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by Giovanni Mellace and Alessandra Pasquini

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MEDIATION ANALYSIS SYNTHETIC CONTROL

by Giovanni Mellace*, Alessandra Pasquini**

Abstract

The synthetic control method (SCM) makes it possible to estimate the causal effect of an intervention in settings where panel data on a small number of treated and control units are available. We show that the existing SCM, as well as its extensions, can easily be modified to estimate how much of the ‘total’ effect goes through observed causal channels. Our new mediation analysis synthetic control (MASC) method requires additional assumptions that are arguably mild in many settings. To illustrate the implementation of the MASC method, we apply it to identify the direct and indirect effects of an anti-smoking policy (California's Proposition 99).

JEL Classification: C21, C23, C31, C33.

Keywords: synthetic control method, mediation analysis, causal mechanisms, direct and indirect effects.

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Contents

1. Introduction	5
2. The Mediation Analysis Synthetic Control Method.....	8
3. A user-friendly empirical example.....	18
4. Conclusions	27
Appendix	28
Appendix A: Derivation of “Synthetic” $Y_{1t}^{0M_1}$	28
Appendix B: Identification of $\delta_{it}(1)$	37
Appendix C: Extra assumptions on the mediator needed for $Y_{1t}^{1M_0}$	39
Appendix D: Derivation of “Synthetic” $Y_{1t}^{1M_0}$	40
Appendix E: Data Sources and Donor Pool	43
Appendix F: Mediator Fits	45
Appendix G: Spillover Effects Testing and Leave-one-out Robustness Checks	46
References	47

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1 Introduction

The Synthetic Control Method (SCM), introduced by Abadie and Gardeazabal (2003) and further developed in Abadie et al. (2010, 2015), is becoming very popular in program evaluation. SCM is attractive, as it allows estimating the causal effect of an intervention even when data on only one treated and a few control units are available. This is possible by using information on the pre-intervention period to construct a “synthetic control”, which mimics what would have happened to the treated unit in the post-intervention period in the absence of the intervention. Gobillon and Magnac (2016) compare SCM to other interactive fixed-effects models and find that it performs very well as soon as in post-intervention periods, the counterfactual outcome of the treated unit lies in the convex hull of the outcomes of the control units. In a recent paper, Xu (2017) further exploits the connection between SCM and interactive fixed-effect models and proposes a new method that combines both. Similarly, Arkhangelsky et al. (2019) combine the SCM with another panel data method, i.e., the differences-in-differences approach, introducing the Synthetic Difference In Differences (SDID). Doudchenko and Imbens (2016) propose a modification of SCM where the weights are not constrained to be positive and do not necessarily add up to one. Ben-Michael et al. (2021) further relax weight constraints and correct for possible covariates imbalances, demonstrating that the synthetic control method can be seen as an inverse propensity score-weighting estimator. Gunsilius (2021) proposes a modification of SCM that allows to estimate the entire counterfactual distribution. Amjad et al. (2018) introduce the Robust Synthetic Control (RSC), which allows to deal with missing data and de-noises the outcome under a different algorithm. Amjad et al. (2019) propose a modification of the RSC which allows for the presence of covariates. Finally, Athey et al. (2017) propose a new method that includes synthetic control and other panel data methods as a special case.

Although all of those methods are very well-suited for estimating the “total” effect of an intervention, they are mostly uninformative about the causal mechanisms that gen-

We have benefited from comments by Simone De Angelis and participants at several seminars, workshops, and conferences.

erated this effect. Often an intervention may have an effect on intermediate variables, which affects the final outcome. It may be interesting, for research purposes, to decompose the total effect into an indirect effect, generated through one of the intermediate variables (hereafter also referred to as the “mediator”), and a residual effect (generally named “direct effect” in the literature), generated either through the rest of the intermediate variables or directly. Sometimes, ignoring the presence of such intermediate variables might weaken the ability to draw a conclusions on the consequences of an intervention. For example, if the direct and indirect effects are both large and similar in magnitude but with opposite signs, merely looking at the total effect may fail to provide a complete picture of its consequences. Consider the consequences of energy crises. The phenomenon increases inflation, both directly and indirectly (i.e., through an increase of production prices). Nonetheless, it may foster innovation in less energy-consuming technologies, thus reducing production prices and inflation. Moreover, quantifying the direct and indirect effects is often important to better target an intervention. Consider the huge decrease in tobacco consumption after the introduction of California’s anti-tobacco law, Proposition 99, estimated in Abadie et al. (2010). Proposition 99 not only increased the tobacco price but also introduced several anti-tobacco informational campaigns. It would be extremely relevant for a policy-maker to know how much of the decrease in tobacco consumption triggered by Proposition 99 is due to a price increase and investment in informational campaigns respectively.

Mediation analysis is a standard approach to dealing with these kind of issues, i.e., once the mediator is chosen according to researchers’ pursuits, it allows to disentangle the direct and the indirect effects. The main challenge in mediation analysis is that the identification of the direct and indirect effects requires knowledge about the potential outcome an individual would get if the potential mediator was set to the value it would have taken under the opposite treatment status than the one observed. This is never observed for any individual. A large part of the literature focuses on identification and estimation of direct and indirect effects under sequential conditional independence (see Pearl 2001; Robins 2003; Imai et al. 2010; Imai and Yamamoto 2013; Vansteelandt and

VanderWeele 2012; Huber 2014; Vansteelandt and VanderWeele 2012; Huber et al. 2016, 2017). The idea behind this approach is that once we check for observed characteristics and the conditionally independent treatment, the potential outcomes are independent of the potential mediators. To the best of our knowledge, none of the existing methods are specifically designed for panel data and cannot directly be applied in settings with one or a few treated units and a few control units.

This motivates the introduction of our mediation analysis synthetic control (MASC) method, a generalization of the SCM that allows decomposing the total effect of an intervention into its direct and indirect components. Given that SCM already requires all unobserved confounders either to be time invariant or to affect treated units and control units identically, the only additional assumption required by MASC is that we are able to find control units with mediator values similar to those of the treated unit after treatment. This is an advantage with respect to the cross-sectional framework, where the additional assumptions required to identify the direct and indirect effects are usually much stronger than the one needed to identify the total effect. Moreover, the plausibility of the assumptions required for MASC can be judged by checking the overlap in pre-treatment outcomes and in covariate values similarly to a standard SCM.

MASC can be easily implemented by using existing SCM algorithms and any of the aforementioned new extensions (with the exception of Arkhangelsky et al. 2019). Indeed, as we will discuss in more detail below, to identify the direct and indirect effects, MASC re-weights control unit post-intervention outcomes by choosing weights that minimize the distance between treated and synthetic unit in pre-intervention observable characteristics (including pre-intervention values of the outcome and the mediator) as well as in post-intervention values of the mediator. Intuitively, this allows us to mimic what would have happened to the treated unit in absence of the intervention if the mediator value were set to the potential mediator under treatment. As we mentioned above, this is the main challenge of mediation analysis. Following Abadie et al. (2010), we illustrate MASC with a simple dynamic factor model with interactive fixed effects and show that both the direct

One exception is Deuchert et al. (2018). However, they consider a framework with a randomized intervention with non-perfect compliance.

and the indirect effect estimators are unbiased as the number of pre-intervention periods goes to infinity.

The rest of the paper is organized as follows: Section 2 introduces MASC; Section 2.3 proposes possible inference procedures; Section 3 includes an empirical application to the introduction of Proposition 99; and Section 4 concludes. All the technical proofs are relegated to the online appendix.

2 The Mediation Analysis Synthetic Control Method

2.1 Notation and theoretical framework

Let us assume that we are interested in the effect of an intervention, D , implemented at time T , on an outcome, Y . Suppose that part of the effect of D on Y goes through an observed intermediate outcome (mediator), M . The total effect of the intervention on the final outcome can be decomposed into an indirect effect, which goes through M , and a residual effect, commonly named “direct effect” in the mediation analysis literature, which could also go through other causal pathways. Although often crucial for policy conclusions, identification of the direct and the indirect effects may be challenging. To see this, let D_{it} be a binary indicator that is equal to one if unit i is exposed to the intervention at time t . We will refer to units that are exposed to the intervention as “treated” and to those that are not exposed as “control”. Using the potential outcome framework (see, e.g., Rubin 1974) for each unit, i , we can define the potential mediator at time t as follows:

$$M_{it}(d) \text{ for } d \in \{0, 1\}.$$

$M_{it}(d)$ is the value that the mediator of unit i would take, at time t , if D_{it} is set to d . Assuming that there are no anticipation effects on the mediator in the pre-intervention period and that the standard stable unit treatment value assumption (SUTVA) holds, the

observed and the potential mediators are related through the following observation rule:

$$M_{it} = M_{it}(0)(1 - D_{it}) + M_{it}(1)D_{it}.$$

Note that M_{it} is always equal to $M_{it}(0)$ for both treated unit and control units in the pre-intervention period, $t < T$, and that we can observe only one of the two potential mediators for each unit in the post-intervention period, $t \geq T$.

Similarly, for each unit i at time t , we define the potential outcomes as

$$Y_{it}(d, M_{it}(d')) \equiv Y_{it}^{d, M_{d'}} \text{ for } d, d' \in \{0, 1\}.$$

$Y_{it}^{d, M_{d'}}$ is the value that the outcome of unit i would take at time t if we set $D_{it} = d$ and $M_{it} = M_{it}(d')$. The potential outcome is a function of both the treatment and the potential mediator. We assume that the potential mediator is only a function of the treatment and unaffected by future values of the outcomes. Under SUTVA, and assuming no anticipation effects in the pre-intervention period, the observed and the potential outcomes are related by the following observation rule:

$$Y_{it} = Y_{it}^{0, M_0}(1 - D_{it}) + Y_{it}^{1, M_1}D_{it}.$$

Differently from the standard setting, each unit has four instead of two potential outcomes. As usual, only one potential outcome between Y_{it}^{0, M_0} and Y_{it}^{1, M_1} can be observed per unit in each period, while Y_{it}^{0, M_1} and Y_{it}^{1, M_0} are never observed for any unit in any period. Assuming no anticipation effects, $Y_{it} = Y_{it}^{0, M_0}$ in the pre-intervention period for all units.

Following the synthetic control literature, we will define our parameters of interest with respect to a single treated unit. This contrasts with the standard mediation analysis literature, in which the total, direct, and indirect effects are defined as averages, either with respect to the whole sample (Pearl 2001; Robins 2003; Imai et al. 2010; Imai and Yamamoto 2013; Vansteelandt and VanderWeele 2012; Huber 2014) or with respect to

We do allow the potential mediator to depend on past values of the outcome.

the treated units (Vansteelandt and VanderWeele 2012; Huber et al. 2017). Nonetheless, if more than one unit is exposed to the intervention (see Gobillon and Magnac 2016; Adhikari 2015) our method can be easily used to decompose the average treatment effect on the treated.

We assume that we observe J units ordered such that units 1 through n are treated, while units $n+1$ through J are controls. Without loss of generality, we will present our results for the first treated unit (unit 1) only. Since we have four potential outcomes instead of two, we can now define more parameters than in the standard synthetic control framework, each measuring the effect implied by a different thought experiment. Indeed, each potential outcome represents a different state of the world, and one can in principle define effects by calculating the difference between a pair of potential outcomes. Intuitively, Y_{1t}^{0,M_0} and Y_{1t}^{1,M_1} measure the value that the outcome of the first treated unit would take with and without intervention. On the other hand, Y_{1t}^{0,M_1} and Y_{1t}^{1,M_0} measure the values that the outcome of the treated unit would take if the value of the mediator were pushed to the value it would take under the opposite treatment status. Intuitively, given that policy-makers typically cannot choose which value the mediator takes under treatment, Y_{1t}^{0,M_1} is arguably a more interesting counterfactual than Y_{1t}^{1,M_0} . Indeed, it is often easier to push M to $M(1)$ in the absence of the intervention, e.g., by implementing alternative policies that target the mediator directly, than M to $M(0)$ with the intervention, as this would require “neutralizing” the effect of the intervention on the mediator and therefore implementing additional policies that have the opposite effects on the mediator. For this reason and since constructing a synthetic Y_{1t}^{1,M_0} requires stronger assumptions, as well as the ability to observe more than one treated unit, we will only focus on parameters that require knowledge of Y_{1t}^{0,M_1} and relegate to Appendix B our identification results for parameters that include Y_{1t}^{1,M_0} .

