The Econometrics Journal

Econometrics Journal (2022), volume **25**, pp. 781–805.
https://doi.org/10.1093/ectj/utac016

Effects of Covid-19 lockdowns on social distancing in Turkey

FIRAT BILGEL

Department of Economics, MEF University, Istanbul 34396, Turkey.
Email: bilgelf@mef.edu.tr

First version received: 8 November 2021; final version accepted: 8 April 2022.

Summary: This paper elucidates the causal effect of lockdowns on social distancing behaviour in Turkey by adopting an augmented synthetic control and a factor-augmented model approach for imputing counterfactuals. By constructing a synthetic control group that reproduces pre-lockdown trajectory of mobility of the treated provinces and that accommodates staggered adoption, the difference between the counterfactual and actual mobility of treated provinces is assessed in the post-lockdown period. The analysis shows that in the short run following the onset of lockdowns, outdoor mobility would have been about 17–53 percentage points higher on average in the absence of lockdowns, depending on social distancing measure. However, residential mobility would have been about 12 percentage points lower in the absence of lockdowns. The findings are corroborated using interactive fixed effects and matrix completion counterfactuals that accommodate staggered adoption and treatment reversals.

Keywords: *Augmented synthetic control*, *interactive fixed effects*, *lockdown*, *matrix completion*, *social distancing behaviour*.

JEL codes: *C54*, *I12*, *I18*.

1. INTRODUCTION

In the absence of effective Covid-19 treatment, almost all economies imposed a number of social distancing policies, known as non-pharmaceutical interventions (NPI). These policies encompassed a large spectrum of measures from blanket lockdowns to closure of non-essential businesses, and were introduced in an attempt to slow down the propagation of the virus (i.e., flattening the epidemiological curve) by limiting human contact, mitigate the overwhelming of health care capacity, and gain time in order to develop effective treatment. NPIs were further reinforced in a number of countries by mask mandates in order to reduce the probability of transmission when physical distancing is not possible. Studying the impact of Covid-19 responses in a developing country like Turkey is policy-relevant and scientifically important, because the impact of lockdown restrictions may differ due to different age structures and state capacity levels. In developed countries, the epidemiological curve flattens as a result of properly enforced lockdown policies and compliance. In developing countries, we do not *ex ante* know whether lockdowns will be properly enforced due to a stringent trade-off between health benefits and economic costs of imposing restrictions, or whether compliance with such restrictions will be near perfect. Therefore, the success of lockdown policies in changing social distancing behaviour is the best indicator of both proper enforcement of and compliance with lockdown restrictions.

Since the outbreak, there has been an exponential growth in the number of studies on Covid-19. A review of this vast literature is beyond the scope of this study. However, Covid-19 literature can be broadly classified under those that deal with the impact of NPI on social distancing (Askitas et al., 2020; Maloney and Taskin, 2020), cases (Bonardi et al., 2020; Courtemanche et al., 2020; Hsiang et al., 2020), deaths, hospitalisations (Juranek and Zoutman, 2020), mental health and well-being (Tubadji et al., 2020; Brodeur, Clark, et al., 2021; Lu et al., 2021), environmental outcomes (Brodeur, Cook, et al., 2021; Orak and Ozdemir, 2021), macroeconomic outcomes (Auray and Eyquem, 2020; Bairoliya and Imrohoroglu, 2020), labour market outcomes (Gupta et al., 2020), and gender and racial inequality (Alon, Doepke, et al., 2020), among others. There has been an increasing use of SIR (susceptible–infected–recovered) epidemiological models of the spread of Covid-19 and its variants to assess various lockdown strategies (Acemoglu et al., 2020; Atkeson, 2020) in conjunction with macroeconomic models (Alon, Kim, et al., 2020; Arnon et al., 2020; Brzezinski et al., 2020; Çakmaklı et al., 2021; Eichenbaum et al., 2021).

This study aims to measure the causal impact of lockdowns of April–May 2020 on social distancing in Turkey, and contributes to the literature in a number of ways. Existing knowledge on the counterfactuals of Covid-19 outbreak in general or of NPIs in particular is very limited in Turkey. Using a susceptible–exposed–infectious–recovered–deceased (SEIRD) model, the counterfactual experiment of Attar and Tekin-Koru (2021) indicates that a month longer than the actual lockdown in mid-2020 would have been highly effective in curbing the progression of Covid-19 in Turkey. First, the impact of NPIs on the Covid-19 burden as a 'reduced-form estimate' for lockdowns would not have affected Covid-19 cases or deaths directly, without first affecting behaviour. Given that a pharmaceutical intervention was not available for Covid-19 in the sample period, the impact of lockdowns on SARS-CoV-2 transmission, and therefore on infections and deaths, was an indirect effect mediated through mobility. While the impact of social distancing on the spread of the virus is straightforward, the impact of lockdowns on social distancing is the policy-relevant portion of this relationship. Second, the current study adopts several counterfactual approaches instead of relying on epidemiological models such as SIR. Third, it aims to impute the level of social distancing at the provincial level in the absence of lockdowns. Although the decision to impose a lockdown is centralised in Turkey, a counterfactual scenario is constructed by leveraging the provincial selectivity on the decision to impose lockdowns.

The nature of lockdowns imposed during April–May 2020 in Turkey posed a number of challenges. First, they were *blanket* yet were switched on and off throughout the sample period. Second, they were imposed in a *backward-staggered* way, in the sense that each lockdown episode targeted a gradually decreasing number of provinces. An augmented synthetic control and a fixed effects counterfactual approach were adopted to deal with these challenges.

The results show that lockdowns are highly effective in increasing social distancing: in the short run following the onset of lockdowns, outdoor mobility measures captured by *retail & recreation*, *grocery & pharmacy*, *parks*, *transit stations*, and *workplace* mobility would have been about 25–29, 53–64, 33–42, 17–19, and 28–33 percentage points higher on average, respectively, relative to baseline, had lockdowns not been imposed. However, *residential* mobility would have been about 12–14 percentage points lower in the absence of lockdowns. A set of statistical tests is performed to evaluate the validity of the identifying assumptions and to gain a holistic view of whether they are likely to hold.

In the strand of the literature that the current study belongs to, empirical evidence points out that NPIs are effective in increasing social distancing/reducing mobility. Employing a multiple event study in a panel dataset of 135 countries, Askitas et al. (2020) assess the impact of lockdown policies on Google mobility patterns, and find that cancellation of public events and restrictions on private gatherings have large and negative effects on retail and recreation, and transit and workplace mobility. In a similar analysis of 45 countries, Wang (2021) finds that social distancing captured by Google mobility reports increases with government stringency. In a panel dataset of 8 to 40 countries depending on income groups as well as in US states, Maloney and Taskin (2020) confirm that NPIs reduce workplace mobility; however, decreased workplace mobility is found to be largely driven by voluntary restrictions in response to increased local and national Covid-19 cases. Akim and Ayivodji (2020) find equally large effects of lockdowns on workplace and residential mobility for a sample of African countries.

While cross-country analyses are suggestive of effective NPIs, little insight can be gained from these studies for developing effective and efficient strategies due to a pooling of legislative/jurisdictional domains and severe heterogeneity in the nature, timing, and the size of the impact of NPIs. Therefore, subnational analyses are potentially more informative in finding good control groups to match treatment groups. Using a regression discontinuity approach, Wellenius et al. (2020) find that implementing one or more social distancing policies reduces mobility by about 25%, with a subsequent reduction of 29% following shelter-in-place orders (SIPO) in the United States. Two subsequent studies for US states using difference in differences (DiD) design find that the adoption of (policies that are equivalent to) state SIPO increases residential mobility by 9–10% (Dave et al., 2021) and by 2.5 percentage points (Abouk and Heydari, 2021). The adoption of a stay-at-home order is also found to be effective at increasing social distancing using county-level data for the United States (Andersen, 2020; Engle et al., 2020; Yan et al., 2021). These studies also point to a substantial voluntary restriction in response to the outbreak.

The nexus between NPIs and social distancing or Covid-19 cases and deaths by and large relies on DiD designs in an attempt to identify causal effects. However, DiD design is not a panacea to the identification of the causal effects, probably even less so in the Covid-19 context. Implementation of several quasi-simultaneous policies to reduce Covid-19 infections, reverse causation, voluntary restriction of mobility, anticipatory effects of lockdown announcements, spatial spillovers, staggered adoption, and measurement and scaling of outcome pose potential threats to valid inference (Goodman-Bacon and Marcus, 2020). Therefore, methods that impose balance, not only on pre-NPI Covid-19 burden, but also on factors that predict social distancing or infections should be adopted. Further, a transparent and convincing analysis should strive to attain the highest rung in Pearl's causal hierarchy (Pearl and Mackenzie, 2018) beyond the effects of interventions. One such method is the synthetic control that offers a solid approach for estimating the counterfactual.

Synthetic control paved its way into the analysis of Covid-19 outbreak, notably in a counterfactual lockdown scenario for Sweden, where a later-rescinded herd immunity policy was adopted at first (Cho, 2020; Born et al., 2021); nitrogen dioxide (NO_2) concentration in the absence of lockdowns in Wuhan, China (Cole et al., 2020); Covid-19 transmission in the absence of school openings in Italy (Alfano et al., 2020); lockdowns in New York (Bayat et al., 2020); quarantines and testing policies in Chile (Bennett, 2021); political elections in Malaysia (Lim et al., 2021); SIPO termination by Wisconsin Supreme Court (Dave et al., 2020); mask policies (Mitze et al., 2020) and jobless claims in the absence of stay-at-home orders (Gibson and Sun, 2020).

Section 2 briefly reviews the methods adopted in this paper for imputing counterfactuals; Section 3 translates the assumptions of these approaches into graphical causal models for transparency and tractability, Section 4 unravels the nature of lockdowns in Turkey and lays out the details of the construction of treated and control units/observations, Section 5 reports the results and performs a battery of falsification tests, and Section 6 concludes. Extensions and additional tests are provided in the Online Appendix.

2. IMPUTING THE COUNTERFACTUAL

2.1. Augmented synthetic control method

The synthetic control method (SCM), developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015), uses a weighted average of outcomes of the controls to construct the counterfactual of treated units. Since its inception, a number of counterfactual models has been developed to extend it to multiple treated units (Ben-Michael et al., 2021a), staggered treatment (Gobillon and Magnac, 2016; Xu, 2017; Ben-Michael et al., 2021b), treatment reversal (Liu et al., 2021), nonparametric estimation of weights (Cerulli, 2019), spatial spillovers (Cao and Dowd, 2019; Grossi et al., 2020), and the Bayesian realm (Kim et al., 2020; Pang et al., 2022).

An extension of SCM, called the augmented synthetic control method (ASCM), is proposed in situations in which the pre-treatment fit is less than perfect (Ben-Michael et al., 2021a). ASCM generalises synthetic control when there are multiple treated units and staggering treatment adoption.

