Sparse Synthetic Controls

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Abstract. This paper introduces a new penalized synthetic control method for policy evaluation. The proposed sparse synthetic control penalizes the number of predictors used in generating the counterfactual to improve pre-treatment fit and select the most important predictors. To motivate the method theoretically I derive, in a linear factor model framework, a model selection consistency result and a mean squared error convergence rate result. Through a simulation study, I then show that the sparse synthetic control achieves lower bias and has better post-treatment fit than the unpenalized synthetic control. Finally, I apply the method to study the effects of the passage of Proposition 99 in California in a setting with a large number of predictors.

Keywords: synthetic controls, lasso, variable selection, linear factor models, high-dimensional observational studies

[PRELIMINARY DRAFT]

1 Introduction

Synthetic controls have become a popular method for making inferences on causal effects of policy interventions. Athey and Imbens 2017 described the method as "arguably the most important innovation in the policy evaluation literature in the last 15 years". Its applications have ranged from evaluating the effects of tax changes (Abadie et al. 2010, Kleven et al. 2013) to more complex policy changes such as the legalization of prostitution (Cunningham and Shah 2018). Beyond the social sciences synthetic controls have also been used in other applied sciences, for example in engineering and the biological sciences (for example, Pieters et al. 2017), and in the private sector, for instance to evaluate the effect of advertising promotions in large tech companies.

In the classical synthetic control setting (Abadie and Gardeazabal 2003) a single aggregate unit (such as a city or a state) is exposed to a policy treatment at period $T_0$, and $J$ units that are never exposed to the policy (called the donor units) are used to generate a counterfactual. The researcher builds the synthetic control by finding the combination of donor units that best matches the pre-treatment characteristics (called the predictors) of the treated unit. An open question remains on how to choose the set of predictors and how to weight the importance of each predictor in building

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the synthetic control. This paper proposes a penalized method to generate synthetic controls that identifies the most important predictors and weights them to optimize pre-treatment fit.

There are two main reasons why the choice of predictor weights matters for computing synthetic controls. First, as noted by Abadie et al. 2010, Abadie and Vives-i-Bastida 2021, Klosner et al. 2018 and others, the predictor weights are important for the performance of the synthetic controls. Weighting the predictors naively can lead to bad pre-intervention fit and synthetic controls with poor post-intervention performance. This, in turn, means that the estimated treatment effects will be biased. Second, the predictor weights may be of interest in their own right. The researcher may be interested in interpreting them to explain what the synthetic control is doing. For example, in the California tobacco control program application that we will re-visit in Section 5, the researcher may be interested in explaining what predictors (alcohol consumption, income etc) are driving the counterfactual. Hence, the choice of predictors is important for the interpretability of the synthetic control.

The main contribution of the paper is to propose a new penalized method to weight the predictors used in synthetic controls, with desirable theoretical and practical properties. The sparse synthetic control uses a data-driven $l_1$ penalty to induce sparsity in the predictor weights. I show that the method is model consistent in a linear factor model framework with a sparse covariate representation (i.e. it is able to identify the predictors that should have zero weight). In the same setting, I also show that the method has a faster mean squared error and bias rate than the un-penalized (standard) synthetic control. I confirm the theoretical results through a simulation study that highlights that the new method has better pre-treatment fit than the un-penalized synthetic control. Finally, I show that the method is a reliable way to produce robust synthetic controls when using a large number of predictors by applying it to the passage of Proposition 99 in California.

**Related work:** This paper draws from the classic synthetic control literature Abadie and Gardeazabal 2003 and Abadie, Diamond, and Hainmueller (2010, 2015), and the growing literature on extensions to the synthetic control method. Most notably, this paper is relevant for two strains of the literature. First, it complements the literature on penalized methods for synthetic controls (Abadie and L’Hour 2016, Doudchenko and Imbens 2016, Ben-Michael, Feller, and Rothstein 2018, and Arkhangelsky, Athey, Hirshberg, Imbens and Wager 2020) by focusing on penalizing the predictor weights rather than the donor unit weights. In doing so, I show that using a penalized method to choose the predictor weights can improve performance. Second, it complements the literature on how to choose the predictor weights (Klosner, Kaul, Pfeifer and Schieler 2018 and Malo, Eskelinen, Zhou and Kuosmanen 2020) by providing a new methodology and a new model selection result. Overall, this paper addresses a new question not previously considered in the literature which is how to consistently select the predictors used in building the synthetic control and provides a method to do it with good performance properties.
The paper is structured as follows. Section 2 describes the sparse synthetic control method and computation algorithm. Section 3 presents the main theoretical results in a linear factor model. Section 4 explains the simulation study results, and finally Section 5 discusses the empirical application to the California tobacco control program.

2 Sparse Synthetic Controls

To define the sparse synthetic control method, consider a setting in which we observe \( J + 1 \) aggregate units for \( T \) periods. The outcome of interest is denoted by \( Y_{it} \) and only unit 1 is exposed to the intervention during periods \( T_0 + 1, \ldots, T \). We are interested in estimating the treatment effect \( \tau_{1t} = Y_{1t}^I - Y_{1t}^N \) for \( t > T_0 \), where \( Y_{1t}^I \) and \( Y_{1t}^N \) denote the outcomes under the intervention and in absence of the intervention respectively. Since we do not observe \( Y_{1t}^N \) for \( t > T_0 \) we estimate \( \tau_{1t} \) by building a counterfactual \( \hat{Y}_{1t}^N \) of the treated unit’s outcome in absence of the intervention.