The effects of interest with regard to unit 1 are the total effect, α_{1t} , which compares the outcomes the treated unit would get with and without the intervention; the direct effect, $\theta_{1t}(M_{1t}(1))$, which compares the treated unit potential outcome *with* the intervention and the outcome *without* intervention when the mediator is set to the value it would have

taken with the intervention; and the indirect effect, $\delta_{1t}(0)$, which measures the effect of pushing the mediator to its level under the intervention but without implementing the intervention. All parameters are assumed to be zero in the pre-intervention period. In the post-intervention period they are defined as

$$\begin{aligned}\alpha_{1t} &= Y_{1t}^{1,M_1} - Y_{1t}^{0,M_0}, \\ \theta_{1t}(M_{1t}(1)) &= Y_{1t}^{1,M_1} - Y_{1t}^{0,M_1}, \\ \delta_{1t}(0) &= Y_{1t}^{0,M_1} - Y_{1t}^{0,M_0}, \quad t \geq T.\end{aligned}$$

It is easy to see that the total effect, α_{1t} , can be decomposed into:

$$\begin{aligned}\alpha_{1t} &= Y_{1t}^{1,M_1} - Y_{1t}^{0,M_0}, \\ &= Y_{1t}^{1,M_1} - Y_{1t}^{0,M_1} + Y_{1t}^{0,M_1} - Y_{1t}^{0,M_0}, \\ &= \theta_{1t}(M_{1t}(1)) + \delta(0).\end{aligned}$$

The decomposition above shows that if α_{1t} is identified, identifying $\theta_{1t}(M_{1t}(1))$ automatically implies identification of $\delta_{it}(0) = \alpha_{1t} - \theta_{1t}(M_{1t}(1))$.

To identify the total effect, SCM takes advantage of the panel structure and uses the pretreatment period to create a ‘‘synthetic control’’ unit that is similar to the treated unit both in terms of observables and in terms of unobservables. The latter need to be somehow stable between the pre- and post-intervention period. For example, the factor model described in Abadie et al. (2010) implicitly assumes that all the unobserved confounders are either time invariant or that they change in the same way for treated unit

In the mediation literature the following alternative decomposition is often also considered:

$$\begin{aligned}\alpha_{1t} &= Y_{1t}^{1,M_1} - Y_{1t}^{0,M_0}, \\ &= Y_{1t}^{1,M_1} - Y_{1t}^{1,M_0} + Y_{1t}^{1,M_0} - Y_{1t}^{0,M_0}, \\ &= \delta_{1t}(1) + \theta_{1t}(M_{1t}(0)),\end{aligned}$$

where $\delta_{1t}(1) = Y_{1t}^{1,M_1} - Y_{1t}^{1,M_0}$ and $\theta_{1t}(M_{1t}(0)) = Y_{1t}^{1,M_0} - Y_{1t}^{0,M_0}$. We decided not to focus on this decomposition for two reasons. First, as we argue above, a policy-maker would need to be able to neutralize the effect of the treatment on the mediator to reproduce Y_{1t}^{1,M_0} . Second, identification of Y_{1t}^{1,M_0} requires additional assumptions and the ability to observe multiple treated units.

and control units. The synthetic control mimics what would have happened to the treated unit in the post-intervention period in the absence of the intervention. In other words, SCM creates a synthetic value of Y_{1t}^{0,M_0} in the post-intervention period. The synthetic control is built by using a linear combination of the control units. Under the assumption that all unobservables are either time invariant or affect the outcome of treated unit and control units in the same way, the synthetic control can be built by a weighted average of the post-treatment outcomes of the control units using weights that are chosen to minimize the distance between the pre-intervention observable characteristics (including pre-intervention outcomes) of the treated unit and synthetic units (see Abadie et al. 2010). Notice that for the SCM to work Y_{1t}^{0,M_0} has to lie in (be close to) the convex hull of the non-treated units post-intervention outcomes.

Most of the methods used in mediation analysis rely on the so called sequential conditional independence assumption (SCIA). Under this assumption, the total effect, direct, and indirect effects can be recovered using control units and controlling for observable characteristics. When there is a single treated unit, however, we cannot invoke the SCIA. To overcome this issue, our MASC uses the SCM idea to create a “synthetic” value of Y_{1t}^{0,M_1} in the post-intervention period. To reconstruct Y_{1t}^{0,M_1} , we propose to use a linear combination of the control unit post-intervention outcomes, choosing weights that minimize the distance between treated unit and control units pre-intervention observable characteristics as well as post-intervention values of the mediator. The intuition is that choosing the weights that minimize the distance between the treated unit and synthetic control with respect to post-treatment values of the mediator as well will mimic what would have happened to the treated unit in the absence of the intervention but fixing the mediator value to its value in presence of the intervention. The additional assumption required by MASC is that Y_{1t}^{0,M_1} has to lie in (be close to) the convex hull of the non-treated units post-intervention outcomes. This is in contrast to the cross-section mediation analysis literature, in which the additional assumptions required to decompose the total effect are far stronger than the one required to identify it.

To better illustrate our approach, in the spirit of Abadie et al. (2010), we will introduce

a factor model and show that, under this model, the bias is bounded by a measure which goes to zero as the number of pre-intervention periods goes to infinity. In our factor model, the potential mediators of unit i are given by

$$M_{it}(d) = \gamma_t + \beta_t Z_i + \vartheta_t \varrho_i + \psi_t d + \nu_{it},$$

where γ_t is an unknown common factor with constant factor loadings across units. Z_i is a $(p \times 1)$ vector of observed covariates; β_t is a $(1 \times p)$ vector of unknown parameters; ϑ_t is a $(1 \times v)$ vector of unobserved common factors; ϱ_i is a $(v \times 1)$ vector of unknown factor loadings; ψ_{it} is an unknown parameter describing the impact of the treatment on the mediator; and ν_{it} are unobserved transitory shocks. Notice that our model does not impose any extra restrictions on the unobservables that are only allowed to change over time in the same way for all units. It is easy to see that if this assumption is violated for the mediator, SCM would not be able to recover Y_{1t}^{0, M_0} , as M would be a post-treatment time-varying confounder, i.e., the assumptions of a standard SCM would also be violated.

Similarly, we assume that the four potential outcomes are given by

$$Y_{it}^{d, M_{d'}} = \zeta_t + \eta_t X_i + \lambda_t \mu_i + \varphi_t(1) M_{it}(d') + \rho_t(M_{it}(d')) \cdot d + \epsilon_{it}$$

where ζ_t is an unknown common factor with constant factor loadings across units; X_i is an $(r \times 1)$ vector of observed covariates that includes all the variables included in Z_i , but might also include other observable variables, which affects the treatment and the outcome but not the mediator; η_t is a $(1 \times r)$ vector of unknown parameters; λ_t is a $(1 \times F)$ vector of unobserved common factors; μ_i is an $(F \times 1)$ vector of unknown factor loadings; ϵ_{it} are unobserved transitory shocks; and $\varphi_{it}(d)$ and $\rho_{it}(M_{it}(d))$ capture the impact on the potential outcomes of the potential mediator and the treatment, respectively. In this

model, the total, direct, and indirect effects of unit 1 are then given by

$$\begin{aligned}\alpha_{1t} &= \varphi_t(1)M_{1t}(1) - \varphi_t(0)M_{1t}(0) + \rho_t(M_{1t}(1)), \\ \theta_{1t}(M_{1t}(1)) &= \rho_t(M_{1t}(1)) + (\varphi_t(1) - \varphi_t(0))M_{1t}(1), \\ \delta_{1t}(0) &= \varphi_t(0)(M_{1t}(1) - M_{1t}(0)).\end{aligned}$$

Notice that the unobservables enter the potential outcomes equations in exactly the same way as in Abadie et al. (2010); thus, no extra restrictions are imposed on them.

Therefore, as also mentioned above, to estimate the total effect we can just use a standard SCM. In particular, we assume that there exists a $(1 \times (J - n))$ vector of weights $L^* = (l_{n+1}^*, \dots, l_J^*)$ that are positive, adding up to 1, and such that in the post-intervention period

$$Y_{1t}^{0, M_0} = \sum_{i=n+1}^J l_i^* Y_{it}.$$

As in Abadie et al. (2015), we assume that $\forall t = 1, \dots, T - 1$, L^* also satisfies

$$\begin{aligned}\sum_{j=n+1}^J l_j^* Y_{jt} &= Y_{1t}, \\ \sum_{j=n+1}^J l_j^* M_{jt} &= M_{1t}, \\ \sum_{j=n+1}^J l_j^* X_j &= X_1.\end{aligned}$$

This justifies the choice of weights that minimize the distance between the observable characteristics of the treated unit and the control units in the pre-treatment period. More formally, let $\Omega_1^\alpha = (X_1, Y_{11}, \dots, Y_{1, T-1}, M_{11}, \dots, M_{1, T-1})$ be a $((2(T - 1) + r) \times 1)$ vector, $\omega_{0i}^\alpha = (X_i, Y_{i1}, \dots, Y_{i, T-1}, M_{i1}, \dots, M_{i, T-1})$ be a $(1 \times (2(T - 1) + r))$ vector, and

$\Omega_0^\alpha = (\omega_{0,n+1}^\alpha, \dots, \omega_{0,J}^\alpha)'$. Then

$$\begin{aligned} L^* &= \min_{l_{n+1}, \dots, l_J} \|\Omega_1^\alpha - L\Omega_0^\alpha\| \\ \text{s.t. } &l_{n+1} \leq 0, \dots, l_J \leq 0, \sum_{i=n+1}^J l_i = 1, \end{aligned}$$

where $\|\Omega_1^\alpha - L\Omega_0^\alpha\| = \sqrt{(\Omega_1^\alpha - L\Omega_0^\alpha)'(\Omega_1^\alpha - L\Omega_0^\alpha)}$. It is also possible to give more weight to specific observable characteristics by using the alternative distance $\|\Omega_1^\alpha - L\Omega_0^\alpha\|_V = \sqrt{(\Omega_1^\alpha - L\Omega_0^\alpha)' V (\Omega_1^\alpha - L\Omega_0^\alpha)}$ (see Abadie et al. 2010 for a data driven procedure to choose V).

