Journal of Development Economics 144 (2020) 102458

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Panel data and experimental design☆

Fiona Burlig[a,*], Louis Preonas[b], Matt Woerman[c]

[a] University of Chicago Harris School of Public Policy and NBER, Keller Center, 1307 E. 60th Street, Chicago, IL, 60637, USA
[b] Department of Agricultural and Resource Economics, University of Maryland, 2200 Symons Hall, 7998 Regents Drive, College Park, MD, 20742, USA
[c] Department of Resource Economics, University of Massachusetts Amherst, Stockbridge Hall, 80 Campus Center Way, Amherst, MA, 01003, USA

ARTICLE INFO

JEL Codes:
B4
C23
C9
O1
Q4

Keywords:
Power
Experimental design
Panel data
Sample size

ABSTRACT

How should researchers design panel data experiments? We analytically derive the variance of panel estimators, informing power calculations in panel data settings. We generalize Frison and Pocock (1992) to fully arbitrary error structures, thereby extending McKenzie (2012) to allow for non-constant serial correlation. Using Monte Carlo simulations and real-world panel data, we demonstrate that failing to account for arbitrary serial correlation *ex ante* yields experiments that are incorrectly powered under proper inference. By contrast, our "serial-correlation-robust" power calculations achieve correctly powered experiments in both simulated and real data. We discuss the implications of these results, and introduce a new software package to facilitate proper power calculations in practice.

1. Introduction

Randomized controlled trials (RCTs) are an increasingly popular method for applied economics research (Card et al. (2011). When designing RCTs, researchers typically use *ex ante* power calculations to evaluate the trade-off between sample size and statistical precision. If the sample is too small, the experiment will be unable to distinguish between true and false null hypotheses; at the same time, overly large samples waste resources.[1] The economics literature on power calculations has focused on single-wave experiments, where units are randomized into treatment and control groups, and researchers observe each unit once.[2] In a widely cited paper based on results from Frison and Pocock (1992), McKenzie (2012) recommends panel data experiments, where multiple observations per unit help to increase statistical power. This is especially attractive in settings where collecting additional waves of data for one individual is cheaper than enrolling more individuals. As a result, panel RCTs have become increasingly common

☆ We thank Andrew Foster, several anonymous referees, Michael Anderson, Maximilian Auffhammer, Patrick Baylis, Joshua Blonz, Severin Borenstein, Hai-Anh Dang, Aureo de Paula, Meredith Fowlie, James Gillan, Erin Kelley, Jeremy Magruder, Aprajit Mahajan, Edward Miguel, Tavneet Suri, Catherine Wolfram, and seminar participants at UC Berkeley, BITSS, NEUDC, and EMEE for valuable comments. Molly Van Dop provided excellent research assistance. Funding for this research was provided by the Berkeley Initiative for Transparency in the Social Sciences, a program of the Center for Effective Global Action (CEGA), with support from the Laura and John Arnold Foundation. Burlig was generously supported by the National Science Foundation's Graduate Research Fellowship Program under grant DGE-1106400, and Preonas was generously supported by Resources for the Future's Joseph L. Fisher Dissertation Fellowship. All remaining errors are our own. Our accompanying STATA package, `pcpanel`, is available from `ssc`.

* Corresponding author.
E-mail addresses: burlig@uchicago.edu (F. Burlig), lpreonas@umd.edu (L. Preonas), mwoerman@umass.edu (M. Woerman).

[1] Bloom (1995) provides an early framework for power calculations, while Duflo et al. (2007) and Glennerster and Takavarasha (2013) detail their implementation in practice. Cohen (1977) and Murphy et al. (2014) are also classic references.

[2] Researchers often collect two waves of data, but estimate treatment effects using post-treatment data only, controlling for the baseline level of the outcome variable (following McKenzie (2012)). Baird et al. (2018) extends the standard cross-sectional setup to randomized saturation designs, capable of measuring spillover and general equilibrium effects. Athey and Imbens (2017) discusses statistical power using a randomization inference approach.

