \overline{\overline{\mathbf{W}}} \in \widehat{\mathbf{W}}\left(\overline{\overline{\mathbf{V}}}\right) \subset \cup_{V \in \mathcal{V}} \widehat{W}(\mathbf{V}) = \widetilde{\mathcal{W}}$, proving that $\widetilde{\mathcal{W}}$ is a compact set. Consequently, Weierstrass' Extreme Value Theorem guarantees that $\widehat{\mathbf{W}}(s, T_0)$ exists.

From assumption 1, we have that $\widehat{Q}_{T_0}(\mathbf{W})$ converges uniformly to $Q_1(\mathbf{W})$ over $\mathcal{W}$. Therefore, for any $\epsilon > 0$, (i) uniform convergence of $\widehat{Q}_{T_0}(\mathbf{W})$ implies that $\widehat{Q}_{T_0}(\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0)) < Q_1(\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0)) + \frac{\epsilon}{3}$ and $Q_1(\overline{\mathbf{W}}) < \widehat{Q}_{T_0}(\overline{\mathbf{W}}) + \frac{\epsilon}{3}$ with probability approaching to one (w.p.a.1), and (ii) convergence in probability of $\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0)$ and continuity of $Q_1(\mathbf{W})$ implies that $Q_1(\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0)) < Q_1(\overline{\mathbf{W}})) + \frac{\epsilon}{3}$ w.p.a.1. Therefore, $\widehat{Q}_{T_0}(\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0)) < \widehat{Q}_{T_0}(\overline{\mathbf{W}}) + \epsilon$ w.p.a.1.

Suppose now that $\widehat{\mathbf{W}}(s, T_0)$ does not converge in probability to $\overline{\mathbf{W}}$. Then $\exists \tilde{\epsilon} > 0$ such that $\text{Lim} Pr(|\widehat{\mathbf{W}}(s, T_0) - \overline{\mathbf{W}}| > \tilde{\epsilon}) \neq 0$ when $T_0 \to \infty$. Since $\mathcal{W}$ is compact and $Q_1(\mathbf{W})$ is uniquely minimized at $\overline{\mathbf{W}}$, then $|\widehat{\mathbf{W}}(s, T_0) - \overline{\mathbf{W}}| > \tilde{\epsilon}$ implies that $\exists \eta > 0$ such that $Q_1(\widehat{\mathbf{W}}(s, T_0)) > Q_1(\overline{\mathbf{W}}) + 3\eta$. Uniform convergence of $\widehat{Q}_{T_0}(\mathbf{W})$ implies that $\widehat{Q}_{T_0}(\widehat{\mathbf{W}}(s, T_0)) > Q_1(\widehat{\mathbf{W}}(s, T_0)) - \eta$ and $Q_1(\overline{\mathbf{W}}) > \widehat{Q}_{T_0}(\overline{\mathbf{W}}) - \eta$ w.p.a.1. Therefore, $\widehat{Q}_{T_0}(\widehat{\mathbf{W}}(s, T_0)) > \widehat{Q}_{T_0}(\overline{\mathbf{W}}) + \eta$ w.p.a.1.

However, if we set $\epsilon = \eta$, then we have $\widehat{Q}_{T_0}(\widehat{\mathbf{W}}(s, V^*(s, T_0), T_0)) < \widehat{Q}_{T_0}(\widehat{\mathbf{W}}(s, T_0))$ w.p.a.1, which contradicts the fact that for all $\tilde{T}_0$ we can always find $T_0 > \tilde{T}_0$ such that $\widehat{\mathbf{W}}(s, T_0) \in \widetilde{\mathcal{W}}$ with $|\widehat{\mathbf{W}}(s, T_0) - \overline{\mathbf{W}}| > \tilde{\epsilon}$ minimizes $\widehat{Q}_{T_0}(\mathbf{W})$ with positive probability. Therefore, it must be that $\widehat{\mathbf{W}}(s, T_0)$ converges in probability to $\overline{\mathbf{W}}$.