Let $\hat{Y}_{1t}^{0,M_0} = \sum_{i=n+1}^J l_i^* Y_{it}$, Abadie et al. (2010) show that if L^* exists, for $t \geq T$

$$E(\hat{Y}_{1t}^{0,M_0}) = Y_{1t}^{0,M_0} + o(T)$$

Consequently, estimating the total effect as $\hat{\alpha}_{1t} = Y_{1t} - \hat{Y}_{1t}^{0,M_0}$ is justified by the fact that

$$\lim_{T \rightarrow \infty} E(\hat{\alpha}_{1t}) = \alpha_{1t} \quad \forall \quad t \geq T \quad (2.1)$$

The estimation of Y_{1t}^{0,M_1} , and thus the direct and indirect effects, requires additional constraints but no extra assumptions on the unobservable in the potential outcomes equations. Our goal is to construct a “synthetic” unit which is identical to the treated unit, not affected by the intervention, and, at the same time, has the same value of the mediator as the treated unit. Similar to standard SCM, we want to find a $(1 \times (J - n))$ vector weights $W_t^* = (w_{n+1,t}^*, \dots, w_{Jt}^*)$ that are positive, adding up to 1, and such that in the post-intervention period

$$Y_{1t}^{0,M_1} = \sum_{i=n+1}^J w_{it}^* Y_{it}.$$

Notice that, in our simple factor model, Y_{1t}^{0,M_1} depends on the value that M takes at time t only. Also notice that the weights need to be calculated in each post-intervention period

It is easy to let Y_{1t}^{0,M_1} depend on all the values that the mediator takes between T and t . This is done by replacing $\Omega_1^{\theta_{t'}(1)}$ and $\omega_{0i}^{\theta_{t'}(1)}$ defined below

in this model. Let $t' \geq T$ be the time at which we want to estimate the direct effect. Similar to Abadie et al. (2010) we assume that $W_{t'}^*$ exists and satisfies $\forall t = 1, \dots, T-1$:

$$\begin{aligned} \sum_{j=n+1}^J w_{jt'}^* Y_{jt} &= Y_{1t}, \\ \sum_{j=n+1}^J w_{jt'}^* X_j &= X_1, \end{aligned}$$

and $\forall t = 1, \dots, T-1, t'$,

$$\sum_{j=n+1}^J w_{jt'}^* M_{jt} = M_{1t}.$$

The vector of weights, $W_{t'}^*$, is then estimated in a similar way as L^* . The only difference is that we now need to include the post-treatment mediator in the distance. More formally, if we let $\Omega_1^{\theta_{t'}(1)} = (X_1, Y_{11}, \dots, Y_{1,T-1}, M_{11}, \dots, M_{1,T-1}, M_{1,t'})$, $\omega_{0i}^{\theta_{t'}(1)} = (X_i, Y_{i1}, \dots, Y_{i,T-1}, M_{i1}, \dots, M_{i,T-1}, M_{i,t'})$, and $\Omega_0^{\theta_{t'}(1)} = (\omega_{n+1}^{\theta_{t'}(1)}, \dots, \omega_J^{\theta_{t'}(1)})'$, then

$$\begin{aligned} W_{t'}^* &= \min_{w_{n+1,t'}, \dots, w_{Jt'}} \|\Omega_1^{\theta_{t'}(1)} - W_{t'} \Omega_0^{\theta_{t'}(1)}\|_V \\ \text{s.t. } & w_{n+1,t'} \leq 0, \dots, w_{Jt'} \leq 0, \sum_{i=n+1}^J w_{it'} = 1, \end{aligned}$$

where $\|\Omega_1^{\theta_{t'}(1)} - W_{t'} \Omega_0^{\theta_{t'}(1)}\|_V = \sqrt{(\Omega_1^{\theta_{t'}(1)} - W_{t'} \Omega_0^{\theta_{t'}(1)})' V (\Omega_1^{\theta_{t'}(1)} - W_{t'} \Omega_0^{\theta_{t'}(1)})}$. Notice that we only have one mediator in the post-intervention period and several pre-intervention variables. Thus, we suggest to choose V such that equal weights are given to pre- and post-intervention information.

Let $\hat{Y}_{1t'}^{0, M_1} = \sum_{i=n+1}^J w_{it'}^* Y_{it'}$, as we show in the appendix, if $W_{t'}^*$ exists, under standard regularity conditions:

$$E(\hat{Y}_{1t'}^{0, M_1}) = Y_{1t'}^{0, M_1} + o(T).$$

This allows us to estimate the direct effect as $\theta_{1t'}(M_{1t}(1))$ and the indirect effect as $\delta_{it'}(0)$

with $\Omega_1^{\theta_{t'}(1)} = (X_1, Y_{11}, \dots, Y_{1,T-1}, M_{11}, \dots, M_{1,T-1}, M_{1,T}, \dots, M_{1,t'})$ and $\omega_{0i}^{\theta_{t'}(1)} = (X_i, Y_{i1}, \dots, Y_{i,T-1}, M_{i1}, \dots, M_{i,T-1}, M_{i,T}, \dots, M_{i,t'})$, respectively.

since

$$\hat{\theta}_{1t'}(M_{1t'}(1)) = Y_{1t'} - \hat{Y}_{1t'}^{0, M_1}, \quad \hat{\delta}_{1t'}(0) = \hat{\alpha}_{1t'} - \hat{\theta}_{1t'}(M_{1t'}(1)),$$

respectively, as it implies

$$\begin{aligned} \lim_{T \rightarrow \infty} E(\hat{\theta}_{1t'}(M_{1t}(1)_{1t})) &= \theta_{1t'}(M_{1t}(1)_{1t}) \quad \forall t \geq T, \\ \lim_{T \rightarrow \infty} E(\hat{\delta}_{it'}(0)) &= \delta_{it'}(0) \quad \forall t \geq T \end{aligned}$$

2.2 Additional assumptions of MASC

Intuitively, W_t^* only exists if, in addition to the assumptions needed for a standard SCM, there is also an overlap in the post-intervention values of the mediator. This assumption basically requires that the post-intervention value of the mediator of the treated units is not an outlier in comparison to the one of the units in the donor pool. This assumption is, of course, not required to identify the total effect and in some applications might be violated, even though the standard SCM assumption are satisfied. However, similarly to the standard SCM, the plausibility of our additional assumption can be graphically assessed by looking at the overlap in the pre-intervention period between the observed outcome, Y_{1t} , and the synthetic outcome \hat{Y}_{1t}^{0, M_1} together with the overlap in the post-intervention period between the mediator of the treated and those of the synthetic unit.

2.3 Inference

Inference can be carried over in a similar manner as for the standard synthetic control method. For example, one can run similar placebo tests as the one suggested in Abadie et al. (2010), Abadie et al. (2015) and Abadie (2021), estimating the effects (in our case also the direct and indirect effects) of the intervention either before its implementation or for units not exposed to it. Another possibility is to follow the approaches outlined in Chernozhukov et al. (2021) and Gobillon and Magnac (2016), to cite a few. We follow Abadie et al. (2010) and base our inference on the estimated placebo effect, excluding all the placebo units having a pre-treatment Root Mean Squared Prediction Error (RMSPE)

which is more than n times bigger than those of the treated units. For unit i and synthetic outcome $\hat{Y}^{d,M_{d'}}$, the pre-intervention RMSPE can be defined as

$$RMSPE_i^{\hat{Y}^{d,M_{d'}},pre} = \frac{\sum_{t=1}^{T-1} (Y_{i,t} - \hat{Y}_{i,t}^{d,M_{d'}})^2}{T-1}.$$

The p-values can be calculated as the portion of selected placebo units whose total (direct or indirect) estimated effect is equal or bigger to those of the treated unit. As underlined in Abadie et al. (2015), in absence of randomization, the p-values can only be interpreted as the probability to obtain an effect at least as large as those of the treated units.

3 A user-friendly empirical example

In this section we apply MASC to estimate how much of the effect of California’s Proposition 99 on cigarettes consumption found in Abadie et al. 2010 can be attributed to an increase in cigarette prices. We take a user-friendly approach by describing step by step all the choices we need to make.

3.1 Institutional framework and data

Proposition 99 was a large-scale tobacco control program introduced in California in 1988 from a voter initiative. It introduced an excise tax of 25 cents per pack of cigarette with the purpose of increasing the final price. The revenues for the tax had to be split mainly between a tobacco prevention (80%) and a cessation program (20%) supporting the health system for tobacco-related issues and tobacco-related research (Balbach et al. 1997). Similarly to previous studies (Siegel 2002), using the SCM, Abadie et al. (2010) find strong negative effects on consumption. Although both the tax and the health education program sought to reduce smoking prevalence, they differ in many respects. The former generates revenues for the state, but might have smaller long-term effects, while the latter might be more successful in the long run, but generates expenses. Therefore, it is crucial to determine to what extent each of its components contributed to the success of Proposition

99. Hu et al. (1995) used time-series monthly data to isolate the impact of the excise tax from those of the media campaign. They found that both had a negative effect on cigarette consumption, although with the excise tax having the largest. We apply MASC to provide further evidence on the role of the tax increase to the efficacy of Proposition 99. We could use the excise tax as a mediator. However, consumers choice is influenced by the total price of cigarette, measured as the sum of retail price and state and federal taxes, rather than on state taxes alone. Therefore, we believe the total cigarette price per package is more interesting as a mediator. Notice that, if needed, the estimation of the direct effect can be repeated multiple times employing multiple mediators to investigate over multiple causal channels. However, to be able to interpret the indirect effects as separate causal channels, the mediators must be independent of each other (see, e.g., Huber (2014)).

Following Abadie et al. (2010), we get data on per-capita cigarette consumption in packs (our outcome), on retail price per pack of cigarette and on the federal and state taxes from Orzechowski and Walker (2019) (the database is publicly available through the Centers for Disease Control and Prevention). The annual state-level database over the period 1970-2000 includes also other relevant control variables (for additional details, see the next section and Abadie et al. (2010)).

3.2 MASC implementation

Selection of the donor pool. As in SCM, MASC implementation requires a valid donor pool. It cannot include any unit that was affected by the intervention. Moreover, neither units in the donor pool nor treated units can experience large idiosyncratic exogenous shocks, affecting the outcome during the period under study. Finally, it is strictly recommended to only include units in the donor pool that are similar to the treated units. Therefore, we exclude from the donor pool all states that adopted large-scale tobacco control program during our sample period and, following Abadie et al. (2010), the District of Columbia as it is unlikely to have characteristics similar to California. The states that implemented tobacco interventions are Massachusetts, Arizona, Oregon, and Florida. Moreover, following Abadie et al. (2010), when re-estimating the total effect of Proposi-

tion 99, we exclude all the states that increased tax excises by more than 50 cents. These are Alaska, Hawaii, Maryland, Michigan, New Jersey, New York and Washington. However, as those states did not complement the tax excise with a tobacco control program (of Medicine (US) 1994), not only we can safely include them when we estimate the direct effect; their inclusion can help us to better find a synthetic unit whose mediator is equal to the one of the treated unit under treatment, i.e., $M(1)$. Our final donor pools included 38 states for the total effect estimation and 45 states for the direct effect estimation (see Appendix E).

Selection of covariates. Our goal is to create a synthetic unit whose outcome resembles as close as possible the one the treated unit would have had in the absence of the intervention. Therefore, following Abadie et al. 2010, we select a set of variables that are predictors of the outcome of interest: real per capita GDP by state (chained at 1997 prices), the percentage of population aged 15-24 and beer consumption per capita. All these predictors were averaged over the period 1980-1988. In addition, as in Abadie et al. 2010, we included the pre-treatment values of the outcome in 1975 and all years between 1980 and 1988. For the same years we also include pre-treatment values of the mediator (as well as the post-treatment years values of the mediator when estimating the direct effect; see the next paragraph). One can include some predictors of the mediator, provided that their inclusion does not deteriorate the pre-treatment fits of the outcome. More details on the applied covariates are available in Appendix E.

Mediator lag selection. In Section 2, we have assumed that all the effects happen instantaneously. Nonetheless, in empirical applications the treatment may have a delayed impact on the mediator and/or the mediator may have a delayed effect on the outcome. The timing of the effects affects the choice of the post-intervention value of the mediator we need to include among the predictors. Suppose that mediator at time $t' - \phi$ affects the outcome at time t' , with $\phi \geq 0$. Consequently, if we want to estimate the effect at time t' ,

Notice that, if the mediator is binary, MASC reduce to excluding from the donor pool all units with a post-treatment value of the mediator different from the one of the treated unit.

we cannot include mediator values at any time $t > t' - \phi$. The choice of ϕ depends on what economic theory suggests. In our application, it is reasonable to think that an increase in cigarette prices should immediately affect consumption (of Medicine (US) 1994); thus, we chose $\phi = 0$. To check the sensitivity of the results to this choice, we repeat the estimation with a one-year lag. The results (available from the authors upon request) do not change substantially.

