REGIONAL POLICY EVALUATION: INTERACTIVE FIXED EFFECTS AND SYNTHETIC CONTROLS

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Abstract—In this paper, we investigate the use of interactive effect or linear factor models in regional policy evaluation. We contrast treatment effect estimates obtained using Bai (2009) with those obtained using difference in differences and synthetic controls (Abadie and coauthors). We show that difference in differences are generically biased, and we derive support conditions for synthetic controls. We construct Monte Carlo experiments to compare these estimation methods in small samples. As an empirical illustration, we provide an evaluation of the impact on local unemployment of an enterprise zone policy implemented in France in the 1990s.

I. Introduction

It is becoming more and more common to evaluate the impact of regional policies using the tools of program evaluation derived from microsettings (see Blundell & Costa-Dias, 2009, or Imbens & Wooldridge, 2011, for surveys). In particular, enterprise and empowerment zone programs have received renewed interest in recent years (see Busso, Gregory, & Kline, 2013; Ham et al., 2012; Gobillon, Magnac, & Selod, 2012). Those programs consist of a variety of locally targeted subsidies aiming primarily at boosting local employment or the employment of residents. Their evaluations use panel data and methods akin to difference in differences that offer the simplest form of control of local unobserved characteristics that can be correlated with the treatment indicator. Nonetheless, specific issues arise when studying regional policies and the tools required to evaluate their impact or to perform a cost-benefit analysis are different from the ones used in more usual microsettings.

The issue of spatial dependence between local units is important in the evaluation of regional policies. Outcomes are likely to be spatially correlated in addition to the more usual issue of serial correlation in panel data. There is thus a need for better control of spatial dependence and, more generally, of cross-section dependence when evaluating regional policies. This is why more elaborate procedures than difference in differences are worth exploring; the use of factors or interactive effects proved to be attractive and fruitful in microstudies (Carneiro, Hansen, & Heckman, 2003). Interactive effect models facilitate the control for cross-section dependence not only because of spatial correlations but also because areas can be close in economic dimensions that depart from purely geographic characteristics. This is the case, for instance, when two local units are affected by the same sector-specific shocks because of sectoral specialization even if these units are not neighbors.

Second, a key issue in policy evaluation is that treatment and outcomes might be correlated because of the presence of unobservables. It should also be acknowledged when using regional data that those unobservables characterizing local units might be multidimensional because the underlying cycles of economic activities of local units are likely to be multiple. Interactive effect models are aimed precisely at allowing the set of unobserved heterogeneity terms or factor loadings that are controlled for to have a moderately large dimension.

Moreover, the estimation of linear factor models in panels is relatively easy, and asymptotic properties of estimates are now well known (Pesaran, 2006; Bai, 2009). Yet there are only a few earlier contributions in the literature that conduct regional policy evaluations using factor models (Kim & Oka, 2014) or using a kindred conditional pseudo-likelihood approach (Hsiao, Ching, & Wan, 2012).

The contributions of this paper are threefold. We first provide results concerning the theoretical setup. We clarify restrictions in linear factor models under which the average treatment on the treated parameter is identified. We analytically derive the generic bias of the difference-in-differences estimator when the true data-generating process has interactive effects and the set of factor loadings is richer than the standard single-dimensional additive local effect. Moreover, we derive from the literature conditions on a number of treatment and control groups, as well as on the number of periods under which factor model estimation delivers consistent estimates of the average treatment on the treated parameter.

Contrasting the estimation of linear factor models with the alternative method of synthetic controls is our second contribution. This alternative method, proposed by Abadie and Gardeazabal (2003), and its properties have been developed and vindicated in a model with factors (Abadie, Diamond, & Hainmueller, 2010). Under the maintained assumption that the true model is a linear factor model, we show that synthetic controls are equivalent to interactive effect methods whenever matching variables (i.e., factor loadings and exogenous covariates) of all treated areas belong to the support of matching variables of control areas, which is assumed to be convex, a case that we call the *interpolation* case. This is no longer true in the *extrapolation* case, that is, when matching variables of one treated area at least do not belong to the support of matching variables in the control group.

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Our third contribution is that we evaluate the relevance and analyze the properties of interactive effect, synthetic control, and difference-in-differences methods by Monte Carlo experiments. We use various strategies for interactive effect estimation. First, a direct method estimates the counterfactual for treated units by linear factor methods in a restricted sample where posttreatment observations for treated units are excluded. The second method estimates a linear factor model that includes a treatment dummy and uses the whole sample. Propensity score matching underlies the third method in which the score is conditioned on factor loading estimates obtained using the first method. Imposing common support constraints on factor loadings when estimating the counterfactual for treated units by linear factor methods provides the fourth method. We contrast these Monte Carlo estimation results with the ones we obtain by using synthetic controls and difference in differences.

We finally provide the results of an empirical application of these methods to the evaluation of the impact of a French enterprise zone program on unemployment exits at the municipality level in the Paris region. This extends our results in Gobillon et al. (2012) in which we were using conditional difference-in-differences methods. We show that the estimated impact is robust to the presence of factors and therefore to cross-section dependence. We also look at other empirical issues of interest, such as the issue of missing data about destination when exiting unemployment and the more substantial issue of the impact of the policy on entries into unemployment.

In the next section, we briefly review the meager empirical literature in which factor models are used to evaluate regional policies. We construct in section III the theoretical setup and write restrictions leading to the identification of the average treatment on the treated in linear factor models. Next, we derive the bias of difference in differences and describe the linear factor model estimation procedures. We derive the conditions that contrast their properties with those of synthetic control methods. Monte Carlo experiments reported in section IV are used to evaluate the small sample properties of the whole range of our estimation procedures. The empirical application and estimation results are presented in section V, and the last section concludes.

II. Review of the Literature

To our knowledge, there are only two earlier empirical contributions, by Hsiao et al. (2012) and Kim and Oka (2014) that apply factor models to the evaluation of regional policies. Interestingly, both papers motivate the use of factor models by contrasting them to the difference-in-differences approach. Hsiao et al. (2012) use an interactive effect model to study the effect on Hong Kong's domestic product of two policies of convergence with mainland China that were implemented at the turn of this century. Their observations consist of various macroeconomic variables measured every quarter over ten years for Hong Kong and countries either in the region or economically associated with Hong Kong. The authors argue that interactive models can be rewritten as models in which interactive effects can be replaced by summaries of outcomes for other countries at the same dates using a conditioning argument. Indeed, common factors can be predicted using this information, but this entails a loss of information since information at the current period only is used to construct these predictions.

Interestingly, Ahn, Lee, and Schmidt (2013) analyze an interactive effect model, and their method, which consists of differencing-out factor loadings, provides potential efficiency improvements over the procedure of Hsiao et al. (2012). The authors indeed show that the parameters of interest are solutions of moment restrictions that do not depend on individual factor loadings. Assuming out any remaining spatial correlation, they show that their GMM estimates are consistent for fixed T.

Kim and Oka (2014) estimate an interactive effect model following Bai (2009) and provide a policy evaluation of the impact of changes in unilateral divorce state laws on divorce rates in the United States. They find that interactive effect estimates are smaller than difference-in-differences estimates and show that the estimation of interactive effect models can bridge the gap between weighted (by state population) and unweighted estimates, which was a cause for debate in the applied literature on the effects of divorce laws.

Overall, in a large N and T environment, the most prominent estimation methods were proposed by Pesaran (2006), who uses regressions augmented with cross-section averages of covariates and outcomes, and by Bai (2009) who uses principal component methods. Westerlund and Urbain (2015) review quite extensively the differences between these methods.

III. Theoretical Setup

Consider a sample composed of $i = 1, \ldots, N$ local units observed at dates $t = 1, \ldots, T$. A simple binary treatment, $D_i \in \{0, 1\}$, is implemented at date $T_D < T$ so that for $t \geqslant T_D > 1$, the units $i = 1, \ldots, N_1$ are treated ($D_i = 1$). Units $i = N_1 + 1, \ldots, N$ are never treated ($D_i = 0$). For each unit, we observe outcomes, y_{it}, which might depend on the treatment, and our parameter of interest is the average effect of the treatment on the treated. In Rubin's notation, we denote by $y_{it}(d)$ the outcome at date t for an individual i whose treatment status is d (where $d = 1$ in case of treatment and $d = 0$ in the absence of treatment). This hypothetical status should be distinguished from random variable D_i describing the actual assignment to treatment in this experiment.

The average effect of the treatment on the treated can be written when $t \geq T_D$:

$$\begin{aligned} &E\left(y_{it}(1) - y_{it}(0) \,|D_i = 1\right) \\ &\quad = E\left(y_{it}(1) \,|D_i = 1\right) - E\left(y_{it}(0) \,|D_i = 1\right). \end{aligned} \tag{1}$$

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A natural estimator of the first right-hand-side term is its empirical counterpart since the outcome in case of treatment is observed for the treated at periods $t \geqslant T_D$. In contrast, the second right-hand-side term is a counterfactual term since the outcome in the absence of treatment is not observed for the treated at periods $t \geqslant T_D$. The principle of evaluation methods relies on using additional restrictions to construct a consistent empirical counterpart to the second right-hand-side term (Heckman & Vytlacil, 2007). For instance, it is well known that difference-in-differences methods are justified by an equal trend assumption,

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1) \\ &\quad = E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0) \text{ for } t \geq T_D, \end{aligned} \tag{2}$$

under which the counterfactual can be written as

$$\begin{aligned} E\left(y_{it}(0) \mid D_i = 1\right) = E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0) \\ + E(y_{i,T_D-1}(0) \mid D_i = 1) \text{ for } t \geq T_D, \end{aligned}$$

in which all terms on the right-hand side are directly estimable from the data.

The object of this section is to generalize the usual setup in which difference in differences provide a consistent estimate of the effect of the treatment on the treated (TT) to a setup allowing for higher-dimensional unobserved heterogeneity terms. Local units treated by regional policies could indeed be affected by various common shocks describing business cycles related, for instance, to different economic sectors. Associated factor loadings would describe the heterogeneity in the exposure of local units to these common shocks. A single-dimensional additive local effect as in the setup underlying difference-in-differences estimation is unlikely to describe this rich economic environment. Furthermore, we know that difference in differences can dramatically fail when heterogeneity is richer than what is modeled (Heckman, Ichimura, & Todd, 1997).