Proof of Corollary 2

Notice that we can write each estimator as:

$$\widehat{\alpha}_{1t}(s, T_0) = Y_{1t} - \sum_{j=2}^{J+1} \widehat{w}_j(s, T_0) Y_{j,t} \text{ for any } s \tag{12}$$

Using the result of proposition 1, under assumption 1:

$$\widehat{\alpha}_{1t}(s, T_0) \to_p Y_{1t} - \sum_{j=2}^{J+1} \overline{w}_j Y_{j,t} \text{ as } T_0 \to \infty \text{ for any } s \tag{13}$$

Hence, for any s and s' such that $L(s, T_0) \to \infty$ and $L(s', T_0) \to \infty$ when $T_0 \to \infty$:

$$\left| \widehat{\alpha}_{1t}(s, T_0) - \widehat{\alpha}_{1t}(s', T_0) \right| \to_p 0 \tag{14}$$

Proof of Corollary 3

Let $\mathbf{y}_{-j,t}$ be the vector of outcomes at time t excluding unit j, $\widehat{\mathbf{W}}_j$ be the SC weights when unit j is used as treated, and $\overline{\mathbf{W}}_j := \arg\min_{\mathbf{W}\in\widetilde{\mathcal{W}}} Q_j(\mathbf{W})$.

Conditional on $\{y_{1,t}, ..., y_{J+1,t}\}_{t=T_0+1}^{T}$, if outcomes are continuous, then we can define $\{(1), ..., (J+1)\}$ such that, with probability one:[40]

$$\frac{\frac{1}{T-T_0}\sum_{t=T_0}^{T}\left(y_{(1),t} - \mathbf{y}'_{-(1),t}\overline{\mathbf{W}}_{(1)}\right)^2}{Q_{(1)}(\overline{\mathbf{W}})} > ... > \frac{\frac{1}{T-T_0}\sum_{t=T_0}^{T}\left(y_{(J+1),t} - \mathbf{y}'_{-(J+1),t}\overline{\mathbf{W}}_{(J+1)}\right)^2}{Q_{(J+1)}(\overline{\mathbf{W}})} \quad (15)$$

From proposition 1, we know that $\widehat{\mathbf{W}}_j \overset{p}{\to} \overline{\mathbf{W}}_j$ and $\frac{1}{T_0}\sum_{t=1}^{T_0}\left(y_{j,t} - \mathbf{y}'_{-j,t}\widehat{\mathbf{W}}_j\right)^2 \overset{p}{\to} Q_j(\overline{\mathbf{W}})$. Therefore, the inequalities in 15 will remain valid w.p.a.1 when we consider the test statistics for the placebo runs.

Sufficient Conditions for Assumption 1

Let $\mathbf{y}_t^0 = (Y_{1,t}^0, ..., Y_{J+1,t}^0)'$. We show that the following assumption is sufficient for Assumption 1.

Assumption 2 $\{\mathbf{y}_t^0\mathbf{y}_t^{0\prime}\}$ is weakly stationarity, each element of $\{\mathbf{y}_t^0\mathbf{y}_t^{0\prime}\}$ has absolutely summable covariances, and $\mathbb{E}\left[\mathbf{y}_t^0\mathbf{y}_t^{0\prime}\right]$ is non-singular.

Let A_t be one element of $\{\mathbf{y}_t^0\mathbf{y}_t^{0\prime}\}$. Under Assumption 2, we can define $\mathbb{E}[A_t] = \mu$ and $\mathbb{E}[(A_t - \mu)(A_{t-j} - \mu)] = \gamma_j$, where $\sum_{j=0}^{\infty}|\gamma_j| < \infty$. Consider a subsequence $\{t_k\}_{k\in\mathbb{N}}$ with $t_k > t_{k-1}$. Note that $\mathbb{E}\left[\frac{1}{K}\sum_{k=1}^{K} A_{t_k}\right] = \mu$. We want to show that $\mathbb{E}\left[\frac{1}{K}\sum_{k=1}^{K}(A_{t_k} - \mu)\right]^2 \to 0$ when $K \to \infty$. Note that:

$$\begin{aligned} K^2\mathbb{E}\left[\frac{1}{K}\sum_{k=1}^{K}(A_{t_k} - \mu)\right]^2 &= (\gamma_0 + \gamma_{|t_1-t_2|} + ... + \gamma_{|t_1-t_K|}) + \\ &\quad +(\gamma_{|t_2-t_1|} + \gamma_0 + ... + \gamma_{|t_2-t_K|}) + \\ &\quad +... + (\gamma_{|t_K-t_1|} + \gamma_{|t_{K-1}-t_1|} + ... + \gamma_0) \\ &= K\gamma_0 + \sum_{k=1}^{K-1}\left[\sum_{l=k+1}^{K} 2\gamma_{|t_l-t_k|}\right] \leq K|\gamma_0| + \sum_{k=1}^{K-1}\left[\sum_{l=k+1}^{K} 2|\gamma_{|t_l-t_k|}|\right] \end{aligned}$$

[40] Continuous outcomes guarantees that ties will happen with probability zero.