As in the standard synthetic control our counterfactual outcome will be given by a weighted average of the donor units’ outcomes, that is \( \hat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt} \) for a set of weights \( w = (w_2, \ldots, w_{J+1})' \). To choose the weight vector \( w \) we use observed characteristics of the units and pre-intervention measures of the outcome of interest. Formally, we let the \( K \times 1 \) design matrix for the treated unit be \( X_1 = (Z_1, \bar{Y}_{K1}^1, \ldots, \bar{Y}_{KM1}^1)' \), where \( \{\bar{Y}_{K1}^1\}_{M} \) represent \( M \) linear combination of the outcome of interest for the pre-intervention period. Similarly, for the donor units, \( X_0 \) is a \( K \times J \) matrix constructed such that its \( j \)th column is given by \( (Z_j, \bar{Y}_{Kj}^1, \ldots, \bar{Y}_{Kj}^{M1})' \). We call the \( K \) rows of the design matrices \( X_0 \) and \( X_1 \) the predictors of the outcome of interest. This can include, for example, lags of the outcome variable and important context dependent characteristics of the aggregate units averaged over the pre-treatment period.

We partition the pre-intervention period into a training set \((X_{0\text{train}}, X_{1\text{train}}, Y_{0\text{train}}, Y_{1\text{train}})\) for \( t \in \{1, \ldots, T_v\} \) and a validation set \((X_{0\text{val}}, X_{1\text{val}}, Y_{0\text{val}}, Y_{1\text{val}})\) for \( t \in \{T_v + 1, \ldots, T_0\} \). This allows for the training and validation design matrices to differ, for example if predictors are averaged over different time periods or different linear combinations of lagged outcome variables are used.

The sparse synthetic control is defined by the tuple of weight vectors \((V^*, w^*)\) computed by solving the following bi-level optimization program:\footnote{I follow the notation of Malo, Eskelinen, Zhou and Kuosmanen 2020, who propose a computational method to solve a similar problem.}

- Upper level problem:
  \[
  (V^*, w^*) \in \arg\min_{V, w} L_V(V, w, \lambda) = \frac{1}{T_{\text{val}}} \|Y_{1\text{val}}^0 - Y_{1\text{val}}^0 w(V)\|^2 + \lambda \|V\|_1,
  \]
  \[\text{s.t. } w(V) \in \psi(V), V \in \mathbb{R}^K_+ .\]

- Lower level problem:
  \[\psi(V) \equiv \arg\min_{w \in \mathcal{W}} L_W(V, w) = \|X_{1\text{train}}^0 - X_{1\text{train}}^{0\text{train}} w\|^2_V,\]
where,

\[ w \in W \equiv \{ w \in \mathbb{R}^J \mid 1^T w = 1, \ w_j \geq 0, \ j = 2, \ldots, J + 1 \}, \]

\[ V \in \mathcal{V} \equiv \{ V \mid V \in \mathbb{R}^{K \times K}, \ \text{trace}(V) = 1, \ V_{kk} \geq 0, \ V_{kl} = 0 \ \text{for} \ k \neq l \}, \]

and \( \| \cdot \|_V \) denotes the semi-norm parametrized by \( V \) such that \( \| A \|_V = (A^T V A)^{1/2} \).

The main idea of the sparse synthetic control is that the \( l_1 \) penalty in the upper level problem induces some predictor weights (the diagonal elements of the weight matrix \( V \)) to be set to zero as the penalty term \( \lambda \) increases. In practice, given the weight restriction \( V \in \mathcal{V} \), we use ex-post weight normalization by initially setting one predictor weight to one (i.e. \( v_{k_0} = 1 \) for some \( k_0 \in \{1, \ldots, K\} \)) and only restricting the \( v_k \) weights to be positive in the upper level program. The following algorithm details the procedure used to choose the hyper-parameter \( \lambda \) and compute the sparse synthetic controls.

**Algorithm 1: Sparse Synthetic Control**

**Result:** \( w^*, V^* \)

**Data:** \((X_{\text{train}}^\text{0}, X_{\text{train}}^\text{1}, Y_{\text{train}}^\text{0}, Y_{\text{train}}^\text{1}), (X_{\text{train}}^\text{0}, X_{\text{train}}^\text{1}, Y_{\text{val}}^\text{0}, Y_{\text{val}}^\text{1})\)

1. set \( v_{k_0} = 1; \)
2. initialize \( v_k \) for \( k \neq k_0 \) to \((X_{\text{train}}^\text{0} X_{\text{train}}^\text{0})^{-1}; \)
3. for each \( \lambda \) in a grid do
   4. get \((V_\lambda, w_\lambda)\) by jointly minimizing \( L_W(V, w, \lambda) \) and \( L_V(V, w) \) for the training data;
   5. s.t. \( w \in W, v_k \geq 0 \ \forall k \neq k_0 \) and \( v_{k_0} = 1; \)
   6. scale \( V_\lambda \) to [0, 1];
   7. get \( w_\lambda^* \) by minimizing \( L_W(V_\lambda, w, \lambda) \) for the training data;
   8. store MSE\((Y_{\text{val}}^\text{1}, Y_{\text{val}}^\text{0} w_\lambda^*)\) and \( V_\lambda; \)
9. end
10. choose \( \lambda^* \) with minimum MSE\((Y_{\text{val}}^\text{1}, Y_{\text{val}}^\text{0} w_{\lambda^*})\);
11. \( V^* = V_{\lambda^*}; \)
12. get \( w^* \) by minimizing \( L_V(V_\lambda^*, w) \) for the shifted training data\(^a\).