In this paper, we restrict our attention to linear models because the number of units is rather small, although extensions to nonlinear settings could follow the line of Abadie and Imbens (2011) at the price of losing the simplicity of linear factor models. The route taken by Conley and Taber (2011) to deal with small-sample issues might also be worth extending to our setting. More specifically, linearity makes one wary of issues of interpolation and extrapolation, which we highlight in the general framework of linear factor models, as well as in the approach of synthetic controls proposed in the seminal paper by Abadie and Gardeazabal (2003).

We present in section IIIA the specification of a linear factor data-generating process, which is maintained throughout the paper, and we discuss identifying assumptions. We show that the conventional difference-in-differences estimate is generically biased. Next, for a linear factor model that includes a treatment indicator, we derive a rank condition for the identification of the average treatment on the treated. We also propose a direct method whereby we construct the counterfactual term in equation (1) using the samples of control and treated units, albeit the latter before treatment only (see Heckman & Robb, 1985, or Athey & Imbens, 2006). Finally, we describe the approach of synthetic controls and analyze its properties when the true model has interactive effects.

A. *Interactive Linear Effects and Restrictions on Conditional Means*

In the conventional case of difference in differences (DID) (see Blundell & Costa-Dias, 2009), the outcome in the absence of treatment is specified as a linear function,

$$y_{it}(0) = x_{it}\beta + \tilde{\lambda}_i + \widetilde{\delta}_t + \varepsilon_{it}, \tag{3}$$

in which x_{it} is a $1 \times K$ vector of individual covariates and $\tilde{\lambda}_i$ and $\widetilde{\delta}_t$ are individual and time effects. A limit to this specification is that individuals are all affected in the same way by the time effects. To allow interactions and make the specification richer, we specify the outcome in the absence of treatment as a function of the interaction between factors varying over time and heterogeneous individual terms called factor loadings as

$$y_{it}(0) = x_{it}\beta + f_t'\lambda_i + \varepsilon_{it}, \tag{4}$$

in which β are the effects of covariates, λ_i is a $L \times 1$ vector of individual effects or factor loadings, and f_t is an $L \times 1$ vector of time effects or factors. Note that this specification embeds the usual additive model, which is obtained when $\lambda_i = \left(\tilde{\lambda}_i, 1\right)'$ and $f_t = \left(1, \widetilde{\delta}_t\right)'$, as in that case, $f_t'\lambda_i = \tilde{\lambda}_i + \widetilde{\delta}_t$.

The true process generating the data is supposed to be given by equation (4) and is completed by the description of the outcome in case of treatment,

$$y_{it}(1) = y_{it}(0) + \alpha_{it}, \tag{5}$$

which, in contrast to the linear specification above, is not restrictive.

A few usual assumptions complete the description of the true data-generating process (DGP) maintained throughout the paper. First, we assume that we know the number of factors in the true DGP described by equation (4). It might be useful to implement tests regarding the number of factors (Bai & Ng, 2002; Moon & Weidner, 2015), but these tests are fragile (Onatski, Moreira, & Hallin, 2013). Moreover, we adopt the assumption that factors are sufficiently strong so that the consistency condition for the number of factors and consequently for factors and factor loadings is satisfied (for alternative views, see Onatski, 2012, or Pesaran & Tosetti, 2011). This condition reflects the fact that factor loadings can be separated from the idiosyncratic random terms at the limit.[1]

[1] It does not mean that the treatment parameter is not identified under alternative assumptions.

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Moreover, we do not specify the dynamics of factors in the spirit of Doz, Giannone, and Reichlin (2011). Their specification imposes more restrictions on the estimation and inference is more difficult to develop. This is why we stick to the limited information framework, which does not impose conditions on the dynamics of factors, although it could be done in the way explained by Hsiao et al. (2012). Furthermore, the only available explanatory variables are not varying over time in our empirical application. This corresponds to the low-rank regressor assumption defined by Moon and Weidner (2013) and under which identifying assumptions are of a particular form. At this stage, however, we prefer to stick to the more general format.

A final comment is worth making. In treatment evaluation, lagged endogenous variables are at times included as matching covariates in order to control for possible ex ante differences. In spirit, this is very close to a model with interactive effects because it is well known that a simple linear dynamic panel data model like

$$y_{it} = \alpha y_{it-1} + \eta_i + u_{it}$$

can be rewritten as a static model,

$$y_{it} = \alpha^t y_{i0} + \left(1 - \alpha^t\right) \frac{\eta_i}{1-\alpha} + \nu_{it},$$

in which ν_{it} is an AR(1) process. Factors are α^t and $1 - \alpha^t$, and factor loadings are y_{i0} and $\frac{\eta_i}{1-\alpha}$. This argument could be generalized to more sophisticated dynamic linear models.

Restrictions on conditional means. To complete the description of the true data-generating process, we now present and comment on the main restrictions on random terms. To keep notation simple and conform with the usual panel data setup, we generally consider that factors f_t are fixed, while factor loadings λ_i are supposed to be correlated random effects.

We first assume that idiosyncratic terms ε_{it} are orthogonal to factor loadings and that explanatory variables are strictly exogenous,[2]

$$\varepsilon_{it} \perp (\lambda_i, x_i),$$

in which $x_i' = (x_{i1}', \ldots x_{iT}')'$ is a $[T, K]$ matrix. This would be without loss of generality when orthogonality is defined as the absence of correlation as in Bai (2009). Because of the next assumption, we prefer to interpret orthogonality as mean independence and the formal translation of the informal statement above is therefore that:

Assumption 1:

$$E(\varepsilon_{it} \mid \lambda_i, x_i) = 0.$$

[2] The extension to the case with weakly exogeneous regressors would follow Moon and Weidner (2013), for instance.

Second, we extend the usual assumption made in difference-in-differences estimation by assuming that the conditioning set now includes unobserved factor loadings,

$$y_{it}(0) \perp D_i \mid (x_i, \lambda_i) \Leftrightarrow \varepsilon_{it} \perp D_i \mid (x_i, \lambda_i),$$

and we write this condition as a mean independence restriction:

Assumption 2:

$$E(\varepsilon_{it} \mid D_i, \lambda_i, x_i) = E(\varepsilon_{it} \mid \lambda_i, x_i).$$

Note that we do not suppose that (λ_i, x_i) and D_i are uncorrelated and selection into treatment can freely depend on observed and unobserved heterogeneity terms.

Finally, define the average treatment effect over the periods after treatment as

$$\alpha_i = \frac{1}{T - T_D + 1} \sum_{t=T_D}^{T} \alpha_{it},$$

so that our main parameter of interest is the average treatment on the treated over the periods after treatment defined as:[3]

Definition ATT:

$$\alpha = E(\alpha_i \mid D_i = 1) = \frac{1}{T - T_D + 1} \sum_{t=T_D}^{T} E(\alpha_{it} \mid D_i = 1).$$

Assumptions 1 and 2 are the main restrictions in our setup, and definition ATT defines our parameter of interest.

B. The Generic Bias of Difference-in-Differences Estimates

If the true data-generating process comprises interactive effects, we now show that the difference-in-differences estimator is generically biased, although we exhibit two interesting specific cases in which the bias is equal to 0. For simplicity, we omit covariates in this section or, since covariates are assumed to be strictly exogenous, implicitly condition on them. We also assume for simplicity that the probability measure of factor loadings in the treated population, $dG(\lambda_i \mid D_i = 1)$, and in the control population, $dG(\lambda_i \mid D_i = 0)$, are dominated by the Lebesgue measure so that both distributions are absolutely continuous.

We shall show that the condition implied by assumption 2,[4]

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1, \lambda_i) \\ &\quad = E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0, \lambda_i) \text{ for } t \geqslant T_D, \end{aligned} \tag{6}$$

[3] In the case $T \to \infty$, those definitions should be interpreted as limits. Note also that it is generally easy to design estimates for time-specific treatment parameters such as $E(\alpha_{it} \mid D_i = 1)$ by restricting the posttreatment observations to period t only.

[4] This condition is slightly weaker than assumption 2 because it considers differences between periods.

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does not imply equation (2) under which the difference-in-differences estimator is consistent. Indeed:

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1) \\ &\quad = E\left[E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1, \lambda_i) \mid D_i = 1\right] \\ &\quad = \int E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1, \lambda_i) dG(\lambda_i \mid D_i = 1). \end{aligned}$$

Replacing the integrand using equation (6) yields

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1) \\ &\quad = \int E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0, \lambda_i) \\ &\qquad \times dG(\lambda_i \mid D_i = 1). \end{aligned} \tag{7}$$

Two special cases are worth noting. First, the integrand in the previous expression does not depend on λ_i in the restricted case in which there is a single factor $f_t = 1$ and a single individual effect associated with this factor. In this case, equation (7) can be written as

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1) \\ &\quad = E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0) \int dG(\lambda_i \mid D_i = 1) \\ &\quad = E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0), \end{aligned}$$

which yields equation (2) describing equality of trends.

Alternatively, (perfectly) controlled experiments also enable identification through difference in differences in spite of using the alternative argument that $dG(\lambda_i \mid D_i = 1) = dG(\lambda_i \mid D_i = 0)$. The same equation (2) holds, and the treatment parameter is consistently estimable by difference in differences.

This implication is not true in general, and we can distinguish two cases. If the conditional distribution of λ_i in the treated population is dominated by the corresponding measure in the control population,

$$\begin{aligned} &\forall \text{ the Borel set } \Lambda,\ \Pr(\lambda_i \in \Lambda \mid D_i = 0) = 0 \\ &\quad \Longrightarrow \Pr(\lambda_i \in \Lambda \mid D_i = 1) = 0, \end{aligned} \tag{8}$$

the support of treated units is included in the support of non-treated units. We shall describe from now on cases in which support condition (8) holds as an instance of interpolation and, if such a condition is not satisfied, as an instance of extrapolation.

In the interpolation case, let

$$r(\lambda_i) = \frac{dG(\lambda_i \mid D_i = 1)}{dG(\lambda_i \mid D_i = 0)} < \infty,$$

which is well defined because of the support condition (8) and because distributions are absolutely continuous. Write equation (7) as

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1) \\ &\quad = \int E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0, \lambda_i) r(\lambda_i) \\ &\qquad \times dG(\lambda_i \mid D_i = 0), \end{aligned} \tag{9}$$

which in turn implies that

$$\begin{aligned} &E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 1) \\ &\quad = E(y_{it}(0) - y_{i,T_D-1}(0) \mid D_i = 0) \\ &\qquad + Cov\left(y_{it}(0) - y_{i,T_D-1}(0), r(\lambda_i) \mid D_i = 0\right). \end{aligned}$$

The second term on the right-hand side can be interpreted as the differential trend in outcomes due to the time-varying effects of factors interacted with unobserved factor loadings. If there is indeed a factor loading associated with a time-varying factor, the second term is not equal to 0 except under special circumstances, as seen above. In the interpolation case, the second term describes the bias in DID estimates.