Notice that we re-estimate the weights separately for each post-intervention period t' . Therefore, once we have chosen ϕ , we can either impose post-treatment constraints on all time periods between T and $t' - \phi$ or only in period $t' - \phi$. Including all periods allows to control for potential dynamic effects of the mediator but increases the complexity of the model and the chance of having a bad overlap in the post-treatment mediator. In our application we have chosen to impose constraints in the entire period (from T to $t' - \phi$). Nevertheless, the overlap remains good for most of the post-intervention period (see Figure 3b in Appendix F). This means that the further we move from the starting period T , the higher the number of post-intervention constraints included.

Control variables weights. We have to choose the weights that are assigned to each constrain (i.e., the matrix V). In the calculation of the total effect, we can select the matrix V using a cross-validation method, as suggested in Abadie et al. (2015). Following this approach, the matrix is not uniquely identified (Klößner et al. 2018). Therefore, in the placebo tests and in the leave-one-out robustness check, we employ the matrix selected in the first step. In the calculation of the direct effect, we have to make one additional choice. There is only one (or few) constraint(s) on the mediator in post-intervention period and several constraints on covariates in the pre-intervention period. Given that it is particularly important to have a good overlap in the post-treatment period mediator of our treated and synthetic control units, we suggest choosing V to ensure that a similar weight is given to the pre- and post-intervention constraints. In the application, we assign 3/4 of the total weights to pre-treatment constraints using cross-validation as for the total effect and the remaining 1/4 equally across the post-treatment constraints.

Inference. For inference, we follow Abadie et al. (2010); i.e., we estimate the placebo effects for all the units in the donor pool. Later on, to make inference for the direct and the total effects, we drop the results for all the placebo units whose pre-treatment RMSPE is more than five times those of the treated unit. For the indirect effect, instead, we drop the results for all the placebo units that have a big RMSPE in the estimation of either the direct or the total effect estimation. Finally, we derive the p-values from the placebo tests as proposed in Abadie et al. (2015) (see section 2.3 for details).

Satisfaction of MASC assumptions. First, we need to check that the overlaps in pre-treatment outcome between the treated unit and all synthetic units (the one used for estimating the total and the direct effects) are good enough. Figures 1a and 1b show that the overlaps for both the total and the direct effect are very good. To estimate the direct effect, it is important that there is a good overlap between the post-treatment values of the mediator. Figure 3b in Appendix F shows a good overlap in the post-treatment value of the mediator using weights obtained in the last estimation period, which is the more demanding, as it includes the highest number of post-intervention constraints (see the discussion about the selection of mediator lags).

Another requirement is that there are no anticipation effects of the intervention both on the outcome and on the mediator. As the policy was introduced in January 1989 after a ballot in November 1988, anticipation effects do not appear to be a major concern.

We cannot have any spillover effects on the units in the donor pool. Abadie et al. (2010) enumerates multiple ways in which this requirement maybe violated in our context. First, the tobacco industry may have diverted funds for campaigns or lobbying from other states to California. Nonetheless, if anything, such a phenomenon would downward bias the treatment effect estimation. Second, the anti-tobacco sentiment may have spread from California to neighboring states. Third, cross-border purchase from neighboring jurisdictions may increase after the introduction of the policy. To check for the last two potential spillovers, we check the robustness of our results to the exclusion of neighboring

Trends in per-capita cigarette sales (in packs)

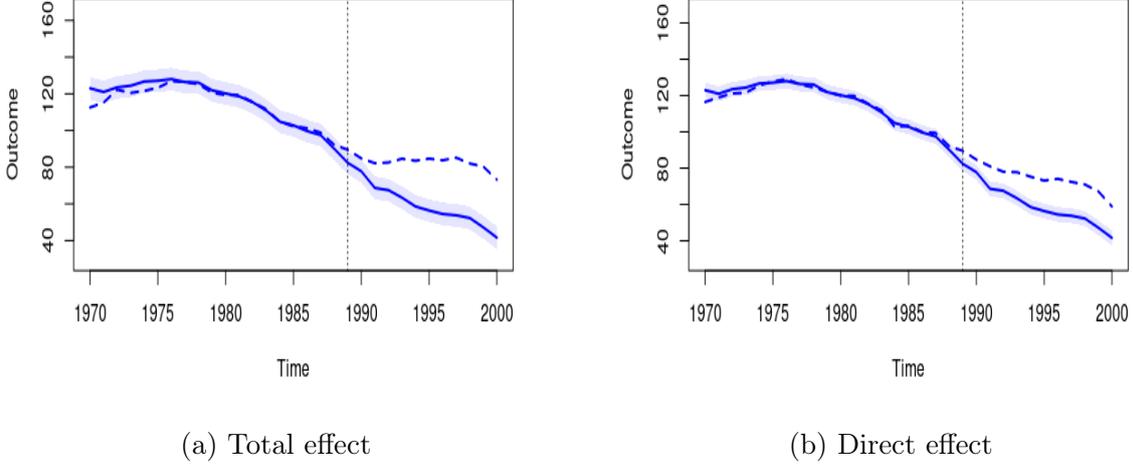


Figure 1: Comparison of the outcome of the treated unit and those of the synthetic units. The solid line represents the treated unit outcome. The dashed line represents the synthetic unit outcome when $Y = Y^{0,M_0}$ (left) and when $Y = Y^{0,M_1}$ (right). The shaded area around the solid line corresponds to two standard deviations of the pre-treatment difference between the outcomes of the treated unit and those of the synthetic units. The vertical line corresponds to the first treatment period. In the direct effect estimation, different weights are used in each post-treatment period. For simplicity, we use those found in the last post-treatment period.

states from the donor pool, i.e., we repeat the estimation excluding Nevada (the other two neighboring states, Oregon and Arizona, were not in the donor pool). As shown in figure 4a in the appendix, our results are fairly robust.

Finally, there has to be no reverse effect of the outcome on the mediator. This requirement is typically less of a concern when it is assumed that the impact of the mediator on the outcome is not instantaneous (i.e., when $\phi > 0$) as in many applications it is reasonable to assume that the outcome at time t does not have an impact on the mediator at time $t - \phi$. As we set $\phi = 0$, the satisfaction of this requirement is not as straightforward. We have to assume that cigarette prices do not instantaneously change to reflect the new consumption level. If there was perfect competition, the decrease in demand would immediately imply a decrease in cigarette prices. However, the empirical evidence suggests that the tobacco industry in the USA looks more like an oligopoly, and when Proposition 99 was implemented, prices did not seem to adjust quickly to the demand shock. The

tax induced an increase in total cigarette prices amounting to 127% of the tax increase (Hu et al. ???). Nevertheless, as a further check, we re-estimated the direct effect using the state tax as a mediator finding qualitatively similar results, which are available upon request.

3.3 Results

Before discussing the results, we want to clarify how the direct and indirect effects should be interpreted in this application (the interpretation of the direct effect may not be as straightforward in other applications). The direct effect captures the effect of the prevention and cessation program, those of the tobacco-related research activity (financed by the revenues of the taxes), and those of the stimulus that Proposition 99 gave to the development of local clean indoor air ordinance (Siegel 2002). The prevention and cessation program was present in state-wide media campaigns, community-based programs, and grants for the development of high school and community programs. In the first year, 82 million dollars was allocated to contribute to it Siegel (2002). The proposition passed in November 1988 and it came into force in January 1989. Since the fiscal year in California goes from July to June, the funds for the campaign were already employed for half of 1989. Notice that as we estimate the direct effect $\theta_{1t}(M_{1t}(d))$ when $d = 1$, we calculate the effect of these programs when accompanied by an increase in cigarette price. This may differ from the effect these programs would have had in its absence.

Given that we estimate the indirect effect $\delta(d)$ when $d = 0$, this is the effect that only an increase in cigarette prices would have had in the absence of other programs (i.e., the prevention and control program, the research activity, the increase in indoor clean air ordinances). As shown in Figure 3a in Appendix F, Proposition 99 induced a strong increase in cigarette prices since 1989 and remained stable up to 1996. After this year, the two prices started to converge until 1999, when the difference between the two prices sharply diverged again.

In Table 1 and Figure 2, we report the estimates for the total, direct, and indirect effects.

The results are robust to leaving one state out from the donor pool (see Appendix G).

Table 1: Effects of Proposition 99

Year	Total	Direct	Indirect
1989	-7.1	-7.17	0.06
P-value	(0.22)	(0.31)	(1)
1990	-7.04	-6.93	-0.11
P-value	(0.3)	(0.19)	(1)
1991	-13.47*	-12.35	-1.12
P-value	(0.07)	(0.13)	(0.75)
1992	-15.05***	-10.49	-4.56
P-value	(0)	(0.25)	(0.38)
1993	-21.18**	-14.36***	-6.81
P-value	(0.04)	(0)	(0.19)
1994	-24.92***	-16.72***	-8.2
P-value	(0)	(0)	(0.13)
1995	-28.23***	-16.77	-11.46***
P-value	(0)	(0.13)	(0)
1996	-29.26***	-19.67***	-9.6*
P-value	(0)	(0)	(0.06)
1997	-31.38***	-18.65***	-12.73***
P-value	(0)	(0)	(0)
1998	-29.72***	-18.72***	-10.99
P-value	(0)	(0)	(0.13)
1999	-33.12***	-19.98*	-13.13***
P-value	(0)	(0.06)	(0)
2000	-31.59**	-17.28	-14.31***
P-value	(0.04)	(0.13)	(0)

P-values are displayed in brackets.

* Significant at 10%.

** Significant at 5%

*** Significant at 1%.

Effects of Proposition 99 on cigarette consumption

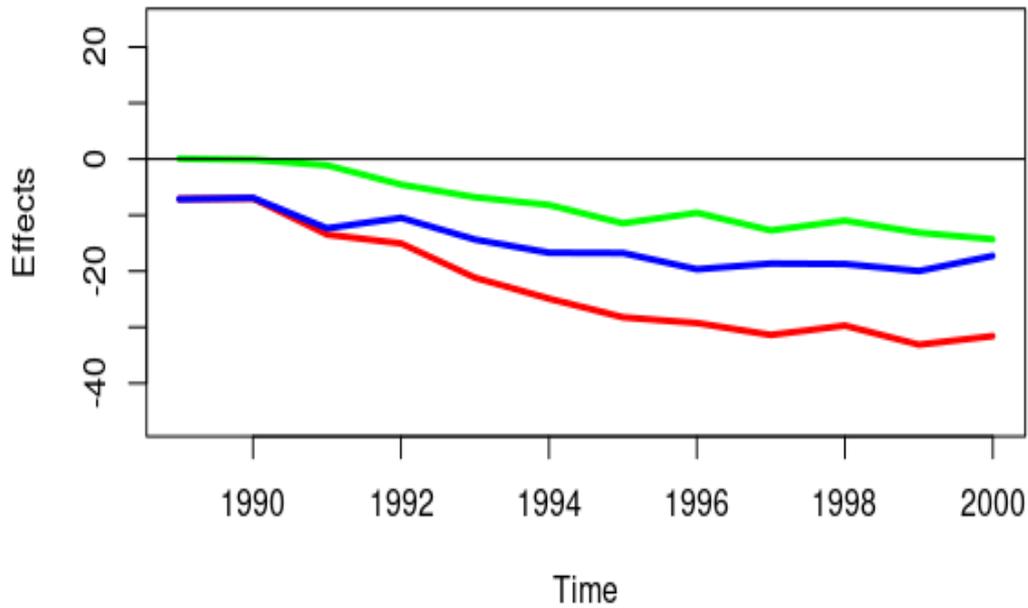


Figure 2: Total (red), direct (blue), and indirect (green) effects evolution over time.

