Synthetic Control Estimator: A Tool for Comparative Case Studies in Economic History

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Abstract

The Synthetic Control Method has become a widely used tool in estimating the causal impact of policies, shocks and interventions of interest on economic and social outcomes. The technique has become particularly popular in estimating the effect of these shocks on a single treated unit. As a transparent and data-driven statistical technique, the goal of the Synthetic Control Method is to construct an artificial control group for the treated unit that has similar pre-treatment characteristics but has not undergone the treatment itself. The synthetic control technique works well when the control group balances pre-intervention outcomes and auxiliary covariates as much as possible. In spite of its widespread adoption, the use of the Synthetic Control Method in comparative economic history has lagged behind other areas of economics. In this article, we critically review the properties of the Synthetic Control Method and discuss the necessary conditions for a plausible application of the technique to comparative economic history in support of research designed to answer some of the long-running historical questions.

Keywords: economic history, comparative economics, Synthetic Control Method
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1 Introduction

"Arguably the most important innovation in the evaluation literature in the last fifteen years is the synthetic control approach developed by Abadie et al. [2010, 2014b] and Abadie and Gardeazabal [2003]. This method builds on difference-in-differences estimation, but uses arguably more attractive comparisons to get causal effects."

Susan Athey and Guido Imbens (Econometrica, 2006)

Using the Synthetic Control Method to evaluate the impact of a particular intervention (say, an event) on particular outcomes of interest (say, GDP growth—the "treated unit") to assess the impact of that event has become a widely used tool in policy evaluation literature (Abadie and Gardeazabal 2003, Abadie et. al. 2010, 2015, Ben Michael et. al. 2018). In a broader sense, the method relies on difference-in-differences estimation and uses pre-intervention trends in the outcomes and auxiliary covariates to simulate a counterfactual outcome scenario reflective of the hypothetical absence of the intervention itself. The general thrust of the method is to construct the outcome of interest from an artificial control group of units that have never experienced the intervention but which have similar outcome dynamics and covariate levels as those of the treated unit in the pre-intervention period. This is achieved by constructing a weighted average of control units that matches the treated unit's outcome and auxiliary covariate values prior to the intervention. This is termed the "artificial unit", "synthetic unit" or "synthetic control group". If the past outcomes and covariates are reasonably well balanced between the treated unit and its control group, the difference in the outcome between the treated unit and its artificial (i.e. synthetic) control group plausibly captures the impact of the policy or intervention in question. In this respect, the idea of the Synthetic Control Method is to compute the gap in the given outcome of interest for the treated unit by assuming that the intervention or policy of interest had never happened thereby exploiting pre-intervention trends in the outcomes and covariates between the treated unit and its control group. In this way, a researcher can obtain a meaningful representation of the intervention impact relative to the evolution of the outcome in the synthetic control group.

The Synthetic Control Method can yield a plausible assessment of the impact of the policy or intervention in question provided that the imbalance in the pre-intervention outcomes and auxiliary covariates is minimized. The key limitation of the method pertains to the difficulty of achieving an exact balance between the treated and control units without producing bias (Ferman and Pinto 2018, Ben Michael et. al. 2018). Several empirical strategies to address the bias that arises from poor pre-intervention matching have been proposed (Abadie and Imbens 2011, Doudchenko and Imbens 2017), including: intending to achieve exact matching such as in the case of using an outcome model with a large pre-intervention period (Garoupa and Spruk 2019); the imposition of negative weighting of the synthetic control group through calibrated propensity scores (Wang and Zubizarreta 2020); the use of root-finding constrained optimization and the interior point method (Vanderbei 1999); and the construction of empirical rejection probabilities to reject the null hypothesis (Firpo and Possebom 2018).

In spite of its wide popularity in policy and impact evaluation studies, the use of the Synthetic Control Method to identify research directions in pursuit of answers to both short-run and long-run questions in economic and institutional history has been scarce. Perhaps the most well-known example of applying the method to economic history was undertaken by Abadie et. al. (2015). In this case, the authors study the impact of German reunification (i.e. Wiedervereinigung) on the economic growth of West Germany. Their approach is to exploit pre-unification trends in economic growth and its determinants to construct the synthetic control group of jurisdictions that best reproduces the pre-unification trajectory of West Germany but that did not experience unification. They use this to construct the counterfactual trajectory of economic growth in the hypothetical absence of reunification. Their results suggest that, in the absence of reunification, West Germany would most likely have achieved significantly higher ongoing economic growth possibly driven by the lack of the large fiscal redistribution from West Germany to the formerly East German territories in the hypothetical case.

Another famous, and perhaps more illuminative, example is that of Billmeier and Nannicini (2013). The authors study the effect of economic liberalization on economic growth in a large sample of countries. By comparing post-liberalization growth trajectories of the affected countries to the weighted trajectories of similar but untreated countries, they uncover the temporally and spatially heterogeneous impact of liberalization on growth. That is, liberalization episodes which occurred early in a country's history had a significant positive growth impact whereas more recent episodes did not.

In this paper we review the Synthetic Control Method and discuss both the scope and context of the applicability of the method in comparative economic history to identify and posit answers to some long-running historical questions. In doing so, we discuss the theoretical and empirical plausibility of the assumptions underlying the validity of the Synthetic Control Method, critically evaluate the state of the literature, and identify research opportunities we consider extant in economic history. The paper is organized as follows; section 2 discusses the prior literature; section 3 provides the key analytical narratives of the synthetic control analysis; section 4 discusses the applications of the method in comparative economic history; section 5 provides general discussion; and section 6 concludes.