In the alternative case of extrapolation, the bias term is derived in a similar way, although its interpretation is less clear since it mixes issues of noninclusive supports with the time-varying effect of factors.

C. *Interactive Effect Estimation in the Whole Sample*

We now explore interactive effect methods and exhibit conditions under which these methods allow the identification of the average treatment on the treated parameter. The observed outcome verifies

$$y_{it} = y_{it}(0)(1 - I_t D_i) + y_{it}(1) I_t D_i,$$

in which D_i is the treatment indicator, and $I_t = 1\{t \geq T_D\}$ is a time indicator of treatment. Using equations (4) and (5) yields

$$y_{it} = \alpha_{it} I_t D_i + x_{it}\beta + f_t' \lambda_i + \varepsilon_{it}. \tag{10}$$

We maintain assumptions 1 and 2 that allow the correlation between D_i and λ_i to be unrestricted so that selection into treatment can depend on factor loadings. Similarly, the correlation between I_t and f_t is unrestricted so that the implementation of the treatment can be correlated with economic cycles that are described here by factors.

We rewrite equation (10) as

$$y_{it} = \alpha I_t D_i + x_{it}\beta + f_t' \lambda_i + \varepsilon_{it} + (\alpha_{it} - \alpha) I_t D_i, \tag{11}$$

in which α is the average treatment on the treated parameter as in definition ATT. Nonetheless, if the number of periods after treatment is greater than 1, this model would not deliver unbiased estimates because of omitted variables. Indeed, we could rewrite model (10) as

$$y_{it} = \alpha_t I_t D_i + x_{it}\beta + f_t' \lambda_i + \varepsilon_{it} + (\alpha_{it} - \alpha_t) I_t D_i, \tag{12}$$

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allowing for a time-varying treatment effect:

$$\alpha_t = E(\alpha_{it} \mid D_i = 1).$$

The omitted variables in equation (11) would be the $T - T_D$ period indicators interacted with the treatment indicator (except 1). For simplicity, we develop our analysis in this section in the simple case in which we have

Assumption 3:

$$\forall t \geq T_D, \qquad \alpha_t = \alpha,$$

so that equation (11) is correctly specified.[5]

We now exhibit further conditions under which α can be identified using interactive effect procedures as proposed by Bai (2009). We start with the case $\beta = 0$, which requires a weak rank condition, and then extend it to the general case with covariates that require an additional assumption that is stronger, albeit easy to interpret.

Average treatment effect on the treated in the absence of covariates. We shall prove that the parameter of interest α is identified under the two conditions that I_t is not equal to a linear combination of factors f_t and that the probability of treatment is positive.

We keep considering that T is fixed, as well as factors f_t and treatment I_t, and we investigate identification as if factors f_t were known. This argument extends to the case in which T tends to infinity by taking limits.

Stack individual observations in individual vectors of dimension $[T, 1]$,

$$y_i = \alpha D_i I_{[1:T]} + F'\lambda_i + \varepsilon_i + \Delta_i I_{[1:T]} D_i, \tag{13}$$

in which $y_i = (y_{i1}, ., y_{iT})'$, $I_{[1:T]} = (I_1, ., I_T)'$, $F = (f_1, ., f_T)$, $\varepsilon_i = (\varepsilon_{i1}, ., \varepsilon_{iT})'$ and Δ_i is a diagonal matrix of dimension $[T, T]$ whose diagonal terms are $(\alpha_{i1} - \alpha, \ldots, \alpha_{iT} - \alpha)$. Set $M_F = I - F'(FF')^{-1}F$, and multiply the previous equation to obtain

$$M_F y_i = \alpha D_i M_F I_{[1:T]} + M_F \varepsilon_i + M_F \Delta_i I_{[1:T]} D_i. \tag{14}$$

A necessary condition for identifying α using equation (14) stacked over the different individual units is therefore

$$I'_{[1:T]} M_F I_{[1:T]} > 0 \text{ and } E(D_i) > 0. \tag{15}$$

This means that $I_{[1:T]}$ is not equal to a linear combination of factors and that the probability of being treated is positive. This is related to the rank condition underlying the identification of parameters in proposition 3 in Bai (2009, p. 1259). Furthermore, this condition is also necessary in equation (13) because the correlation between λ_i and D_i is unrestricted.

[5] The identification of equation (12) can be established using similar developments. The proof is available on request.

This condition is also sufficient. This is because $E(D_i) I'_{[1:T]} M_F I_{[1:T]}$ is invertible using condition (15) and because we can show that

$$\begin{aligned} \alpha &= (E(D_i I'_{[1:T]} M_F I_{[1:T]}))^{-1} E(D_i I'_{[1:T]} M_F y_i) \\ &= (E(D_i) I'_{[1:T]} M_F I_{[1:T]})^{-1} E(D_i I'_{[1:T]} M_F y_i). \end{aligned} \tag{16}$$

Indeed, the covariance between the two right-hand-side terms of equation (14), the regressor $D_i M_F I_{[1:T]}$, and the error term $M_F \varepsilon_i + M_F(\alpha_i - \alpha) I_{[1:T]} D_i$ is equal to 0. There are two terms in this correlation that we analyze in turn.

The first term is equal to 0 by construction (assumption 2) because

$$E(I'_{[1:T]} M_F D_i M_F \varepsilon_i) = E(I'_{[1:T]} M_F D_i M_F E(\varepsilon_i \mid D_i)) = 0, \tag{17}$$

since D_i is a scalar random variable and variables in the time dimension are supposed to be fixed.

The second term of the correlation above is more interesting and involves

$$\begin{aligned} &E(I'_{[1:T]} M_F D_i M_F \Delta_i I_{[1:T]} D_i) \\ &\quad = E(I'_{[1:T]} M_F D_i M_F E(\Delta_i \mid D_i) I_{[1:T]} D_i), \end{aligned} \tag{18}$$

which is equal to 0 by construction of Δ_i since $E(\Delta_i \mid D_i = 1)$ is a diagonal matrix whose diagonal terms are

$$E(\alpha_{it} - \alpha \mid D_i = 1) = \alpha_t - \alpha = 0,$$

by assumption 3. The correlation in equation (18) is then equal to 0.

Finally, multiplying equation (14) by $I'_{[1:T]} M_F D_i$ and taking the expectation gives equation (16). This ends the proof that the average treatment on the treated parameter α is identified under rank condition (15).

The Case with Covariates. In the general case with covariates, we can write equation (11) as

$$y_i = \alpha D_i I_{[1:T]} + x_i \beta + F'\lambda_i + \varepsilon_i + \Delta_i I_{[1:T]} D_i.$$

Multiplying this equation by M_F, we obtain

$$M_F y_i = \alpha D_i M_F I_{[1:T]} + M_F x_i \beta + M_F \varepsilon_i + M_F \Delta_i I_{[1:T]} D_i. \tag{19}$$

Denote the linear prediction of D_i as a function of x_i as

$$D_i = vec(x_i)'\gamma + D_{ix},$$

and rewrite equation (19) as

$$M_F y_i = \alpha D_{ix} M_F I_{[1:T]} + M_F \tilde{\varepsilon}_i + M_F \Delta_i I_{[1:T]} D_i, \tag{20}$$

in which $\tilde{\varepsilon}_i = \varepsilon_i + x_i \beta + \alpha . vec(x_i)' \gamma I_{[1:T]}$. Because x_i and $vec(x_i)$ are uncorrelated with D_{ix}, the same noncorrelation condition as in equation (17) is valid since we have from

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assumptions 1 and 2 that $E(\varepsilon_i | D_i, x_i) = 0$. Thus, the second condition derived from equation (18) that remains to be checked refers to the equality to 0 of

$$\begin{aligned} &E(\Delta_i I_{[1:T]} D_i D_{ix}) \\ &\quad = E(\Delta_i I_{[1:T]} D_i D_i) - E((\Delta_i I_{[1:T]} D_i vec(x_i)'\gamma) \\ &\quad = -E(\Delta_i I_{[1:T]} D_i vec(x_i)'\gamma), \end{aligned}$$

because of the argument employed after equation (18) that uses definition ATT. This term is equal to 0 under the sufficient condition given by

$$\forall t \geq T_D, \qquad E(\alpha_{it} \mid D_i = 1, x_i) = E(\alpha_{it} \mid D_i = 1),$$

since it implies that

$$E(\Delta_i \mid D_i = 1, x_i) = E(\Delta_i \mid D_i = 1) = 0,$$

by assumption 3 and definition ATT as above. This condition is stronger than necessary as it would be sufficient to condition on the scalar variable $vec(x_i)\gamma$.[6] Note also that the interactive effect model could be generalized by conditioning on covariates in an unrestricted way or interacting covariates with the treatment indicator, and this would substantially weaken this condition as in the static evaluation case (Heckman & Vytlacil, 2007).

Consistency and other asymptotic properties of this method can be derived from Bai (2003) when $N \longrightarrow \infty$ and $T \to \infty$. Note also that condition (15) also implies that N_1 tends to ∞ when $N \longrightarrow \infty$. Estimation could also proceed with the estimation method proposed by Ahn et al. (2013) and thus dispense with the assumption that $T \to \infty$. Note that when T is small, Bai's estimator is inconsistent unless errors are white noise (Ahn, Lee, & Schmidt, 2001).