In line with those of Abadie et al. (2010), our results show that Proposition 99 reduced the cigarette per-capita consumption from a minimum of 7 to a maximum of 33 packs each year. The direct effect, which is visible from the very first year of treatment, seems to have contributed the most to this decrease, although the contribution of the indirect effect increases over time. Notice that the total effect is significantly different from zero since 1991, the direct effect since 1993, and the indirect effect only in the second half of the period. The direct effect decreased slightly in 1992. This is explained by the fact that in 1992, Governor Pete Wilson suspended the media campaign that was re-established the next year, following a lawsuit by the American Lung Association. The fact that the decrease in 1992 is only visible in the direct effect while the indirect effect is unaffected, increases our confidence that we are correctly decomposing the total effect.

Our results suggest that the contribution of the direct effect is higher than what was found by Hu et al. (1995) for the media campaign. However, we need to emphasize that our direct effect incorporates not only the effect of the media campaign but also the effects of the other programs (i.e., community-based programs, grants and research activity). Our estimates of the effects of the cigarette prices increase are also lower than in their estimation. Overall, our results suggest that the two policies combined were more successful in reducing cigarette consumption than only imposing a higher tax would have been.

4 Conclusions

We introduced MASC, a new method that combines the synthetic control method (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015) with the mediation analysis approach and allows us to identify direct and indirect effects in a panel data frameworks with a low number of treated and control units. MASC is intuitive and easy to implement (i.e., publicly available SCM algorithms can be employed). Even though we show how to implement our method based on the original SCM algorithm of Abadie et al. (2010, 2015), one can alternatively use many of the new SCM-based methods such as Athey et al. (2017); Xu (2017); Kreif et al. (2016); Ben-Michael et al. (2021); and Doudchenko and Imbens (2016). Finally, we provide an illustrative example where we decompose the “total” effect of Proposition 99 on cigarette consumption, and we show that an increase in cigarette prices alone would have had a lower impact than in combination with the other programs.

Appendix

A Derivation of “Synthetic” $Y_{1t}^{0M_1}$

To ease the notation, the subscript t is dropped from the weights. Following Abadie et al. (2010), consider a generic vector of weights $W = (w_{n+1}, \dots, w_J)'$ such that $w_j \geq 0$ for all $j = n+1, \dots, J$ and $w_{n+1} + \dots + w_J = 1$. With these weights (and considering the factor model introduced in the text) the synthetic value of $Y_{1t}^{0M_1}$ is given by

$$\sum_{j=n+1}^J w_j Y_{jt} = \zeta_t + \eta_t \sum_{j=n+1}^J w_j X_j + \lambda_t \sum_{j=n+1}^J w_j \mu_j + \varphi_t(0) \sum_{j=n+1}^J w_j M_{jt}(0) + \sum_{j=n+1}^J w_j \epsilon_{jt}.$$

The difference between the real potential outcome and the synthetic one is then

$$\begin{aligned} Y_{1t}^{0, M_1} - \sum_{j=n+1}^J w_j Y_{jt} &= \eta_t \left(X_1 - \sum_{j=n+1}^J w_j X_j \right) + \lambda_t \left(\mu_1 - \sum_{j=n+1}^J w_j \mu_j \right) \\ &+ \varphi_t(0) \left(M_{1t}(I\{t \geq T\}) - \sum_{j=n+1}^J w_j M_{jt}(0) \right) \\ &+ \sum_{j=n+1}^J w_j (\epsilon_{1t} - \epsilon_{jt}). \end{aligned} \quad (\text{A.1})$$

Let Y_i^P be the $((T-1) \times 1)$ vector with t th element equal to Y_{it} , ϵ_i^P the $((T-1) \times 1)$ vector with t th element equal to ϵ_{it} , η^P the $((T-1) \times r)$ matrix with t th row equal to η_t and λ^P the $((T-1) \times F)$ matrix with t th row equal to λ_t . Moreover, let $\varphi^P(0)$ be the $((T-1) \times 1)$ vector with t th element equal to $\varphi_t(0)$ and $M_i^P(0)$ the $((T-1) \times 1)$ vector with t th element equal to $M_{it}(0)$. We can now write

$$\begin{aligned} Y_1^P - \sum_{j=n+1}^J w_j Y_j^P &= \eta^P \left(X_1 - \sum_{j=n+1}^J w_j X_j \right) + \lambda^P \left(\mu_1 - \sum_{j=n+1}^J w_j \mu_j \right) \\ &+ \varphi^P(0) \left(M_{1t}^P(0) - \sum_{j=n+1}^J w_j M_{jt}^P(0) \right) + \left(\epsilon_1^P - \sum_{j=n+1}^J w_j \epsilon_j^P \right). \end{aligned}$$

Note that we have $M_{1t}^P(0)$ as $t < T$. It is easy to see that:

$$\begin{aligned}
\lambda^P \left(\mu_1 - \sum_{j=n+1}^J w_j \mu_j \right) &= Y_1^P - \sum_{j=n+1}^J w_j Y_j^P - \eta^P \left(X_1 - \sum_{j=n+1}^J w_j X_j \right) \\
&- \varphi^P(0) \left(M_{1t}^P(0) - \sum_{j=n+1}^J w_j M_{jt}^P(0) \right) \\
&- \left(\epsilon_1^P - \sum_{j=n+1}^J w_j \epsilon_j^P \right)
\end{aligned} \tag{A.2}$$

Similar to Abadie et al. (2010), assume that

Assumption 1. $\sum_{t=1}^{T-1} \lambda_t' \lambda_t$ is non-singular.

Assumption 1 is equivalent to assuming no perfect-collinearity among unobserved common factors and implies that $(\lambda^{P'} \lambda^P)^{-1}$ exists. We can then multiply both sides of A.2 by $(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'}$ to get

$$\begin{aligned}
\mu_1 - \sum_{j=n+1}^J w_j \mu_j &= (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left\{ Y_1^P - \sum_{j=n+1}^J w_j Y_j^P - \eta^P \left(X_1 - \sum_{j=n+1}^J w_j X_j \right) \right. \\
&\left. - \varphi^P(0) \left(M_{1t}^P(0) - \sum_{j=n+1}^J w_j M_{jt}^P(0) \right) - \left(\epsilon_1^P - \sum_{j=n+1}^J w_j \epsilon_j^P \right) \right\}.
\end{aligned}$$

Substituting in A.1 and considering a generic post-intervention period $t' \geq T$, we have

$$\begin{aligned}
Y_{1t'}^{0, M_1} - \sum_{j=n+1}^J w_j Y_{jt'} &= \lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left(Y_1^P - \sum_{j=n+1}^J w_j Y_j^P \right) \\
&+ \left(\eta_{t'} - \lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \eta^P \right) \left(X_1 - \sum_{j=n+1}^J w_j X_j \right) \\
&- \lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left[\varphi^P(0) \left(M_1^P(0) - \sum_{j=n+1}^J w_j M_j^P(0) \right) \right] \\
&+ \varphi_{t'}(0) \left(M_{1t'}(1) - \sum_{j=n+1}^J w_j M_{jt'}(0) \right) \\
&- \lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left(\epsilon_1^P - \sum_{j=n+1}^J w_j \epsilon_j^P \right) + \sum_{j=n+1}^J w_j (\epsilon_{1t'} - \epsilon_{jt'}).
\end{aligned}$$

If we now assume, as we did in the main text, that there exists a set of positive and summing up to 1 weights W^* that satisfies, $\forall t = 1, \dots, T - 1$

$$\begin{aligned}\sum_{j=n+1}^J w_j^* Y_{jt} &= Y_{1t}, \\ \sum_{j=n+1}^J w_j^* X_j &= X_1,\end{aligned}$$

and $\forall t = 1, \dots, T - 1, t'$, also satisfies

$$\sum_{j=n+1}^J w_j^* M_{jt} = M_{1t},$$

replacing in the post-intervention period, the generic weights with W^* , we get

$$Y_{1t'}^{0, M_1} - \sum_{j=n+1}^J w_j^* Y_{jt'} = -\lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left(\epsilon_1^P - \sum_{j=n+1}^J w_j^* \epsilon_j^P \right) + \sum_{j=n+1}^J w_j^* (\epsilon_{1t'} - \epsilon_{jt'}).$$

From here, the proof is identical to the one in Abadie et al. (2010). We can write

$$Y_{1t'}^{0, M_1} - \sum_{j=n+1}^J w_j^* Y_{jt'} = R_{1t'} + R_{2t'} + R_{3t'}$$

where

$$R_{1t'} = \lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \sum_{j=n+1}^J w_j^* \epsilon_j^P \tag{A.3}$$

$$R_{2t'} = -\lambda_{t'} (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \epsilon_1^P \tag{A.4}$$

$$R_{3t'} = \sum_{j=n+1}^J w_j^* (\epsilon_{jt'} - \epsilon_{1t'}) \tag{A.5}$$

Following Abadie et al. (2010), we impose the following assumptions

Assumption 2. $\epsilon_{it} \perp \epsilon_{jt} \forall i \neq j$ with $i, j = 1, \dots, J$.

Assumption 3. $\epsilon_{it} \perp \epsilon_{it''} \forall t \neq t''$ with $t, t'' = 1, \dots, t'$.

Assumption 4. $E(\epsilon_{it}|X_i, \mu_i, M_{it}(I\{t \geq T\})) = E(\epsilon_{it}) = 0$ for $i \in \{1, n+1, \dots, J\}$ and for $t = 1, \dots, t'$

Taking the expected value on both sides of A.4, we get

$$\begin{aligned} E(R_{2t'}) &= E(-\lambda_{t'}(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \epsilon_1^P) \\ &= -\lambda_{t'}(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} E(\epsilon_1^P) \\ &= 0 \end{aligned}$$

where the second equality follows from the fact that $-\lambda_{t'}(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'}$ is non-stochastic and the third equality follows from assumption 4. Taking the expectation on both sides of A.5

$$\begin{aligned} E(R_{3t'}) &= E\left(\sum_{j=n+1}^J w_j^* (\epsilon_{jt'} - \epsilon_{1t'})\right) = \sum_{j=n+1}^J [E(w_j^* \epsilon_{jt'}) - E(w_j^* \epsilon_{1t'})] \\ &= \sum_{j=n+1}^J [E(w_j^*)E(\epsilon_{jt'}) - E(w_j^*)E(\epsilon_{1t'})] = 0 \end{aligned}$$

where the third equality follows from the fact that weights $W^* = w_{n+1}^*, \dots, w_J^*$ are determined using constraints on covariates, pre-treatment period outcomes and the mediator, which under assumptions 2, 3 and 4 are independent from the error terms at time $t' \geq T$. The fourth equality follows from assumption 4. The remaining A.3 can be rewritten as:

$$R_{1t'} = \sum_{j=n+1}^J w_j^* \sum_{s=1}^{T-1} \lambda_{t'} \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js} \quad (\text{A.6})$$

As in Abadie et al. (2010), we further assume that

Assumption 5. Let $\varsigma(M)$ be the smallest eigenvalue of

$$\frac{1}{M} \sum_{t=T-M+1}^{T-1} \lambda'_t \lambda_t,$$

$\varsigma(M) \geq \underline{\varsigma} > 0$ for each positive integer M .