2 Prior Literature

The Synthetic Control Method has gained significant momentum in the scholarly literature and has been applied in several different settings in a wide range of applications in economics and political science. These applications include evaluating the impact of: terrorism, civil wars and political risk (Abadie and Gardeazabal 2003, Bove et. al. 2017, Montalvo 2011, Yu and Wang 2013); natural disasters (Coffman and Noy 2012, Barone and Mocetti 2014, Cavallo et. al. 2013, Smith 2015); economic and trade liberalization (Billmeier and Nannicni 2013, Gathani et. al. 2013, Hosny 2012); health policy (Abadie et al. 2010, Bauhoff 2014, Kreif et. al. 2016, Spruk and Kovac 2020); political reforms and regimes changes (Billmeier and Nannicini 2009, Garcia Ribeiro et. al. 2013, Carrasco et. al. 2014, Abadie et. al. 2015, Spruk 2019, Campos et. al. 2019); social and political connections (Acemoglu et. al. 2016);

labor (Bohn et. al. 2014, Calderon 2014, De Souza 2014); and local development policies (Kirkpatrick and Bennear 2014, Gobillon and Magnac 2016, Possebom 2017, Ando 2015), among several others.

As described above, the Synthetic Control Method has been designed for comparative case studies in small and moderately sized samples as a transparent and data-driven way of constructing a synthetic control group for use in comparing the outcome of interest in an affected unit with the outcome of that control group without the direct exposure of that control group to the intervention of interest. Data-driven processes of constructing the synthetic control group are based on selecting weights of past outcomes and auxiliary covariates through the diagonal matrix comparison that determines which comparison units' outcome process characteristics are within the convex hull of the affected unit to best reproduce the outcome trajectory for the treated unit in the hypothetical absence of the intervention.

To date, the application of the Synthetic Control Method to comparative economic history has been minimal. Campos et. al. (2019) examine the contribution of institutional integration to economic growth and use the Synthetic Control Method to assess the growth effects of European Union membership for non-founding states. They find heterogeneous and largely positive effects of EU membership on growth. By simulating the economic growth trajectory without EU membership, they use a sample of non-EU countries as a donor pool to construct the country-level synthetic control groups that best capture pre-EU growth trajectories of non-founding EU members. The reported and somewhat positive effect of being an EU member on growth appears to be robust across more than 10,000 randomly generated donor samples. Somewhat intriguingly, they find that Greece is the only exception to the positive effects since its growth trajectory without EU membership appears to be better than the actual growth trajectory with the EU membership.

Another prominent example of the use of Synthetic Control Method to answer important historical questions comes from Grier and Maynard (2016) who examine the economic growth and development of the Venezuelan economy during the Chavez administration. A controversial figure, Chavez won three democratic elections, rewrote the Venezuelan constitution, restructured its

Supreme Court, and survived numerous coup and recall attempts. The proponents of *Chavismo* argue that poverty rates dropped massively during his term in office alongside reduced inequality and the rapid expansion of access to health and education for the poor. Opposing this, critics argue that Venezuela under Chavez underwent a rampant deterioration of institutional quality that paved the way for an increase in crony capitalism with numerous industries nationalized, the expropriation of private property and the gross mismanagement of Venezuela's largest oil company amidst the introduction of rigid food and price control policies. Empirically, the two antithetic views open an intriguing question: was Chavez the hero of the poor, supporting them with the beneficial economic growth impact of his policies, or simply the precursor of the resource disease and Venezuela's current disaster. Grier and Maynard (2016) apply the Synthetic Control Method to study the economic and social impact of Chavez's policies. In particular, they simulate the trajectories of various economic and social outcomes in the hypothetical absence of Chavez using other countries without the influence of Chavez as a control group. Their results indicate no discernible improvement in poverty, health and inequality outcomes during the Chavez administration compared to the control group of countries that best capture Venezuela's pre-Chavez economic, health and inequality-related outcomes.

Similarly, the notion whether the long-standing socialist rule of Fidel Castro helped or hurt the Cuban economy has also received much attention in the scholarly debate. Deploying a reasonably large sample of countries for the period 1920-2000, Jales et. al. (2018) find a moderate negative impact of the Castro regime on the economic growth trajectory of Cuba relative to the synthetic control group, which appears to be robust to various alterations of the original synthetic control estimator. Moreover, Bologna Pavlik and Geloso (2018) employ the Synthetic Control Method to test the hypothesis as to whether the policies of the Castro regime led to lower mortality rates. They use the set of Latin American countries as a control sample to build the counterfactual distribution of mortality rates as an approximation of the Castro regime's effect on infant mortality. Contrary to the popular narrative, they find that relative to the synthetic control group, Cuban infant mortality rates increased. The general narrative of these experiments is to describe the nature of an institutional shock with respect to its impact on economic growth and development.