Remarks. First, when we let the number of periods grow, it is interesting to consider again the difference-in-differences estimator that might be consistent when $T \to \infty$ even if the sufficient conditions of section IIIB are not fulfilled.[7] In the absence of covariates, the difference-in-differences estimator is the OLS estimator of the demeaned equation,

$$\begin{aligned} &y_{it} - y_{.t} - y_{i.} + y_{..} \\ &\quad = \alpha(D_i - D_.)(I_t - I_.) + (f_t - f_.)'(\lambda_i - \lambda_.) + \tilde{\varepsilon}_{it}, \end{aligned}$$

in which the notation ".", which replaces an index, points at the average of the variable running over this index, say, for instance, $y_{i.} = \frac{1}{T}\sum_{t=1}^{T} y_{it}$ and $\tilde{\varepsilon}_{it}$ is the demeaned version of the errors. When $N \to \infty$, the bias in the OLS estimator of this equation converges to a term that is proportional to

$$\begin{aligned} &plim_{N\to\infty} \frac{1}{NT} \sum_{i,t} (D_i - D_.)(I_t - I_.)(f_t - f_.)'(\lambda_i - \lambda_.) \\ &\quad = \frac{1}{T} \sum_t (I_t - I_.)(f_t - f_.)' \, plim_{N\to\infty} \frac{1}{N} \\ &\qquad \times \sum_{i,t} (D_i - D_.)(\lambda_i - \lambda_.). \end{aligned} \tag{21}$$

As assumed above, we generically have $plim_{N\to\infty} \frac{1}{N} \sum_{i,t} (D_i - D_.)(\lambda_i - \lambda_.) \neq 0$ because the correlation between D_i and λ_i is different from 0. Even in this case, the DID estimate can nonetheless be consistent when $T \to \infty$ if

$$plim_{T\to\infty} \frac{1}{T} \sum_t (I_t - I_.)(f_t - f_.)' = 0.$$

This condition states that in the long run, treatment and factors are uncorrelated, yet this is not an assumption that one would like to make in all policy evaluations.

Second, it is interesting to develop the reverse of the underspecified case developed in section IIIB. Overspecification arises when a factor model is estimated while the true data-generating process is that of a standard panel with additive individual and time effects. We speculate that results of Moon and Weidner (2015) might be used to show that not only there is no bias but also there is no loss of precision when using a greater number of factors than necessary, at least asymptotically.

D. *Direct Estimation of the Counterfactual*

Assumptions 1 and 2 imply that a direct estimation strategy for the effect of treatment on the treated can also be adopted. Estimate first the interactive effect model (4) using the sample composed of nontreated observations over the whole period and of treated observations before the date of the treatment $t < T_D$. Orthogonality assumption 2 makes sure that excluding observations (i, t) with $i \in \{1, ., N_1\}$ and $t \geq T_D$ does not generate selection. Second, orthogonality assumption 1 renders conditions stated by Bai (2009) valid, and the derived asymptotic properties of linear factor estimates hold.

Various asymptotics can be considered:

- If N and T tend to ∞, then β, f_t, and λ_i for the nontreated are consistently estimated (Bai, 2009).
- If the number of periods before treatment T_D tends to ∞, then λ_i for the treated units are consistently estimated.

As for the counterfactual term to be estimated in equation (1), we have for $t \geqslant T_D$,

$$E(y_{it}(0) | D_i = 1) = E\left(x_{it}\beta + \lambda_i' f_t | D_i = 1\right). \tag{22}$$

To estimate this quantity, we replace parameters λ_i, $i = 1, \ldots, N_1$, β and f_t when $t \geqslant T_D$ by their consistently estimated values in the right-hand-side expression (computed

[6] In this case, developments following Wooldridge (2005) might be appropriate, but we do not follow up on this route in this paper.

[7] We address here additional points made by referees, whom we thank for their suggestions.

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as detailed in the online appendix) and take the empirical counterpart of the expectation. Namely, the treatment on the treated at a given period is derived by using equation (1) and can be written as

$$\begin{aligned} &E\left(y_{it}(1)-y_{it}(0)\,|D_i=1\right)\\ &\quad=E(\alpha_{it}\mid D_i=1)\\ &\quad=E\left(y_{it}(1)\,|D_i=1\right)-E\left(x_{it}\beta+\lambda_i' f_t\,|D_i=1\right), \end{aligned} \tag{23}$$

and its estimate is obtained by replacing unknown quantities by their empirical counterparts. The average treatment on the treated effect is then obtained by exploiting definition ATT and averaging equation (23) over the periods after treatment.[8]

An additional word of caution about identification is in order since the rank conditions (15) developed in the previous section are also necessary. The second condition of equation (15), $E(D_i) > 0$, is straightforward while the first condition in that equation is not as simple to derive. This is summarized in proposition 1.

Proposition 1. *Suppose that the first rank condition in equation (15) does not apply and that the treatment vector $I_{1:T}$ is a linear function of factors,*

$$I_{1:T}=F'\delta,$$

in which δ is an $[L,1]$ vector and F is the matrix of factors as defined above. Then for any value of the treatment effect α, there exists an observationally equivalent factor model in which the value of the treatment effect is equal to 0.

Proof. Let α be any value and write equation (13) as

$$y_i=\alpha I_{1:T}D_i+F'\lambda_i+\tilde{\varepsilon}_i,$$

in which $\tilde{\varepsilon}_i$ includes any idiosyncratic variation of the treatment effect across individuals and periods. By replacing $I_{1:T}=F'\delta$, we get

$$\begin{aligned} y_i&=\alpha F'\delta D_i+F'\lambda_i+\tilde{\varepsilon}_i,\\ &=F'(\alpha\delta D_i+\lambda_i)+\tilde{\varepsilon}_i, \end{aligned}$$

which provides the alternative factor representation in which the value of the treatment effect is equal to 0.

This shows the necessity of condition (15) for the estimation method derived in this section, as well as for any other estimation method analyzed below.

E. A Single-Dimensional Factor Model

It is well known since Rubin and Rosenbaum (1983) that conditions 1 and 2 imply the condition

$$E(\varepsilon_{it}\mid D_i=1,p(x_i,\lambda_i))=0,$$

in which the distinction between observed variables x_i and unobserved variables λ_i does not matter. Let $\mu_i=p(x_i,\lambda_i)$ denote the propensity score.

The condition above suggests the following strategy:

1. Estimate factors and factor loadings using the sample of controls and the subsample of treated observations before treatment as detailed in section IIID.
2. Regress D_i on x_i and $\hat{\lambda}_i$, and construct the predictor of the score $\hat{\mu}_i$.
3. Match on the propensity score à la Heckman, Ichimura, and Todd (1998) or, under some conditions, use a single factor model associated to $\hat{\mu}_i$.

F. Synthetic Controls

The technique of synthetic controls proposed by Abadie and Gardeazabal (2003) and further explored by Abadie, Diamond, and Hainmueller (2010, ADH thereafter) proceeds differently. It focuses on the case in which the treatment group is composed of a single unit and uses a specific matching procedure of this treated unit to the control units whereby a so-called synthetic control is constructed. We proceed in the same way, although as we have potentially more treated units, we repeat the procedure for each of them and then aggregate the result over various synthetic controls to yield the average treatment on the treated.[9]

Presentation. We follow the presentation by ADH (2010). An estimator of $y_{it}(0)$ for a single treated unit $i\in\{1,.,N_1\}$ after treatment $t\geq T_D$ is the outcome of a synthetic control "similar" to the treated unit that is constructed as a weighted average of nontreated units. We impose the similarity of characteristics x_{it} between treated units and synthetic controls by weighting characteristics x_{jt} of control units, $j\in\{N_1+1,.,N\}$ in such a way that

$$\sum_{j=N_1+1}^{N}\omega_j^{(i)}x_{jt}=x_{it}\text{ for }t=1,.,T, \tag{24}$$

where $\omega_j^{(i)}$ is the weight of unit j in the synthetic control (such that $\omega_j^{(i)}\geqslant 0$ and $\sum_{j=N_1+1}^{N}\omega_j^{(i)}=1$).

Similarity between pretreatment outcomes is also imposed in ADH (2010),

$$\sum_{j=N_1+1}^{N}\omega_j^{(i)}y_j^{(k)}=y_i^{(k)}, \tag{25}$$

[8] The variance of the estimator can be computed using formulas in Bai (2003, 2009).

[9] An alternative would be to aggregate the treated units into a single unit first. By analogy with what is done in nonparametric matching, this procedure seems more restrictive because using a single synthetic control leads to less precise estimates than when constructing various synthetic controls. Nonetheless, support conditions for the validity of the synthetic control method that we find might justify such an approach because support requirements are weaker in the aggregate case.

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where $y_j^{(k)} = \sum_{t=1}^{T_D-1} k_t y_{jt}$ is a weighted average of pretreatment outcomes in which $k = (k_1, ., k_{T_D-1})$ are weights differing across periods ($y_i^{(k)}$ for the treated unit is defined similarly). A set of such pretreatment outcome summaries can be generated using various vectors of weights, k. Nevertheless, the most general setting is when we consider all pretreatment outcomes, y_{jt}, for $t = 1, \ldots, T_D - 1$. Indeed, taking linear combinations of pretreatment outcomes or considering the original ones is equivalent in this general formulation, and we dispense with the construction of $y_j^{(k)}$ and $y_i^{(k)}$.

The average treatment on the treated for unit i is estimated as

$$\hat{\alpha}_i = \frac{1}{T - T_D + 1} \sum_{t \geq T_D} \left[y_{it} - \sum_{j=N_1+1}^{N} \omega_j^{(i)} y_{jt} \right]. \tag{26}$$

In practice, one needs to determine the weights that allow the construction of the synthetic control. Weights should ensure that the synthetic control is as close as possible to the treated unit i and thus that conditions (24) and (25) are verified. Denote $z_j = (y_{j1}, ., y_{j,T_D-1}, x_{j1}, ., x_{jT})'$ (resp. z_i) the list of variables over which the synthetic control is constructed (i.e., pretreatment outcomes and exogenous variables). Weights are computed using the following minimization program,

$$\underset{\omega_j^{(i)} \middle| \omega_j^{(i)} \geqslant 0, \sum_{j=N_1+1}^{N} \omega_j^{(i)} = 1}{\text{Min}} \left(\sum_{j=N_1+1}^{N} \omega_j^{(i)} z_j - z_i \right)' \times M \left(\sum_{j=N_1+1}^{N} \omega_j^{(i)} z_j - z_i \right) \tag{27}$$

in which M is a weighting matrix.[10] Note that the resulting weight $\omega^{(i)}$ is a function of the data $(z_i, z_{N_1+1}, ., z_N)$.

Synthetic controls and interactive effects. We now describe this procedure in an interactive effect model setting as first suggested by ADH (2010). Nonetheless, we show that the absence of bias implies constraints on the supports of factor loadings and exogenous variables and is related to the developments in section IIIB.