The idea is to run an outcome model, to obtain model fit $\hat{m}(X_i)$ using pre-lockdown covariates X_i, to choose the synthetic control weights $\hat{\gamma}_i$ to minimise the pre-lockdown gap between the actual and the synthetic outcome for the treated unit, and then to debias the original estimate by subtracting an estimate of the remaining difference from the post-lockdown gap; hence, it is doubly robust. Assuming that unit 1 is being treated, $\hat{y}_1(0)$ is the outcome in (2.1) that would be obtained for the treated unit in the absence of treatment, and $\mathbb{C}$ being the pool of units that never goes under a lockdown, the ASCM estimator for the treated unit is:

$$\begin{aligned}\hat{y}_1(0) &= \underbrace{\sum_{i\in\mathbb{C}}\hat{\gamma}_i y_i}_{\text{SCM}} + \underbrace{\hat{m}(X_1) - \sum_{i\in\mathbb{C}}\hat{\gamma}_i\hat{m}(X_i)}_{\text{debias}} \\ &= \hat{m}(X_1) + \sum \hat{\gamma}_i\left(y_i - \hat{m}(X_i)\right). \end{aligned} \tag{2.1}$$

The following assumptions need to hold for the identification of treatment effects in ASCM setting: (a) *conditional ignorability*: treatment is independent of potential outcomes, conditional on observed covariates; (b) *stable unit treatment value assumption (SUTVA)*: the treatment does not have cross-sectional spillover effects and there are no multiple versions of the treatment; and (c) *no anticipatory effects*: treatment should have no effect on the outcome before the actual date of intervention.

A number of quantities of interest can be derived for the ASCM with multiple treated units. Let J denote the number of treated units, $j = 1, \ldots, J$ and T_j be the treatment period for the j^{th} treated unit such that the duration after the onset of lockdowns is defined as $k = t - T_j$. The unit-specific effect for the j^{th} treated unit at time k is τ_{jk}, which is defined as the difference between the outcome that is observed under treatment and the potential outcome that would be observed in the absence of treatment.

For any treated unit j, the unit-specific effects, averaged across K post-lockdown period is the average treatment effect on the treated for unit j, $\mathrm{ATT}_j = \frac{1}{K}\sum_{k=1}^{k}\tau_{jk}$. For any post-lockdown period k, the unit-specific effects averaged across J treated units is the average treatment effect on the treated at period k, $\mathrm{ATT_k} = \frac{1}{J}\sum_{j=1}^{j}\tau_{jk}$. Averaging across k yields the average post-treatment effect, $\mathrm{ATT} = \frac{1}{K}\sum_{k=1}^{k}\mathrm{ATT_k}$.

2.2. *Fixed effects counterfactual*

ASCM does not accommodate both the staggered and the on-and-off nature of treatment. An approach that can accommodate both scenarios is the fixed effects counterfactual estimators (FECT) of Bai (2009), Gobillon and Magnac (2016), Xu (2017), and Bai and Ng (2021), and discussed in Liu et al. (2021).

The following assumptions need to hold in a counterfactual estimators setting (Liu et al., 2021): (a) *parallel trends*: potential outcomes should follow similar trends in the absence of treatment; (b) *no cross-sectional spillover*: treatment should not have an effect on control units; (c) *no feedback*: past outcomes should not affect current treatment; (d) *no temporal carry-over*: treatment effects should not endure once treatment is over; (e) *additive separability*: treatment effects are separable from the influences of unobserved factors and covariates; and (f) *strict exogeneity*: model errors are independent of treatment, observed covariates and unobserved attributes (i.e., unit and time fixed effects). Strict exogeneity rules out the use of lagged dependent variables as covariates in this setting.

Using a factor-augmented model to impute the counterfactual, two special cases are adopted in this paper. The *interactive fixed effects* (IFE) uses a model that relaxes the assumption of no latent time-varying confounders, when such confounders can be decomposed into interactive time and unit-specific factors. For all provinces and time periods under no treatment, the counterfactual outcome for the treated observation is:

$$y_{it}(0) = \mathbf{X}_{it}\beta + \alpha_i + \xi_t + \phi_i' f_t + \varepsilon_{it}, \tag{2.2}$$

where $\mathbf{X}_{it}$ is a vector of covariates, α_i and ξ_t are the unit and time-specific factors, $\phi_i' f_t$ are the interactive fixed effects and ε_{it} are the idiosyncratic errors.

Matrix completion (MC) is a technique borrowed from computer science that explicitly imputes the missing potential outcome for treated units. MC is a generalised factor-augmented model that uses regularisation to complete a lower-rank $N \times T$ matrix representation of the outcome with missing elements that occur when the observation is under treatment (Athey et al., 2021). Using matrix notation, the counterfactual outcome is:

$$\mathbf{y}(0) = \mathbf{X}\beta + \mathbf{L} + \varepsilon, \tag{2.3}$$

where $\mathbf{y}(0)$ is an $N \times T$ matrix of untreated outcomes, $\mathbf{X}$ is an $N \times T \times k$ array of covariates, $\mathbf{L}$ is the lower-rank matrix and is a product of two matrices of fixed effects, $\mathbf{L} = \varPhi\mathbf{F}$ in which $\varPhi$ is an $N \times r$ matrix of factor loadings, $\mathbf{F}$ is an $r \times T$ matrix of factors, and ε is an $N \times T$ matrix of errors.

The objective is to estimate $\mathbf{L}$ directly using regularisation by adding a penalty term in (2.4):

$$\hat{\mathbf{L}} = \arg\min_{\mathbf{L}} = \left\{ \sum_{(i,t)\in\mathcal{O}} \frac{(Y_{it} - L_{it})^2}{|\,\mathcal{O}\,|} + \lambda_L \parallel \mathbf{L} \parallel \right\}, \tag{2.4}$$

in which $\mathcal{O}$ denotes the observations under control, λ_L is the tuning parameter and $\parallel \mathbf{L} \parallel$ is the choice of norm of matrix $\mathbf{L}$. The chosen norm for the penalty term, $\lambda_L \parallel \mathbf{L} \parallel$, is the *nuclear norm* due to its desirable imputation properties regarding prediction error.

The number of factors, r for IFE and the hyperparameter λ_L for MC are chosen by predicting $y_{it}(0)$ for each treated observation on a training set of untreated observations until convergence

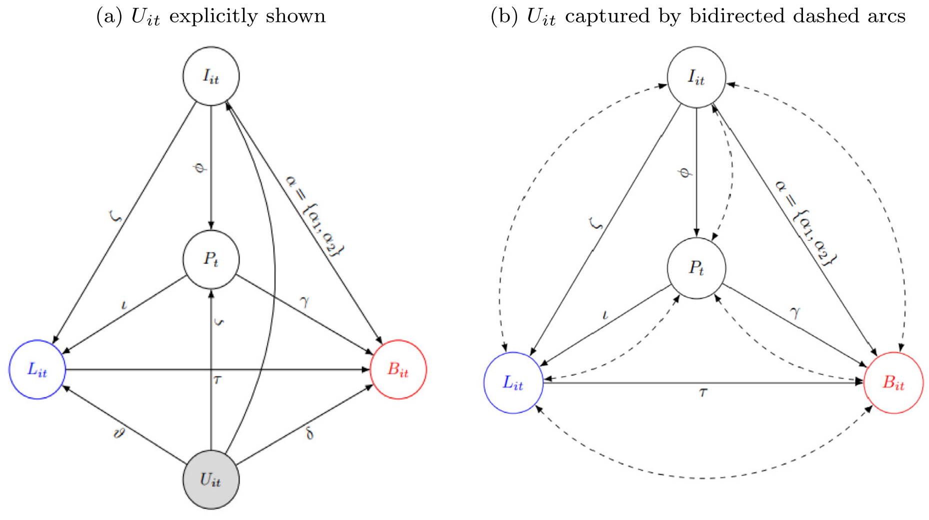

Figure 1. DAG $\mathcal{G}_0$.

is achieved. The optimal r for IFE and λ_L for MC are chosen based on MSPE using a *k-fold cross-validation* (CV) technique (Liu et al., 2021).[1]

The estimation involves fitting (2.2) or (2.3), predicting $y_{it}(0)$ for each treated observation (i.e., $\hat{y}_{it}(0)$), estimating unit-specific effect or gap (i.e., $\hat{\tau}_{it}$), and finally taking the average of unit-specific effects to produce the ATT (Liu et al., 2021).

3. A GRAPHICAL CAUSAL MODEL OF LOCKDOWNS

Let B_{it} be the behavioural variable in province i at period t (outcome), L_{it} be a binary variable capturing lockdown in province i at period t (exposure), I_{it} be the information node that consists of time-invariant, but unit-varying pre-treatment confounders such as human capital and population density (I_i); and unit-invariant, but time-varying confounders such as Covid-19 burden (I_t) and P_t be the time-varying, but unit-invariant NPIs other than lockdown.

The directed acyclic graph (**DAG**) $\mathcal{G}_0$ that captures these nodes is given in Figure 1. It is a modified version of those given in Carneiro et al. (2020) and Chernozhukov et al. (2021). Both graphs are equivalent except that a common latent confounder, U_{it}, is shown explicitly by a node (Figure 1a) and by the bidirected dashed arcs (Figure 1b).

Time-invariant pre-treatment confounders that vary across provinces, such as human capital, determine how people perceive the available information in the country and subsequently adjust their behaviour. Notably, everything else being fixed, provinces with higher human capital levels

[1] The type of CV and the asymptotic approximation being used can affect whether the correct model is consistently selected. The *k-fold* CV splits the observations into k equal-sized subsets, uses each subset as a validation set, and the remaining as a training set. In contrast, *leave-one-out cross-validation* (LOOCV) holds out a single observation for validation and the *leave-p-out-cross-validation* (LpOCV) holds out p observations or p fraction of the sample for validation. While LOOCV is inconsistent in a regression with a fixed number of covariates, LpOCV is consistent (Shao, 1993). The *k-fold* CV can be thought as an approximation of LpOCV and should therefore consistently select the correct model. I thank an anonymous referee for pointing this out.

are more likely to take the pandemic seriously and more likely to process the available information accurately.