\(^a\) The shifted training data is the training data but with time dependent variables shifted to the \( T_v \) periods before \( T_0 \).

### 3 Theoretical Results for a Linear Factor Model

To motivate sparse synthetic controls theoretically, consider a standard setting in which the outcomes in absence of the intervention are given by a linear factor model as in Abadie et al. 2010:

\[ Y_{it}^N = \delta_t + \theta_i Z_i + \lambda_i \mu_i + \epsilon_{it}, \]

where \( \delta_t \) is a common factor with equal loadings, \( Z_i \) is a \((k \times 1)\) vector of observed features, \( \theta_i \) is a \((1 \times k)\) vector of unknown parameters, \( \lambda_i \) is a \((1 \times F)\) vector of unobserved common factors, \( \mu_i \) is
an \((F \times 1)\) vector of unknown factor loadings, and \(\epsilon_{it}\) is a unit-level transitory shock, modeled as \(N(0, \sigma^2)\). Similar models have been used to motivate synthetic control and diff-in-diff estimators (see Arkhangelsky et al. 2020, Ferman and Pinto 2018 or Abadie and Vives-i-Bastida 2021), and the setting and main results could be extended to more general models.

To study under which conditions sparse synthetic controls select the most important predictors, I assume a sparse representation of the predictors, \(\theta_t\) is partitioned conformably into \((\tilde{\theta}_t, 0)\)' where \(\tilde{\theta}_t\) is a \((k_1 \times 1)\) vector of non-zero parameters and \(0\) is a \((k_2 \times 1)\) vector such that \(k = k_1 + k_2\). Similarly, \(Z_i = (Z_i^1, Z_i^2)\), and throughout the paper I will refer to the \(Z_i^1\) predictors as the “useful” predictors and the \(Z_i^2\) predictors as the nuisance predictors.

For the sparse synthetic control to be interpretable we need that given a large set of predictors it successfully recovers the useful ones. That is, we require the method to consistently select the true model in the sparse representation. The main result, Theorem 1, provides conditions under which the sparse synthetic control is model consistent when \(T_0\) is large and \(k_1\) is fixed.

**Theorem 1 (Model Selection).** If \(\psi\) is an injective function and \(\hat{\lambda}_{T_0}\) converges to zero slower than \(1/T_0\)-rate, then for a fixed \(k_1\), as \(T_0 \to \infty\) for all \(m \in \{1, \ldots, k\}\) the following holds \(\theta_{tm} = 0\) for all \(t \in \{1, \ldots, T\} \Rightarrow v^*_m = o_p(1)\), where \(v^*_m\) is the predictor weight for predictor \(m\) assigned by the sparse synthetic control algorithm.

In words Theorem 1 states that the sparse synthetic control will not use the nuisance predictors with high probability as the number of pre-treatment periods increases. It is important to note that the implication does not go the other way. The method could set a useful predictor weight to zero, but it will not assign a nuisance predictor a non-negative weight with high probability. This result is important because it justifies the use of sparse synthetic controls to identify important predictors. A researcher that is unsure about what predictors to use to generate the synthetic control can use the sparse synthetic control with many predictors and be confident that it will use the “useful” predictors. This suggests using the method as an alternative to predictor search by researchers and possibly as a way to prevent specification search.

Next, I show that the sparse synthetic control also has desirable performance properties when compared to the standard (un-penalized) method. In particular, it has a faster convergence rate for bias and mean-squared-error of the treatment effect estimator. To show this, recall that the treatment effect of interest is \(\tau_{1t} = Y_{it}^1 - Y_{N1}^1\) for \(t > T_0\). To estimate it we generate a counterfactual \(\hat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt}^N\) for a synthetic control \(w = (w_2, \ldots, w_{J+1})'\). Therefore, the estimated treatment effect indexed by a synthetic control \(w\) is given by

\[
\hat{\tau}_{1t}^w = \theta_t \left( Z_1 - \sum_{j=2}^{J+1} w_j Z_j \right) + \lambda_t \left( \mu_1 - \sum_{j=2}^{J+1} w_j \mu_j \right) + \sum_{j=2}^{J+1} w_j (\epsilon_{1t} - \epsilon_{jt}).
\]

This assumption is not necessary, we could model the noise as a sub-gaussian random variable and the main results would follow.

Here, \(\hat{\lambda}_{T_0}\) is the cross-validated hyper-parameter of the penalty function denoted by \(\lambda^*\) in Algorithm 1.
It is well known that when the synthetic control does not have perfect pre-treatment fit it is biased. The following lemma gives an upper bound on the bias that depends on two terms that do not vanish asymptotically.