To proceed, we need to introduce additional notation. Our linear factor model can be written at each time period as

$$\begin{aligned} &Y_t(0) = \beta' X_t' + f_t' \Lambda_U + \varepsilon_t \text{ for the untreated,} \\ &y_{it}(0) = \beta' x_{it}' + f_t' \lambda_i + \varepsilon_{it} \text{ for each treated individual,} \end{aligned} \tag{28}$$

where $\Lambda_U = \left(\lambda_{N_1+1}, \ldots, \lambda_N\right)$ is $(L, N - N_1)$ and f_t is a L column vector. Similarly, $Y_t(0)$ and ε_t are $(N - N_1)$ row vectors, and X_t is a $(N - N_1, K)$ matrix.

Weights $\omega^{(i)} = \left(\omega_{N_1+1}^{(i)}, \ldots, \omega_N^{(i)}\right)$ are obtained by equation (27), and we have

$$\begin{cases} y_{it}(0) = Y_t(0)\,\omega^{(i)} + \eta_{it} \text{ for } t < T_D, \\ x_{it}' = X_t' \omega^{(i)} + \eta_{itX} \text{ for } t = 1, \ldots, T. \end{cases} \tag{29}$$

Note that the construction of the synthetic control by equation (29) is allowed to be imperfectly achieved and the discrepancy is captured by the terms η_{it} and η_{itX}. We thus acknowledge that characteristics of the treated unit, $z_i = (y_{i1}, ., y_{i,T_D-1}, x_{i1}, ., x_{iT})'$, might not belong to the convex hull, C_U, of the characteristics of control units. First, there are small sample issues when the number of pretreatment periods, $T_D - 1$, and of covariates, KT, is larger than the number of untreated units, $N - N_1$. In other words, the convex hull C_U lies in a space whose dimension is lower than the number of vector components, $T_D - 1 + KT$. Second, and more important, even if $T_D - 1 + KT < N - N_1$, vector z_i might not belong to this convex hull because supports of characteristics for treated and control units differ. Terms η_{it} and η_{itX} capture this discrepancy.

We now analyze what consequences this construction has on the estimation of the treatment effect. The estimated treatment effect given by equation (26) is a function of

$$\begin{aligned} y_{it} - \sum_{j=N_1+1}^{N} \omega_j^{(i)} y_{jt} &= y_{it}(1) - Y_t(0)\,\omega^{(i)} \\ &= \alpha_{it} + y_{it}(0) - Y_t(0)\,\omega^{(i)} \\ &= \alpha_{it} + \eta_{it}, \end{aligned}$$

in which we have extended definition (29) to all $t \geq T_D$. The absence of bias for the left-hand side estimate with respect to $E(\alpha_{it})$ can thus be written as $E(\eta_{it}) = 0$. To write this condition as a function of primitives, we need to replace dependent variables by their values in the model described by equation (28). This gives

$$\begin{aligned} \eta_{it} &= y_{it}(0) - Y_t(0)\,\omega^{(i)} \\ &= \beta' x_{it}' + f_t' \lambda_i + \varepsilon_{it} - (\beta' X_t' + f_t' \Lambda_U + \varepsilon_t)\omega^{(i)}, \\ &= \beta'(x_{it}' - X_t' \omega^{(i)}) + f_t'(\lambda_i - \Lambda_U \omega^{(i)}) + \varepsilon_{it} - \varepsilon_t \omega^{(i)}. \end{aligned}$$

Considering that β and f_t are fixed and taking expectations yields

$$\begin{aligned} E(\eta_{it}) &= \beta' E(x_{it}' - X_t' \omega^{(i)}) + f_t' E(\lambda_i - \Lambda_U \omega^{(i)}) \\ &\quad + E(\varepsilon_{it} - \varepsilon_t \omega^{(i)}), \\ &\simeq \beta' E(x_{it}' - X_t' \omega^{(i)}) + f_t' E(\lambda_i - \Lambda_U \omega^{(i)}), \end{aligned}$$

in which we have used the result derived by ADH (2010) that $E(\varepsilon_{it} - \varepsilon_t \omega^{(i)})$ tends to 0 when the number of pretreatment

[10] M can be chosen in various ways (see Abadie et al., 2010, for some guidance). In our case, we set M to the identity matrix. There could also exist multiple solutions to this program if the treated observation belongs to the convex hull of the controls. Abadie, Diamond, and Hainmueller (2015) suggest using a refinement by selecting the convex combination of the specific points that are the closest to the treated observation (see their footnote 12).

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periods T_D tends to ∞.[11] This expression should be true for any value of β and f_t and the absence of bias thus implies that

$$E(x'_{it} - X'_t\omega^{(i)}) = 0 \text{ and } E(\lambda_i - \Lambda_U\omega^{(i)}) = 0. \tag{30}$$

The following sufficient condition is established in the appendix:

Lemma 1. *If the support of exogenous variables and factor loadings of the treated units is a subset of the support of exogenous variables and factor loadings of the nontreated units and this latter set is convex and bounded, then condition (30) is satisfied at the limit when $N - N_1 \to \infty$.*

We call this the interpolation case, and this relates to the familiar support condition in the treatment effect literature and to the domination relationship between probability measures in the treated and control groups seen in equation (8).

If the support of controls does not contain the support of treated observations, the synthetic control method is based on extrapolation since it consists of projecting λ_i and x_{it} onto a convex set to which they do not belong, and this generates a bias. For instance, to compute the distance between λ_i and the convex hull of the characteristics of the controls denoted $conv\,(\Lambda_U)$, we could use the support function (see Rockafellar, 1970) and show that

$$d\,(\lambda_i, conv\,(\Lambda_U)) = \inf_{q\in\mathbb{R}^L}\left[\max_{j\in\{N_1+1,\ldots,N\}}\left(q'\lambda_j\right) - q'\lambda_i\right]$$

in which λ_j is the jth column of Λ_U. Statistical methods to deal with inference in this setting could be derived from recent work by Chernozhukov, Lee, and Rosen (2013), but this is out of the scope of this paper.

More generally, synthetic control is a method based on convexity arguments and thus needs assumptions based on convexity. The case of discrete regressors is a difficult intermediate case between interpolation and extrapolation that inherits the "bad" properties of extrapolation. In consequence, we conjecture that the synthetic cohort estimate is generically biased.

IV. Monte Carlo Experiments

A. The Setup

The data-generating process is supposed to be given by a linear factor model,

$$y_{it} = \alpha_i I_t D_i + f_t'\lambda_i + \varepsilon_{it},$$

in which the treatment effect, α_i, is homogeneous or heterogeneous across local units but not time and the number of factors, L, is variable. We always include additive individual and time effects (i.e., $\lambda_i = (\lambda_{i1}, 1, \lambda_{i2}, \ldots)'$ and $f_t = (1, f_{t1}, f_{t2}\ldots)'$) as most economic applications would require. We did not include any other explanatory variables than the treatment variable itself.

The data-generating process is constructed around a baseline experiment and several alternative experiments departing from the baseline in different dimensions, such as the distribution of disturbances, the assumption that they are identically and independently distributed (i.i.d.), the number of local units and periods, the correlation of treatment assignment and factor loadings, the structure of factors, the support of factor loadings, and the heterogeneity of the treatment effect, α_i. Experiments are described in detail below or in the online appendix. The Monte Carlo aspect of each experiment is given by drawing new values of $\{\varepsilon_{it}\}_{i=1,..,N,t=1,..,T}$ only, and the number of replications is set to 1,000.

In the baseline, individual and period shocks ε_{it} are i.i.d. and drawn in a zero-mean and unit-variance normal distribution.

The number of treated units, N_1 (resp. total, N) and the number of periods before treatment, T_D (resp. total, T), as well as the number of factors L, are fixed at relatively small values in line with our empirical application developed in the next section, and more generally, with data used in the evaluation of regional policies. In the baseline experiment, we fix $(N_1, N) = (13, 143)$, $(T_D, T) = (8, 20)$, and $L = 3$ (including one additive factor). We also experiment with L varying in the set $\{2, 4, 5, 6\}$.

The values of factors f_t and factor loadings λ_i are drawn once and for all in each experiment. Factors f_t, for $t = 1, ., T$, are drawn in a uniform distribution on $[0, 1]$ (except the first factor, which is constrained to be equal to 1). Alternatively, we also experiment by fixing the second factor in f_t to the value $a\sin(\pi t/T)$ with $a > 0$ large enough.

The support of factor loadings, λ_i, is the same for treated units as for untreated units in our baseline experiment. They are drawn in a uniform distribution on $[0, 1]$ (except the second factor loading, which is constrained to be equal to 1). In an alternative experiment, we construct overlapping supports for treated and untreated units. This is achieved by shifting the support of factor loadings of treated units by .5 or, equivalently, by adding .5 to draws. In another experiment, supports of treated and untreated units are made disjoint by shifting the support of treated units by 1. Because the original support is $[0, 1]$, this means that the intersection of the supports of treated and nontreated units is now reduced to a point. Note that adding .5 (resp. 1) to draws of treated units spawns a positive correlation between factor loadings and the treatment dummy D_i equal to .446 (resp. .706).

In the baseline experiment, the treatment effect is fixed to a constant, $\alpha_i = .3$, which is a value close to ten times the one obtained in our empirical application.

[11] The main difficulty there is to take into account that ω is a random function of z_i and z_j

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B. Estimation Methods

We evaluate six estimation methods:

1. A direct approach using pretreatment period observations for control and treated units and posttreatment periods for the nontreated only to estimate factors f_t and λ_i in the equation

$$y_{it}(0) = f_t'\lambda_i + \varepsilon_{it}, \tag{31}$$

as in section IIID. The estimation procedure follows Bai's methodand is based on an EM algorithm, which is detailed in online appendix A.1. A parameter estimate of α is then recovered from equation (23) replacing the right-hand-side quantities by their empirical counterparts. This estimator is labeled "interactive effects, counterfactual."

2. An approach whereby we estimate parameter α applying Bai's method to the linear model in which a treatment dummy is the only regressor,

$$Y_{it} = \alpha I_t D_i + f_t'\lambda_i + \varepsilon_{it},$$

as in section IIIC. The resulting estimator is labeled "interactive effects, treatment dummy."