[3] Recent panel RCTs include Bloom et al. (2013); Blattman et al. (2014); Jessoe and Rapson (2014); Bloom et al. (2015); Fowlie et al. (2017); Atkin et al. (2017a, 2017b); McKenzie (2017); and Fowlie et al. (2018).

https://doi.org/10.1016/j.jdeveco.2020.102458
Received 15 June 2018; Received in revised form 26 August 2019; Accepted 17 February 2020
Available online 4 March 2020
0304-3878/© 2020 Elsevier B.V. All rights reserved.

in recent years.[3]

At the same time, panel data pose challenges for statistical inference, due to non-constant within-unit serial correlation. Bertrand, Duflo, and Mullainathan (2004) demonstrate that failing to account for this correlation structure can bias standard errors towards zero, raising the probability of a Type I error. In order to achieve correct false rejection rates, applied econometricians often implement the cluster-robust variance estimator (CRVE), or use "clustered standard errors", which accommodates arbitrary serial correlation within panel units.[4]

Besides affecting the false rejection rate, serial correlation can also impact statistical power. Hence, it is important to properly account for serial correlation during both *ex post* analysis and *ex ante* power calculations.[5] However, McKenzie (2012) only allows for *constant* serial correlation *ex ante* (equivalent to assuming i.i.d. errors after removing unit fixed effects), despite evidence that panel data typically exhibit more complex serial correlation (Bertrand et al. (2004)). While Frison and Pocock (1992) propose a framework for incorporating non-constant serial correlation into *ex ante* power calculations, they rely on restrictive assumptions—homogeneous error structures across units and deterministic time shocks—which are likely unrealistic in practice. As a result, there is a gap in the existing literature that may preclude researchers from designing properly powered experiments using panel data.

In this paper, we derive analytical expressions for the variance of panel estimators under *arbitrary* non-i.i.d. error structures, while also allowing for random time shocks. We use these expressions to (i) formalize a power calculation formula for difference-in-differences estimators that is robust to arbitrary serial correlation, and (ii) devise a method for estimating the required inputs to this formula from real data.[6]

We conduct Monte Carlo analyses using both simulated and real data, and demonstrate that the methods outlined in McKenzie (2012) yield experiments that are incorrectly powered in the presence of non-constant serial correlation, even with proper *ex post* inference. These methods tend to yield dramatically overpowered experiments in short panels and dramatically underpowered experiments in long panels. By allowing for non-constant serial correlation *ex ante*, our "serial-correlation-robust" power calculation approach achieves the desired power in both simulated and real data. Ultimately, we provide researchers with both the theoretical insights and practical tools to design properly powered experiments in panel data settings.

We make three main contributions to the economics literature on experimental design. First, we show that standard power calculation methods for panel RCTs (discussed in McKenzie (2012)) fail in the presence of arbitrary serial correlation. Second, we derive a new power calculation formula for difference-in-differences, allowing for *arbitrary* serial correlation, which extends Frison and Pocock (1992) by accommodating heterogeneous error structures across units and random time shocks. This serial-correlation-robust formula enables researchers to calibrate panel RCTs to the desired power. Finally, we provide guidance for designing panel RCTs in real experimental settings, and introduce an accompanying STATA package (`pcpanel`) to facilitate proper power calculations in practice.

The paper proceeds as follows. Section 2 presents analytical power calculations for panel data with arbitrary serial correlation, and uses Monte Carlo simulations to evaluate the performance of these results. Section 3 extends these simulation results to real experimental data. Section 4 discusses practical issues related to power calculations, and introduces our accompanying software package. Section 5 concludes.

2. Power calculations for panel data

Power calculations provide an *ex ante* estimate of the smallest effect size that an experiment, with a given sample size and experimental design will be able to statistically detect. Most power calculations take the following form:

$$MDE = \left(t^d_{1-\kappa} + t^d_{\alpha/2}\right)\sqrt{\text{Var}\left(\hat{\tau} \mid \mathbf{X}\right)} \tag{1}$$

where $\text{Var}(\hat{\tau} \mid \mathbf{X})$ is the exact finite sample variance of the treatment effect estimator, conditional on independent variables $\mathbf{X}$; $t^d_{\alpha/2}$ is the critical value of a t distribution with d degrees of freedom associated with the probability of a Type I error, α, in a two-sided test against a null hypothesis of $\tau = 0$; and $t^d_{1-\kappa}$ is the critical value associated with the probability of correctly rejecting a false null, κ.[7] These parameters determine the minimum detectable effect (*MDE*), the smallest value $|\tau| > 0$ for which the experiment will (correctly) reject the null $\tau = 0$ with probability κ at the significance level α.