Finally, and extending the narrative, Garoupa and Spruk (2019) propose a taxonomy of institutional changes distinguishing between: gradual institutional changes that help improve long-run growth without a major deviation; institutional changes imposed by a shock resulting in a temporary deviation of the growth path from its long-run equilibrium that eventually fizzles out; and structural breaks with a major and permanent deviation of long-run growth equilibrium. This approach allows for a clear empirical distinction between temporary deviations of growth equilibrium and structural breaks whilst alternative empirical techniques tend to blur the two phenomena. The presence or absence of a structural break in the post-intervention period is clearly of particular interest to the practitioners of Synthetic Control Method.

3 Synthetic Control Method - Analysis

3.1 Framework

In its simplest form, the synthetic control setup to study the impact of a certain intervention on the outcome of interest involves the set of $i = 1, 2, \ldots J + 1$ affected units, which may be either countries, regions, cities, firms or households, which are exposed to the intervention of interest. Without a loss of generality, we assume that only one unit is exposed to the intervention while the remaining *J* number of units are potential candidates in the donor pool to be used to construct the synthetic control group and evaluate the impact of the intervention.

Let $\ln y_{i,t}^{N}$ be the outcome of interest for the i-th unit in the absence of the intervention at time t within the discrete time horizon t = 1,2,…T and let T_0 denote the number of pre-intervention periods from the discrete time horizon such that $1 \leq T_0 < T$. Suppose that the outcome of interest in the presence of the intervention is denoted as $\ln y_{i,t}^{I}$, and assume that the period of intervention lasts from $T_0 + 1$ to T .

Two standard assumptions underly the validity of the Synthetic Control Method in providing a plausible interpretation of the impact of the intervention. First, the intervention of interest at the time of its implementation is independent of the outcome of interest in the pre-intervention period. This implies that if the intervention of interest invokes anticipatory effects prior to the timing of the intervention, it is not likely to yield a plausible representation of its impact. This assumption in turn implies that the intervention of interest should have no prior impact on the outcome of interest. That said, if the intervention of interest is anticipated, the outcome prior to the intervention may react to it, which most likely violates the assumption. Secondly, the units exposed to the intervention should not interfere in the intervention itself suggesting that potentially interfering units should be excluded from the donor pool in order to isolate the impact of the intervention on the treated unit. For instance, suppose the researcher is interested in the impact of democratization on the growth of regions in an affected country. To estimate and isolate the impact of the transition to democracy on regional growth, and ensure that both assumptions are not violated, all potentially interfering regions must be excluded from the donor pool. This implies that the treated region in question should be matched with the set of countries or regions that have not been affected by democratization.

An example of this is provided by Melcarne and Spruk (2019). The authors examine the impact of the institutional transition from monarchy to democracy on the economic growth of Italian regions in the one-hundred-year period from 1870-1970. To exclude potentially interfering regions, they effectively match every Italian region with the set of other countries that have not undergone the transition to democracy to prevent the impact of democratization from being contaminated by the presence of democracy in the potential donor countries.

Mathematically, this can be described as follows. Let $\lambda_1 = \ln y_{i,t}^{I} - \ln y_{i,t}^{N}$ describe the effect of the intervention of interest for country i at time t where $\lambda_1 = \left(\lambda_{1,T_0+1}, \ldots, \lambda_{1,T}\right)$ captures the full set of the effects of intervention in the post-treatment period. Further assume that

$D_{i,t} = 1 \cdot \left[\left(i \in J+1 \right) \rightarrow \left\{ 0,1 \right\} \right]$ is a simple linear indicator function that takes the value of 1 if the *i*-th country is exposed to the intervention at time T_0, and 0 otherwise. Hence, the outcome of interest for country *i* at time *t* is given by:

$$\ln y_{i,t} = \ln y_{i,t}^{N} + \lambda_{i,t} \cdot D_{i,t} \tag{1}$$

The practitioners of the Synthetic Control Method often aim to estimate the effect of the given intervention on the outcome of interest. The general thrust of Eq. (1) is that the level of outcome of interest in the absence of the intervention is, by default, unobserved to the econometrician. Under standard conditions, the vector of post-treatment effects of the intervention of interest $\lambda_1 = \left(\lambda_{1,T_0+1}, \ldots, \lambda_{1,T} \right)$ can only be estimated for the period $t > T_0$ which implies that $\lambda_{1t} = \ln y_{1,t}^{I} - \ln y_{1,t}^{N} = \ln y_{1,t} - \ln y_{1,t}^{N}$. Since $y_{1,t}^{N}$ is unobserved to the econometrician. Abadie et. al. (2010) advocate the use of the latent factor model where observed components of the outcome together with the matrix of observed and unobserved common factors are used to approximate $y_{1,t}^{N}$ in the pre-intervention period. The latent factor model provides a valid counterfactual trajectory of the outcome provided that transitory shocks exhibit a zero conditional mean independence assumption.

Since $y_{1,t}^{N}$ can only be estimated, a researcher using the synthetic control estimator must rely on the behavior of the outcome of interest in the donor pool not being exposed to the intervention of interest. This allows for the construction of the synthetic control group for the treated unit where observed covariates of y are set to match the treated unit on the set of observable time-varying and time-invariant characteristics prior to the timing of the intervention. Matching the treated unit with the control group unaffected by the intervention ensures that unobserved heterogeneity bias is not projected out of the counterfactual outcome trajectory since common time factors are matched automatically between the treated unit and its control group.