Other observable confounders, such as population density, also determine, in part, locations where lockdowns are imposed. However, a more prominent factor in the government's accounting of lockdowns, and where and how to impose them is the Covid-19 burden. Unfortunately, data on provincial weekly Covid-19 incidences were available only after February 8, 2021, and data on provincial Covid-19 deaths were not disclosed. The only publicly available information on Covid-19 incidences or deaths in Turkey during the sample period is the national figures that affect the timing and places of lockdowns. Although provincial (or disaggregated) Covid-19 burden is available to the government only, if available at all, one can get a rough idea of the local Covid-19 burden by the revealed preference of the government with respect to the provinces where curfews were imposed. This revealed preference might be a source of an endogenous or voluntary restriction of mobility prior to lockdowns. This endogenous or voluntary change in behaviour is given by the edge $I_{it} \to B_{it}$ as in (3.1). Information also affects lockdowns as in (3.2) and other NPIs as in (3.3), such as curfews for the elderly, congregational praying bans, business closures, mandatory mask, and so on. Finally, a common latent confounder, U_{it}, that affects information variables, behavioural variable, lockdowns, and other NPIs is introduced into the model. The graphical causal model in Figure 1 can be expressed by the following linear structural form:

$$B_{it} = \alpha_1 I_i + \alpha_2 I_t + \tau L_{it} + \delta U_{it} + \gamma P_t + \varepsilon_{it}^B \tag{3.1}$$

$$L_{it} = \zeta_1 I_i + \zeta_2 I_t + \vartheta U_{it} + \iota P_t + \varepsilon_{it}^L \tag{3.2}$$

$$P_t = \phi I_t + \varsigma U_t + \varepsilon_t^P. \tag{3.3}$$

Substituting (3.3) into (3.1), we obtain a reduced form for B:

$$B_{it} = \alpha_1 I_i + \pi_2 I_t + \tau L_{it} + \theta U_{it} + \epsilon_{it}, \tag{3.4}$$

where $\pi_2 = \alpha_2 + \gamma\phi$, $\theta = \delta + \gamma\varsigma$, and $\epsilon_{it} = \gamma\varepsilon_{it}^P + \varepsilon_{it}^B$. Given that U_{it} is unobservable, but correlated with L_{it}, L_{it} will be correlated with ϵ_{it} and the causal effect of L on B cannot be identified. Graphically, this is equivalent to the failure to operationalise the backdoor criterion in causal graph $\mathcal{G}_0$. A solution is to perform an instrumental variable (IV) estimation, provided that a valid IV is found to instrument L. At best, any potential instrument that is correlated with L will also be correlated with the unobservable factors that affect B. Therefore IV-admissible variables are unlikely to exist.

Although standard regression tools are unable to identify this effect, a latent factor model can be derived from causal graph $\mathcal{G}_0$. Let α_1 vary over time, $\alpha_1 = \alpha_t$ and $\pi_2 I_t = \xi_t$ represent time-specific factors with constant factor loadings across provinces. Then under the potential outcomes, the reduced form in (3.4) is identical to the factor model of the canonical SCM.

If we let $\alpha_i = \alpha_1 I_i$ and $\pi_2 I_t = \xi_t$ denote the unit-specific and time-specific factors respectively, allow heterogenous treatment effects (i.e., τ_{it}), and assume that latent confounder U_{it} can be decomposed into interactive time and unit-specific factors, $\phi_i' f_t$, then under the potential outcomes the reduced form in (3.4) is identical to the IFE model in (2.2), but without covariates:

$$B_{it}(0) = \alpha_i + \xi_t + \phi_i' f_t + \varepsilon_{it}. \tag{3.5}$$

The identifying assumptions that lead to the special case given in (3.5) are shown in causal graph $\mathcal{G}_1$ in Figure 2, adopted from Liu et al. (2021). The assumption of constant treatment effects

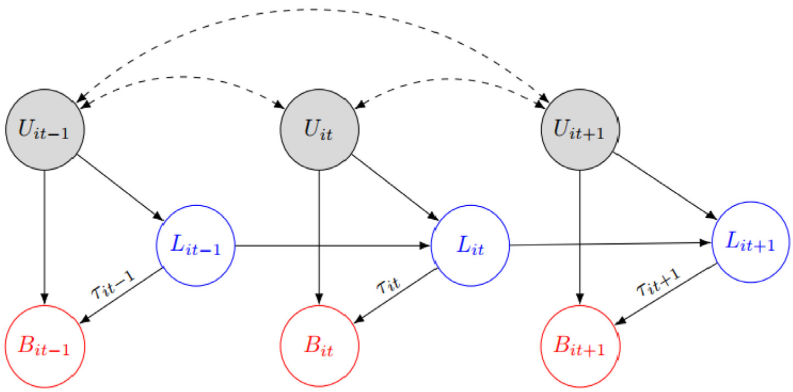

Figure 2. DAG $\mathcal{G}_1$.

is relaxed and the treatment effect heterogeneity (i.e., $\tau_{it} = \tau$) is captured by the unique directed edges $L_{t-1} \rightarrow B_{t-1}$, $L_t \rightarrow B_t$, and $L_{t+1} \rightarrow B_{t+1}$. Notice that the DAG in Figure 2 satisfies the no-carry-over effects (absence of $L_t \rightarrow B_{t+1}$), no feedback (absence of $B_{t-1} \rightarrow L_t$), and no lagged outcome (absence of $B_t \rightarrow B_{t+1}$). The no-carry-over effects assumption can be relaxed in this setup under a staggered adoption (Liu et al., 2021).

However, it is likely that current behaviour (B_t) affects future Covid-19 cases, which, in turn, affects future lockdowns (L_{t+1}) and future behaviour (B_{t+1}). If provincial Covid-19 cases are unobservable and contained in U, this would imply additional edges, $B_{t-1} \rightarrow U_t$ and $B_t \rightarrow U_{t+1}$, in Figure 2, and would violate the *no-lagged outcome* and *no-feedback* assumptions, respectively, through the paths $B_{t-1} \rightarrow U_t \rightarrow B_t$ and $B_{t-1} \rightarrow U_t \rightarrow L_t$. A proxy for provincial Covid-19 cases blocks these paths partly if not fully.[2] Hence, the IFE model in (3.5), augmented by this measure to be defined in Subsection 5.2, is used to estimate the unobserved $\phi_i' f_t$ using non-treated data (Liu et al., 2021). Equation (3.5) can be further augmented by the inclusion of exogenous covariates. Unfortunately, the available variables are not eligible because they are either time-varying, but unit-invariant (e.g., national Covid-19 burden) or unit-varying, but time-invariant (e.g., schooling, population density).

4. EMPIRICAL STRATEGY

This study uses provincial (Nomenclature of Territorial Units for Statistics 3—NUTS-3) data that comes from the Turkish Statistical Office (Turkstat, 2020), national Covid-19 data that comes from the Ministry of Health of Turkey (MoH, 2020), and panel data from the Google mobility report (Google, 2020) covering 81 provinces in Turkey in 2020. Six Google mobility measures are chosen as the outcome variables to capture social distancing (B): *retail & recreation*, *grocery & pharmacy*, *parks*, *transit stations*, *workplace*, and *residential*. Google mobility data show the change in the length of stay compared to a baseline, measured before the outbreak. This baseline is the median for the corresponding day of the week during the five weeks between January 3 and February 6, 2020.[3] The sample period covers 95 days between February 15 and May 19, 2020,

[2] I thank an anonymous referee for pointing this out.
[3] Detailed information on Google mobility data and the place categories is available at https://www.google.com/covid19/mobility/data_documentation.html?hl=en.

inclusive. February 15, 2020, is the first day for which Google mobility measures are available at the provincial level. The sample ends on May 19, 2020, as this was the last day of lockdown for the treated provinces. A seventh final lockdown was imposed between May 23 and May 26, 2020, however, it was effective in all 81 provinces of Turkey and therefore excluded.

4.1. Lockdowns and the evolution of Google mobility trends

Figure 3 shows the trends in Google mobility for the six place categories in the sample period. The left y-axis shows mobility separately for the control and lockdown provinces and the right y-axis shows the national Covid-19 daily new cases. The lockdown date is normalised to zero so that the x-axis shows days relative to lockdown. The vertical dashed line indicates the first day of lockdown episodes, which is April 11, 2020.

Two other important events are marked by the vertical short dashes. The first is the announcement of the first Covid-19 positive case on March 11 and the second is the first death due to Covid-19 on March 17. Notice that from February 15 (56 days prior to lockdown) to the announcement of the first Covid-19 positive case, all mobility trends are flat. After the announcement of the first case, outdoor mobility exhibits a sharp decline relative to the baseline, and the downward trend continues following the announcement of the first death. Given that all NPIs introduced in this period were non-selective, the mobility measures for the treated and the control provinces are very much aligned until lockdown. This is taken as evidence of a previously documented endogenous or voluntary restriction of mobility as a response to the available information (Andersen, 2020; Abouk and Heydari, 2021; Born et al., 2021; Yan et al., 2021). In causal graph $\mathcal{G}_0$ of Figure 1, this is the direct effect of I on B (α).

Due to its centralised nature, lockdown enforcement was uniform across lockdown-imposed provinces with no room for local authority discretion on the scope of restrictions. These restrictions can be thought of as a centralised *stay-at-home order*, requiring all citizens to remain indoors for anything other than essential activities. They covered (extended) weekends and all economic activity including restaurants, workplaces, groceries, recreational areas, and transportation. Additionally, inter-provincial mobility was sealed off during lockdowns and required permits for travelling across cities.

The circular announced by the Ministry of Interior for each lockdown episode set out the dates and times during which the lockdown will be effective, lays out the businesses and other workplaces that will remain open during lockdown, and those that are exempt from lockdown regulations.[4] Additionally, emergency exemptions were set out by a presidential decree and were subject to the permission of the governor's office. Violation of lockdown restrictions were subject to an administrative fine, set by the Public Sanitation Law no.1593 (*Umumi Hıfzıssıhha Kanunu*) of 1930. The timeline of events and notable Covid-19 NPIs are provided in Table 1.

At the time of announcement of the first lockdown on April 11–12, daily Covid-19 cases peaked in the sample period. From day 0 and onwards, the government imposed a series of lockdowns in the treated provinces. This is shown by the six vertical grey areas in Figure 3. Not surprisingly, weekend or extended lockdowns in lockdown provinces translated into an abrupt fall in outdoor mobility and an abrupt jump in indoor or *residential* mobility. However, each episode of lockdown is followed by a quick recovery of mobility measures for the treated provinces up to the level of those of the control provinces. The government on and off imposed a total of 17 days of lockdowns out of 39 days of the post-lockdown period.

[4] The circular of the Ministry of Interior can be found at https://icisleri.gov.tr.