This paper's core contribution is our "serial-correlation-robust" (SCR) power calculation formula for designing experiments with panel data. This section outlines our model and resulting SCR formula for the difference-in-differences (DD) estimator, which extends Frison and Pocock (1992) and McKenzie (2012) by incorporating arbitrary (non-constant) serial correlation. Using simulations, we demonstrate the importance of accounting for this correlation *ex ante* in order to achieve the desired statistical power *ex post*. We also consider three sensitivities: short panels, which are traditionally of interest in development economics; alternative treatment effect estimators; and alternative assumptions on the data generating process (DGP).

2.1. Serial-correlation-robust power calculations

We begin with a model in which there are J units, P proportion of which are randomized into treatment. The researcher collects outcome data Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For treated units, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for control units, $D_{it} = 0$ in all periods.[8]

Assumption 1. (Data generating process).

The data are generated according to the following model:

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \delta_t + \omega_{it}$$

where υ_i is a unit-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\upsilon)$; δ_t is a time-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\delta)$; ω_{it} is an idiosyncratic

[3] See White (1984), Arellano (1987), and Cameron and Miller (2015) for more details on the CRVE. Abadie et al. (2017) underscore the need to use the CRVE in experiments where treatment assignment is correlated within clusters—as in most panel RCTs, where treated units remain treated throughout the experiment.

[4] If researchers do not adjust their standard errors *ex post* to account for within-unit serial correlation in panel data, they will likely over-reject true null hypotheses. If they adjust their standard errors *ex post* but do not adjust their *ex ante* power calculations to account for within-unit serial correlation, they introduce a mismatch between *ex ante* and *ex post* assumptions that will likely yield incorrectly powered experiments.

[5] Recent experiments published in top economics journals use either the difference-in-differences estimator or the ANCOVA estimator. We discuss ANCOVA in Sections 2.2.2 and 3.1, where standard power calculation techniques similarly ignore non-constant serial correlation within panel units. However, analytically deriving the variance of the ANCOVA estimator necessitates restrictive assumptions on the data generating process, causing analytical ANCOVA power calculations to perform poorly with real data. For this reason, we focus on power calculations for difference-in-differences.

[6] For one-sided tests, $t^d_{\alpha/2}$ can be replaced with t^d_α. $1-\kappa$ gives the probability of a false rejection, or a Type II error. The degrees of freedom, d, will depend on the dimensions of $\mathbf{X}$ and the treatment effect estimator in question.

[7] Put differently, we assume that there is a control group of units that is never treated in the sample period, and a treatment group of units for which treatment turns on in a particular time period (and persists through all subsequent periods). This is a standard design for panel RCTs in economics.

error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma_\omega^2)$; and τ *is the treatment effect, which is assumed to be homogeneous across all units and all time periods.*[9]

Assumption 2. (Strict exogeneity).

$E[\omega_{it} \mid \mathbf{X}] = 0$, *where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$, $J - 1$ *unit dummies, and* $(m + r) - 1$ *time dummies. This follows from random assignment of* D_{it}.

Assumption 3. (Balanced panel).

The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.

Assumption 4. (Independence across units).

$E[\omega_{it}\omega_{js} \mid \mathbf{X}] = 0, \forall i \neq j, \forall t, s.$

Assumption 5. (Symmetric covariance structures). *Define:*

$$\psi^B \equiv \frac{2}{Jm(m-1)} \sum_{i=1}^{J} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} Cov(\omega_{it}, \omega_{is} \mid \mathbf{X})$$

$$\psi^A \equiv \frac{2}{Jr(r-1)} \sum_{i=1}^{J} \sum_{t=1}^{r-1} \sum_{s=t+1}^{r} Cov(\omega_{it}, \omega_{is} \mid \mathbf{X})$$

$$\psi^X \equiv \frac{1}{Jmr} \sum_{i=1}^{J} \sum_{t=-m+1}^{0} \sum_{s=1}^{r} Cov(\omega_{it}, \omega_{is} \mid \mathbf{X})$$

to be the average pre-treatment, post-treatment, and across-period covariance between different error terms of the same unit, respectively. Define ψ_T^B, ψ_T^A, *and* ψ_T^X *analogously, where we consider only the PJ treated units; also define* ψ_C^B, ψ_C^A, and ψ_C^X *analogously, where we consider only the* $(1 - P)J$ *control units. Using these definitions, assume that* $\psi^B = \psi_T^B = \psi_C^B$; $\psi^A = \psi_T^A = \psi_C^A$; *and* $\psi^X = \psi_T^X = \psi_C^X$.[10]