3. A matching approach (section IIIE) by which equation (31) is first estimated as in the first estimation method. This yields estimates of λ_i from which a propensity score discriminating treated and untreated units is computed. We use a logit specification for the score and construct the counterfactual outcome in the treated group in the absence of treatment at periods $t \geqslant T_D$ using the kernel method proposed by Heckman et al. (1998). If we denote the score predicted by the logit model by $\hat{\mu}_i$, the counterfactual of the outcome for a given treated local unit i at a given posttreatment period is constructed as

$$\widehat{E}(y_{it}(0)\,|D_i = 1) = \sum_{j=N_1+1}^{N} K_h(\hat{\mu}_i - \hat{\mu}_j)\, y_{jt} \Bigg/ \sum_{j=N_1+1}^{N} K_h(\hat{\mu}_i - \hat{\mu}_j) \quad \text{for } t \geqslant T_D,$$

where $K_h(\cdot)$ is a normal kernel whose bandwidth is chosen using a rule of thumb (Silverman, 1986). An estimator of the average treatment on the treated is the average of $y_{it} - \widehat{E}(y_{it}(0)\,|D_i = 1)$ over the population of treated local units for dates $t \geqslant T_D$. The resulting estimator is labeled "interactive effects, matching."

4. An approach similar to "interactive effects, counterfactual" in which we impose the constraint $\lambda_i = \Lambda_U \omega^{(i)}$ for any unit i when estimating equation (31). Λ_U is the $L \times (N - N_1)$ matrix comprising untreated factor loadings, and $\omega^{(i)}$ are weights obtained in the synthetic control method. The estimation method is detailed in online appendix A.2 and the estimator of α is recovered from equation (23), replacing right-hand-side quantities by their empirical counterpart. This estimator is labeled "interactive effects, constrained."
5. The synthetic control approach (section IIIF), whereby the average treatment on the treated is obtained by averaging equation (26) over the population of treated units. The resulting estimator is labeled "synthetic controls."
6. A standard difference-in-differences approach whereby we compute the FGLS estimator taking into account the covariance matrix of residuals (written in first difference). Recent research presented in Brewer, Crossley, and Joyce (2013) suggests that this is the appropriate procedure if assumptions underlying difference in differences are satisfied. The resulting estimator is labeled "diff-in-diffs."

In our simulations, the number of iterations for Bai's method involved in methods 1 to 4 is fixed to 20, and the number of iterations for the EM algorithm involved in method 1 and 4 is fixed to 1. When an estimation method using Bai's approach is implemented, we use the true number of factors.[12]

C. Results

Our parameter of interest is α, and we report the empirical mean bias, median bias, and standard error of each estimator for every Monte Carlo experiment. Results in the baseline case are presented in column 1 of table 1 and, unsurprisingly, show that the estimated treatment parameter exhibits little bias for all methods controlling for interactive factors: "Interactive effects, counterfactual," "Interactive effects, treatment dummy," "Interactive effects, matching," "Interactive effects, constrained" and "Synthetic controls." Similarly, the difference-in-differences method, is unbiased in spite of not accounting for interactive factors since factor loadings are orthogonal to the treatment indicator in the baseline experiment.

Interestingly, among methods allowing for interactive factors, those with constraints are the ones achieving the lowest standard errors ("Interactive effects, constrained" and "Synthetic controls") since using constraints that bind in the true model increases power. Note also that the standard error is larger when using the method "Interactive effects, counterfactual" than when using the method "Interactive effects, treatment dummy" as the structure of the true model after treatment in the treated group is not exploited. Difference-in-differences standard errors lie between those values.

In columns 2 and 3 of table 1, we report results when shifting by .5 or 1 the support of individual factors for the treated.

[12] Monte-Carlo simulations are implemented in R. Weights $\omega^{(i)}$ in methods 4 and 5 are computed using the R procedure "lsei" and the minimization algorithm "solve.QP."

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TABLE 1.—MONTE CARLO RESULTS: VARIATION OF SUPPORT

Support Difference	0	.5	1
Interactive effects, counterfactual	**0.009**	**−0.045**	**−0.115**
	0.004	*−0.046*	*−0.122*
	[0.174]	[0.204]	[0.248]
Interactive effects, treatment dummy	**0.009**	**−0.043**	**−0.093**
	0.005	*−0.046*	*−0.100*
	[0.155]	[0.172]	[0.284]
Interactive effects, matching	**0.007**	N.A.	N.A.
	0.006	N.A.	N.A.
	[0.154]	N.A.	N.A.
Interactive effects, constrained	**−0.008**	**0.413**	**0.732**
	−0.005	*0.418*	*0.720*
	[0.107]	[0.128]	[0.238]
Synthetic controls	**−0.017**	**0.661**	**1.510**
	−0.018	*0.660*	*1.510*
	[0.104]	[0.121]	[0.185]
Difference-in-differences	**0.016**	**−0.052**	**−0.130**
	0.020	*−0.044*	*−0.134*
	[0.136]	[0.135]	[0.134]

Data-generating process: Number of observations: $(N_1, N) = (13, 143)$; number of periods: $(T_D, T) = (8, 20)$; number of individual effects (including an additive one): $L = 3$; treatment parameter: $\alpha = .3$; time and individual effects of the nontreated drawn in a uniform distribution [0, 1]; individual effects of the treated drawn in a uniform distribution $[0 + s, 1 + s]$ with $s \in \{0, .5, 1\}$ reported at the top of column, errors drawn in a normal distribution with mean 0 and variance 1.

Estimation methods are detailed in section IVA. $S = 1{,}000$ simulations are used. The average (resp. median) estimated bias is reported in bold (resp. italic). The empirical standard error is reported in brackets. Results for "Interactive effects, matching" are not reported when $s \in \{.5, 1\}$, as in some simulations, some treated and nontreated observations might be completely separated. As a consequence, the logit model used to construct the propensity score is not identified.

TABLE 2.—MONTE CARLO RESULTS; VARIATION OF SUPPORT, ONE SINUSOIDAL FACTOR

Support Difference	0	.5	1
Interactive effects, counterfactual	**0.004**	**0.007**	**0.030**
	0.010	*0.014*	*0.026*
	[0.158]	[0.166]	[0.233]
Interactive effects, treatment dummy	**0.002**	**−0.009**	**−0.002**
	0.006	*−0.015*	*−0.007*
	[0.143]	[0.154]	[0.209]
Interactive effects, matching	**0.002**	N.A.	N.A.
	0.006	N.A.	N.A.
	[0.136]	N.A.	N.A.
Interactive effects, constrained	**0.005**	**0.426**	**0.798**
	0.009	*0.425*	*0.805*
	[0.104]	[0.119]	[0.213]
Synthetic controls	**0.010**	**0.633**	**1.420**
	0.013	*0.637*	*1.420*
	[0.102]	[0.120]	[0.206]
Difference-in-differences	**−0.087**	**0.209**	**0.518**
	−0.087	*0.204*	*0.519*
	[0.134]	[0.134]	[0.137]

Data-generating process: Number of observations: $(N_1, N) = (13, 143)$; number of periods: $(T_D, T) = (8, 20)$; number of individual effects (including an additive one): $L = 3$; treatment parameter: $\alpha = .3$; one interactive time effect is the deterministic sinusoid $5.\sin(\pi t/T)$; other time effects and individual effects of the nontreated drawn in a uniform distribution [0, 1]; individual effects of the treated drawn in a uniform distribution $[0 + s, 1 + s]$ with $s \in \{0, .5, 1\}$ reported at the top of column; errors drawn in a normal distribution with mean 0 and variance 1.

Estimation methods are detailed in section IVA. $S = 1000$ simulations are used. The average (resp. median) estimated bias is reported in bold (resp. italic). The empirical standard error is reported in brackets. Results for "Interactive effects, matching" are notreported when $s \in \{.5, 1\}$ as, in some simulations, some treated and nontreated observations might be completely separated. As a consequence, the logit model used to construct the propensity score is not identified.

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These shifts have two consequences. First, the validity conditions are now violated for interactive effect estimation, which uses support constraints ("Interactive effects, constrained"), and for synthetic controls. Second, they make factor loadings correlated with the treatment dummy. Results show that all methods are severely biased except "Interactive effects, counterfactual," "Interactive effects, treatment dummy," and, more surprising, difference in differences. The two first methods are designed to properly control for interactive effects and factor loadings whatever the assumption about supports or about correlations between factor loadings and treatment. The bias for "Diff-in-diffs" is close to 0 because the correlation between the factors and time indicators of treatment is close to 0 (see equation [21]). We investigate further below the bias in a case in which they are correlated.

The method "Interactive effects, matching" does not work well because nontreated units close to treated units in the space of factor loadings are hard to find since the support for the treated has been shifted. We thus abstain from reporting the related results. As expected, the bias obtained for "Interactive effects, constrained" and "Synthetic controls" is large. These methods indeed impose that individual effects of treated units can be expressed as a linear combination of individual effects of nontreated units. These constraints are violated with a positive probability when the treated unit support is shifted by .5, and they are always violated when the support is shifted by 1.

To investigate further the cause of the surprising small bias of "Diff-in-diffs" in the previous table, we modified the structure of factors in the experiment. The first factor in f_t is now fixed to $5\sin(\pi t/T)$, and this implies that factors and time indicators of treatment, I_t, are now correlated.[13] Table 2 shows that the "Diff-in-diffs" method can generate much larger biases in this alternative setting while biases of other methods remain the same. It is even the case that small sample biases of "Interactive effects, counterfactual," and "Interactive effects, treatment dummy" become smaller in this alternative experiment.

We then make the number of individual effects vary between two and six (including one individual additive effect) to assess to what extent the accuracy of estimates decreases with the number of factors. Results reported in table 3 show that for the first three methods—"Interactive effects, counterfactual," "Interactive effects, treatment dummy," and "Interactive effects, matching"—the bias does not vary much and remains below 10%. Interestingly, whereas the standard error markedly increases with the number of factors for the method "Interactive effects, counterfactual," it increases much more slowly for the method "Interactive effects, treatment dummy." This occurs because factor loadings of the treated are estimated using pretreatment periods only in the former case, whereas in the latter case, all periods contribute to the estimation of factor loadings. When using methods with constraints "Interactive effects, constrained" and "Synthetic controls," the bias can be larger than 10%, but standard errors remain small. As in the baseline case, the bias of "Diff-in-diffs" is rather small,

[13] This correlation disappears when $T \to \infty$ as noted by a referee.