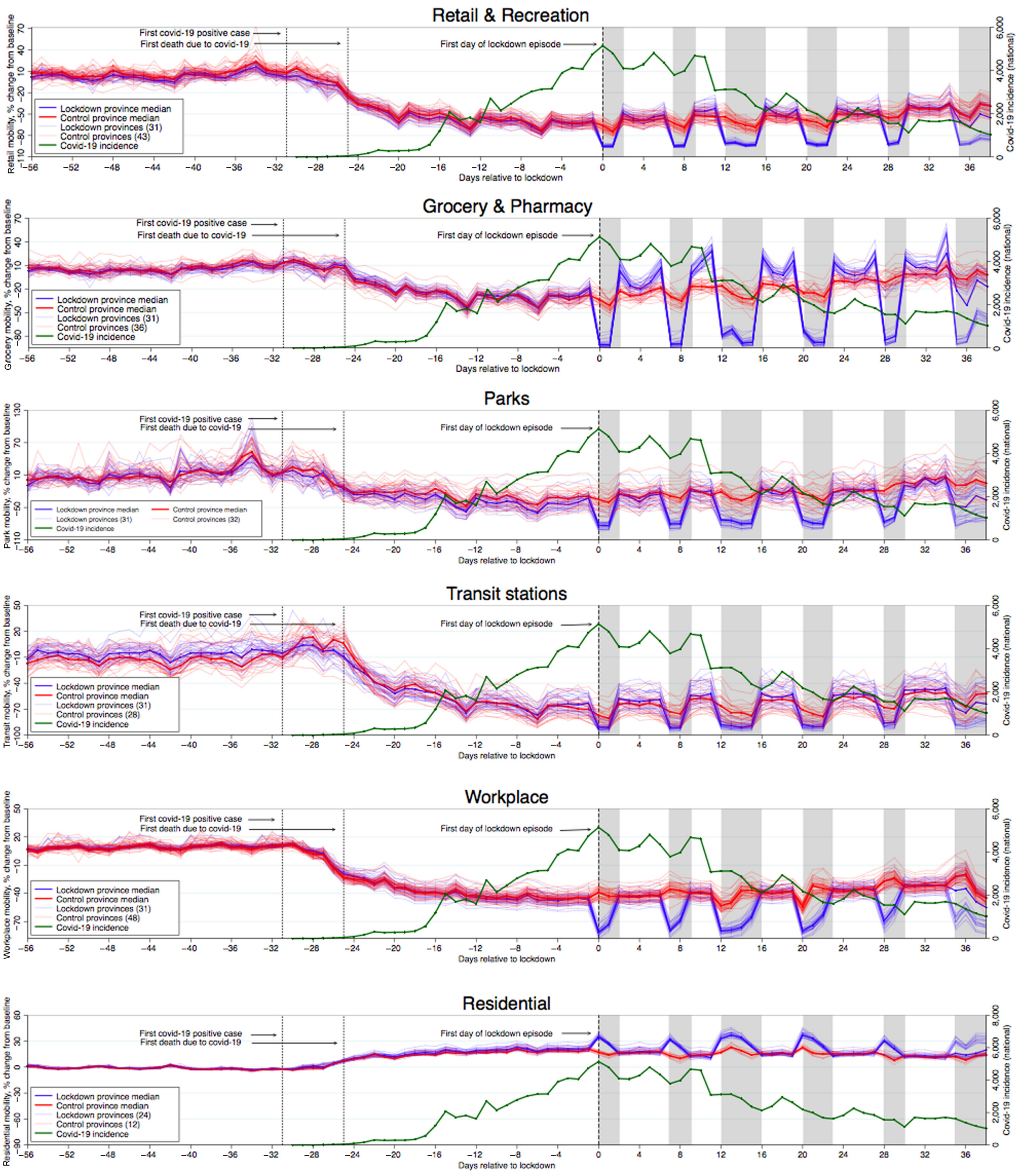

Figure 3. Google mobility trends, lockdowns, and Covid-19 burden for February–May 2020. The graph shows the evolution of Google mobility between February 15, 2020 (Day 1), and May 19, 2020 (Day 95). The first of a series of lockdowns was imposed on April 11, 2020 (Day 57). Vertical grey areas show the periods during which a lockdown was in effect.

Two challenges emerge from Figure 3. First, the number of lockdown provinces diminishes from 31 to 24 provinces between the fourth and the fifth lockdown episodes, and then from 24 to 15 provinces between the fifth and the sixth episodes (see Table 1). In event or comparative

Table 1. Timeline of events and notable Covid-19 NPIs in Turkey (April–May 2020).

Effective date	Event
February 5, 2020	All flights between Turkey and China cancelled
March 11, 2020	The first Covid-19 positive case
March 13, 2020	Travel ban for civil servants
March 15, 2020	149,382 workplaces suspended economic activity
March 16, 2020	Online primary and secondary education
March 16, 2020	Sporting events without spectators
March 16, 2020	Congregational praying ban
March 16, 2020	Suspension of activities of theatres, performance and concert halls, restaurants, cafes, bistros, spas, gyms, pools, recreational facilities
March 17, 2020	First death due to Covid-19
March 19, 2020	Suspension of all sporting events and congregations
March 20, 2020	Designation of pandemic hospitals
March 21, 2020	Lockdown for individuals 65+ years of age and those with chronic illnesses
April 3, 2020	Lockdown for those who were born after January 1, 2000
April 11–12, 2020	Lockdown no. 1 (31 provinces, 2 days)
April 18–19, 2020	Lockdown no. 2 (31 provinces, 2 days)
April 23–26, 2020	Lockdown no. 3 (31 provinces, 4 days)
May 1–3, 2020	Lockdown no. 4 (31 provinces, 3 days)
May 9–10, 2020	Lockdown no. 5 (24 provinces, 2 days)
May 16–19, 2020	Lockdown no. 6 (15 provinces, 4 days)

case studies, treatment adoption typically follows a staggering pattern in which the number of treated units gradually increases over time. In our case, however, the number of lockdown provinces decreases over time. We nevertheless refer to this phenomenon as *staggered lockdown*. Second, lockdowns are imposed on and off as shown by the grey vertical areas (i.e., *treatment reversal*). In the following section, we introduce two strategies/methods to accommodate both aspects.

4.2. Selection of treated provinces/observations

There were a total of six episodes of lockdowns in Turkey between April 11 and May 19, 2020. Either a weekend lockdown or an extended lockdown that covered consecutive days of weekends and national holidays was imposed. The first four lockdowns shown in Figure 3 were implemented in 30 metropolitan municipalities and the province of Zonguldak during weekends or extended holidays between April 11 and May 3, 2020. The fifth lockdown covered only 23 metropolitan municipalities and the province of Zonguldak on May 9–10, 2020, and the sixth and the last lockdown covered 14 metropolitan municipalities and the province of Zonguldak between May 16 and May 19, 2020, inclusive. A time series cross-section view of these lockdowns is given in Figure 4 for *workplace* mobility as an example, and a similar chart can be constructed for other place categories.

Figure 4a shows the design for the ASCM in which all 31 lockdown provinces are used as multiple treated units. Of the remaining 50 provinces, 2 (Ardahan and Tunceli) were excluded

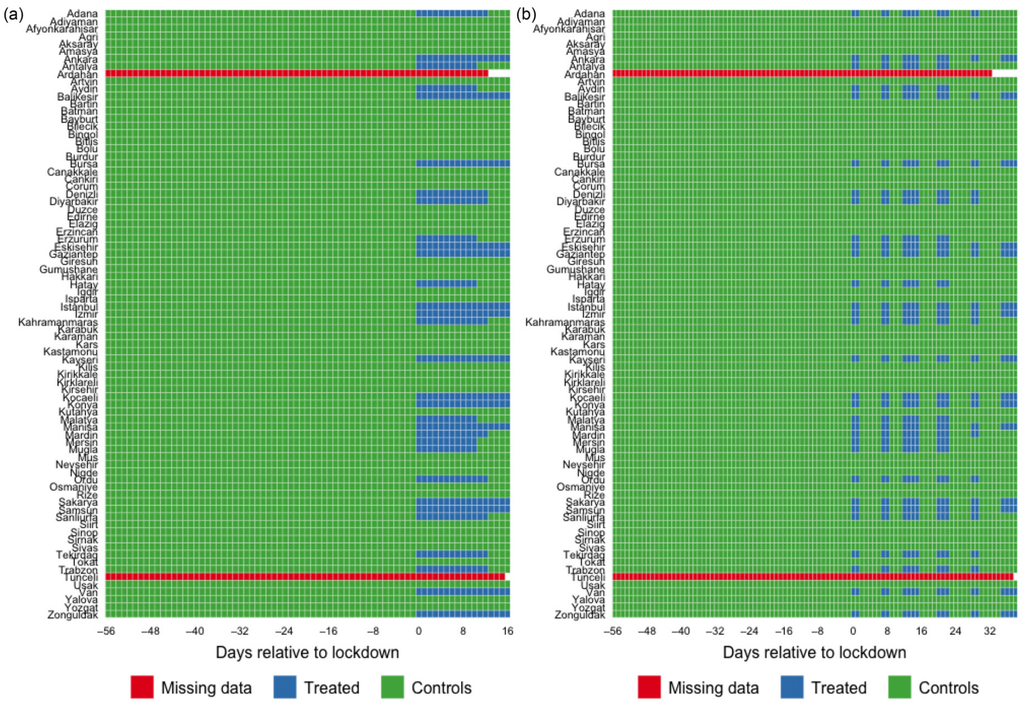

Figure 4. Treatment status under lockdown for workplace mobility (February 15–May 19, 2020). Panels (a) and (b) respectively show the treatment status under lockdown for the ASCM and the FECT.

due to missing data (shown by the 'missing data' observations in Figure 4).[5] Therefore, the donor pool under *workplace* mobility consists of 48 provinces that were not exposed to any lockdown (shown by the 'controls' observations).[6] ASCM does not directly address on and off treatment. A solution to get around this problem is to remove days without lockdown in the post-lockdown period and concatenate all lockdowns to obtain a single, uninterrupted lockdown period. This yields a pre-lockdown period of 56 days and a post-lockdown period of 17 concatenated days.

Our efforts to compensate for the violation of 'no treatment reversal' assumption of the ASCM serve as a robustness check against the FECT that accommodates staggered treatment and treatment reversals. Figure 4b shows the exemplary design for the FECT approach for *workplace* mobility. As in ASCM, two provinces (Ardahan and Tunceli) were excluded due to missing observations. In contrast to ASCM, the accommodation of treatment reversal in FECT implies that the treatment period is the maximum duration of any of the six episodes of lockdowns (i.e., 4 days). Under workplace mobility, the final setup for FECT consists of 449 treated day–province observations in 31 provinces and 7,056 non-treated

[5] The panelView package in R, is available at https://yiqingxu.org/softwares/panelview-visualizing-panel-data/panelview.

[6] The treated provinces and the donor pool under the remaining five place categories are given in the legends of Figure 3.