**Lemma 1 (Bias Bound).** Assume that $\epsilon_{i,t}$ are mean independent of $\{Z, \mu\}$, that there exists a $\lambda$ such that $|\lambda_{ft}| \leq \bar{\lambda}$ for all $t$ and $f$, and that the smallest eigenvalue of $\sum_{t=1}^{T_0} \lambda_t^t \lambda_t$ is bounded below by $\xi$. Then, for a synthetic control $w$,

$$\mathbb{E}|\hat{\tau}_w| \leq \frac{\gamma}{T_0} \sum_{m=1}^{T_0} \mathbb{E}|Y_{1m} - \sum_{j=2}^{J+1} w_j Y_{jm}| + \left|\bar{\theta} \left(1 - \frac{\gamma}{T_0}\right)\right| \sum_{k=1}^{k_1} \mathbb{E}|Z_{1k}^1 - \sum_{j=2}^{J+1} w_j Z_{1k}^1| + O\left(T_0^{-1}\right),$$

where $\gamma = \left(\frac{\bar{\lambda}^2 F}{\xi}\right)$ is a constant independent of $T_0$, $\bar{\theta}$ is the maximum value of $\bar{\theta}_t$, $F$ is the number of unobserved common factors and $k_1$ is the number of “useful” predictors.

The bias bound in Lemma 1 provides three insights. First, if we don’t have perfect pre-treatment fit the bias does not vanish asymptotically as $T_0$ increases. This means that our treatment effect estimate will be biased even if we have many pre-treatment periods. Second, the bias depends on the mean absolute deviation of the pre-treatment outcomes. Pre-treatment fit is, therefore, very important for controlling the bias. This leads to the suggestion of not using synthetic controls when the pre-treatment fit is bad. Third, and most relevant for the sparse synthetic controls, the bias depends on the fit of the “useful” predictors $Z_{1i}$ and linearly in $k_1$. Therefore, even if we have a large number of pre-treatment periods and the pre-treatment fit is good (the first term in the bound is small), the bias could be large if the predictor fit is bad. Hence, a synthetic control that minimizes bias should attempt to perfectly match the useful predictors and disregard the nuisance predictors.

To formalize the improvement in terms of bias and mean-square-error of the sparse synthetic control over the standard synthetic control I derive the finite sample rate for $\text{MSE}(Z_0w)$.

**Theorem 2 (MSE Rate).** Let $Z_1 = Z_0w^* + u$, for $u_i \sim_{\text{ind}} \text{subG}(\sigma^2_z)$. Then, for the sparse synthetic control $\hat{w}$,

$$\text{MSE}(Z_0\hat{w}) \lesssim \frac{\sigma_z \sqrt{k_1}}{k} \sqrt{2 \log J}.$$  

For the standard synthetic control $\tilde{w}$,

$$\text{MSE}(Z_0\tilde{w}) \lesssim \sigma_z \sqrt{\frac{2 \log J}{k}}.$$  

Theorem 2 describes how the mean squared error of the predictor match changes with the number of predictors and donor units. The main difference between the sparse synthetic control and the standard method is that the rate is of order $O(\sqrt{k_1}/k)$ instead of $O(1/\sqrt{F})$. This means that in a sparse setting, when $k_1$ is small with respect to $k$, the sparse synthetic control has a faster MSE rate. In practice, this is important because it implies that the sparse synthetic control will achieve lower MSE than the standard synthetic controls when both methods use the same number
of predictors. More so, given Lemma 1, a faster predictor MSE rate also implies that the sparse synthetic control will achieve lower bias than the standard method. In section 4 we explore these properties further by estimating the expected mean absolute deviations empirically and show in a simulation study that the sparse synthetic control achieves both better outcome pre-treatment fit and better "useful" predictor fit than the standard synthetic control.

4 Simulation Study

In this section we study the performance of the sparse synthetic control in relation to two benchmark models: the standard synthetic control with \( \mathbf{V} \) chosen as \((\mathbf{X}_0^{\text{train}}\mathbf{X}_0^{\text{train}})^{-1}\), which we label SCM, and the synthetic control with \( \mathbf{V} \) chosen to minimize \( \frac{1}{T_{\text{val}}} \| \mathbf{Y}^{\text{val}}_1 - \mathbf{Y}^{\text{val}}_0 \mathbf{w}(\mathbf{V}) \|_2^2 \) as proposed by Abadie et al. 2015, which we label SCM with \( \lambda = 0 \) as it can be understood as the unpenalized version of the sparse synthetic control.

The analysis below relies on the following simulation design for \( B = 1000 \) draws:

\[
T = 30, \ T_0 = 20, \ T_v = 10,
\]

\[
\mathbf{Z}_i = [\mathbf{Z}_1^i, \mathbf{Z}_2^i], \text{ where } \mathbf{Z}_1^i, \mathbf{Z}_2^i \sim_{\text{iid}} U[0,1],
\]

\[
\mathbf{Z}_1^i = \frac{1}{2} \mathbf{Z}_2^i + \frac{1}{2} \mathbf{Z}_3^i,
\]

\( \lambda_t \) follows an AR(1) with coefficient \( \rho = 0.5 \),

\[
\epsilon_{it} \sim N(0,\sigma^2) \text{ with } \sigma = 0.25,
\]

\( F = 7 \) in groups of 3 units and \( J + 1 = 21 \).