Assumption 6.

$$\exists \underline{\lambda} \text{ s.t. } |\lambda_{tf}| \leq \underline{\lambda} \quad \forall t=1, \dots, t' \text{ and } f=1, \dots, F.$$

Assumption 5 guarantees that the matrix $\sum_{t=1}^T \lambda'_t \lambda_t$ and, consequently, its inverse, are symmetric and positive definite. Thus, for the Cauchy-Schwarz inequality, we find that

$$\begin{aligned} \left(\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \right)^2 &= |\langle \lambda_t, A \lambda'_s \rangle|^2 \leq \|A \lambda_t\|^2 \|A \lambda_s\|^2 \\ &= \left(\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_t \right) \left(\lambda_s \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \right) \end{aligned} \quad (\text{A.7})$$

Where $A = \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1}$. Since A is a symmetric matrix $B = (T-1)A$ is symmetric as well. Thus, it can be decomposed as $B = GOG^{-1}$. Where G is orthogonal and $G^{-1} = G'$ and O is a diagonal matrix with the eigenvalues of B as elements. Thus,

$$\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_t = \frac{1}{T-1} (\lambda_t B \lambda'_t) = \frac{1}{T-1} (\lambda_t G O G' \lambda'_t)$$

Defining $b_t = \lambda_t G$, we have

$$\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_t = \frac{1}{T-1} (b_t O b'_t) = \frac{1}{T-1} \left(b_{t1}^2 \frac{1}{\varsigma_1} + \dots + b_{tF}^2 \frac{1}{\varsigma_F} \right)$$

where ς_i are the eigenvalues of matrix B . From assumption 5, imposing $M = T - 1$, we'll have that $\frac{1}{\varsigma_i} \leq \frac{1}{\underline{\varsigma}}$ for $i = 1, \dots, F$. Indeed, the eigenvalues of the inverse of a matrix are given by the inverse of the matrix eigenvalues, and B is the inverse of the matrix in

assumption 5. Consequently:

$$\begin{aligned}\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_t &= \frac{1}{T-1} \sum_{f=1}^F \frac{b_{tf}^2}{\varsigma_f} \leq \frac{1}{(T-1)_{\underline{\varsigma}}} \sum_{f=1}^F b_{tf}^2 \\ &= \frac{1}{(T-1)_{\underline{\varsigma}}} \|b_t\|^2 = \frac{1}{(T-1)_{\underline{\varsigma}}} \|\lambda_t G\|^2\end{aligned}$$

As we noticed before, G is an orthogonal and thus isometric matrix, hence $\|\lambda_t G\| = \|\lambda_t\|$.

Consequently,

$$\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_t \leq \frac{1}{(T-1)_{\underline{\varsigma}}} \|\lambda_t\|^2 = \frac{\sum_{f=1}^F \lambda_{tf}^2}{(T-1)_{\underline{\varsigma}}} \leq \frac{\sum_{f=1}^F \lambda^2}{(T-1)_{\underline{\varsigma}}} = \frac{F\lambda^2}{(T-1)_{\underline{\varsigma}}}$$

where the last inequality follows from assumption 6. Applying the same idea to the second part of A.7, we get

$$\begin{aligned}\left(\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \right)^2 &\leq \left(\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_t \right) \left(\lambda_s \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \right) \\ &\leq \left(\frac{F\lambda^2}{(T-1)_{\underline{\varsigma}}} \right)^2\end{aligned}\tag{A.8}$$

Following Abadie et al. (2010), we define

$$\bar{\epsilon}_j^t = \sum_{s=1}^{T-1} \lambda_T \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js}\tag{A.9}$$

for $j = n+1, \dots, J$. Assume that

Assumption 7. *The p^{th} moment of $|\epsilon_{jt}|$ for some even p exists for $j = 2, \dots, J$ and $t = 1, \dots, T-1$*

Using Hölder's inequality and taking into account that $0 \leq w_j^* \leq 1$ for $j = n+1, \dots, J$

we find that:

$$\begin{aligned} \sum_{j=n+1}^J w_j^* |\overline{\epsilon_j^L}| &= \sum_{j=n+1}^J w_j^* |\overline{\epsilon_j^L} * 1| \leq \left(\sum_{j=n+1}^J w_j^* |\overline{\epsilon_j^L}|^p \right)^{1/p} \left(\sum_{j=n+1}^J w_j^* |1|^q \right)^{1/q} \\ &= \left(\sum_{j=n+1}^J w_j^* |\overline{\epsilon_j^L}|^p \right)^{1/p} \left(\sum_{j=n+1}^J w_j^* \right)^{1/q} = \left(\sum_{j=n+1}^J w_j^* |\overline{\epsilon_j^L}|^p \right)^{1/p} \leq \left(\sum_{j=n+1}^J |\overline{\epsilon_j^L}|^p \right)^{(1/p)} \end{aligned}$$

where the last equality follows from $w_{n+1}^* + \dots + w_J^* = 1$ and the last inequality follows from the condition that $w_{n+1}^* \leq 1, \dots, w_J^* \leq 1$. Applying Hölder's inequality again, we get

$$E \left[\sum_{j=n+1}^J w_j^* |\overline{\epsilon_j^L}| \right] \leq \left(E \left[\sum_{j=n+1}^J |\overline{\epsilon_j^L}|^p \right] \right)^{1/p} \quad (\text{A.10})$$

Applying Rosenthal's inequality, we have

$$\begin{aligned} E \left[|\overline{\epsilon_j^L}|^p \right] &= E \left[\left| \sum_{s=1}^{T-1} \lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js} \right|^p \right] \\ &\leq C(p) \max \left(\sum_{s=1}^{T-1} E \left[\left| \lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js} \right|^p \right] \right. \\ &\quad \left. , \left(\sum_{s=1}^{T-1} E \left[\left| \lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js} \right|^2 \right] \right)^{p/2} \right) \end{aligned}$$

where $C(p)$ is the p th moment of -1 plus a Poisson random variable with mean 1 (see Abadie et al. (2010)). Consider the two elements of $\max(\cdot)$. For the first element, we have

$$\begin{aligned} \sum_{s=1}^{T-1} E \left[\left| \lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js} \right|^p \right] &= \sum_{s=1}^{T-1} E \left[\left(\lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \right)^{2*(p/2)} |\epsilon_{js}|^p \right] \\ &\leq \sum_{s=1}^{T-1} E \left[\left(\frac{F \underline{\lambda}^2}{(T-1) \underline{\varsigma}} \right)^{2*(p/2)} |\epsilon_{js}|^p \right] \\ &= \left(\frac{F \underline{\lambda}^2}{\underline{\varsigma}} \right)^p \frac{1}{(T-1)^p} \sum_{s=1}^{T-1} E (|\epsilon_{js}|^p) \end{aligned}$$

where the first equality follows from the distributivity of the power and the inequality follows from A.8. For the second element in $\max(\cdot)$, we have

$$\begin{aligned} \left(\sum_{s=1}^{T-1} E \left[\left| \lambda_t \left(\sum_{h=1}^{T-1} \lambda'_h \lambda_h \right)^{-1} \lambda'_s \epsilon_{js} \right|^2 \right] \right)^{p/2} &\leq \left[\sum_{s=1}^{T-1} E \left(\left(\frac{F\bar{\lambda}^2}{(T-1)\underline{\zeta}} \right)^2 \epsilon_{js}^2 \right) \right]^{p/2} \\ &= \left(\frac{F\bar{\lambda}^2}{\underline{\zeta}} \right)^p \left[\sum_{s=1}^{T-1} \frac{1}{(T-1)^2} E(\epsilon_{js}^2) \right]^{p/2} \end{aligned}$$

where the first inequality follows from A.8. Putting all these results together, we have

$$E \left[|\bar{\epsilon}_j^L|^p \right] \leq C(p) \left(\frac{F\bar{\lambda}^2}{\underline{\zeta}} \right)^p \max \left(\frac{1}{(T-1)^p} \sum_{s=1}^{T-1} E(|\epsilon_{js}|^p), \left[\sum_{s=1}^{T-1} \frac{1}{(T-1)^2} E(\epsilon_{js}^2) \right]^{p/2} \right)$$

As Abadie et al. (2010), we define $\sigma_{js}^2 = E|\epsilon_{js}|^2$, $\sigma_j^2 = (1/(T-1) \sum_{s=1}^{T-1} \sigma_{js}^2)$, $\bar{\sigma}^2 = \max_{j=n+1, \dots, J} \sigma_j^2$ and $\bar{\sigma} = \sqrt{\bar{\sigma}^2}$. Similarly, we define $\tau_{p,jt} = E|\epsilon_{jt}|^p$, $\tau_{p,j} = \frac{1}{(T-1)} \sum_{t=1}^{T-1} \tau_{p,jt}$, and $\bar{\tau}_p = \max_{j=n+1, \dots, J} \tau_{p,j}$. We can write the first element of $\max(\cdot)$ as

$$\frac{1}{(T-1)^p} \sum_{s=1}^{T-1} E(|\epsilon_{js}|^p) = \frac{1}{(T-1)^{p-1}} \frac{1}{(T-1)} \sum_{t=1}^{T-1} \tau_{p,jt} = \frac{1}{(T-1)^{p-1}} \tau_{p,j}$$

Similarly, the second element can be written as

$$\left[\sum_{s=1}^{T-1} \frac{1}{(T-1)^2} E(\epsilon_{js}^2) \right]^{p/2} = \left(\frac{1}{T-1} \frac{1}{T-1} \sum_{s=1}^{T-1} \sigma_{js}^2 \right)^{p/2} = \left(\frac{1}{T-1} \sigma_j^2 \right)^{p/2}$$

Thus, defining $\varpi = C(p)\left(\frac{F\bar{\lambda}^2}{\underline{s}}\right)^p$, we have

$$\begin{aligned}
E \left[|\bar{\epsilon}_j^L|^p \right] &\leq \varpi \max \left(\frac{1}{(T-1)^{p-1}} \tau_{pj}, \left(\frac{1}{T-1} \sigma_j^2 \right)^{p/2} \right) \\
\sum_{j=n+1}^J E \left[|\bar{\epsilon}_j^L|^p \right] &= E \left[\sum_{j=n+1}^J |\bar{\epsilon}_j^L|^p \right] \\
&\leq \varpi \max \left(\frac{1}{(T-1)^{p-1}} \sum_{j=n+1}^J \tau_{pj}, \sum_{j=n+1}^J \left(\frac{1}{T-1} \sigma_j^2 \right)^{p/2} \right) \\
&= \varpi \max \left(\frac{J-n-1}{(T-1)^{p-1}} \frac{1}{J-n-1} \sum_{j=n+1}^J \tau_{pj}, \frac{1}{(T-1)^{p/2}} \sum_{j=n+1}^J \sigma_j^{2*p/2} \right) \\
\left(E \left[\sum_{j=n+1}^J |\bar{\epsilon}_j^L|^p \right] \right)^{1/p} &\leq \varpi^{1/p} \max \left(\frac{\left(\frac{J-n-1}{(T-1)^{p-1}} \right)^{1/p} \left(\sum_{j=n+1}^J \tau_{pj} \right)^{1/p}}{(J-n-1)^{1/p}}, \frac{\left(\sum_{j=n+1}^J \sigma_j^{2*p/2} \right)^{1/p}}{(T-1)^{(p/2)*(1/p)}} \right) \\
&= \varpi^{1/p} \max \left(\left(\frac{J-n-1}{(T-1)^{p-1}} \right)^{1/p} \bar{\tau}_p^{1/p}, \frac{1}{(T-1)^{1/2}} \left(\sum_{j=n+1}^J \bar{\sigma}^{2*(p/2)} \right)^{1/p} \right)
\end{aligned}$$

where the last equality follows from $\frac{1}{J-n-1} \sum_{j=n+1}^J \tau_{pj} = E(\tau_{pj}) \leq \max_j(\tau_{pj}) = \bar{\tau}_p$. Thus,