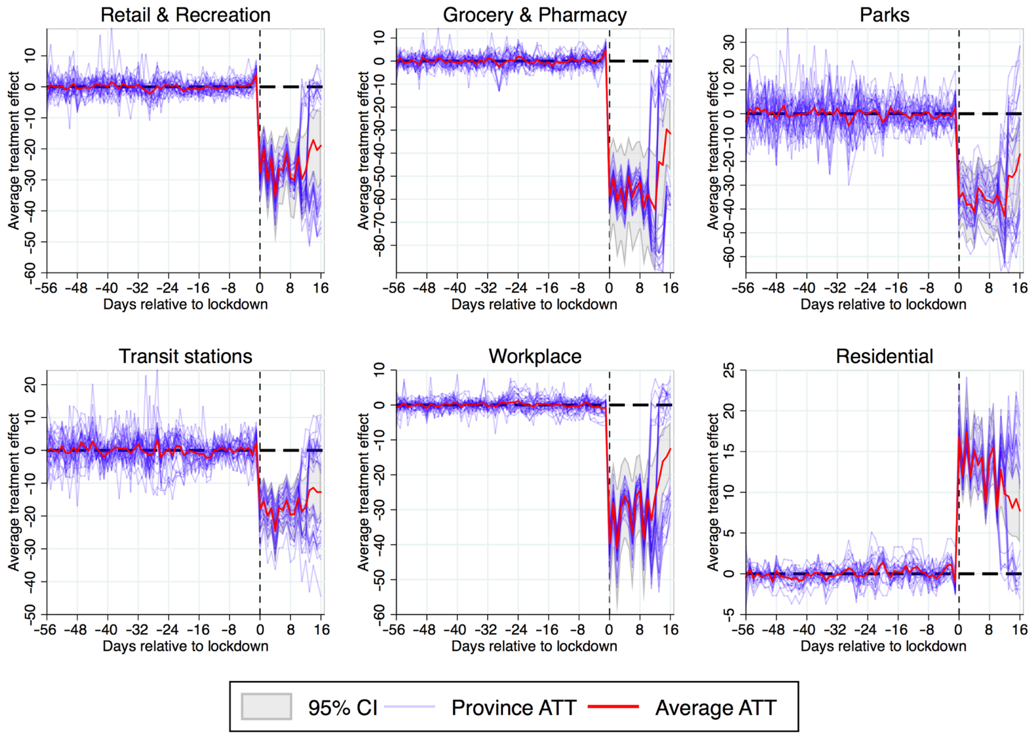

Figure 5. Treatment effect of lockdown on social distancing, partially pooled ASCM estimates ($\nu = 0.5$). The vertical dashed line is the day of the first lockdown. Synthetic units are constructed using the respective mobility of the control provinces for every day in the pre-lockdown period. Days without lockdowns in the post-lockdown period are removed and the remaining lockdown days are concatenated, yielding a post-lockdown period of 17 days.

day–province observations in 48 provinces for a total of 7,505 day–province observations in the sample.

5. RESULTS AND INFERENCE

5.1. Augmented synthetic control estimates

The treatment effect of lockdown on social distancing using ASCM is given in Figure 5 for each of the six place categories.[7] The average ATTs are the partially pooled synthetic control estimates in order to improve province-specific pre-lockdown fits with the intermediate choice, $\nu = 0.5$.[8] In Figure 5, the post-lockdown period is reshaped by removing no-lockdown days and concatenating the remaining 17 lockdown days. The thin trajectories show the province-specific ATTs and the thick trajectory shows the average ATT (average treatment effect on the

[7] Augsynth package in R is available at https://github.com/ebenmichael/augsynth.
[8] A robustness check with varying ν is provided in Section S.1.1 of the Online Appendix.

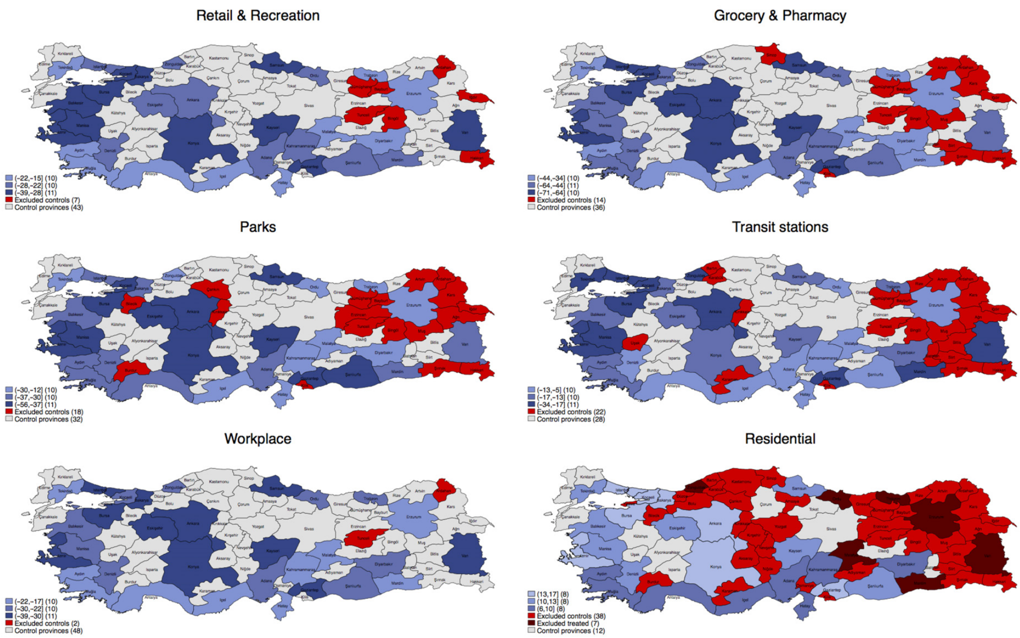

Figure 6. Geographical distribution of province-specific ATTs, ASCM estimates.

treated) along with the post-lockdown 95% wild bootstrapped confidence interval. Notice that the trajectory of a number of provinces near the end of the post-lockdown period in Figure 5 reaches the zero ATT line due to the fact that the last two episodes of lockdowns (fifth and the sixth episodes) were not imposed in 7 and 16 out of 31 treated provinces, respectively. Although this reduces the average ATT, it is statistically distinguishable from zero at conventional test levels.

Results indicate that all outdoor mobility measures would be 17 to 53 percentage points higher had lockdown not been imposed. Specifically, *grocery & pharmacy* is the most affected place category from lockdowns (by 53 percentage points) followed by *parks* (33 percentage points), *workplace* (28 percentage points), *retail & recreation* (25 percentage points), and *transit stations* (17 percentage points). However, *residential* mobility would be about 12 percentage points lower had lockdown not been imposed.[9]

The geographical distribution of province-specific ATTs is displayed in Figure 6. It shows the size of ATT in tertiles, the included controls, excluded controls and the excluded treated.

Figure 7 shows the placebo ASCM estimates. The placebo lockdown date is set to 4 and 10 days prior to the actual lockdown date. The average placebo ATT estimates for each place category with a placebo lockdown of 4 (10) days prior are 2.30 (1.47) for *retail & recreation*, 2.14 (2.57) for *grocery & pharmacy*, −0.13 (−0.81) for *parks*, 0.65 (1.29) for *transit stations*, −1.04 (−0.53) for *workplace* and 0.47 (0.06) for *residential* mobility. The average placebo ATT estimates are statistically indistinguishable from zero at a 95% confidence interval.

[9] An assessment of the sensitivity of the results to the exclusion of a particular province from the donor pool is provided in Section S.1.2 of the Online Appendix.

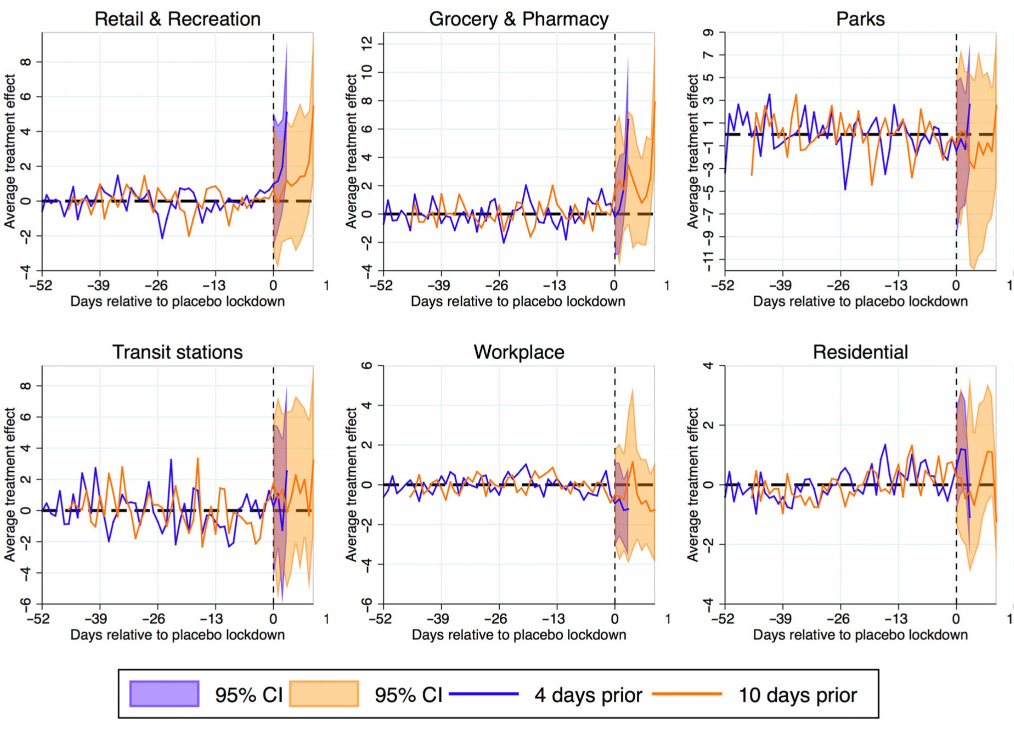

Figure 7. Placebo treatment effect of lockdown on social distancing, ASCM.

5.2. *Fixed effects counterfactual estimates*

The FECT does not allow unit-varying only covariates. Therefore, such variables (e.g., years of schooling, population density, old dependency ratio) are excluded from the imputation of the counterfactual mobility. All unit-varying only factors are already captured by the unit-fixed effects α_i in (2.2). Similarly, all time-varying only factors are already captured by the time-fixed effects ξ_t in (2.2).

Provincial Covid-19 incidence (new cases) is likely to be an important time-varying confounder in estimating the effect of lockdown on mobility because people voluntarily reduce their mobility when the number of cases is high. However, data on provincial incidence did not exist in the sample period. A solution is to assume that incidence is proportional to provincial population size. Hence, national incidence (time-varying only), is multiplied by a factor equal to the share of the provincial population out of the total country population (unit-varying only) to obtain a proxy for provincial Covid-19 incidence. In all FECT imputations, this proxy is used as the only covariate.