This simulation design implies that unit 1 (the treated unit) can be perfectly replicated (up to noise) by an average of units 2 and 3. Hence, the optimal synthetic control would choose \( w_2 = w_3 = \frac{1}{2} \). I study two different predictor settings. The first is one in which there are a similar amount of useful and useless predictors \( (k_1 = k_2 = 5) \); the second, includes only one “useful” predictor \( (k_1 = 1 \text{ and } k_2 = 9) \). In both cases I add 10 lags of the outcome variable to the design matrix, for a total of 20 predictors. Note that this is a challenging design for the method as both useful and useless predictors are drawn from the same distribution. I summarize the simulation results in two Figures that show the performance of the different methods and explore the theoretical insights from Section 3.

First, focus on the post-treatment mean squared error (MSE) of the outcome variable. Given that the MSE is an informative measure of fit that includes both bias and variance it gives us an idea of the performance of the estimator. In particular, lower post-treatment MSEs will imply lower standard errors for the treatment effect of interest. Figure 1 shows the distribution of MSEs for the simulations for the two settings. Panels (a) and (b) show that the sparse synthetic control has on average lower MSE and less dispersion than the two benchmark methods. Panels (c) and (d) show the distribution of the gaps between the predicted outcome and the real outcome for all periods.

\[\text{I also set } \delta_t = 100, \text{ but without loss of generality } \delta_t \text{ could be set to zero.}\]
The sparse synthetic control has tighter confidence intervals both before and after treatment and sets unit weights closer to the optimal control (83% of the weight is given to units 2 and 3). Furthermore, observe that whereas the benchmarks methods perform worse in the $k_1 = 1, k_2 = 9$ setting, the sparse synthetic control is able to perform similarly in both settings. This shows the ability of the method to perform well regardless of the number of predictors.

![Graph](image1.png)

(a) $k_1 = 5$.

(b) $k_1 = 1, k_2 = 9$.

![Graph](image2.png)

(c) $k_1 = 5$.

(d) $k_1 = 1, k_2 = 9$.

Fig. 1: Mean Squared Errors.

**Notes:** Panels (a) and (b) show the kernel density across simulations of the MSEs for the outcome variable in the post-treatment period, with average values in parenthesis. Panels (c) and (d) show the inter-quartile range of the $Y_{1t} - \hat{Y}_{1t}$ with $w^*_2 + w^*_3$ in parenthesis. SCM lmb=0 refers to the unpenalized synthetic control with $\lambda = 0$.

Next, recall that in section 3 the bias bound crucially depends on the MAD of the pre-treatment outcomes and useful predictors. Figure 2 shows the EMADs for the outcome and useful predictors in the pre-treatment period. Panels (a) and (b) show that the pre-MADs of the outcome variable are on average lower for the sparse synthetic control. As in Figure 1, the sparse synthetic control is able to perform well in both settings whereas the benchmark methods perform poorly in the
$k_1 = 1, k_2 = 9$ setting. Panels (c) and (d) show that the MADs for the useful predictors are lower for the \textit{sparse} synthetic control than for the benchmark models. Interestingly, for $k_1 = 1, k_2 = 9$ the standard synthetic control has lower average MAD because the distribution has a flatter tail, but as can be seen in the figure the \textit{sparse} synthetic control has more mass closer to zero.

To corroborate the insight from Theorem 2 (Model Selection) I plot the histogram of the predictor weight values ($v_k$) for the useful and useless predictors across simulations. Panels (e) and (f) in Figure 2 compare the SCM with $\lambda^* = 0$ and \textit{sparse} synthetic control for the $k_1 = 1, k_2 = 9$ setting. Whereas the standard SCM does not clearly distinguish between the useful and useless covariates, the \textit{sparse} synthetic control correctly assigns zero weight to the useless covariates most of the time. The stark difference between the two models is evidence that the \textit{sparse} synthetic control is able to successfully perform variable selection.

Finally, I also note that the \textit{penalized} synthetic control is stable across the simulations. By stability I mean that independently of the optimal amount of regularization chosen, the synthetic control weights are similar. This can be seen in Figure 3, where I plot the share of the $V$ and $w$ weights that are zero for the different values of $\lambda^*$. The main takeaway is that the unit weights $w^*$ have the same amount of sparsity regardless of the magnitude of the optimal regularization. This is evidence to motivate the technical assumption that $\psi$ is injective in the proof of Theorem 2, and confirms that the method can reliably achieve a unique optimum. Note that we do see some instability for large values of $\lambda^*$ in cases in which only one predictor is used.

5 The California Smoking Program

In 1988 proposition 99 increased California’s cigarette excise tax by 25 cents per pack and shifted public policy towards a clean air agenda. This policy intervention has been used extensively to compare the validity and performance of various synthetic controls and diff-in-diff estimators. The outcome of interest is cigarette sales per capita in packs in California and the donor pool includes 38 states without similar policy interventions. The original data set used in the Abadie et al. 2010 study used eight predictors: Ln(GDP per capita), percent aged 15–24, retail price, beer consumption per capita and three lags of cigarette sales per capita. In this study the standard synthetic control built using this design matrix falls outside the convex hull of the predictors, but has very good pre-treatment fit.