TABLE 3.—MONTE CARLO RESULTS: VARIATION OF THE NUMBER OF FACTORS

Number of Individual Effects	2	3	4	5	6
Interactive effects, counterfactual	**0.020**	**0.020**	**0.022**	**0.016**	**0.010**
	0.019	*0.024*	*0.020*	*0.019*	*−0.011*
	[0.160]	[0.173]	[0.226]	[0.301]	[0.610]
Interactive effects, treatment dummy	**0.021**	**0.019**	**0.013**	**0.015**	**0.013**
	0.020	*0.022*	*0.015*	*0.019*	*0.010*
	[0.147]	[0.147]	[0.167]	[0.182]	[0.192]
Interactive effects, matching	**0.018**	**0.015**	**0.011**	**0.021**	**0.015**
	0.018	*0.017*	*0.010*	*0.016*	*0.025*
	[0.149]	[0.157]	[0.174]	[0.206]	[0.234]
Interactive effects, constrained	**0.009**	**−0.005**	**−0.027**	**−0.011**	**−0.028**
	0.009	*−0.007*	*−0.029*	*−0.014*	*−0.031*
	[0.111]	[0.107]	[0.109]	[0.112]	[0.118]
Synthetic controls	**0.003**	**−0.016**	**−0.045**	**−0.022**	**−0.040**
	0.004	*−0.017*	*−0.047*	*−0.023*	*−0.04*
	[0.110]	[0.105]	[0.105]	[0.110]	[0.116]
Difference-in-differences	**0.023**	**0.020**	**0.018**	**0.028**	**0.024**
	0.022	*0.023*	*0.019*	*0.024*	*0.021*
	[0.137]	[0.132]	[0.136]	[0.136]	[0.136]

Data-generating process: Number of observations: $(N_1, N) = (13, 143)$; number of periods: $(T_D, T) = (8, 20)$; number of individual effects (including an additive one): $L \in \{2, 3, 4, 5, 6\}$ with L reported at the top of column; treatment parameter: $\alpha = .3$, time and individual effects drawn in a uniform distribution [0, 1]; errors drawn in a normal distribution with mean 0 and variance 1. Estimation methods are detailed in section IVA. $S = 1{,}000$ simulations are used. The average (resp. median) estimated bias is reported in bold (resp. italic). The empirical standard error is reported in brackets.

although we know from the previous analysis that changing the structure of factors could make the bias larger.

Two interesting conclusions in this analysis bear on our empirical application. First, the method of "Interactive effect, counterfactual" seems to be dominated in terms of bias and precision by the method "Interactive effect, treatment dummy" in all experiments, and we thus retain only the second method. Second, the three methods "Interactive effects, matching," "Interactive effects, constrained," and "Synthetic controls" seem to behave similarly. Therefore, we retain only one method, synthetic controls, for our application.

D. *Other Experiments*

In online appendix B, we detail additional Monte Carlo simulation results when the distribution of errors is uniform, when there are fewer pretreatment and posttreatment periods, and when the number of local units is larger. Results conform with intuition.

We also report there, results when disturbances are not i.i.d. Heteroskedasticity is introduced by drawing variances from a distribution with two points of support, with probability 1/2 for each point. We change the ratio of the two variance values across experiments. Alternatively, serial dependence is modeled as autoregressive of order 1, and we change serial correlation across experiments. This allows us to show that the number of periods that we considered $T = 20$ in line with our empirical application below is sufficiently large for the asymptotic results developed in Bai (2009) to be valid. We find very little evidence of bias, and the asymptotic variance of estimates obtained in the i.i.d. setting is a rather good approximation to the experimental variance. In other words, small sample biases shown by Ahn et al. (2001) could be neglected when $T = 20$.

V. Empirical Application

Our application is motivated by the evaluation reported in Gobillon et al. (2012) of an enterprise zone program implemented in France on January 1, 1997. A survey of enterprise zone programs in the United States and the United Kingdom is presented in this article, as well as many particulars that we do not have the space to develop here. The fiscal incentives given by the program to enterprise zones were uniform across the country and consisted of a series of tax reliefs on property holding, corporate income, and, above all, wages. The key measure was that firms needed to hire at least 20% of their labor force locally (after the third worker hired) in order to be exempted from employers' contributions to the national health insurance and pension system. This is a significant tax exemption that represents around 30% of whole labor costs (gross wage). It was expected that this measure would affect labor demand for residents of these zones and decrease unemployment. This is why we analyze the impact of such a program on unemployment entries and exits over this period.

We restrict our analysis to the Paris region in which nine enterprise zones (*Zones franches urbaines*) were created in 1997. Municipalities or groups of municipalities had to apply to the program, and projects were selected taking into account their ranking given by a synthetic indicator. This indicator, whose values have never been publicly released, aggregates five criteria: the population of the zone, its unemployment rate, the proportion of young people (less than 25 years old), the proportion of workers with no skill, and finally the income level in the municipality in which the enterprise zone would be located. An additional criterion is that the proposed zone should have at least 10,000 inhabitants. Nevertheless, the views of local and central government representatives who intervened in the geographic delimitation of the zones also played a role in the selection process. It thus suggests that although the selection of treated areas should be conditioned on the criteria of the synthetic indicator, it is likely that there is sufficient variability in the selection process due to political tampering. As a consequence, assumptions underlying matching estimates are not a priori invalid if observed heterogeneity is controlled for. Indeed, the supports of the propensity score in treated and nontreated municipalities largely overlap, though there are some outliers, as shown in online appendix C.3.

In Gobillon et al. (2012), we provided evidence that controlling for the effect of individual characteristics of the unemployed when studying unemployment exits only moderately affects the treatment evaluation. This is why we use raw data at the level of each municipality in this empirical analysis. Furthermore, the destination after an unemployment exit, to either a job or unemployment, is quite uncertain in the data since an unemployment spell is often terminated

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because the unemployed worker is absent at a control. Many exits to a job might be hidden in the category "Absence at a control." The empirical contribution of our paper is that we investigate not only exits to a job, as in Gobillon et al. (2012), but also unknown exits, as well as entries into unemployment. More generally, we assess the robustness of the results when using estimation methods that deal with the presence of a larger set of unobserved heterogeneity terms than difference in differences.

A. Data

We use the historical file of job applicants to the National Agency for Employment (Agence nationale pour l'emploi, ANPE) for the Paris region. This data set covers the large majority of unemployment spells in the region given that registration with the national employment agency is a prerequisite for unemployed workers to claim unemployment benefits in France. We use a flow sample of unemployment spells that started between July 1989 and June 2003 and study exits from unemployment between January 1993 and June 2003. This period includes the implementation date of the enterprise zone program (January 1, 1997) and allows us to study the effect of enterprise zones not only in the short run but also in the medium run. These unemployment spells may end when the unemployed find a job, drop out of the labor force, leave unemployment for an unknown reason, or when the spell is right-censored.

Regarding the geographic scale of analysis, given that enterprise zones are clusters of a significant size within or across municipalities, it would be desirable to try to detect the effect of the policy at the level of an enterprise zone and comparable neighboring areas. Nevertheless, our data do not let us work at such a fine scale of disaggregation, and we retain municipalities as our spatial units of analysis. Municipalities have on average twice the population of the enterprise zone they contain. As a consequence, any effect at the municipality level measures the effect of local job creation net of within-municipality transfers.

The Paris metropolitan region on which we focus is inhabited by 10.9 million people and subdivided into 1,300 municipalities. We use only municipalities that have between 8,000 and 100,000 inhabitants, as every municipality comprising an enterprise zone has a population within this range. Using propensity score estimation, we select as controls municipalities whose score is close to the support of the score for treated municipalities, and this further restricts our working sample to 148 municipalities (135 controls and 13 treatments). On average, about 300 unemployed workers find a job each half-year in each of those municipalities. In view of these figures, we chose half-years as our time intervals, since using shorter periods would generate too much sampling variability.

Descriptive statistics relative to exits to a job, exits to unemployment, and exits for unknown reasons can be found in online appendix C.2.

B. Results

In Table 4, we report estimation results of the enterprise zone treatment effect obtained with the most promising methods that were evaluated in the Monte Carlo experiments.[14] As explained at the end of the previous section, we use the interactive effect model with a treatment dummy and the synthetic control approach, and contrast them with the most popular method of difference in differences. Standard errors of the "Interactive effect, treatment dummy" estimates are computed using i.i.d. disturbances, an assumption we justify below.

We also derive a confidence interval for the synthetic control estimate that, as far as we know, has not been derived in the literature. We construct this confidence interval by inverting a test statistic whose distribution is obtained by using permutations between local units under the (admittedly strong) assumption of exchangeable disturbances across local units. The procedure is as follows. Subtract the synthetic control estimate $\hat{\alpha}$ from posttreatment outcomes of treated units. Next, draw 10,000 times without replacement 13 units in the whole population (treated and controls) and consider them as the new treated units, while the other 135 are the new controls. Construct synthetic controls in each sample, and estimate the average treatment effect. Derive the estimated quantiles $\hat{q}_{0.025}$ and $\hat{q}_{0.975}$ from the empirical distribution of estimates. Consider now any null hypothesis $H_0 : \alpha = \alpha_0$ and reject it at level 5% when $\alpha_0 - \hat{\alpha}$ does not belong to the interval bounded by those quantiles. Inverting this test yields the confidence interval, $[\hat{\alpha}+\hat{q}_{0.025}, \hat{\alpha}+\hat{q}_{0.975}]$, reported in table 4. Note that we use a nonpivotal statistic in the absence of any result about asymptotic standard errors of the synthetic control estimates. As a consequence, the confidence interval has no refined asymptotic properties.

We analyze three outcomes at the level of municipalities constructed for each six-month period between July 1993 and June 2003: exit from unemployment to a job, exit from unemployment for unknown reasons, and entry into unemployment. The outcome describing unemployment exits (to a job or for unknown reasons) is defined as the logarithm of the ratio between the number of unemployed workers exiting during the period and the number of unemployed at risk at the beginning of the period. Entries are defined in the same way. Table 4 reports results using our three estimation methods for each outcome.

Starting with exits to a job, we find a small positive and significant treatment effect using the interactive effect method in line with the "Diff-in-diffs" estimate and with the findings in Gobillon et al. (2012) in which we used difference in differences but with a more limited number of periods.[15] The size of the interactive effect estimate is

[14] The only slight modification is that for the FGLS first difference estimate, the covariance matrix is kept general enough to allow for serial correlation of unknown form.