The ATTs are estimated under two versions of the FECT. In the first version, the original post-lockdown period is used, with lockdown switching on and off. In the second version, days without lockdown in the post-lockdown period are removed and all lockdowns are concatenated, as in Subsection 5.1, for a comparison against the ASCM estimates and to assess how much of the difference is due to different methods or different sample construction. All

Table 2. Automated method selection using the *k-fold* CV procedure.

Place category	IFE		MC		Recommended method
	r^*	MSPE	λ_L^*	MSPE	
Retail & recreation	6	10.230	0.075	10.323	IFE
Grocery & pharmacy	6	20.754	0.075	26.474	IFE
Parks	6	61.162	0.032	49.403	MC
Transit stations	6	24.333	0.075	18.788	MC
Workplace	6	2.892	0.075	2.462	MC
Residential	6	0.846	0.075	0.698	MC

Notes: IFE: Interactive fixed effects. MC: Matrix completion. MSPE: Mean squared prediction error. r^* is the optimal number of factors for the IFE, λ_L^* is the optimal tuning parameter for the MC.

period-by-period results and tests for the identifying assumptions of the FECT below, pertain to the first version. The analyses for the second version are relegated to Section S.1.3 of the Online Appendix.

The *k-fold* CV procedure is performed to automatically determine which method (i.e., IFE/MC) to use based on the out-of-sample prediction performance for each place category. This procedure helps determine the tuning parameter such that the method with the minimum MSPE is selected. Table 2 displays the results. For all place categories, with the exception of *retail & recreation* and *grocery & pharmacy*, the automated selection procedure indicates that the minimum MSPE is lower for the MC. For *retail & recreation* and *grocery & pharmacy* the algorithm selects the IFE method, as indicated by the lower minimum MSPE.

For each place category, Figure 8 displays period-by-period ATTs produced by IFE or MC along with a 95% confidence interval and the number of treated observations per period relative to the onset of lockdown shown on the right y-axis.[10] Liu et al. (2021) notes that nonparametric bootstrap procedure to obtain standard errors works well when the number of treated units is greater than 40, and recommends the use of jackknife when the number of treated units is small. Given that the number of treated units in the sample is 31, the uncertainty estimates are obtained via jackknife by iteratively dropping the entire time series of one unit from the dataset.

Results show ATTs of magnitude slightly stronger than those reported under ASCM. In the absence of lockdowns, outdoor mobility measures for post-lockdown treated provinces would be at least 19 percentage points higher and *residential* mobility would be about 14 percentage points lower. Again, the most affected category is *grocery & pharmacy* with a statistically significant ATT of about −64 percentage points, followed by *parks*, *workplace*, *retail & recreation*, and *transit stations*.

Two types of statistical tests are performed, as suggested by Liu et al. (2021), to assess the absence of time-varying unobservable confounders. The first is a test of *no pre-trend* or *equivalence* under the null hypothesis that the average residuals for any pre-lockdown period fall within a pre-specified range, called *the equivalence range*. Rejection of the null is taken as evidence that equivalence holds. Additionally, a minimum range is calculated as the smallest symmetric bound within which the null of inequivalence can be rejected. A rule of thumb is that

[10] Fect package in R is available at http://yiqingxu.org/software/#panel-data-methods/fect.html.

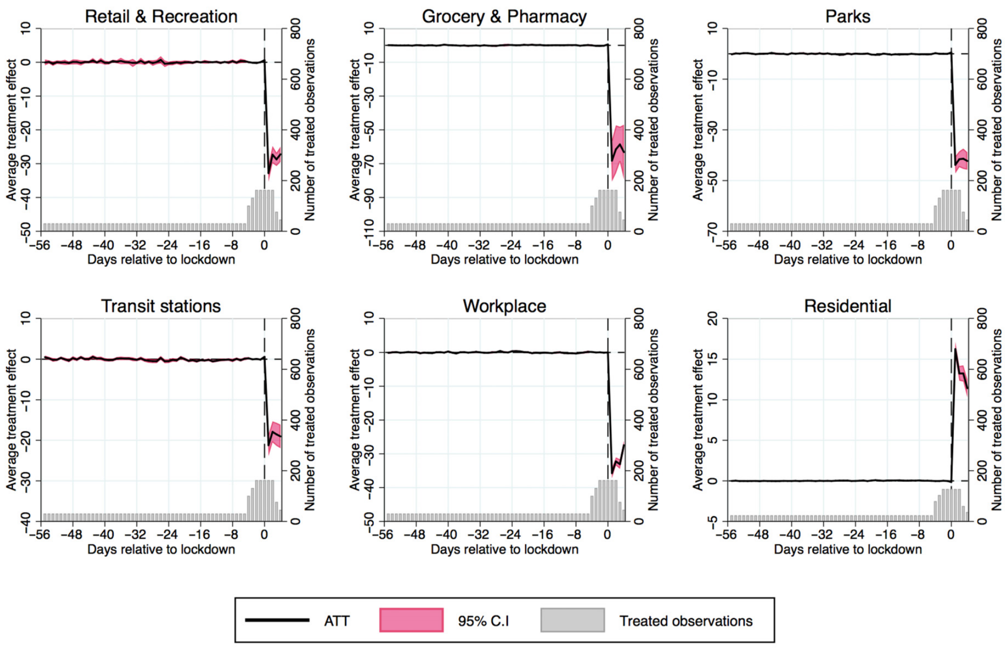

Figure 8. Treatment effect of lockdown on social distancing, FECT. The method for *retail & recreation* and *grocery & pharmacy* is interactive fixed effects (IFE). The method for the remaining place categories is matrix completion (MC). See Table 2 for details.

the test is considered passed when the minimum range falls within the equivalence range (Liu et al., 2021).[11]

Figure 9 shows the results for the test of no pre-trend. The black solid line shows the residual average of the outcome, the dark dashed lines represent the equivalence range and light grey dashed lines represent the minimum range. The equivalence range is outside the minimum range for all place categories indicating that the *no time-varying unobservable confounder* assumption is met at a 90% confidence interval and equivalence holds.

The second is an *out-of-sample placebo test* in which the onset of lockdown is set to S periods prior to the actual lockdown date for each unit in the treatment and the counterfactual is imputed as before, using IFE or the MC estimators. For the *no-time-varying confounder* assumption to hold, the placebo ATT should be statistically indistinguishable from zero.[12] Liu et al. (2021) suggests that S should not be set too large. S is therefore set to 5. The results of the placebo test are shown in Figure 10.

The placebo ATTs are statistically distinguishable from zero at a 5% test level for all measures with the exception of *workplace* mobility, suggesting that the *no-time-varying confounder* assumption does not hold for these place categories.

[11] An alternative to testing for the absence of pre-trends (leave-one-out approach) is conducted in Section S.2.1 of the Online Appendix.

[12] A variant of the placebo test (carry-over effects) is conducted in Section S.2.2 of the Online Appendix.

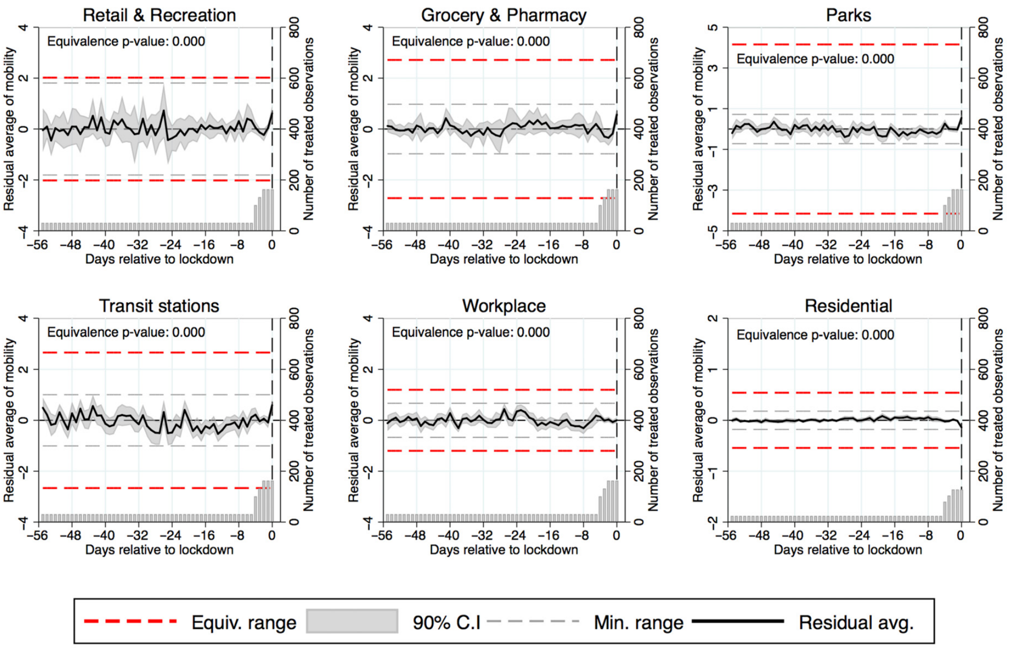

Figure 9. Test of absence of pre-trend, FECT. The method for *retail & recreation* and *grocery & pharmacy* is IFE. The method for the remaining place categories is MC. See Table 2 for details. The test shows the *p*-value for the null hypothesis of inequivalence.

6. CONCLUSION

This study used province-level panel data covering February–May 2020 to elucidate the causal impact of Covid-19 lockdowns on social distancing in Turkey. The comparative attributes, setup and results are summarised in Table 3. An ASCM is employed in order to improve province-specific pre-lockdown fits and to accommodate for staggered lockdowns (column 1). ASCM estimates for two placebo lockdown dates confirmed that the average placebo ATTs are statistically indistinguishable from zero at conventional test levels. The estimated ATTs for the actual lockdown date showed that *retail & recreation*, *grocery & pharmacy*, *parks*, *transit stations*, and *workplace* mobility would be about 25, 53, 33, 17, and 28 percentage points higher, respectively, and residential mobility would be 12 percentage points lower had lockdown not been imposed.

For purposes of cross-validation, a FECT approach that accommodates staggered lockdowns and lockdown reversals is adopted (Table 3, column 2). For the fact that unit- and time-varying covariates are unavailable, and that unit and time fixed effects are already captured in the model, the ATT is estimated using a proxy for Covid-19 incidence as the only covariate. The assumption of *no-time-varying confounder* is tested using an equivalence and a placebo test. While all models pass the equivalence test, only *workplace* mobility passes the placebo test and, hence, satisfies the assumption of *no-time-varying confounder*.