To study the potential benefits of using the \textit{sparse} synthetic control, I augment the original dataset with 50 extra predictors. These are obtained from the IPPSR (MSU) dataset on policy correlates and include demographic variables, income related variables, political participation measures and government spending statistics. I then compare the SCM with $\lambda^* = 0$, the standard Diff-in-Diff, the Synthetic Diff-in-Diff (SDID) proposed by Arkhangelsky et al. 2020 and the \textit{penalized} synthetic control. To choose the optimal predictor weights, I divide the pre-treatment period in a training period (14 years) and a validation period (5 years). Figure 4 shows the \textit{sparse} synthetic control for the augmented data setting. It can be seen that the synthetic control achieves relatively good pre-treatment fit in both the training and validation periods.
Fig. 2: Pre-treatment Fit and Variable Selection.

Notes: Panels (a) - (b) show the kernel density across simulations of MADs of the outcome variable for the pre-treatment period, with average values in parenthesis. Panels (c) - (d) show the kernel density across simulations of MADs of the useful predictors, with average values in parenthesis. Panels (e) and (f) show the histogram for each group of predictors for the $k_1 = k_2 = 5$ setting. SCM lmb=0 refers to the unpenalized synthetic control with $\lambda = 0$. 

(a) $k_1 = k_2 = 5$. 

(b) $k_1 = 1$, $k_2 = 9$. 

(c) $k_1 = k_2 = 5$. 

(d) $k_1 = 1$, $k_2 = 9$. 

(e) SCM $\lambda^* = 0 V^*$ 

(f) Sparse SCM $V^*$
Fig. 3: Sparsity of \( \mathbf{w}^* \) and \( \mathbf{V}^* \).

Table 1: Treatment effect estimates.

|                | DID   | SCM   | SDID  | Sparse SCM | SCM + | Sparse SCM + |
|----------------|-------|-------|-------|------------|-------|--------------|
| \( \hat{\tau} \) estimate | -27.4 | -19.8 | -13.4 | -18.8      | -22.9 | -17.6        |
| s.e.           | (16.4) | (7.7) | (7.6) | ( . )      | ( . ) | ( . )        |

Notes: DID is the standard diff-in-diff estimator, SCM is the standard synthetic control with \( \mathbf{V} \) chosen to minimize \( \frac{1}{t_{\text{val}}} \| \mathbf{Y}_{\text{val}}^\text{tmt} - \mathbf{Y}_{\text{val}}^\text{t0} \mathbf{w}(\mathbf{V}) \|^2 \) without penalization (\( \lambda^* = 0 \)), SDID is the synthetic diff-in-diff estimator, and the ’+’ indicates the augmented data setting. Standard errors are taken from Arkhangelsky et al. 2020.
Table 1 shows the treatment effects for each method. The standard DID estimator is negatively biased because the parallel trends assumption is not satisfied. Therefore, we can use it as a lower bound on the bias of our treatment effects. For the non-augmented data setting observe that the standard SCM and the sparse SCM are very similar. This is because the standard synthetic control already has almost perfect pre-treatment fit and good covariate fit, so the sparse SCM does not have much room to improve on it. The SDID estimate is significantly smaller (in absolute value) as it uses a different pre-treatment assumption for valid inference as discussed in Arkhangelsky et al. 2020.

On the other hand, in the augmented data setting (labelled with a ′+′), due to the increased number of covariates the standard SCM has worse pre-treatment fit and appears to be slightly biased. Recall that the bias bound (Theorem 1) tells us that the bias is linearly increasing in the predictor fit, which explains why the standard SCM is likely to be biased in this setting. The sparse SCM, however, penalizes the predictors and is able to improve pre-treatment fit. This leads to an estimated treatment effect that is closer to the one in the original study. Additionally, the sparse SCM is able to identify the useful predictors. Besides using the predictors of the original study, the sparse synthetic control also uses some additional income and demographic variables. In particular, it uses household debt and savings measures, household demographics (such as number of households with children), quality of education measures and other drug consumption measures. It does not use however 70% of the predictors, in particular setting to zero geographic variables and other statistics that have little to do with consumer behavior.

The main two takeaways from the empirical application are (1) that the standard SCM can be biased when we use a large number of predictors and (2) that the sparse synthetic control is a potential solution to this problem. Furthermore, it suggests that researchers that have many predictors at their disposal can avoid having to search manually for the best predictors to use as the sparse synthetic control method will automatically select the useful ones.

6 Conclusion

Researchers and policy makers are increasingly drawn towards synthetic control methods to analyze policy interventions. A key step in using these methods is deciding what predictors to use in building the synthetic control. Using a large number of predictors is not a valid alternative as it can lead to biased treatment effects and poor post-treatment performance. In this paper, I advocate for a data-driven penalized method, the sparse synthetic control, that automatically chooses the important predictors. I suggest that this method can be used as an alternative to the standard synthetic control when the researcher has many predictors at its disposal but does not know which ones should be used.