Under these assumptions, the OLS estimator with unit and time fixed effects is $\hat{\tau} = (\ddot{\mathbf{D}}'\ddot{\mathbf{D}})^{-1}\ddot{\mathbf{D}}'\ddot{\mathbf{Y}}$, with $E[\hat{\tau}] = \tau$. Assumptions 1–5 yield a power calculation formula that is robust to arbitrary serial correlation[11]:

$$MDE = (t_{1-\kappa}^J + t_{\alpha/2}^J) \times \sqrt{\underbrace{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma_\omega^2 + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]}_{\text{Var}(\hat{\tau}|\mathbf{X})}} \tag{2}$$

Throughout the remainder of the paper, we refer to Equation (2) as the "serial-correlation-robust" (SCR) power calculation formula. Note that under cross-sectional randomization, this expression for the variance of $\hat{\tau}$ still holds in expectation, even in the presence of within-period error correlations across units:

Lemma 1. *In a panel difference-in-differences model with treatment randomly assigned at the unit level,* $\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma_\omega^2 + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]$ *is an unbiased estimator of the expectation of* $\text{Var}(\hat{\tau} \mid \mathbf{X})$, *even in the presence of arbitrary within-period cross-sectional correlations. See Appendix A.3 for a proof, and see Appendix A.2.3 for a more general model that relaxes Assumptions 4-5.*

2.1.1. Accounting for serial correlation

Our SCR power calculation formula generalizes the Frison and Pocock (1992) (henceforth FP) difference-in-differences formula to accommodate fully arbitrary correlation structures. Whereas FP assume that all units share a homogeneous correlation structure, our SCR formula accommodates arbitrary heterogeneous correlation structures across units. We also allow for random time shocks across all units, which are characteristic of most economic datasets.[12] As a result, our SCR formula is able to flexibly accommodate more realistic panel data structures.

McKenzie (2012) is the most widely cited reference for power calculations using panel data in economics. McKenzie's results follow from FP's original derivations, yet they impose a more restrictive assumption on the data generating process: constant serial correlation between any two time periods, within each cross-sectional unit.[13] Using our notation, McKenzie's difference-in-differences power calculation formula becomes[14]:

$$MDE = \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\left(\frac{\sigma_\omega^2}{P(1-P)J}\right)\left(\frac{m+r}{mr}\right)} \tag{3}$$

This is equivalent to the SCR formula when ψ^A, ψ^B, and ψ^X are all equal to zero—allowing only *constant* serial correlation of the *composite* error term (ε_{it}, in McKenzie's notation), or an i.i.d. idiosyncratic error term (ω_{it}, in our notation). As highlighted by Bertrand et al. (2004, henceforth BDM), this assumption is likely unrealistic because most panel

[9] While we assume a homogeneous treatment effect, our derivations also hold if τ varies across time periods (as in Frison and Pocock (1992), and McKenzie (2012)). In addition, this data generating process is quite general and nests versions with no random unit or time shocks; reducing σ_υ^2 (σ_δ^2) to zero is equivalent to removing random unit (time) shocks from the data generating process. Section 2.2.3 explores data generating processes where υ_i and δ_t are deterministic, rather than random shocks. In Section 3.1 below, we explore the performance of our serial-correlation-robust power calculations in a real data setting where the true data generating process is unknown.