Finally, a sample for the FECT is constructed by concatenating the post-lockdown period to assess how much of the difference between the FECT and the ASCM is attributable to methodical

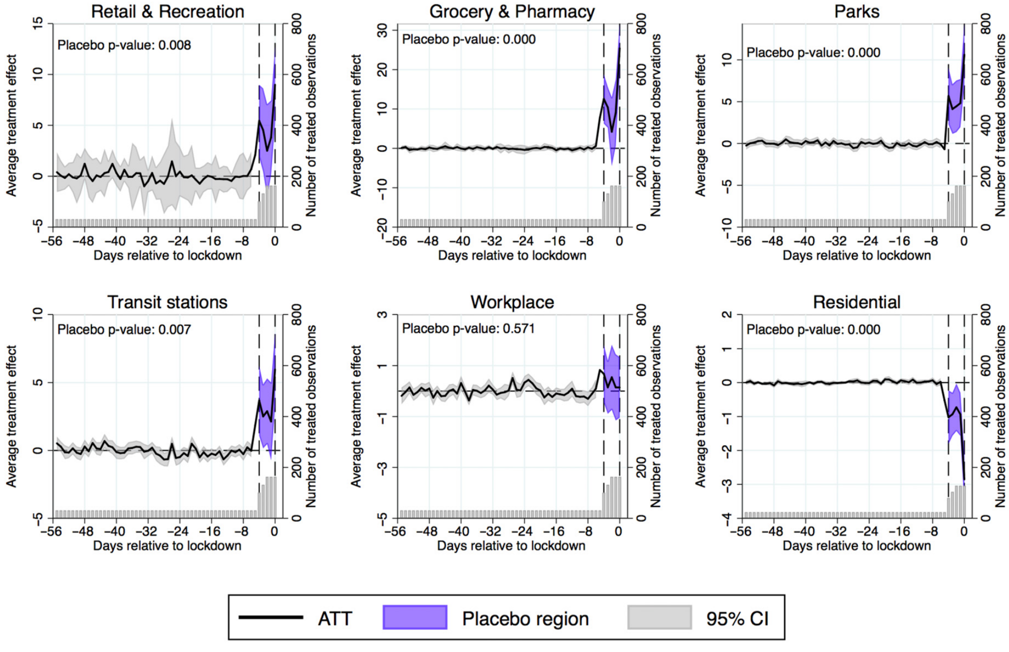

Figure 10. Placebo tests, FECT. The method for *retail & recreation* and *grocery & pharmacy* is IFE. The method for the remaining place categories is MC. See Table 2 for details. The test shows the *p*-value for the null hypothesis of no placebo effects five days prior to the actual lockdown date.

differences (Table 3, column 3). The results for this exercise show that for all six place categories, the ATT estimates are somewhere between those of ASCM and the original FECT sample. This indicates that post-lockdown concatenation may partly explain the discrepancy between the ASCM and the IFE/MC estimates. Under a concatenated post-lockdown period, the models for all place categories, but *grocery & pharmacy* and *transit stations* pass both the equivalence and the placebo test.[13]

All three approaches point out to estimates of very similar magnitude of the causal effect of lockdown on social distancing behaviour. The analysis provides evidence that individuals comply with lockdown restrictions in Turkey and suggests that if considered to be a policy tool, either on its own in the absence of effective treatment or as a complement to pharmaceutical interventions, lockdowns succeed in limiting human contact and thereby reduce transmission. As much as this study shows that early lockdowns in Turkey were effective, the efficiency of these lockdowns remain to be explored due to trade-offs between economic and public health costs/benefits of NPIs of varying severity. Epidemiological models suggest that age-dependent or selective NPIs, coupled with school closures are equally effective, but more efficient than blanket lockdowns (Acemoglu et al., 2020; Bairoliya and Imrohoroglu, 2020).

There are a number of potential threats to valid causal inference. First, all related prior studies point out to substantial voluntary restriction of mobility before lockdowns, and Turkey is no

[13] ATT trajectories, the equivalence, and the placebo tests of Table 3, column 3, are relegated to Section S.1.3 of the Online Appendix.

Table 3. Comparative attributes, setup and results.

	ASCM	FECT (IFE or MC)	
	Ben-Michael et al. (2021b) Ben-Michael et al. (2021a)	Liu et al. (2021)	
	(1)	(2)	(3)
Attributes			
Robust to parallel trends violation?	Yes	No	No
Accommodates staggered treatment?	Yes	Yes	Yes
Accommodates on-and-off treatment?	No	Yes	Yes
Accommodates multiple treated units?	Yes	Yes	Yes
Accepts time-varying only covariates?	No	No	No
Accepts unit-varying only covariates?	Yes	No	No
Setup			
Covariates used	All pre-lockdown outcome values	Covid-19 incidence	
Action taken for staggered treatment	None	None	None
Action taken for treatment reversal	Post-lockdown concatenation	None	Post-lockdown concatenation
Post-lockdown period	17 days	39 days	17 days
Number of treated provinces[a]	31	31	31
In-time placebo test			
Retail & recreation	Pass	Fail	Pass
Grocery & pharmacy	Pass	Fail	Fail
Parks	Pass	Fail	Pass
Transit stations	Pass	Fail	Fail
Workplace	Pass	Pass	Pass
Residential	Pass	Fail	Pass
Test of no pre-trend			
Retail & recreation	–	Pass	Pass
Grocery & pharmacy	–	Pass	Pass
Parks	–	Pass	Pass
Transit stations	–	Pass	Pass
Workplace	–	Pass	Pass
Residential	–	Pass	Pass
ATT (SE)			
Retail & recreation	– 25.08 (5.49)	– 29.38 (1.114)	– 28.01 (1.073)
Grocery & pharmacy	– 53.10 (10.43)	– 63.75 (6.821)	– 58.24 (1.289)
Parks	– 33.45 (7.69)	– 41.91 (1.634)	– 39.23 (1.737)
Transit stations	– 16.76 (3.90)	– 19.11 (1.262)	– 18.29 (1.279)
Workplace	– 27.61 (6.37)	– 33.03 (0.611)	– 32.86 (0.619)
Residential	12.02 (2.04)	14.11 (0.413)	13.61 (0.449)

Notes: [a]Only residential mobility covers 24 provinces. ASCM: Augmented synthetic control. FECT: Fixed effects counterfactuals. IFE: Interactive fixed effects. MC: Matrix completion. SE: Standard error. ATT: Average treatment effect on the treated. The ATT for ASCM in column 1 are the partially pooled synthetic control estimates with $\nu = 0.5$. Based on *k-fold* CV results, the ATTs in column 2 for *retail & recreation* and *grocery & pharmacy* are obtained via IFE; the ATTs for the remaining place categories in column 2 are obtained via MC. All ATTs in column 3 are obtained via MC. See the Online Appendix for details. Standard errors in parentheses are obtained via wild bootstrap in column 1 and jackknife in columns 2 and 3.

exception. Google mobility data shows that all provinces in Turkey take precautions before any lockdown was in place. In the DiD context, this would confound the effect of lockdown on mobility. However, in the synthetic control, both treated and control provinces are equally subject to voluntary precautions, hence, such behaviour is unlikely to invalidate the estimates. A second potential threat is the anticipatory effects, likely to be of concern when governments announce lockdowns ahead of time. The time of government's announcement and the adoption of these lockdowns in Turkey during April–May 2020 were only a few hours apart and typically occurred at midnight. Therefore, such anticipatory effects on mobility are unlikely to exist. What may be a well-grounded concern is that lockdowns that curbed *workplace* mobility in treated provinces may also have helped to curb *workplace* mobility in control provinces. This spatial spillover effect may have emerged as a result of concurrent inter-provincial mobility restrictions that affected non-essential businesses. One strategy to deal with this threat is to create a buffer zone around treated provinces by excluding neighbouring controls from the donor pool (Bennett, 2021). However, this strategy could not be adopted due to a large number of treated provinces in the sample, leaving almost no control units after building a buffer zone. Although it is hard to tell the extent to which such spillovers would have attenuated our estimates for *workplace* mobility, caution is advised in interpreting some of our results.[14]

A natural extension of this study is to assess whether early NPIs inhibit the propagation of the virus or deaths in Turkey. Unfortunately, such efforts have been undercut due to underreporting (Uçar et al., 2020). Estimating the progression of Covid-19 cases or deaths in the absence of lockdown or the causal impact of social distancing on Covid-19 was not possible due to absence of data on provincial Covid-19 burden in Turkey during the sample period. However, anecdotal evidence suggests that an overwhelming proportion of Covid-19 infections are workplace and school related, exacerbated by infecting family members at home. Future research in Turkey should concentrate on modelling disaggregated Covid-19 cases, which were made available by February 2021, in order to answer these challenging causal queries while significant measurement errors need to be addressed.

Conflict of interest statement. Potential conflicts, direct or indirect financial or personal relationships that might bias this study do not exist.

REFERENCES

Abadie, A., A. Diamond and J. Hainmueller (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association 105*, 493–505.

Abadie, A., A. Diamond and J. Hainmueller (2015). Comparative politics and the synthetic control method. *American Journal of Political Science 59*(2), 495–510.

Abadie, A. and J. Gardeazabal (2003). The economic costs of conflict: A case study of the Basque country. *American Economic Review 93*, 113–32.

Abouk, R. and B. Heydari (2021). The immediate effect of Covid-19 policies on social-distancing behavior in the United States. *Public Health Reports 136*(2), 245–52.

Acemoglu, D., V. Chernozhukov, I. Werning and M. Whinston (2020). A multi-risk SIR model with optimally targeted lockdown. Working paper 27102, NBER, Cambridge, MA.

[14] There exists two unpublished papers dealing with spatial spillovers in synthetic control context (Cao and Dowd, 2019; Grossi et al., 2020). However, the code to perform the proposed method is not available.

Akim, A. and F. Ayivodji (2020). Interaction effect of lockdown with economic and fiscal measures against Covid-19 on social-distancing compliance: Evidence from Africa. SSRN: http://dx.doi.org/10.2139/ssrn.3621693.

Alfano, V., S. Ercolano and L. Cicatiello (2020). A synthetic control method analysis of schools opening and Covid-19 outbreak in Italy. Working paper 8784, CESifo, Munich, Germany.

Alon, T. M., M. Doepke, J. Olmstead-Rumsey and M. Tertilt (2020). The impact of Covid-19 on gender equality. Working paper 26947, NBER, Cambridge, MA.

Alon, T. M., M. Kim, D. Lagakos and M. VanVuren (2020). How should policy responses to the Covid-19 pandemic differ in the developing world? Working paper 27273, NBER, Cambridge, MA.

Andersen, M. (2020). Early evidence on social distancing in response to Covid-19 in the United States. SSRN: http://dx.doi.org/10.2139/ssrn.3569368.

Arnon, A., J. Ricco and K. Smetters (2020). Epidemiological and economic effects of lockdown. *Brookings Papers on Economic Activity Fall 2020*(3), 61–108.