Further, suppose that the donor pool comprises $J-1$ units excluding the affected unit. By relying on the behavior of the outcome in the synthetic control group, *J×1* vector of weights allows us to reweight the behavior of the outcome in the control group such that its outcome of interest will mimic the characteristics of the treated unit as much as possible given the similarities in covariates and past outcome realizations. The vector of weights may be described by $\mathbf{W}=(w_2,w_3,...,w_{J+1})'$ such that $w_j \geq 0$ for $j=2,...,J+1$ and $w_2+...+w_{J+1}=1$. Notice that each particular value of the vector W represents a potential control unit with which to construct the synthetic control group. Under these circumstances, the synthetic control group is a weighted average of control countries sharing similar pre-intervention characteristics captured by observed covariates and pre-intervention outcomes. A standard theorem underlying the ability of the synthetic control estimator to reproduce the outcome of the treated unit in the absence of the intervention suggests that the discrepancy in the outcome of interest between the treated unit and its synthetic control group will disappear provided that the pre-intervention period is sufficiently large. This implies that under the standard conditions, the synthetic control group may provide a plausible characterization of the missing counterfactual scenario. An approximately unbiased estimator of $\lambda_{1,t}$ is then given by the underlying difference between the observed outcome and synthetic control group that holds the scale of transitory shocks constant provided that pre-intervention characteristics of the treated unit can be matched with its control group through a data-generating process.

One of the standard issues in synthetic control analysis arises from the potential lack of fit in the counterfactual outcome trajectory relative to the observed outcome of the affected unit prior to the intervention. On several occasions, the fit may be poor due to interpolation biases being large relative to the sample size. The traditional approach advocated by Abadie et. al. (2010), Cavallo et. al. (2013) is to adjust the underlying specification with the appropriate covariates set to avoid a poorly fit synthetic control unit, or remove the observations with a pre-intervention root mean square prediction error (RMSE) of greater than $\sqrt{3}$ multiplied by average pre-intervention RMSE (Acemoglu et. al. 2016). More recently, Ferman et. al. (2018) propose several specification-searching empirical strategies based on the prediction error of each specification and placebo estimations.

An important question regarding the ability of the synthetic control estimator to capture the effect of the intervention of interest concerns the composition of the control group for the treated unit. Under the most plausible circumstances, if the assumption on conditional independence of the intervention and prior outcomes holds, the affected unit should exhibit similar trends in past outcomes and covariates to serve as a meaningful representation of the unobserved counterfactual scenario. To avoid excessively large counterfactuals, numerous adjustments of the RMSE have been proposed to allow for large but plausible treatment effects without an artificially inflated counterfactual scenario arising from the lack of fit (Adhikari and Alm 2016, Dube and Zipperer 2015, Ferman et. al. 2018).

In the absence of the similarity in the trend of covariates and past outcomes used in the synthetic control specifications, the estimated counterfactual scenario is most likely plagued by the lack of fit. On the contrary, the synthetic control estimator allows the researcher to find an error-minimizing combination of weights of covariates and past outcomes that lie within the convex hull of the treated unit before the intervention. Recall that $\mathbf{W}$ is a J×1 vector of non-negative weights such that $\mathbf{W} = (w_2, \ldots, w_{J+1})'$ for $j = 2, \ldots J+1$ where $w_2 + \ldots + w_{J+1} = 1$. Each value from W represents the weighted average of the control unit's covariates that lie within the convex hull of the treated unit and serves as a synthetic control group facilitating the assessment of the effect of the intervention. The convexity of the combinations from the untreated units ensures that the weights are additive by themselves.

The key question pertains regarding the similarity of covariate-level characteristics between the affected unit and its control units unaffected by the intervention. The standard approach to minimize the pre-intervention distance between the affected and unaffected units, denoted as

$\|\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}\|$, is to consider Abadie and Gardeazabal (2003) and Abadie et. al. (2010) semi-definite fully symmetric *r×r* matrix V that captures characteristics-based distance between the affected unit and unaffected ones, viz:

$$\|\mathbf{X}_{1,j} - \mathbf{X}_{0,i}\mathbf{W}\|_{\mathbf{V}} = \sqrt{(\mathbf{X}_{1,j} - \mathbf{X}_{0,i}\mathbf{W})'\mathbf{V}(\mathbf{X}_{1,j} - \mathbf{X}_{0,i}\mathbf{W})} \tag{2}$$

where **W** is the distance-minimizing vector of non-negative weights used to match the treated and untreated units in terms of the covariate characteristics before the intervention, where $\mathbf{X}_{1,j}$ is the covariate-level vector for the treated unit, $\mathbf{X}_{0,i}$ is the covariate-level vector of the unaffected unit, and V is the positive semi-definite weighing matrix that captures the degree of similarity in terms of covariate values prior to the intervention. By extracting the similarity in terms of covariates and past outcomes, one is able to determine the quality of the fit between the treated unit and its unaffected counterparts, and to gauge whether the synthetic control group is a plausible representation of the treated unit's outcome trajectory in the hypothetical absence of the intervention.