Let $lim \sum_{l=0}^{T} |\gamma_l| = C$. Now note that, for each k, $\sum_{l=k+1}^{K} 2|\gamma_{|t_l - t_k|}|$ is the sum of a subsequence of $\{|\gamma_l|\}$. Therefore, for any k, we have that $\sum_{l=k+1}^{K} 2|\gamma_{|t_l - t_k|}| \leq \sum_{l=1}^{t_K} 2|\gamma_l| \leq C$. Therefore:

$$\mathbb{E}\left[\frac{1}{K}\sum_{k=1}^{K}(A_{t_k} - \mu)\right]^2 \leq \frac{1}{K}|\gamma_0| + \frac{K-1}{K^2}C$$

which implies that $\mathbb{E}\left[\frac{1}{K}\sum_{k=1}^{K}(A_{t_k} - \mu)\right]^2 \to 0$ when $K \to \infty$. Therefore, we have that all elements of the pre-treatment averages of $\{\mathbf{y}_t^0 \mathbf{y}_t^{0\prime}\}$ for any subsequence $\{t_k\}_{k\in\mathbb{N}}$ converge in probability to their corresponding expected values.

Since $\frac{1}{K}\sum_{k=1}^{K}\left(Y_{j,t_k}^0 - \mathbf{y}_{-j,t_k}^0{}'\mathbf{W}\right)^2$ is a linear combination of pre-treatment averages of elements of $\{\mathbf{y}_t^0 \mathbf{y}_t^{0\prime}\}$ for a given subsequence $\{t_k\}_{k\in\mathbb{N}}$, for any $\mathbf{W} \in \mathcal{W}$, we have that:

$$\widetilde{Q}_K(\mathbf{W}) \equiv \frac{1}{K}\sum_{k=1}^{K}\left(Y_{j,t_k}^0 - \mathbf{y}_{-j,t_k}^0{}'\mathbf{W}\right)^2 \xrightarrow{p} \mathbb{E}\left[\left(Y_{j,t}^0 - \mathbf{y}_{-j,t}^0{}'\mathbf{W}\right)^2\right] \tag{16}$$

where $\mathbb{E}\left[\left(Y_{j,t}^0 - \mathbf{y}_{-j,t}^0{}'\mathbf{W}\right)^2\right]$ is continuous and strictly convex.

Finally, we show that this convergence in probability is uniform. For any $\mathbf{W}', \mathbf{W} \in \mathcal{W}$, using the mean value theorem, we can find $\widetilde{\mathbf{W}} \in \mathcal{W}$ such that:

$$\left|\widetilde{Q}_K(\mathbf{W}') - \widetilde{Q}_K(\mathbf{W})\right| = \left|2\left(\frac{1}{K}\sum_{k=1}^{K}\mathbf{y}_{-j,t_k}^0 Y_{j,t_k}^0 - \frac{1}{K}\sum_{k=1}^{K}\mathbf{y}_{-j,t_k}^0 \mathbf{y}_{-j,t_k}^0{}'\widetilde{\mathbf{W}}\right)\cdot(\mathbf{W}' - \mathbf{W})\right| \tag{17}$$

$$\leq \left[\left(2\left|\left|\frac{1}{K}\sum_{k=1}^{K}\mathbf{y}_{-j,t_k}^0 Y_{j,t_k}^0\right|\right| + \left|\left|\frac{1}{K}\sum_{k=1}^{K}\mathbf{y}_{-j,t_k}^0 \mathbf{y}_{-j,t_k}^0{}'\right|\right| \times \left|\left|\widetilde{\mathbf{W}}\right|\right|\right) ||\mathbf{W}' - \mathbf{W}||\right]^{\frac{1}{2}} \tag{18}$$

Define $B_K = 2\left|\left|\frac{1}{K}\sum_{k=1}^{K}\mathbf{y}_{-j,t_k}^0 Y_{j,t_k}^0\right|\right| + \left|\left|\frac{1}{K}\sum_{k=1}^{K}\mathbf{y}_{-j,t_k}^0 \mathbf{y}_{-j,t_k}^0{}'\right|\right| \cdot C$. Since $\mathcal{W}$ is compact, $\left|\left|\widetilde{\mathbf{W}}\right|\right|$ is bounded, so we can find a constant C such that $\left|\widetilde{Q}_K(\mathbf{W}') - \widetilde{Q}_K(\mathbf{W})\right| \leq B_K\left(||\mathbf{W}' - \mathbf{W}||\right)^{\frac{1}{2}}$. From assumption 2, B_K converges in probability to a positive constant, so $B_K = O_p(1)$. Note also that $\mathbb{E}\left[\left(Y_{j,t}^0 - \mathbf{y}_{-j,t}^0{}'\mathbf{W}\right)^2\right]$ is uniformly continuous on $\mathcal{W}$. Therefore, from corollary 2.2 of Newey (1991), we have that $\widetilde{Q}_K$ converges uniformly in probability to $\mathbb{E}\left[\left(Y_{j,t}^0 - \mathbf{y}_{-j,t}^0{}'\mathbf{W}\right)^2\right]$ for any subsequence $\{t_k\}_{k\in\mathbb{N}}$.