Askitas, N., K. Tatsiramos and B. Verheyden (2020). Lockdown strategies, mobility patterns and Covid-19. IZA Discussion paper 13293, Institute of Labor Economics, Bonn, Germany.

Athey, S., M. Bayati, N. Doudchenko, G. Imbens and K. Khosravi (2021). Matrix completion methods for causal panel data models. *Journal of the American Statistical Association 116*(536), 1716–30.

Atkeson, A. (2020). What will be the economic impact of Covid-19 in the US? Rough estimates of disease scenarios. Working paper 26867, NBER, Cambridge, MA.

Attar, A. and A. Tekin-Koru (2021). Dealing with epidemiological underestimation: A simple algorithm for Covid-19. Working paper, Hacettepe University, Ankara, Turkey. Available at: https://bit.ly/3vQKOsk.

Auray, S. and A. Eyquem (2020). The macroeconomic effects of lockdown policies. *Journal of Public Economics 190*, 104260.

Bai, J. (2009). Panel data models with interactive fixed effects. *Econometrica 77*(4), 1229–79.

Bai, J. and S. Ng (2021). Matrix completion, counterfactuals, and factor analysis of missing data. *Journal of the American Statistical Association 116*(536), 1746–63.

Bairoliya, N. and A. Imrohoroglu (2020). Macroeconomic consequences of stay-at-home policies during the Covid-19 pandemic. *Covid Economics 13*, 71–90.

Bayat, N., C. Morrin, Y. Wang and V. Misra (2020). Synthetic control, synthetic interventions, and Covid-19 spread: Exploring the impact of lockdown measures and herd immunity. *arXiv: Computers and Society* arXiv:2009.09987.

Ben-Michael, E., A. Feller and J. Rothstein (2021a). The augmented synthetic control method. *Journal of the American Statistical Association 116*(536), 1789–803.

Ben-Michael, E., A. Feller and J. Rothstein (2021b). Synthetic controls with staggered adoption. Working paper 28886, NBER, Cambridge, MA.

Bennett, M. (2021). All things equal? Heterogeneity in policy effectiveness against Covid-19 spread in Chile. *World Development 137*, 105208.

Bonardi, J.-P., Q. Gallea, D. Kalanoski and R. Lalive (2020). Fast and local: How did lockdown policies affect the spread and severity of the Covid-19. *Covid Economics 23*, 325–51.

Born, B., A. M. Dietrich and G. J. Müller (2021). The lockdown effect: A counterfactual for Sweden. *PLoS ONE 16*(4), e0249732.

Brodeur, A., A. E. Clark, S. Fleche and N. Powdthavee (2021). Covid-19, lockdowns and well-being: Evidence from Google trends. *Journal of Public Economics 193*, 104346.

Brodeur, A., N. Cook and T. Wright (2021). On the effects of Covid-19 safer-at-home policies on social distancing, car crashes and pollution. *Journal of Environmental Economics and Management 106*, 102427.

Brzezinski, A., V. Kecht and D. van Dijcke (2020). The cost of staying open: Voluntary social distancing and lockdowns in the US. Economics Series Working papers 910, University of Oxford, UK. SSRN: http://dx.doi.org/10.2139/ssrn.3614494.

Çakmaklı, C., S. Demiralp, Ş. Kalemli-Özcan, S. Yeşiltaş and M. Yıldırım (2021). Covid-19 and emerging markets: A SIR model, demand shocks and capital flows. Working paper 27191, NBER, Cambridge, MA.

Cao, J. and C. Dowd (2019). Estimation and inference for synthetic control methods with spillover effects. *arXiv: Econometrics* arXiv:1902.07343.

Carneiro, C., I. Ferreira, M. Medeiros, H. Pires and E. Zilberman (2020). Lockdown effects in US states: An artificial counterfactual approach. *arXiv: Applications* arXiv:2009.13484.

Cerulli, G. (2019). A flexible synthetic control method for modeling policy evaluation. *Economics Letters 182*, 40–4.

Chernozhukov, V., H. Kasahara and P. Schrimpf (2021). Causal impact of masks, policies, behavior on early Covid-19 pandemic in the US. *Journal of Econometrics 220*(1), 23–62.

Cho, S. (2020). Quantifying the impact of nonpharmaceutical interventions during the Covid-19 outbreak: The case of Sweden. *Econometrics Journal 23*(3), 323–44.

Cole, M. A., R. J. Elliott and B. Liu (2020). The impact of the Wuhan Covid-19 lockdown on air pollution and health: A machine learning and augmented synthetic control approach. *Environmental and Resource Economics 76*(4), 553–80.

Courtemanche, C., J. Garuccio, A. Le, J. Pinkston and A. Yelowitz (2020). Strong social distancing measures in the United States reduced the Covid-19 growth rate. *Health Affairs 39*(7), 1237–46.

Dave, D., A. Friedson, K. Matsuzawa, D. McNichols and J. Sabia (2020). Did the Wisconsin Supreme Court restart a Covid-19 epidemic? Evidence from a natural experiment. Working paper 27322, NBER, Cambridge, MA.

Dave, D., A. Friedson, K. Matsuzawa and J. Sabia (2021). When do shelter-in-place orders fight Covid-19 best? Policy heterogeneity across states and adoption time. *Economic Inquiry 59*(1), 29–52.

Eichenbaum, M. S., S. Rebelo and M. Trabandt (2021). The macroeconomics of epidemics. *Review of Financial Studies 34*(11), 5149–87.

Engle, S., J. Stromme and A. Zhou (2020). Staying at home: Mobility effects of Covid-19. SSRN: http://dx.doi.org/10.2139/ssrn.3565703.

Gibson, J. and X. Sun (2020). Understanding the economic impact of Covid-19 stay-at-home orders: A synthetic control analysis. SSRN: http://dx.doi.org/10.2139/ssrn.3601108.

Gobillon, L. and T. Magnac (2016). Regional policy evaluation: Interactive fixed effects and synthetic controls. *Review of Economics and Statistics 98*(3), 535–51.

Goodman-Bacon, A. and J. Marcus (2020). Using difference-in-differences to identify causal effects of Covid-19 policies. Discussion paper 1870, DIW Berlin, Germany.

Google. (2020). Google Covid-19 community mobility reports. https://www.google.com/covid19/mobility/ (accessed: February 17, 2021).

Grossi, G., P. Lattarulo, M. Mariani, A. Mattei and Ö. Öner (2020). Synthetic control group methods in the presence of interference: The direct and spillover effects of light rail on neighborhood retail activity. *arXiv: Applications* arXiv:2004.05027.

Gupta, S., L. Montenovo, T. D. Nguyen, F. Lozano-Rojas, I. M. Schmutte, K. I. Simon, B. A. Weinberg and C. Wing (2020). Effects of social distancing policy on labor market outcomes. Working paper 27280, NBER, Cambridge, MA.

Hsiang, S., D. Allen, S. Annan-Phan, K. Bell, I. Bolliger, T. Chong, H. Druckenmiller, et al. (2020). The effect of large-scale anti-contagion policies on the Covid-19 pandemic. *Nature 584*(7820), 262–7.

Juranek, S. and F. Zoutman (2020). The effect of social distancing measures on the demand for intensive care: Evidence on Covid-19 in Scandinavia. Working paper 8262, CESifo, Munich, Germany.

Kim, S., C. Lee and S. Gupta (2020). Bayesian synthetic control methods. *Journal of Marketing Research 57*(5), 831–52.

Lim, J. T., K. Maung, S. T. Tan, S. E. Ong, J. M. Lim, J. R. Koo, H. Sun, et al. (2021). Estimating direct and spill-over impacts of political elections on Covid-19 transmission using synthetic control methods. *PLoS Computational Biology 17*(5), e1008959.

Liu, L., Y. Wang and Y. Xu (2021). A practical guide to counterfactual estimators for causal inference with time-series cross-sectional data. SSRN: http://dx.doi.org/10.2139/ssrn.3555463.

Lu, H., P. Nie and L. Qian (2021). Do quarantine experiences and attitudes towards Covid-19 affect the distribution of mental health in China? A quantile regression analysis. *Applied Research in Quality of Life 16*(5), 1925–42.

Maloney, W. and T. Taskin (2020). Determinants of social distancing and economic activity during Covid-19: A global view. Policy Research Working paper 9242, World Bank, Washington, DC.

Mitze, T., R. Kosfeld, J. Rode and K. Wälde (2020). Face masks considerably reduce Covid-19 cases in Germany. *Proceedings of the National Academy of Sciences 117*(51), 32293–301.

MoH. (2020). Republic of Turkey, Ministry of Health, Covid-19 Information Platform. https://covid19asi.saglik.gov.tr/?_Dil=2.

Orak, N. and O. Ozdemir (2021). The impacts of Covid-19 lockdown on PM_{10} and SO_2 concentrations and association with human mobility across Turkey. *Environmental Research 197*, 111018.

Pang, X., L. Liu and Y. Xu (2022). A Bayesian alternative to synthetic control for comparative case studies. *Political Analysis 30*(2), 269–88.

Pearl, J. and D. Mackenzie (2018). *The Book of Why: The New Science of Cause and Effect*. New York: Basic Books.

Shao, J. (1993). Linear model selection by cross-validation. *Journal of the American Statistical Association 88*(422), 486–94.

Tubadji, A., F. Boy and D. Webber (2020). Narrative economics, public policy and mental health. *Center for Economic Policy Research 20*, 109–31.

Turkstat. (2020). Address-based population registry system. Turkish Statistics Office. https://biruni.tuik.gov.tr/medas.

Uçar, A., Ş. Arslan and A. Yapalak (2020). Türkiye Covid-19 pandemisinde resmi ve tahmini sayılar. Presentation, Istanbul University. https://bilimakademisi.org/wp-content/uploads/2020/12/abdullah-ucar-sunum.pdf.

Wang, Y. (2021). Government policies, national culture and social distancing during the first wave of the Covid-19 pandemic: International evidence. *Safety Science 135*, 105138.

Wellenius, G. A., S. Vispute, V. Espinosa, A. Fabrikant, T. C. Tsai, J. Hennessy, B. Williams, et al. (2020). Impacts of state-level policies on social distancing in the United States using aggregated mobility data during the Covid-19 pandemic. *arXiv: Populations and Evolution* arXiv:2004.10172.

Xu, Y. (2017). Generalized synthetic control method: Causal inference with interactive fixed effects models. *Political Analysis 25*(1), 57–76.

Yan, Y., A. A. Malik, J. Bayham, E. P. Fenichel, C. Couzens and S. B. Omer (2021). Measuring voluntary and policy-induced social distancing behavior during the Covid-19 pandemic. *Proceedings of the National Academy of Sciences 118*(16), e2008814118.

SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Online Appendix
Replication Package

Co-editor Victor Chernozhukov handled this manuscript.