I show that, in a linear factor model setting, the sparse synthetic control is model consistent and can successfully recover which predictors are useless when the number of pre-treatment periods is

5 Missing standard errors will be computed in a future iteration of the paper.
large. Furthermore, by deriving an MSE convergence rate result and a bias bound result, I show that the sparse synthetic control has better theoretical performance properties than the unpenalized method. Motivated by this insight I then show through a simulation study that the sparse synthetic control is able to reduce both bias and MSE measures with respect to the unpenalized synthetic control.

Finally, I show the practical relevance of the method by applying it to the passage of Proposition 99 in California in a setting with 8 predictors versus a setting with 58 predictors. Whereas the standard synthetic control estimate becomes more biased when the number of predictors is increased, the sparse synthetic control is robust to the predictor increase. A natural next step for future work is to explore in which settings the sparse synthetic control can also improve the standard errors of the treatment effect estimates.

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Appendix

6.1 Proof of Lemma 1

Consider the linear factor model described in Section 1. Our counterfactual will be a weighted average of the outcome variable for the donor pool

\[
\sum_{j=2}^{J+1} w_j Y_{1t}^N = \theta_t + \theta_t \sum_{j=2}^{J+1} w_j Z_j + \lambda_t \sum_{j=2}^{J+1} w_j \mu_j + \sum_{j=2}^{J+1} w_j \epsilon_{jt}.
\]

As a result, the treatment effect \( \tau_{1t}^w = Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{j1}^N \) for some weight vector \( w = \{w_j\}_{j=2}^{J+1} \) takes the form

\[
\tau_{1t}^w = \theta_t \left( Z_1 - \sum_{j=2}^{J+1} w_j Z_j \right) + \lambda_t \left( \mu_1 - \sum_{j=2}^{J+1} w_j \mu_j \right) + \sum_{j=2}^{J+1} w_j (\epsilon_{1t} - \epsilon_{jt}).
\]

Under some additional definitions and mean independence of the error term we have that:

\[
\tau_{1t}^w = \lambda_t (\lambda^P \lambda^P)_{-1} \lambda^P \left( Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{j1}^P \right) + \left( \theta_t - \lambda_t (\lambda^P \lambda^P)_{-1} \lambda^P \theta_t \right) \left( Z_1 - \sum_{j=2}^{J+1} w_j Z_j \right) - \lambda_t (\lambda^P \lambda^P)_{-1} \lambda^P \left( \epsilon_{1t} - \sum_{j=2}^{J+1} w_j \epsilon_j \right) + \sum_{j=2}^{J+1} w_j (\epsilon_{1t} - \epsilon_{jt}).
\]

In ADH 2010, a bias bound is derived in the case in which we perfectly replicate the treated unit and the first two terms are zero. They show that the second to terms are \( O \left( \frac{1}{T_0} \right) \). This paper explores the case in which we have many covariates and therefore are likely to fall outside the convex hull of the donor pool. In such cases, the synthetic control will not be able to replicate the design matrix of the treated unit and the pre-treatment fit will not be perfect.

Assume that \( \sum_{t=1}^{T_0} \lambda_t' \lambda_t \) is positive semi-definite with smallest eigenvalue bounded away from zero by \( \xi \) and \( |\lambda_{1f}| < \bar{\lambda}, |\theta_{1f}| < \bar{\theta} \) for all \( t, f \). Then, it can be shown by the C-S inequality that:

\[
\left( \lambda_t \left( \sum_{s=1}^{T_0} \lambda_s' \lambda_t \right)^{-1} \lambda_t' \right)^2 \leq \left( \frac{\lambda^2 \lambda^P}{T_0 \xi} \right)^2.
\]

Then, consider the first term in the decomposition of the treatment effect,

\[
\lambda_t (\lambda^P \lambda^P)_{-1} \lambda^P \left( Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{j1}^P \right) = \sum_{m=1}^{T_0} \lambda_t \left( \sum_{s=1}^{T_0} \lambda_s' \lambda_t \right)^{-1} \lambda_m \left( Y_{1m} - \sum_{j=2}^{J+1} w_j Y_{jm} \right) \leq \sum_{m=1}^{T_0} \lambda_t \left( \sum_{s=1}^{T_0} \lambda_s' \lambda_t \right)^{-1} \lambda_m \left| Y_{1m} - \sum_{j=2}^{J+1} w_j Y_{jm} \right| \leq \left( \frac{\lambda^2 \lambda^P}{T_0 \xi} \right) \sum_{m=1}^{T_0} Y_{1m} - \sum_{j=2}^{J+1} w_j Y_{jm} \leq \left( \frac{\lambda^2 \lambda^P}{\xi} \right) \text{MAD} \left( Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{j1}^P \right).
\]

Therefore, the bias contribution from the first term \( R_{1t} \) is bounded by:
Let \( T \) where \( \bar{\theta} \)

Rewriting the objective function with our model assumptions:

\[
\epsilon \text{ with } \psi
\]

We require that

\[
6.2 \text{ Proof of Theorem 1}
\]

\[
\text{optimization problem. Denote this solution } X \text{ positive definite so that } X
\]

\[
\text{must be zero. Hence, if } \theta_{ik} = 0 \text{ for all } t \text{ and } \lambda \text{ does not go to a zero at a faster rate than } T_0 \text{ then } v_k^* = 0.
\]