3.2 Inference

The effect of the intervention of interest on the outcome in question also hinges on the statistical significance of the outcome gap between the treated and untreated unit as an approximation of the counterfactual scenario. The standard approach used to tackle the statistical significance of the outcome gap is to run a series of (i) in-space and (ii) in-time placebo tests. More specifically, in-space placebo tests allow the researcher to determine whether the effect of the intervention appears to be specific to the treated unit or whether it is also perceivable in the control sample. For example, Abadie et. al. (2010) perform a series of placebo checks to determine the impact of Proposition 99 (i.e. a large-scale anti-tobacco legislation introduced in California in 1989) on the prevalence of smoking. They assign Proposition 99 to all other states that did not implement Proposition 99 and thereby shift California from the treatment sample to the control sample. The underlying intuition of this approach is straightforward. If the effect of Proposition 99 appears to be specific to California, the effect of Proposition 99 on the smoking rates in other states should be either imperceptible or driven by the lack of fit. Under these circumstances, the evidence would lend support to the argument that a significant drop in smoking rates occurred in California in the post-Proposition period. On the other hand, if the gap in smoking rates in California in the post-intervention period is similar, then the analysis most likely does not provide evidence of the significant impact of the Proposition on the smoking rates. Hence, if the distribution of placebo effects of the intervention yields many effects as large as the baseline estimated, then the estimated impact of the intervention is most likely observed by chance and clearly not driven by the intervention in question. By default, such a non-parametric test does not impose any distribution of the random error term. Abadie et. al. (2010) extend this approach further and also consider the ratio of post-intervention and pre-intervention RMSE to judge the uniqueness of the effect.

Further suppose the effect of the intervention of interest is described by $\hat{\lambda}_{1t}$ and that the distribution of the in-space placebo effects is given by $\hat{\lambda}_{1t}^{Placebo} = \left\{ \hat{\lambda}_{jt} : j \neq 1 \right\}$. The two-tailed p-value on the effect of the intervention of interest may be computed as $\mathrm{P} = \mathrm{Prob}\left(\left| \hat{\lambda}_{1t}^{Placebo} \right| \geq \left| \hat{\lambda}_{1t} \right| \right) = \left(\sum_{j \neq 1} 1 \cdot \left| \hat{\lambda}_{jt} \right| \geq \left| \hat{\lambda}_{1t} \right| \right)^{-\left(\frac{1}{J}\right)}$ whereas one-tailed p-values for strictly positive effects are given by $\mathrm{P} = \mathrm{Prob}\left(\left| \hat{\lambda}_{1t}^{Placebo} \right| \geq \left| \hat{\lambda}_{1t} \right| \right) = \left(\sum_{j \neq 1} 1 \cdot \left| \hat{\lambda}_{jt} \right| \geq \left| \hat{\lambda}_{1t} \right| \right)$. Notice that since the intervention in most cases is not randomly distributed across the sample, the placebo distribution serves as a typical randomization inference. Since the p-values on the effect of the intervention of interest are derived non-parametrically, the probabilities represent the proportion of control units that have the estimated impact of the intervention at least as large as the treated unit. One caveat should be that of the case of states. Specifically, the placebo effects may be relatively large if the treated and control units are not well matched in the period preceding the intervention for reasons other than the lack of fit. The standard approach to partially mitigate the dissimilarity issue is to adjust the use of placebo coefficients $\hat{\lambda}_{1t}$ for the quality of pre-intervention match in two steps. In the first step, the quality of the match is adjusted by the multiple of the placebo effects to consider only those placebo gaps that match reasonably well. Abadie et. al. (2010) employ this restriction and iteratively exclude those placebos that are between five times and two times the size of California's pre-intervention RMSE and show that the impact remains largely unchanged, even under the least lenient RMSE restrictions. In the second step, the placebo effects are divided by the pre-shock match quality parameter to obtain the distribution of pseudo t-statistics and compute the relevant p-values, which allows us to conduct the statistical inference on the intervention of interest.

By contrast, an in-time placebo test is based on the assignment of the intervention of interest to a deliberately false date. The underlying intuition is both simple and straightforward. If the effect of the intervention of interest is perceivable in the given year, shifting the intervention period to a wrong date should yield implausible effects that do not arise from the year of the true treatment. In a much-debated example, Abadie et. al. (2015) study the economic growth impact of the reunification of West and East Germany in 1990. They conduct a simple in-time placebo test and shift the year of the unification from 1990 to 1970 as the mid-range of the sample period. Their findings suggest that the trajectory of real West Germany and its synthetic counterpart are identical before and after 1970 as a deliberately wrong year of the intervention.

4 Applications in Comparative Economic History: Scope

The framework of the Synthetic Control Method posits a vast potential for the application of the method to identify key questions and to assist in answering some of the long-running historical questions. Before these questions can be answered, a few pressing questions concerning research design and the composition of the samples remain. Given the scope of the analysis in estimating plausible counterfactual scenarios, the question that perhaps warrants heightened attention is: how should a valid synthetic control analysis of the important historical questions look?