B Model with time-invariant covariates

In Section 3, we provide evidence that specifications 6 (pre-treatment outcome mean as economic predictor) and 7 (initial, middle and final years of the pre-intervention period as economic predictors) fail to take into account the time-series dynamics of the data, which implies that the SC estimator using these specification do not converge to the SC estimators using the other specifications, that satisfy the conditions of proposition 1 and corollaries 2 and 3. As a consequence, the possibilities for specification searching do not vanish even when the number of pre-treatment periods is large in contrast to the behavior of the specifications within the scope of our theoretical results. However, in most applications that use specifications 6 and 7, other time-invariant covariates are also considered as economic predictors. Here we consider an alternative MC simulation where we include time-invariant covariates, and we show that the same pattern observed in Section 3 can arise even when we consider specifications that also include time-invariant covariates as economic predictors.

The alternative DGP is given by:

$$Y_{j,t}^0 = \delta_t + \lambda_t^k + \theta_t Z_i + \epsilon_{jt} \tag{19}$$

where $Z_i = 1$ for $i = 1, ..., 10$ and $Z_i = 0$ for $i = 11, ..., 20$. As in our DGP from Section 3, we consider $K = 10$.[41] We consider that λ_t^k is normally distributed following an AR(1) process with 0.5 serial correlation parameter, $\delta_t \sim N(0, 1)$, $\epsilon_{j,t} \sim N(0, 0.1)$, and $\theta_t \sim N(0, 1)$. We consider the same seve specifications as in Section 3, except that we also include Z_i as economic predictor.

In columns (1) and (2) of Table A.3, we show that the variability of specifications 6 and 7 in comparison to specification 1 remain constant with T_0, which implies that there is still substantial differences in the SC estimators even when T_0 is large. Mort importantly, this suggests that the additional of time-invariant covariates do not help to alleviate the poor finite sample behavior of the specifications that do not satisfy the conditions of proposition 1.

In columns (3) and (4), we present the proportion of misallocated weights for specifications 6 and 7. Although the misallocation of weights is slightly less intense than in the DGP of the main text (table 5), specifications that do not satisfy the conditions of proposition 1 still misallocate

[41] Therefore, units 1 and 2 follow the trend λ_t^1, units 3 and 4 follow the trend λ_t^2, and so on.

significantly more weight relative to other specifications, and, importantly, the misallocation of weights remains constant when T_0 increases.[42]

Given that specifications 6 and 7 remains poorly behaved in comparison to the other specifications even with large T_0, the possibilities for specification searching remain high for large T_0, as presented in columns (5) and (6) of Table A.3. This is similar to our findings in Section 3. The intuition is that including Z_i as an economic predictor helps prevent that the SC estimator allocates positive weights to units $i = 11, ..., 20$. However, specification 6 and 7 still fails to capture the time-series dynamics when allocating weights among units $i = 2, ..., 10$.

[42]The misallocation for the other specifications is similar to the stationary model considered in Section 3. Results available upon request.

C Appendix Tables

Table A.1: **Specification searching - Alternative Models**

	Model (8) with $\epsilon_{j,t} \sim N(0,1)$		Model (8) with $K=2$	
	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)
$T_0 = 12$	0.139	0.246	0.142	0.25
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.132	0.235	0.147	0.247
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 100$	0.13	0.235	0.133	0.243
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.119	0.218	0.129	0.23
	(0.003)	(0.004)	(0.003)	(0.004)

Note: Rejection rates are estimated based on 10,000 observations and on six specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. *z% test* indicates that the nominal size of the analyzed test is z% and T_0 is the number of pre-treatment periods.