The bias contribution from the second term \((R_{2i})\) is then given by:

\[
\mathbb{E}|R_{2i}| \leq K \left| \hat{\theta} \left( 1 - \frac{\hat{X}^2 \lambda}{T_0 \xi} \right) \right| \mathbb{E}\text{MAD} \left( Z_1, \sum_{j=2}^{J+1} w_j Z_j \right).
\]

Given that the other two terms are \( O \left( \frac{1}{T} \right) \) the result follows.

\[6.2 \text{ Proof of Theorem 1}\]

We require that \( \psi : V \to W \) is an injective function. This is the case when every subset of \( k \) columns of \( X_0 \) spans \( \mathbb{R}^k \) (that is, the columns in every subset are linearly independent) and when \( V \) is diagonal and positive definite so that \( X_0' V X_0 \) is non-singular. Then, we can find a unique solution to the lower level optimization problem. Denote this solution \( w^*(V) \).

Suppose for simplicity that

\[ Y_{ki}^N = \theta_k Z_i + \epsilon_{ki}, \]

with \( \epsilon \sim N(0, \sigma^2) \). The proof can be extended to the full linear factor model.

Consider the upper level problem that involves solving the following penalized program:

\[
\min_V \frac{1}{T_{\text{val}}} \| Y_{\text{val}}^l - Y_{\text{val}}^0 w^*(V) \|^2 + \lambda \sum_{k=1}^{K} v_k.
\]

Rewriting the objective function with our model assumptions:

\[
\frac{1}{T_0} \sum_{t=1}^{T_0} \left( \theta_t \left( Z_1 - \sum_{j=2}^{J+1} w_j Z_j \right) + \epsilon_{it} - \sum_{j=2}^{J+1} w_j \epsilon_{jt} \right)^2 + \lambda \sum_{k=1}^{K} v_k.
\]

Let \( T_0 \to \infty \), ADH 2010 show that the error part is \( o_p(1) \). So we consider the following condition for \( v_k \):

\[
\tilde{\theta}_k^2 \left( Z_{1k} - \sum_{j=2}^{J+1} w_j Z_{jk} \right) - \sum_{j=2}^{J+1} \partial_j w_j \partial_k Z_{jk} \right) + o_p(1) \leq \lambda,
\]

where \( \tilde{\theta}_k^2 \) is the maximum value of \( \theta_{ik}^2 \) for all \( t \). If this condition holds then by KKT the optimal value of \( v_k^* \) must be zero. Hence, if \( \theta_{ik} = 0 \) for all \( t \) and \( \lambda \) does not go to a zero at a faster rate than \( T_0 \) then \( v_k^* = 0 \).
6.3 Proof of Theorem 2: MSE Rates

First, we focus on the covariate matching problem with subGaussian noise:

\[ Z_1 = Z_0 w^* + \epsilon, \quad \epsilon \sim \text{ind subG}(\sigma_z^2). \]

Assume a sparse representation where only \( k_1 \) predictors are non-zero. The model selection result suggests rewriting the problem as:

\[
\min_{V,W} L_V(V,W) = \frac{1}{T_{\text{val}}} \| Y_{\text{val}} \|^2 - Y_{\text{val}}^0 W(V) \| V, \\
\text{s.t.} \quad W(V) \in \psi(V), \\
\quad V \in \mathcal{V},
\]

where \( \psi: \mathcal{V} \mapsto W \) maps the upper level solutions to the lower level optima

\[
\psi(V) \equiv \arg\min_{W \in \mathcal{W}} L_W(V,W) = \| X_{\text{train}}^1 - X_{\text{train}}^0 \|^2, \\
\quad W \equiv \{ W \in \mathbb{R}^J \mid \mathbf{1}'W = 1, \ W_j \geq 0, \ j = 2, \ldots, J + 1 \} \equiv \Delta^J, \\
\quad V \equiv \mathcal{B}_0(k_1) \cup \mathbb{R}^k_{\geq 0}.
\]

First, consider the lower level program of matching the covariates. For simplicity, restrict design matrix to the covariates, without including transformations of the outcome variable. Furthermore, let \( Z \) be bounded such that \( \max_j \| Z_j \| \leq \sqrt{k} \). Denote the minimizer of the lower level program by \( \hat{w} \),

\[
\| Z_1 - Z_0 \hat{w} \|^2 \leq \| Z_1 - Z_0 w^* \|^2. \\
\| Z_0 w^* - Z_0 \hat{w} \|^2 \leq 4(V^T \epsilon, \frac{Z_0 w^* - Z_0 \hat{w}}{\| Z_0 w^* - Z_0 \hat{w} \|^2}) \\
\quad \leq \sup_{b \in X_0 \Delta^J, \| b \|=1} 4(V^T \epsilon, b)^2
\]

Given our assumptions it can be shown that \( Z_1^T V^T \epsilon \sim \text{subG}(k_1 \sigma_z^2) \). Therefore, using a maximal inequality it follows that

\[
MSE(Z_0 \hat{w}) = \frac{1}{k} E \max_b (V^T \epsilon, b) \lesssim \frac{\sigma_z \sqrt{k_1}}{k} \sqrt{2 \log J}.
\]

The rate for the standard synthetic control can be derived in a similar way.