4.1 Treatment scope

The scope of treatment used to study the impact of historical shocks, policy-related or institutional changes on the economic outcomes of interest should satisfy three specific criteria. First, the shock assigned to the affected unit should not be easily anticipated by prior economic conditions. If these conditions predict the timing of the treatment, the conditional independence assumption on the treatment of interest most likely fails and masks the estimated counterfactual scenario with the pre-shock outcome dynamics that is not necessarily attributed to the treatment of interest. A good example of a well-defined and unanticipated treatment is that of a natural disaster. For instance, Barone and Mocetti (2014) consider two large-scale earthquakes that occurred in two different Italian regions: one in 1976 (in Friuli Venezia Giulia) and the other in 1980 (in Irpinia) to estimate the impact of natural disasters on within-country economic growth. The use of natural disasters does not fall short of the conditional independence assumption since the timing cannot be anticipated. As a treatment, it provides a plausible shock that mimics the characteristics of the random assignment. Apart from providing strength to the feasibility of the assumption, natural disasters may provide an in-depth perspective of how local institutions and economic actors react to such shocks, and whether such disasters have either temporary or long-term economic impact. The authors find an almost zero short-term effect of the earthquake in both cases. In the long term they find opposite results.[5] The authors argue that the positive nature of the shock in Friuli Venezia Giulia and the negative nature of the shock in Irpinia reflect the strength of local institutions in combating the economic effects of the earthquake. In the latter case, the construction of better infrastructure to replace that damaged might increase the potential output and enhance the long-term growth trajectory. In the former, the construction of better infrastructure is hindered by the diversion and misallocation of public funds by rent-seeking behaviour that distorts markets and reduces the potential output, hence, inflicting a permanent

[5] More specifically, 20 years after the earthquake, they find that the observed per capita income in Friuli Venezia Giulia is 23 percent higher than in the synthetic control group, while the observed per capita income of Irpinia is 12 percent lower than in the synthetic control group.

economic growth penalty from the earthquake. In this respect, the interaction between large-scale natural disasters and the subsequent institutional changes should not be neglected.

In terms of a further example, Cavallo et. al. (2013) examine the causal impact of catastrophic natural disasters on economic growth by leveraging both between- and within-country growth variation in 196 countries for the period 1970-2008. By applying the synthetic control estimator to the countries undergoing large-scale natural disasters, they are able to construct a counterfactual growth scenario in the hypothetical absence of the disaster. By controlling for the intensity of disaster, they show that large-scale disasters have a permanent economic growth effect only if followed by the necessary radical political revolution that fundamentally reshapes the institutional structure of the society. They construct two comparative case studies of such disasters, namely the 1972 earthquake in Nicaragua and 1978 earthquake in Iran, to support their argument.

An important caveat to keep in mind concerns multiple cases of the same treatment. For instance, several scholars have tried to estimate the economic growth effects of institutional integration (Campos et. al. 2019, Garoupa and Spruk 2020, Maseland and Spruk 2020). One such example is the membership of the European Union and the effect of the institutional bonus of the EU-wide institutional framework on within-country economic growth. To isolate the effect of EU membership on growth, Campos et. al. (2019) and Garoupa and Spruk (2020) set up the synthetic control designed to include only one treated country and several non-treated ones. This implies that a single EU country is used in the treatment sample whilst the control samples include only non-EU units to capture the effect of EU membership on the economic growth trajectory. If other EU countries were included in the donor sample, the estimated treatment effect of EU membership would not be valid. Relatedly, Possebom (2017) applies the Synthetic Control Method to Brazilian city-level data for the period 1920-1999 to study the impact of Free Trade Zone of Manaus, and compares Manaus against 48 MCAs (i.e. Minimum Comparable Areas) in the Brazilian Northern Region to isolate the impact of free-trade zone on a range of economic outcomes. These results are further confirmed once the social impact of the free-trade zone is considered (Castilho et. al. 2017). Furthermore, Gobillion and Magnac

(2016) use time-varying regional data to study the impact of enterprise zone policy on local unemployment in France in 1990s and pool difference-in-differences with Synthetic Control Methods into an interactive effect model. Interestingly, they find only small short-run effects of the zone policy on the local employment rate (Gobillon et. al. 2012).

The general thrust of the treatment selection is its uniqueness. If the treatment is experienced by multiple units, the units undergoing the treatment at the same time must be excluded from the donor pool to provide a valid and plausible counterfactual scenario. For instance, Castillo et. al. (2017) investigate the impact of tourism policy on the employment rate using a comprehensive Tourism Development Policy implemented in the province of Salta in Argentina during the period 2003-2010. By adopting the synthetic control estimator to estimate the counterfactual employment rate in the absence of the policy, they first exclude the provinces that have undertaken a similar policy initiative in their sample period, namely, Buenos Aires City, Buenos Aires Province, Córdoba and Río Negro. In a similar vein, Ando (2015) uses the synthetic control estimator to estimate the impact of establishing nuclear power facilities in Japan in 1970s and 1980s on local per capita income. To establish a plausible counterfactual scenario, the author compares a single municipality where such a facility was established against a sample of non-nuclear municipalities.

4.2 Placebo analysis

4.2.1 In-time placebo analysis

One of the key questions arising from the estimated counterfactual scenario is the assessment of whether the gap in the outcome between the actual treated unit and its synthetic control group is not driven by pre-treatment trends. An intuitive approach has been suggested by Abadie et. al. (2015). In the study of the economic growth impact of German unification, they use a deliberately wrong date of the unification by pushing the 1990 treatment year to the middle of the pre-treatment period. They find that the growth trajectories of actual West Germany and its synthetic counterpart under a falsely assigned unification year are almost identical, which gives empirical support to the argument on the negative economic growth effect of unification on West German economy. If the pre-existing trends are perceived when the treatment is applied to a false year, the influence of pre-treatment events and shocks cannot be excluded and most likely taints the counterfactual scenario with the influence of these changes. Under these circumstances, the adjustment of the time period to better capture the impact of the treatment of interest is warranted alongside the control sample that does not contain any notion of the treatment of interest. In a similar example. Emery and Spruk (2020) examine the causal impact of factional politics on long-run development by exploiting the within-country growth variation using the 1958 civil conflict in Lebanon. They perform an in-time placebo study and assign the 1958 conflict to 1975 civil war and find that the structural break between actual Lebanon and its synthetic version does not occur in the year of the civil war, but rather is apparent with the 1958 conflict. Rigorous empirical evidence using the synthetic control estimator to examine the causal impact of the intervention of interest should not be able to reject the null hypothesis of in-time placebo effects in the wrongly assigned year of the intervention, and should be able to confirm the underlying post-treatment effect that begins to unfold in the year of the intervention.