Table A.2: **Specification searching - Excluding specifications 2 and 3**

	Stationary Model		Non-stationary Model	
	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)
	Panel A: Unconditional			
$T_0 = 12$	0.125	0.225	0.123	0.224
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.131	0.232	0.138	0.251
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.131	0.237	0.139	0.248
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.127	0.23	0.138	0.245
	(0.003)	(0.004)	(0.004)	(0.005)
	Panel B: Conditional on $\tilde{R}^2 > 0.80$			
$T_0 = 12$	0.099	0.179	0.104	0.185
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.092	0.164	0.116	0.211
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.086	0.152	0.128	0.226
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.084	0.15	0.138	0.245
	(0.003)	(0.004)	(0.004)	(0.005)
	Panel C: Conditional on $\tilde{R}^2 > 0.95$			
$T_0 = 12$	0.178	0.178	0.109	0.109
	(0.007)	(0.007)	(0.004)	(0.004)
$T_0 = 32$	0.202	0.202	0.101	0.101
	(0.013)	(0.013)	(0.004)	(0.004)
$T_0 = 100$	0.087	0.087	0.098	0.098
	(0.081)	(0.081)	(0.003)	(0.003)
$T_0 = 400$	-	-	0.122	0.122
	-	-	(0.004)	(0.004)

Note: Rejection rates are estimated based on 10,000 observations and on five specifications — (1) all pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values.. *z% test* indicates that the nominal size of the analyzed test is z% and T_0 is the number of pre-treatment periods. Our measure of goodness of fit is defined by equation (10). We consider two definitions of good fit; $\tilde{R}^2 > 0.80$ and $\tilde{R}^2 > 0.95$.

Table A.3: **Model with time-invariant covariates**

	Variability of Weights		Misallocation of Weights		Specification Searching	
	Spec 6	Spec 7	Spec 6	Spec 7	5% test	10% test
	(1)	(2)	(3)	(4)	(5)	(6)
$T_0 = 12$	0.494	0.197	0.625	0.287	0.142	0.232
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)
$T_0 = 32$	0.527	0.203	0.615	0.263	0.141	0.224
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)
$T_0 = 100$	0.543	0.204	0.611	0.251	0.136	0.215
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
$T_0 = 400$	0.553	0.202	0.612	0.247	0.125	0.200
	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)

Note: This table presents results based on 10,000 observations of the MC simulations described in Appendix B. Columns (1) and (2) presents the variability of weights for specifications (spec) 6 and 7 in comparison with spec 1 when we include Z_i as economic predictors. Columns (3) and (4) presents the the misallocation of weights for spec 6 and spec 7 when we include Z_i as economic predictors. Columns (5) and (6) present the probability of rejecting the null in at least one specification at, respectively, 5% and 10% significance level.

Table A.4: **Infeasible Test**

	Stationary Model		Non-stationary Model	
	5% test	10% test	5% test	10% test
	(1)	(2)	(3)	(4)
	Panel A: Including All Specifications			
$T_0 = 12$	0.201	0.344	0.192	0.330
	(0.004)	(0.005)	(0.004)	(0.005)
$T_0 = 32$	0.176	0.308	0.185	0.320
	(0.004)	(0.005)	(0.005)	(0.006)
$T_0 = 100$	0.155	0.274	0.167	0.291
	(0.004)	(0.005)	(0.004)	(0.005)
$T_0 = 400$	0.134	0.240	0.152	0.266
	(0.004)	(0.005)	(0.005)	(0.006)
	Panel B: Excluding Specifications 6 and 7			
$T_0 = 12$	0.152	0.266	0.146	0.259
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 32$	0.130	0.231	0.132	0.234
	(0.003)	(0.004)	(0.004)	(0.005)
$T_0 = 100$	0.102	0.184	0.105	0.191
	(0.003)	(0.004)	(0.003)	(0.004)
$T_0 = 400$	0.078	0.148	0.083	0.154
	(0.003)	(0.004)	(0.004)	(0.005)

Note: This table presents results for the unfeasible test. This test is based on the true distribution of the test statistics in our Monte Carlos. In the first panel, we present the results for the non-stationary and for the stationary model. In the second panel, columns (1) and (2) present the results for the stationary model when K equals 2, columns (3) and (4) contain the results for the stationary model with covariates and the last two columns (5) and (6) present the results for stationary model with an alternative distribution of the transitory shock,$\epsilon_{j,t} \sim N(0,1)$. Rejection rates are estimated based 10,000 observations and on seven specifications — (1) all pre-treatment outcome values, (2) the first three quarters of the pre-treatment outcome values, (3) the first half of the pre-treatment outcome values, (4) odd pre-treatment outcome values, (5) even pre-treatment outcome values, (6) the mean of all pre-treatment outcome values, and (7) three outcome values. *z% test* indicates that the nominal size of the analyzed test is z% and T_0 is the number of pre-treatment periods.