[10] These terms come from the derivation of the variance of the DD estimator (see Appendix A.2.2). Each ψ averages covariances over units with potentially heterogeneous error structures (ω_{it} from the underlying DGP). In addition, the ψ terms depend both on the error structure and on the length of the experiment (m and r). We choose the letters "B" to indicate the Before-treatment period, and "A" to indicate the After-treatment period. We index the m pre-treatment periods $\{-m+1, \ldots, 0\}$, and the r post-treatment periods $\{1, \ldots, r\}$. In a randomized setting, $E\left[\psi^B\right] = E\left[\psi_T^B\right] = E\left[\psi_C^B\right]$, $E\left[\psi^A\right] = E\left[\psi_T^A\right] = E\left[\psi_C^A\right]$, and $E\left[\psi^X\right] = E\left[\psi_T^X\right] = E\left[\psi_C^X\right]$, making this a reasonable assumption *ex ante*. However, it is possible for treatment to alter the covariance structure of treated units only.

[11] We present the formal derivation of this formula in Appendix A.2.2. Note that if $m = 1$ (or $r = 1$), ψ^B (or ψ^A) is not defined and is multiplied by 0 in Equation (2).

[12] If we impose FP's assumptions, our SCR formula collapses to the FP formula for the difference-in-differences model with unequal correlations.

[13] The preceding paragraph describes the most general FP model, reported on page 1701. McKenzie draws from the more restrictive FP model, reported on p. 1693.

[14] See Appendix A.2.1 for the derivation. This formula is analogous to McKenzie's theoretical formula. McKenzie (2012, p. 215) suggests an alternative approach for empirical applications with non-constant serial correlation, which we explore in Appendix C.4 using both simulated and real data. We find that this approach, while effective in panels with only 1 pre- and 1 post-treatment period, delivers improperly powered experiments in longer panels. We also find this approach to be less effective as the degree of autocorrelation increases. In Appendices D.1 and E, we derive a method to use real pre-existing data to perform power calculations using the SCR method, which is effective even in settings where the true DGP is unknown.

datasets in economics exhibit non-constant serial correlation.[15]

To further illustrate the difference between the McKenzie and SCR models, consider two cross-sectional units (indexed $\{i,j\}$) and four time periods (indexed $\{0,1,2,3\}$), with the data generating process described in Assumption 1. The vector of idiosyncratic errors, $\boldsymbol{\omega}$, and the corresponding variance-covariance matrix, $\boldsymbol{\Omega}$, can be represented as follows[16]:

$$\boldsymbol{\omega} = \begin{bmatrix} \omega_{i0} \\ \omega_{i1} \\ \omega_{i2} \\ \omega_{i3} \\ \omega_{j0} \\ \omega_{j1} \\ \omega_{j2} \\ \omega_{j3} \end{bmatrix} \quad \boldsymbol{\Omega} = \begin{bmatrix} \sigma^2_{i0} & & & & & & & \\ \sigma_{i0,i1} & \sigma^2_{i1} & & & & & & \\ \sigma_{i0,i2} & \sigma_{i1,i2} & \sigma^2_{i2} & & & & & \\ \sigma_{i0,i3} & \sigma_{i1,i3} & \sigma_{i2,i3} & \sigma^2_{i3} & & & & \\ & & & & \sigma^2_{j0} & & & \\ & & & & \sigma_{j0,j1} & \sigma^2_{j1} & & \\ & & & & \sigma_{j0,j2} & \sigma_{j1,j2} & \sigma^2_{j2} & \\ & & & & \sigma_{j0,j3} & \sigma_{j1,j3} & \sigma_{j2,j3} & \sigma^2_{j3} \end{bmatrix}$$

Serial correlation within each unit is represented by the (potentially non-zero) covariance terms $\sigma_{it,is}$ and $\sigma_{jt,js}$, for all $t \neq s$. In contrast, McKenzie's model allows only for constant serial correlation—that is, $\upsilon_i \neq 0$ and ω_{it} i.i.d., in our notation. The assumption of i.i.d. idiosyncratic errors would force all off-diagonal covariance elements in $\boldsymbol{\Omega}$ to equal zero, thereby precluding the types of non-constant serial correlation that BDM highlight (e.g., autoregressive processes).