4.2.2 In-space placebo analysis

Another important question concerns the statistical significance of the effect of the intervention of interest. Whilst an in-time placebo study is able to rule out the presence or absence of pre-intervention outcome trends, it does not entail any notion of statistical significance. For instance, how likely is it that the estimated effect of the intervention is obtained by chance? The standard way to evaluate the significance of synthetic control estimates is ask whether the results are randomly driven (Abadie et. al. 2010, Ferman and Pinto 2017). More specifically, how likely would we obtain an effect of similar size if we were to choose a unit other than the treated unit?

To address this issue, Bertrand et. al. (2004) advocate the use of in-space placebo tests. Such tests involve the application of the synthetic control estimator to the units that did not implement the intervention of interest. In the famous example of the study of terrorism on economic growth, Abadie and Gardeazabal (2003) perform a series of in-space placebo tests by applying the synthetic control estimator to a region from the synthetic control group for the Basque Country that was not exposed to terrorism. For this purpose, they select Catalonia and show that it remains unaffected by terrorist activity compared to the Basque Country. Moreover, Abadie et. al. (2010) then extend this approach by advocating the iterative application of the synthetic control estimator to all control units from the donor pool that were not exposed to the intervention of interest. The underlying thrust of the placebo studies concerns the similarity of the outcome gaps in the post-intervention period between the treated unit and all control units. If placebo analysis creates outcome gaps similar to the one for the actual treated unit, then the synthetic control analysis is quite unlikely to provide evidence of the significant impact of the intervention. On the other hand, if placebo analysis uncovers an outcome gap for the treated unit that is unusually large compared to the gaps for the control units, then the evidence of a significant effect becomes plausible. This implies that the ratio of the post- and pre-intervention prediction error should be large for the treated unit and gradually smaller for the untreated units.

Furthermore, Galiani and Quistorff (2017) automate the computation of the placebo gaps along with a non-parametric set of p-values indicating how likely it is that the effect is driven by chance across the full range of post-intervention years. As suggested by Ferman and Pinto (2017), the inference on the placebo gaps is tainted by the size distortions even when the post-/pre-intervention prediction error ratio has the same marginal distribution for all placebo runs. Since such p-values are non-parametric and indicate the proportion of control units with the effect as large as the effect on the treated unit, Garoupa and Spruk (2019) perform difference-in-differences test of the uniqueness of the placebo gaps that can be applied to single or multiple treated units, and show that the test performs reasonably well in large samples.

4.3 Leave-one-out analysis

The next question concerns the effect of ambiguity that arises from the intervention of interest. Klößner et. al. (2018) review the findings by Abadie et. al. (2015) on the economic growth effect of German unification and address the variation in the post-unification economic growth gap by performing a series of effect simulations with a different composition of the donor pool. More specifically, they find that the economic growth gap between West Germany and its synthetic control group is strongly influenced by US data, and show that excluding the United States of America from the donor pool renders the estimated growth gap much smaller and no longer significant in the in-space placebo analysis. The composition of the donor pool matters greatly for the size of the underlying effect and has, by default, immediate implications for statistical significance.

Extending this discussion with another example, Spruk and Keseljevic (2020) examine the causal impact of the Yugoslav war on the economic growth trajectory of former Yugoslav republics. They confront a significant variation in the composition of synthetic control groups across the treated countries[6].

To address this issue, the authors undertake a "leave-one-out" analysis by excluding the country with the largest weight share from the control group for each treated country. They show that the covariate balance remains unaffected since the exclusion does not alter the pre-war prediction error. Moreover, the size and significance of the growth effect of the Yugoslav war remains unaffected by the exclusion of the most powerful donor from the control group, suggesting that ambiguity is not an issue *per se*.

Leave-one-out analyses can be further extended by considering a more rigorous exclusion set such as the exclusion of the entire synthetic control group from the donor pool to check whether the size and significance of the effect are stable or not. Such analysis is contingent on the sample size such that it is sufficiently large to permit such exclusions without invoking an under-powered research design where the inference is typically driven by a few control units.

4.4 Specification search

[6] For instance, given the covariate weights, the economic growth trajectory of Slovenia before the Yugoslav war can be best synthesized by Switzerland (37%), Malta (28%), Iceland (12%), Japan (9%), and Oman (8%). On the other hand, Kosovo's pre-war growth trajectory can be best reproduced by Cape Verde (32%), Malta (26%), Romania (23%), Mozambique (10%), and a few others with minor weight shares.