$$\begin{aligned}
\left(E \left[\sum_{j=n+1}^J |\bar{\epsilon}_j^L|^p \right] \right)^{1/p} &\leq \varpi^{1/p} \max \left(\frac{(J-n-1)^{1/p} \bar{\tau}_p^{1/p}}{(T-1)^{1-1/p}}, \frac{(J-n-1) \bar{\sigma}^{2*(p/2)}}{(T-1)^{1/2}} \right)^{1/p} \\
&= \varpi^{1/p} (J-n-1)^{1/p} \max \left(\frac{\bar{\tau}_p^{1/p}}{(T-1)^{1-\frac{1}{p}}}, \frac{\sqrt{\bar{\sigma}^2}}{(T-1)^{1/2}} \right) \quad (\text{A.11})
\end{aligned}$$

this implies

$$\begin{aligned}
E [|R_{1t'}|] &= E \left[\left| \sum_{j=n+1}^J w_j^* \epsilon_j^L \right| \right] \\
&\leq E \left[\sum_{j=n+1}^J w_j^* |\epsilon_j^L| \right] \\
&\leq \left(E \left[\sum_{j=n+1}^J |\epsilon_j^L|^p \right] \right)^{1/p} \\
&\leq \varpi^{1/p} (J-n-1)^{1/p} \max \left(\frac{\bar{\tau}_p^{1/p}}{(T-1)^{1-\frac{1}{p}}}, \frac{\bar{\sigma}}{(T-1)^{1/2}} \right)
\end{aligned}$$

where, in the second equation, the first equality follows from A.4 and A.9, the first inequality follows from the triangular inequality, the second follows from A.10 and the third from A.11. It follows that

$$E|R_{1t'}| \leq C(p)^{1/p} \frac{\overline{\lambda^2 F}}{\underline{\varsigma}} (J - n - 1)^{1/p} \max \left\{ \frac{\overline{\tau_p^{1/p}}}{(T - 1)^{1-1/p}}, \frac{\overline{\sigma}}{(T - 1)^{1/2}} \right\}.$$

Thus, the difference between the expected value of Y_{1t}^{0, M_1} and its synthetic counterpart can be bound by something that goes to zero when the number of pre-intervention periods goes to infinity, namely

$$E \left(Y_{1t'}^{0, M_1} - \sum_{j=n+1}^J w_j^* Y_{jt'} \right) = E(R_{1t'}) = o(T).$$

B Identification of $\delta_{it'}(1)$

Finding a “synthetic” value of Y_{1t}^{1, M_0} is more challenging and requires more than 1 treated unit. First, we need to estimate what value the mediator of unit 1 would have taken in the absence of the intervention ($M_{1t}(0)$). This could be done with a standard SCM, using the mediator as an outcome. Second, we propose to treat the remaining treated units as control units in a SCM where we also use the distance between the first step estimate of $M_{1t}(0)$ and the other treated units mediators, in computing the weights. If the number of treated units is big enough, we can also create a “synthetic” $Y_{it'}^{1, M_0}$. This is done in two steps. In a first step, we estimate $M_{1t'}(0)$ by $\hat{M}_{1t'}(0) = \sum_{i=n+1}^J k_{it'}^* M_{it'}$ with $K_{t'}^* = (k_{n+1, t'}^*, \dots, k_{J, t'}^*)$ chosen with a standard SCM. Note that also those weights need to be calculated for each t' . In a second step, we need to find a vector of positive and adding up to 1 weights $Q_{t'}^* = (q_{2t'}^*, \dots, q_{nt'}^*)$, such that $Y_{it'}^{1, M_0} = \sum_{i=2}^n q_{it'}^* Y_{it'}$. $Q_{t'}^*$ is estimated with a SCM but using only the other treated units. More specifically, let $\Omega_1^{\delta_{t'}(1)} = (X_1, Y_{11}, \dots, Y_{1, T-1}, M_{11}, \dots, M_{1, T-1}, \hat{M}_{1t'}(0))$, $\omega_{0i}^{\theta_{t'}(1)} = (X_i, Y_{i1}, \dots, Y_{i, T-1}, M_{i1},$

$\dots, M_{i,T-1}, M_{i,t'}),$ and $\Omega_0^{\theta_{t'}(1)} = (\omega_2^{\theta_{t'}(1)}, \dots, \omega_n^{\theta_{t'}(1)})'$, then

$$Q_{t'}^* = \min_{q_{n+1,t'}, \dots, q_{Jt'}} \|\Omega_1^{\theta_{t'}(1)} - Q_{t'} \Omega_0^{\theta_{t'}(1)}\|_V$$

$$s.t. \quad q_{n+1,t'} \leq 0, \dots, q_{Jt'} \leq 0, \sum_{i=n+1}^J q_{it'} = 1,$$

where the distance and V are defined as above for $Y_{it'}^{0,M_1}$.

Let $\hat{Y}_{1t'}^{1,M_0} = \sum_{i=2}^n q_{it'}^* Y_{it'}$, similar as before, we assume that $Q_{t'}^*$ exists and satisfies $\forall t = 1, \dots, T-1$

$$\sum_{j=2}^n q_{jt'}^* Y_{jt} = Y_{1t},$$

$$\sum_{j=2}^n q_{jt'}^* X_j = X_1,$$

$$\sum_{j=2}^n q_{jt'}^* M_{jt} = M_{1t},$$

$\forall t = 1, \dots, T-1$ and

$$\sum_{j=2}^n q_{jt'}^* M_{jt'} = \hat{M}_{1t'}(0).$$

Under extra standard conditions and assuming that $\rho_{t'}(\cdot)$ is a linear function

$$E(\hat{Y}_{1t'}^{1,0}) = Y_{1t'}^{1,M_0} + o(T).$$

The latter assumption can admittedly be restrictive in many applications. However, it is substantially weaker than assuming a constant $\rho_{t'}$. Then, we can estimate the indirect effect $\delta_{it'}(1)$ and the direct effect as $\theta_{1t'}(M_{1t}(0))$ as

$$\hat{\delta}_{1t'}(1) = Y_{1t'} - \hat{Y}_{1t'}^{1,M_0}, \quad \hat{\theta}_{1t'}(M_{1t'}(0)) = \hat{\alpha}_{it'} - \hat{\delta}_{1t'}(1),$$

respectively. Intuitively, $Q_{t'}^*$ exists under the similar assumptions as the one discussed in the main text. However, if the number of treated units is too small $\hat{Y}_{1t'}^{1,M_0}$ will be a very

poor approximation of $Y_{1t'}^{1,M_0}$. In this setting, it is only possible to estimate $\delta_{it'}(0)$ and $\theta_{it'}(1)$.

C Extra assumptions on the mediator needed for $Y_{1t}^{1M_0}$

To create a synthetic $Y_{1t}^{1M_0}$, we need to impose the standard SCM assumptions on the mediator, which are:

Assumption 8. $\sum_{t=1}^{T-1} \vartheta'_t \vartheta_t$ is non-singular.

Assumption 9. $\nu_{it} \perp \nu_{jt} \forall i \neq j$ with $i, j \in \{1, n+1, \dots, J\}$.

Assumption 10. $\nu_{it} \perp \nu_{it''} \forall t \neq t''$ with $t, t'' = 1, \dots, t'$.

Assumption 11. $E(\nu_{it} | \{Z_i, \varrho_i\}_{i \in \{1, n+1, \dots, J\}}) = E(\nu_{it}) = 0$ for $i \in \{1, n+1, \dots, J\}$ and for $t = 1, \dots, t'$

Assumption 12. $\kappa(M) \geq \underline{\kappa} > 0$ for each positive integer M , where $\kappa(M)$ is the smallest eigenvalue of

$$\frac{1}{M} \sum_{t=T-M+1}^{T-1} \vartheta'_t \vartheta_t. \quad (\text{C.1})$$

Assumption 13.

$$\exists \underline{\vartheta} \text{ s.t. } |\vartheta_{tv}| \leq \underline{\vartheta} \quad \forall t=1, \dots, t' \text{ and } v=1, \dots, V. \quad (\text{C.2})$$

Assumption 14. \exists a p^{th} moment of $|\nu_{jt}|$ for some even p and for $j = n+1, \dots, J$ and $t = 1, \dots, t'$

D Derivation of “Synthetic” $Y_{1t}^{1M_0}$

As for $Y_{1t}^{0M_1}$, we drop the subscript t from the weight and write

$$\begin{aligned} \sum_{j=2}^n q_j Y_{jt} = \zeta_t &+ \eta_t \sum_{j=2}^n q_j X_j + \lambda_t \sum_{j=2}^n q_j \mu_j + \varphi_t (I\{t \geq T\}) \sum_{j=2}^n q_j M_{jt} (I\{t \geq T\}) \\ &+ \sum_{j=2}^n q_j \rho_t (M_{jt} (I\{t \geq T\})) I\{t \geq T\} + \sum_{j=2}^n q_j \epsilon_{jt}. \end{aligned}$$

Thus,

$$\begin{aligned} Y_{1t}^{1,M_0} - \sum_{j=2}^n q_j Y_{jt} &= \eta_t \left(X_1 - \sum_{j=2}^n q_j X_j \right) + \lambda_t \left(\mu_1 - \sum_{j=2}^n q_j \mu_j \right) \\ &+ \varphi_t (I\{t \geq T\}) \left(M_{1t}(0) - \sum_{j=2}^n q_j M_{jt}(I\{t \geq T\}) \right) \\ &+ \left(\rho_t (M_{1t}(0)) - \sum_{j=2}^n q_j \rho_t (M_{jt} (I\{t \geq T\})) \right) I\{t \geq T\} \\ &+ \sum_{j=2}^n q_j (\epsilon_{1t} - \epsilon_{jt}) \end{aligned}$$

Using the same notation as before in the pre-intervention period, we have

$$\begin{aligned} Y_1^P - \sum_{j=2}^n q_j Y_j^P &= \eta^P \left(X_1 - \sum_{j=2}^n q_j X_j \right) + \lambda^P \left(\mu_1 - \sum_{j=2}^n q_j \mu_j \right) \\ &+ \varphi^P (0) \left(M_1^P (0) - \sum_{j=2}^n q_j M_j^P (0) \right) + \left(\epsilon_1^P - \sum_{j=2}^n q_j \epsilon_j^P \right) \end{aligned}$$

Thus,

$$\begin{aligned} \lambda^P \left(\mu_1 - \sum_{j=2}^n q_j \mu_j \right) &= Y_1^P - \sum_{j=2}^n q_j Y_j^P - \eta^P \left(X_1 - \sum_{j=2}^n q_j X_j \right) \\ &- \varphi^P (0) \left(M_1^P (0) - \sum_{j=2}^n q_j M_j^P (0) \right) - \left(\epsilon_1^P - \sum_{j=2}^n q_j \epsilon_j^P \right) \end{aligned}$$

Multiplying both sides by $(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'}$, we get

$$\begin{aligned} \mu_1 - \sum_{j=2}^n q_j \mu_j &= (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left\{ \left(Y_1^P - \sum_{j=2}^n q_j Y_j^P \right) - \eta^P \left(X_1 - \sum_{j=2}^n q_j X_j \right) \right. \\ &\quad \left. - \varphi^P(0) \left(M_1^P(0) - \sum_{j=2}^n q_j M_j^P(0) \right) - \left(\epsilon_1^P - \sum_{j=2}^n q_j \epsilon_j^P \right) \right\}. \end{aligned}$$