The magnitudes of these off-diagonal covariance terms directly affect the variance of the DD estimator. The three ψ terms defined above, along with the error variance and experimental design parameters, are sufficient to fully characterize the true variance of the treatment effect estimator in this model. To fix ideas, using the four-period model above and supposing treatment is administered beginning at $t = 2$, these covariance parameters are:

$$\psi^B = \frac{\sigma_{i0,i1} + \sigma_{j0,j1}}{2}$$

$$\psi^A = \frac{\sigma_{i2,i3} + \sigma_{j2,j3}}{2}$$

$$\psi^X = \frac{\sigma_{i0,i2} + \sigma_{i1,i2} + \sigma_{i0,i3} + \sigma_{i1,i3} + \sigma_{j0,j2} + \sigma_{j1,j2} + \sigma_{j0,j3} + \sigma_{j1,j3}}{8}$$

Alternatively, if treatment is administered beginning at $t = 1$, these covariance terms become:

$$\psi^B = \text{(not defined for only 1 pre-treatment period)}$$

$$\psi^A = \frac{\sigma_{i1,i2} + \sigma_{i1,i3} + \sigma_{i2,i3} + \sigma_{j1,j2} + \sigma_{j1,j3} + \sigma_{j2,j3}}{6}$$

$$\psi^X = \frac{\sigma_{i0,i1} + \sigma_{i0,i2} + \sigma_{i0,i3} + \sigma_{j0,j1} + \sigma_{j0,j2} + \sigma_{j0,j3}}{6}$$

Assumption 5 generalizes this structure to a model with J units across m pre-treatment periods and r post-treatment periods. Equation (2) shows that greater average covariance in the pre- or post-treatment periods (ψ^B or ψ^A) increases the *MDE*. Intuitively, as errors for treated and control units are more serially correlated, the benefits of collecting multiple waves of pre- and post-treatment data are eroded. However, cross-period covariance (ψ^X) enters Equation (2) negatively. This highlights a key property of the DD estimator: because DD identifies the treatment effect off of differences between post- and pre-treatment outcomes, greater serial correlation between pre- and post-treatment observations makes differences caused by treatment easier to detect.

Assuming that the within-unit correlation structure does not vary systematically across time periods, positively correlated errors will imply positive ψ^B, ψ^A, and ψ^X. Because ψ^B and ψ^A enter the SCR formula positively, while ψ^X enters negatively, serial correlation may either increase or decrease the *MDE* relative to the McKenzie i.i.d. case. Specifically, serial correlation will increase the *MDE* if and only if:

$$\left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A > 2\psi^X \tag{4}$$

This inequality is more likely to hold in longer panels, for two reasons. First, as the number of pre- and post-treatment periods increases, $\left(\frac{m-1}{m}\right)$ and $\left(\frac{r-1}{r}\right)$ approach one. Second, the covariance terms contributing to ψ^X lie farther away from the diagonal of the variance-covariance matrix than the covariance terms contributing to ψ^B and ψ^A. Because errors from non-adjacent time periods are likely to be less correlated than errors from adjacent time periods, and because the number of far-off-diagonal covariances increases relatively more quickly for ψ^X as the panel becomes longer, ψ^X is increasingly likely to be smaller than ψ^B and ψ^A in longer panels.[17]

2.1.2. Monte Carlo simulations

If a randomized experiment relies on a power calculation that fails to properly account for serial correlation *ex ante*, its realized power may be different from the desired κ. To understand the extent to which this matters in practice, we conduct a series of Monte Carlo simulations comparing the McKenzie model and the SCR model over a range of panel lengths and error correlations. We simulate three cases and compute the Type I error rate and the statistical power for each: (i) experiments that fail to account for serial correlation in ω_{it} (i.e. the *idiosyncratic* component of error term) both *ex ante* and *ex post*; (ii) experiments that fail to account for serial correlation in ω_{it} *ex ante* but apply the CRVE to account for serial correlation *ex post*; and (iii) experiments that both account for serial correlation in ω_{it} *ex ante* and apply the CRVE *ex post*.