[15] This was based on an analysis distinguishing short-run and long-run effects of the program.

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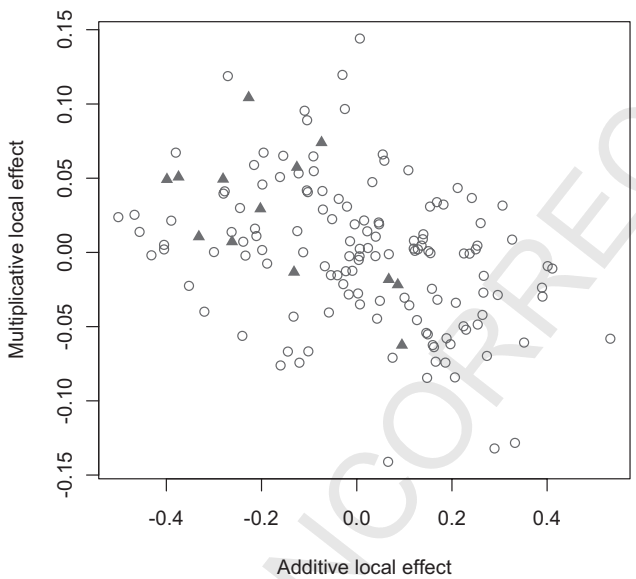
TABLE 4.—ESTIMATED ENTERPRISE ZONE PROGRAM EFFECTS ON UNEMPLOYMENT EXITS AND ENTRY

Number of Individual Effects	2	3	4	5	6
Exit rate to a job					
Interactive effects,	0.032	0.036	0.039	0.043	0.046
treatment dummy	[−0.001; 0.065]	[−0.001; 0.073]	[0.006; 0.072]	[0.010; 0.076]	[0.015; 0.077]
Synthetic controls			−0.026		
			[−0.081; 0.013]		
Diff-in-diffs			0.028		
			[−0.003; 0.059]		
Exit rate for unknown reasons					
Interactive effects,	0.025	0.003	0.002	0.004	0.005
treatment dummy	[−0.012; 0.062]	[−0.032; 0.038]	[−0.029; 0.033]	[−0.027; 0.035]	[−0.024; 0.034]
Synthetic controls			0.046		
			[0.000; 0.091]		
Diff-in-diffs			0.019		
			[−0.012; 0.050]		
Entry rate					
Interactive effects,	0.007	0.006	0.004	0.008	0.007
treatment dummy	[−0.022; 0.036]	[−0.021; 0.033]	[−0.021; 0.029]	[−0.023; 0.039]	[−0.022; 0.036]
Synthetic controls			0.007		
			[−0.019; 0.034]		
Diff-in-diffs			0.020		
			[−0.004; 0.044]		

Outcomes are computed in logarithms at the municipality level. The number of observations $(N_1, N) = (13, 148)$ and the number of periods $(T_D, T) = (8, 20)$. The estimated coefficient is the first reported figure. Its 95% confidence interval is given in brackets. For the estimation method "Interactive effects, treatment dummy," the confidence interval is computed considering that errors are i.i.d. For the estimation method "Diff-in-diffs," the feasible general least square estimator is computed assuming a constant within-municipality unrestricted covariance matrix. For "*Synthetic controls*," the confidence interval is computed as explained in the text under the assumption of exchangeable errors.

FIGURE 1.—ADDITIVE AND MULTIPLICATIVE LOCAL EFFECTS, EXIT TO A JOB



Local effects are estimated using the method "Interactive model, treatment dummy" for the specification including the treatment dummy, an additive local effect and one multiplicative local effect only. Circles: control municipalities; triangles: treated municipalities.

slightly larger than the difference-in-differences estimate and tends to increase with the number of factors included in the estimation. In contrast, the "Synthetic control" estimate is negative, although the estimated confidence interval is so large that this estimate is not significantly different from 0 at a level of 5%.

In the Monte Carlo experiment, differences between interactive effect estimates and other estimates were interpreted as an issue of disjoint supports. We plot in figure 1 the additive local effect (i.e. the factor loading associated to the constant factor) and the multiplicative factor loading for each control unit (circle) and each treated unit (triangle) in the case in which the model includes two individual effects only. This graph does not exhibit any evidence against the hypothesis that the support of factor loadings for the treated units is included in the corresponding support for the controls. We tried to construct a test using permutation techniques (Good, 2005) and failed to reject the null hypothesis of inclusion of the supports. In the absence of formal analyses of this test in the literature, we do not know, however, if this result is due to the low power of such a test.

Another cause of the discrepancy between synthetic controls and interactive effects could be the presence of serial correlation. When a single local effect is considered as in the difference-in-differences method, serial correlation is still substantial, and the estimate of the autocorrelation of order 1 is around .35. In contrast, estimates of the serial correlation in the interactive effect model are close to 0. Factor models exhaust serial time dependence, and this is also true for spatial dependence.[16] By contrast, we do not know much about the behavior of synthetic controls when serial correlation and spatial correlation are substantial. Interestingly, the within estimate without any correction for serial correlation is also on the negative side and close to the synthetic control estimate.

Results for other outcomes confirm the diagnostic that synthetic control estimates seem to have a behavior different from interactive effect estimates and difference-in-differences estimates. While interactive effect estimates of

[16] This result is obtained using a Moran test when the distance matrix is constructed using the reciprocal of the geographical distance. Other contiguity schemes (e.g., when using discrete distance matrices constructed using 5 km and 10 km thresholds) capture positive spatial correlations, although they diminish with the number of factors.

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the treatment effect are undistinguishable from 0 when we analyze exits from unemployment for unknown reasons, difference in differences yield a positive but insignificant estimate and synthetic controls a positive and significant estimate. As we have reason to believe that the treatment effect should be larger for the outcome recording exits to a job than for the outcome recording exits for unknown reasons, synthetic control estimates seem slightly incoherent. Nonetheless, it is also true that synthetic control and interactive effect estimates for the effect of treatment on entries are very similar, while difference-in-differences estimates seem surprisingly positive and nearly significant.

As a robustness check, we report in the online appendix C.4 the treatment effect estimates when the propensity score is introduced as a regressor. Results are very similar with those presented in the text.

VI. Conclusion

In this paper, we compared different methods of estimation of the effect of a regional policy using time-varying regional data. Spatial and serial dependence are captured by a linear factor structure that permits conditioning on an extended set of unobserved local effects when applying methods of policy evaluation. We show how difference-in-differences estimates are biased and how interactive effect methods following Bai (2009) can be applied. We compare different versions of these interactive effect methods with a synthetic control approach and with a more traditional difference-in-differences approach in Monte Carlo experiments. We finally apply the different methods to the evaluation of an entreprise zone program introduced in France in the late 1990s. In both Monte Carlo experiments and the empirical application, interactive effect estimates behave well with respect to competitors.

There are quite a few interesting extensions worth exploring in empirical analyses. First, there is a tension between two empirical strategies in regional policy evaluations (Blundell et al., 2004). On the one hand, choosing areas in the neighborhood of treated areas as controls might lead to biased estimates since neighbors might be affected by spillovers or contamination effects of the policy. On the other hand, nonneighbors might be located too far away from the treated areas to be good matches and therefore good controls. This paper tackles this issue in a somewhat automatic way by letting factor loadings pick out spatial correlation in the data. A richer robustness analysis would allow the modification of the populations of controls and treatments by playing on the distance between municipalities and locally treated areas as was done in Gobillon et al. (2012).

Second, it is easy to extend the interactive effect procedures we have analyzed to the case in which the treatment date varies with time. This is particularly easy in the linear factor model, and this setup is used by Kim and Oka (2014). In addition, the variability of treatment dates facilitates the identification of the treatment effect since the rank condition, equation (15), used in section IIIC for identification purposes, is no longer needed, although endogeneity issues might become more severe. The synthetic control approach can also be adapted when the treatment date varies across treated units by using a variable number of pretreatment outcomes to construct the synthetic control.

A word of caution is also in order in case of extrapolation. When supports of exogenous variables and factor loadings of the treated units are not included in the corresponding supports of the control units, we have seen that unconstrained interactive effect estimation methods perform better than matching methods such as a constrained Bai method or synthetic controls. This conclusion is nonetheless due to our Monte Carlo setting in which the true data-generating process has linear factors. If it was nonlinear, this asymmetry between methods would disappear, and no method would be likely to dominate each other. Extrapolation is indeed a case in which any technique needs some untestable assumptions to achieve identification. Bounds on outcome variations might, however, lead to partial identification of treatment effects.

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APPENDIX

Proof of Lemma 1

Let Y and X be some real random vectors whose supports denoted S_Y and S_X are included in $\mathbb{R}^K$. Assume that S_X is convex and bounded.

Denote D the distance between Y and its projection on the convex hull generated by n independent copies of X. Namely, let this convex hull be defined as

$$\hat{S}_{X,n} = \left\{ Z; Z = \sum_{j=1}^{n} \omega_j X_j, \omega_j \geq 0, \sum_{j=1}^{n} \omega_j = 1 \right\},$$

so that

$$D = \left\| Y - Proj_{\hat{S}_{X,n}}(Y) \right\|.$$

We use the result that if $n \to \infty$, $\hat{S}_{X,n} \to S_X$ in probability in the Hausdorf sense, that is,

$$d_H(\hat{S}_{X,n}, S_X) = o_P(1),$$

in which d_H is the Hausdorf distance. The proof of this result is in Dumbgen and Walther (1996).

Assume that $S_Y \subset S_X$. Consider any realization y of Y and a realization $\hat{S}_{x,n}$ of $\hat{S}_{X,n}$. If $y \in \hat{S}_{x,n}$, then the realization of D is 0. If $y \notin \hat{S}_{x,n}$, then the realization of D is bounded since S_X is bounded. As by the result above $d_H(\hat{S}_{X,n}, S_X) = o_P(1)$ and $y \in S_X$ then

$$\begin{aligned} E(D) &= E(D \mid Y \in \hat{S}_{X,n}) \Pr(Y \in \hat{S}_{X,n}) + E(D \mid Y \notin \hat{S}_{X,n}) \Pr(Y \notin \hat{S}_{X,n}) \\ &= E(D \mid Y \notin \hat{S}_{X,n}) \Pr(Y \notin \hat{S}_{X,n}) \to 0 \text{ when } n \to \infty. \end{aligned}$$

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