Substituting in D.1 and considering a generic post-intervention period t' , we have

$$\begin{aligned} Y_{1t'}^{1, M_0} - \sum_{j=2}^n q_j Y_{jt'} &= (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left(Y_1^P - \sum_{j=2}^n q_j Y_j^P \right) \\ &\quad + \left(\eta_{t'} - (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \eta^P \right) \left(X_1 - \sum_{j=2}^n q_j X_j \right) \\ &\quad - (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \varphi^P(0) \left(M_1^P(0) - \sum_{j=2}^n q_j M_j^P(0) \right) \\ &\quad + \varphi_{t'}(1) \left(M_{1t'}(0) - \sum_{j=2}^n q_j M_{jt'}(1) \right) \\ &\quad + \left(\rho_{t'}(M_{1t'}(0)) - \sum_{j=2}^n q_j \rho_{t'}(M_{jt}(1)) \right) \\ &\quad - (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \left(\epsilon_1^P - \sum_{j=2}^n q_j \epsilon_j^P \right) + \sum_{j=2}^n q_j (\epsilon_{1t'} - \epsilon_{jt'}) \end{aligned}$$

Assume, as we did in the main text, that weights q_2^*, \dots, q_n^* exists that satisfy $\forall t = 1, \dots, T -$

1

$$\begin{aligned} \sum_{j=2}^n q_j^* Y_{jt} &= Y_{1t}, \\ \sum_{j=2}^n q_j^* X_j &= X_1, \\ \sum_{j=2}^n q_j^* M_{jt} &= M_{1t}, \end{aligned}$$

and it also satisfies

$$\sum_{j=2}^n q_j^* M_{jt'} = \hat{M}_{1t'}(0).$$

Substituting the generic weights with q_2^*, \dots, q_n^* in the post-intervention period t' , we get

$$\begin{aligned} Y_{1t'}^{1, M_0} - \sum_{j=2}^n q_j^* Y_{jt'} &= \left(\rho_{t'}(M_{1t'}(0)) - \sum_{j=2}^n q_j^* \rho_{t'}(M_{jt'}(1)) \right) \\ &- \left(\lambda^{P'} \lambda^P \right)^{-1} \lambda^{P'} \left(\epsilon_1^P - \sum_{j=2}^n q_j^* \epsilon_j^P \right) + \sum_{j=2}^n q_j^* (\epsilon_{1t'} - \epsilon_{jt'}) \end{aligned}$$

Note that, as by assumption $\sum_{j=2}^n q_j^* M_{jt'} = \hat{M}_{1t'}(0)$, and $\hat{M}_{1t'}(0)$ is estimated using a standard SCM

$$E \left(\varphi_{t'}(1) \left(M_{1t'}(0) - \sum_{j=2}^n q_j^* M_{jt'}(1) \right) \right) = o(T).$$

As mentioned earlier, for identification we have to impose an extra assumption, namely:

Assumption 15. $\rho_{t'}(\cdot)$ is a linear function

Under assumption 15, we have

$$\begin{aligned} E \left[\left(\rho_{t'}(M_{1t'}(0)) - \sum_{j=2}^n q_j^* \rho_{t'}(M_{jt'}(1)) \right) \right] &= E \left[\left(\rho_{t'}(M_{1t'}(0)) - \rho_{t'} \left(\sum_{j=2}^n q_j^* M_{jt'}(1) \right) \right) \right], \\ &= E \left[\left(\rho_{t'}(M_{1t'}(0)) - \rho_{t'}(\hat{M}_{1t'}(0)) \right) \right], \\ &= \rho_{t'}(M_{1t'}(0)) - \rho_{t'}(E(\hat{M}_{1t'}(0))) = o(T). \end{aligned}$$

Thus,

$$Y_{1t'}^{1, M_0} - \sum_{j=2}^n q_j^* Y_{jt'} = - \left(\lambda^{P'} \lambda^P \right)^{-1} \lambda^{P'} \left(\epsilon_1^P - \sum_{j=2}^n q_j^* \epsilon_j^P \right) + \sum_{j=2}^n q_j^* (\epsilon_{1t'} - \epsilon_{jt'}).$$

This can be shown to imply

$$E(Y_{1t'}^{1, M_0} - \sum_{j=2}^n q_j^* Y_{jt'}) = o(T).$$

E Data Sources and Donor Pool

The variables employed stem from different sources. As mentioned in 3.1, data on the outcome and on the mediator stem from the Tax Burden on Tobacco compilation (Orzechowski and Walker 2019). We obtained the data on population from the US Census Bureau. We obtained the data on the real GDP per capita, chained at 1997 prices, from the Bureau of Economic Analysis of the US Department of Commerce. Finally, we obtained the data on beer consumption per capita from Kaplan (2018).

In Table 2, we present the donor pool for the total (first two columns) and for the direct (third and fourth columns) effect estimations with the corresponding weights. In the direct effect estimation, different weights are used in each post-treatment period. For simplicity, we report those found in the last post-treatment period.

Table 2: Donor Pool

Donor Pool	Weights Total Effect	Weights Direct Effects
Alaska	-	0.046
Alabama	<0.001	<0.001
Arkansas	<0.001	<0.001
Colorado	<0.001	<0.001
Connecticut	0.014	<0.001
Delaware	<0.001	<0.001
Georgia	<0.001	<0.001
Hawaii	-	0.135
Iowa	<0.001	<0.001
Idaho	0.005	<0.001
Illinois	<0.001	<0.001
Indiana	<0.001	<0.001
Kansas	<0.001	<0.001
Kentucky	<0.001	<0.001
Louisiana	<0.001	<0.001
Maryland	-	0.001
Maine	<0.001	<0.001
Michigan	-	<0.001
Minnesota	<0.001	<0.001
Missouri	<0.001	<0.001
Mississippi	<0.001	<0.001
Montana	0.061	<0.001
North Carolina	0.005	<0.001
North Dakota	<0.001	<0.001
Nebraska	<0.001	<0.001
New Hampshire	0.107	<0.001
New Jersey	-	0.001
New Mexico	0.349	<0.001
Nevada	0.032	0.263
New York	-	0.249
Ohio	<0.001	<0.001
Oklahoma	<0.001	<0.001
Pennsylvania	<0.001	<0.001
Rhode Island	0.006	<0.001
South Carolina	<0.001	<0.001
South Dakota	<0.001	<0.001
Tennessee	<0.001	<0.001
Texas	0.001	<0.001
Utah	0.247	0.302
Virginia	<0.001	<0.001
Vermont	<0.001	<0.001
Washington	-	<0.001
Wisconsin	0.001	<0.001
West Virginia	0.169	<0.001
Wyoming	<0.001	<0.001

F Mediator Fits

Trends in total cigarette price per pack

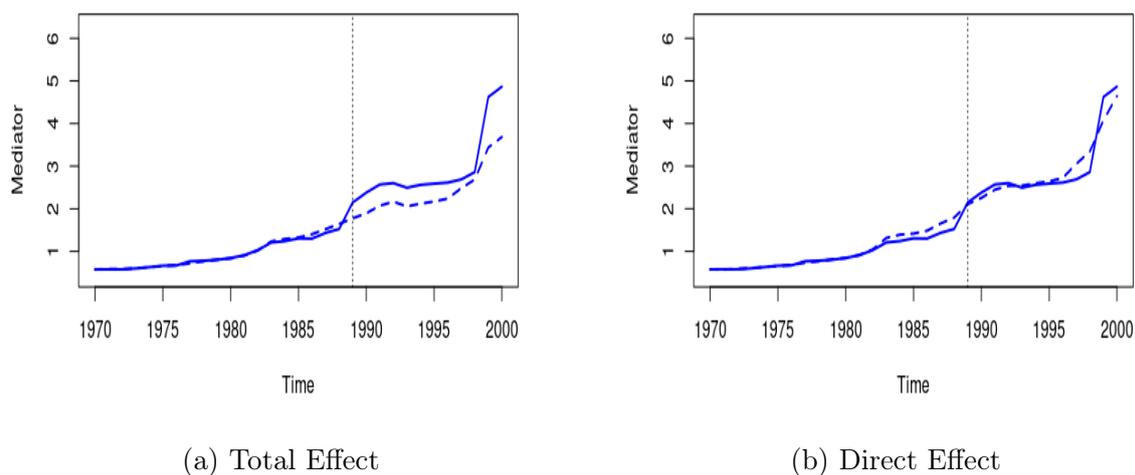
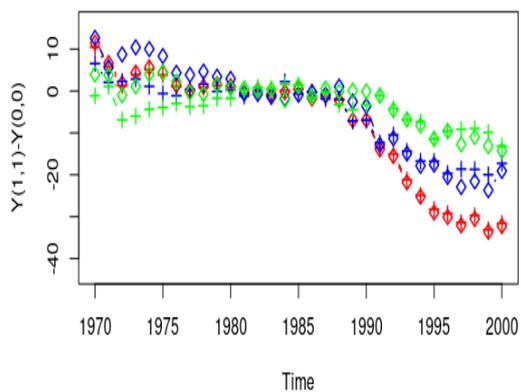
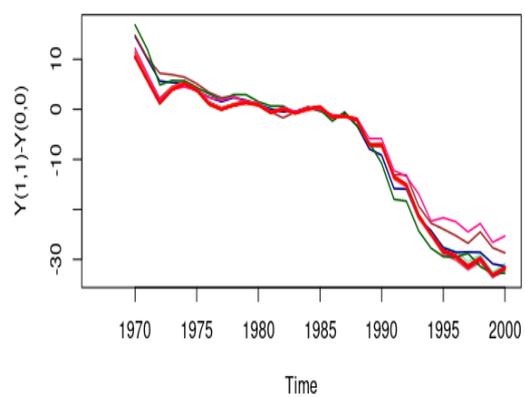


Figure 3: Constraints on the mediator for the estimation of the total (left) and those of the direct (right) effects. We are mostly interested in post-treatment overlaps in the right panel. The solid line represents the treated unit mediator. The dashed line represents the synthetic unit mediator. The vertical line corresponds to the first treatment period. In the direct effect estimation, different weights are used in each post-treatment period. For simplicity, we use those found in the last post-treatment period.

G Spillover Effects Testing and Leave-one-out Robustness Checks

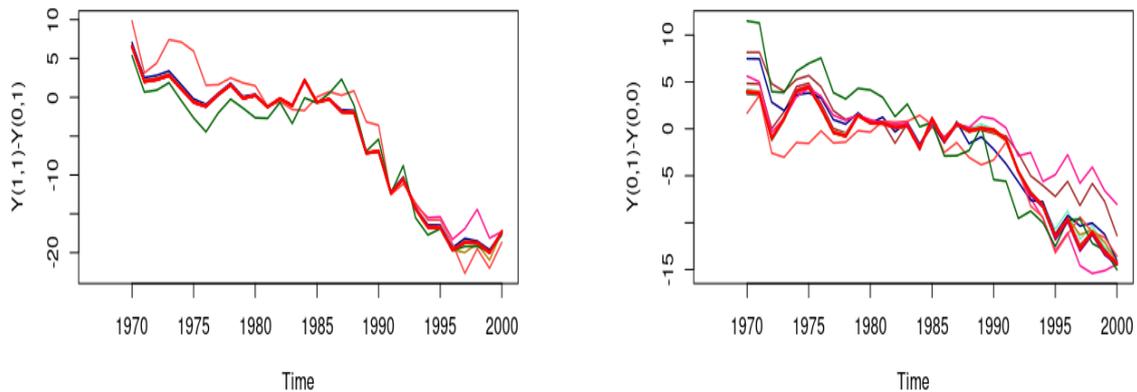


(a) Sensitivity of the results to the exclusion, from the donor pool, of the state of Nevada. Total, direct, and indirect effects are represented, respectively, in red, blue and green. The diamond-line represents baseline estimation. The x-line represents the spillover-free estimation.



(b) Leave-one-out robustness checks for the total effect. The thick red line represents the baseline estimation.

Figure 4: Robustness checks



(a) Leave-one-out robustness checks for the direct effect. The thick red line represents the baseline estimation.

(b) Leave-one-out robustness checks for the indirect effect. The thick red line represents the baseline estimation.

Figure 5: Robustness checks

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