For each set of parameter values characterizing both a data generating process and an experimental design, we first calculate two treatment effect sizes: τ^{McK} equal to the *MDE* from the McKenzie formula, and τ^{SCR} equal to the *MDE* from our SCR formula. Second, we use these parameter values to create a panel dataset from the following DGP:

$$Y_{it} = \beta + \upsilon_i + \delta_t + \omega_{it} \tag{5}$$

where ω_{it} follows an AR(1) process:

$$\omega_{it} = \gamma\omega_{i(t-1)} + \xi_{it} \tag{6}$$

Third, we randomly assign treatment, with effect sizes τ^{McK}, τ^{SCR}, and $\tau^0 = 0$ at the unit level, to create three separate outcome variables. Fourth, we regress each of these outcome variables on their respective treatment indicators and include unit fixed effects and time fixed effects. Fifth, we compute both OLS standard errors and CRVE standard errors clustered at the unit level, for all three regressions. We repeat

[15] Our SCR formula differs from that of McKenzie in one additional way: McKenzie's empirical specification does not include a unit fixed effect, whereas the estimator underlying the SCR model does. This fixed effect absorbs any constant serial correlation of composite errors, leaving no remaining serial correlation between the idiosyncratic error terms of our SCR model. Thus, constant serial correlation in McKenzie's framework translates to no serial correlation in our SCR framework. Section 2.2.3 discusses the implications of removing constant serial correlation from the true DGP, and Section 2.2.2 considers alternative treatment effect estimators.

[16] We show only the lower diagonal of the variance-covariance matrix because $\boldsymbol{\Omega}$ is symmetric. We also omit the cross-unit covariance terms for notational convenience, which are zero under Assumption 4.

[17] This analytical result illustrates how the correlation structure impacts the variance of estimators that use both pre- and post-treatment data. Fig. 10 below demonstrates that, in cases with strong serial correlation, adding time periods can actually increase the MDE. This is not specific to the DD estimator: Appendix Fig. C3 shows the same pattern for power calculations using the ANCOVA estimator.

steps two through five 10,000 times for each set of parameters, calculating rejection rates of the null hypothesis $\tau = 0$ across all simulations. For τ^{McK} and τ^{SCR}, this rate represents the realized power of the experiment. For the placebo τ^0, it represents the realized false rejection rate.

We test five levels of the AR(1) parameter: $\gamma \in \{0, 0.3, 0.5, 0.7, 0.9\}$. For each γ, we simulate symmetric panels with an equal number of pre-treatment and post-treatment periods, with panel lengths ranging from 2 periods ($m = r = 1$) to 40 periods ($m = r = 20$). We hold J, P, β, σ^2_υ, σ^2_δ, α, and κ fixed across all simulations, and we adjust the variance of the white noise term σ^2_ξ such that every simulation has a fixed idiosyncratic variance σ^2_ω.[18] This allows γ to govern the proportion of σ^2_ω that is serially correlated.[19] The covariance terms ψ^B, ψ^A, and ψ^X have closed-form expressions under the AR(1) structure, and we use these expressions to calculate τ^{SCR}.[20] This causes τ^{SCR} to vary both with the degree of serial correlation and panel length, whereas τ^{McK} varies only with panel length.

Fig. 1 displays the results of this exercise. The left column shows rejection rates under the McKenzie formula using OLS standard errors, which assumes zero serial correlation in ω_{it} both *ex ante* and *ex post*. The middle column shows rejection rates under the FP formula using CRVE standard errors, which accounts for serial correlation in ω_{it} *ex post* only. The right column show rejection rates under our SCR formula using CRVE standard errors, which allows for serial correlation in ω_{it} both *ex ante* and *ex post*. The top row plots realized power as a function of the number of pre/post-treatment periods, which should equal $\kappa = 0.80$ in a properly designed experiment. The bottom row plots the corresponding realized false rejection rates, which should equal the desired $\alpha = 0.05$. Only the SCR formula, in conjunction with CRVE standard errors, achieves the desired 0.80 and 0.05 across all panel lengths and AR(1) parameters.

The left column confirms the BDM result that failing to appropriately account for serial correlation leads to false rejection rates dramatically higher than $\alpha = 0.05$. Even a modest AR(1) parameter of $\gamma = 0.5$ yields a 20 percent probability of a Type I error, for panels with $m = r > 5$. This underscores the fact that randomization cannot correct serial correlation in panel settings, and experiments that collect multiple waves of data from the same cross-sectional units should account for within-unit correlations over time. By contrast, the middle and right columns apply the CRVE and reject placebo effects at the desired rate of $\alpha = 0.05$.