One of the key steps to undertake the synthetic control analysis is to identify the covariates of the outcome variable. In the ideal setup, these covariates are able to account for the bulk of the outcome variance in the pre-intervention years. The natural question to ask is how to select the covariates to build both comprehensive and compact synthetic control specification. As a rule of thumb, Hahn and Shi (2017) advocate for a large number of covariates compared to the size of the donor pool as a way to improve the choice of weights assigned to each control unit given the set of predictor weights. Since both time-varying and time-invariant covariates can be considered, one potentially appealing candidate is the lagged outcome variable. Apart from addressing the standard endogeneity concerns inherent in within/between-type of outcome variation, the inclusion of the lagged outcome variable largely ameliorates the standard omitted variable bias. Furthermore, Athey and Imbens (2006) argue that including covariates other than the lagged outcome variable implies that these covariates rarely matter.

Against this backdrop, Klößner et. al. (2018) suggest that researchers should avoid using the values of outcome variables for all pre-intervention years since such a strategy would nullify the effect of the auxiliary covariates. In addition, Ferman et. al. (2018) show that the lack of clear theoretical guidance on the selection of covariates creates numerous specification-searching problems. They recommend the consideration of different sets of lags and covariates and report them all whilst discarding specifications with the average of pre-treatment outcome since it fails to exploit the pre-intervention outcome dynamics. To successfully exploit the specification-searching opportunities, McClelland and Gault (2017) recommend the choice of a small number of outcome variable lags that follow the outcome trend in the pre-intervention period.

4.6 Differential trend analysis

Lastly, the ability of the synthetic control estimator to estimate the causal impact of the intervention of interest depends on the researcher's ability to determine whether the outcome in the treated unit and its synthetic control group follows a different trend in the post-intervention period. Ideally, the outcome trends in the treated unit and its synthetic control group should be indistinguishable whilst being statistically distinguishable in the post-treatment period. To address this issue, Spruk and Kovac (2020) test the similarity of outcome trends between the treated unit and its synthetic control group before and after the intervention. Their study examines the causal impact of banning trans fats on cardiovascular diseases and obesity rates by using the ban passed by Denmark in 2003. More specifically, they construct 95% confidence intervals for the trend slopes of mortality rates in Denmark and its synthetic control group before and after the ban. By comparing the change in the slope of actual Denmark and its synthetic counterpart in the pre- vs. post-intervention period. They undertake a triple difference test to determine whether the policy intervention induced the structural break in the mortality trajectory. By embedding a simple Chow test into the synthetic control setup, they reject the null hypothesis on the absence of differential trends with a parametric p-value. Such analysis can potentially uncover the presence or absence of differential trends in the treated unit and its synthetic control group before and after the intervention using the triple difference test. The ability to empirically defend the differential trend assumption most likely lends further credence and support to the validity and plausibility of the synthetic control analysis in estimating the causal impact of the intervention of interest.

5 Conclusion

In this paper we review the Synthetic Control Method, analyse some of its most pressing problems, propose solutions that scholars and practitioners might effectively deploy, and identify opportunities for the application of the method to comparative economic history in order to uncover the long-term effects of historical shocks and institutional changes. The key question concerns the appropriate estimate of the counterfactual scenario that is *ex ante* unobserved to the econometrician, in response to an historical shock or change to provide plausible evidence of the impact.

Our discussion suggests a few discernible and easily tractable guidelines for practitioners in comparative economic history to undertake synthetic control analysis. More specifically, we outline the contours of treatment selection and the necessity of undetectable treatment in the donor pool and the synthetic control group. If the treatment is present in the control group for the treated unit, the application of the synthetic control estimator to the treated unit most likely yields misleading results. By reviewing a couple of established case studies, we show that the unanticipated treatments or interventions of interest tend to have the largest potential to uncover the causal effect of interest provided that the control group is not directly tainted by the same shock. Moreover, we advocate a combined spatial and temporal placebo analysis of the underlying intervention. This implies that to gauge the significance of the effect, the researchers are advised to proceed in two distinctive directions. First, they are advised to assign the intervention of interest to the treated unit into a deliberately false year to gauge whether pre-existing trends affect the counterfactual scenario. And second, they are advised to apply the synthetic control estimator to all untreated units and determine whether the effect appears to be unusually large for the treated unit. Furthermore, we discuss additional considerations pertaining to the ambiguity of the effect, specification search guidelines, and the detection of differential trends between the treated unit and its synthetic control group before and after the intervention.

In essence, synthetic control analysis offers numerous opportunities to tackle the effects of historical shocks by estimating the counterfactual scenario. Such scenarios allow the identification of historical shocks either as gradual changes, temporary changes or permanent changes of the long-run economic equilibrium (Garoupa and Spruk 2019). These scenarios also permit a detailed investigation into the size and significance of the effect. Moreover, the method should not be confined to estimating the long-term growth impact of certain interventions, shocks or institutional changes. For example, in principle, using comparative constitutional data, the method can be applied to discern the impact of specific events, both political and non-political, on *de facto* constitutional protection by going back to the years before the 19th century. By estimating the counterfactual scenario, the differences in the temporary and permanent effect of specific events on constitutional protection can be discussed with the reasonably clear empirical evidence at hand. Many other applications are possible as long as data is available. Armed with an additional battery of placebo tests, when appropriately applied without *ex ante* ideological or other hindsight bias, the synthetic control estimator can reveal insightful evidence on the long-term economic and social consequences of the institutional shocks that advances the state of causal inference both in economics and economic history.

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