The middle column shows how failing to properly account for serial correlation *ex ante* can yield dramatically overpowered or underpowered experiments. Particularly for longer panels with $m = r > 5$, performing power calculations via Equation (3) may actually produce experiments with less than 50 percent power, even though researchers intended to achieve power of 80 percent (i.e., $\kappa = 0.80$). For a relatively high serial correlation of $\gamma = 0.7$, simulations based on the conventional power calculation formula yield power less than 32 percent for $m = r > 10$. This is consistent with the BDM finding that applying the CRVE reduces statistical power, even though doing so achieves the desired Type I error rate. By contrast, the right column applies both the SCR power calculation formula and the CRVE, and these simulations achieve the desired power of $\kappa = 0.80$ for each value of γ.

The middle column also highlights how failing to account for serial correlation *ex ante* may either increase *or* decrease statistical power, as shown in Equation (4). For shorter panels, using the FP formula instead of our SCR formula yields dramatically overpowered experiments. While this may seem counterintuitive, Equation (4) is increasingly unlikely to hold as m and r decrease to 1. In the extreme case where $m = r = 1$, ψ^B and ψ^A do not enter, and the only covariance term in the SCR formula is ψ^X, which enters negatively. These simulations reveal that just as higher γ yields more dramatically underpowered experiments for longer panels, higher γ yields more dramatically *over*powered experiments for shorter panels.[21]

These results are striking. For even a modest degree of AR(1) serial correlation, applying the McKenzie power calculation formula will not yield experiments of the desired statistical power. By contrast, the SCR formula achieves the desired power for all panel lengths and AR(1) parameters. While AR(1) is a relatively simple correlation structure, it serves as a reasonable first-approximation for more complex forms of serial correlation. Given that real-world panel datasets exhibit enough serial correlation to produce high Type I error rates, it stands to reason that such serial correlation can similarly impact the statistical power of experiments if not properly accounted for *ex ante*.

2.2. Sensitivities

2.2.1. Short panels

While experiments with long panels have become increasingly common, short-panel experiments remain a development economics staple. Our SCR formula generalizes to panels as short as 2 periods ($m = r = 1$), where Equation (2) simplifies to:

$$MDE = (t^J_{1-\kappa} + t^J_{\alpha/2})\sqrt{\left(\frac{1}{P(1-P)J}\right)\left[2\sigma^2_\omega - 2\psi^X\right]} \tag{7}$$

Notably, ψ^B and ψ^A are no longer defined for $m = r = 1$. However, ψ^X remains, meaning that the wedge between the SCR and McKenzie formulas also remains. By omitting ψ^X (which enters Equation (7) negatively), the McKenzie formula will tend to yield over-powered two-period experiments.

It may be unintuitive that serial correlation still matters in a two-period panel, which is isomorphic to a cross-sectional first-differences model, and does not require clustered standard errors to achieve the desired Type I error rate. However, unlike the Type I error rate, statistical power (i.e. 1 minus the Type II error rate) is a function of the variance of the treatment effect estimator, which depends on ψ^X. Hence, ignoring ψ^X from Equation (7) will bias *MDE* upward, likely leading researchers to choose an unnecessarily large J.[22]

To illustrate the difference between the SCR and McKenzie formulas for short panel experiments, we extend Fig. 1 for panels with between 1 and 6 pre/post-treatment periods. We again simulate data with serially correlated errors of varying AR(1) parameters γ, and calibrate treatment effect sizes using each power calculation formula. Fig. 2 displays realized power for panels with $1 \leq m \leq 6$ and $1 \leq r \leq 6$, varying m and r independently. Across all levels of non-zero serial correlation ($\gamma > 0$), the McKenzie formula yields over-powered experiments for panels with either 1 pre-treatment or 1 post-treatment period. Consistent with Fig. 1, realized power under the McKenzie formula decreases

[18] Note that σ^2_ω is a parameter of the DGP, as described in Assumption 1; it is not a function of a particular realization of data, desired experiment length, or estimating equation. In this section, we ignore the practicalities of estimating σ^2_ω (and other DGP parameters) because we know their true underlying values. See Appendix D.1 for a detailed discussion of how to estimate these parameters from data.

[19] In an AR(1) model, the relationship between the variance of the AR(1) process and the variance of the white noise disturbance depends on γ, with $\sigma^2_\omega = \frac{\sigma^2_\xi}{1-\gamma^2}$.

[20] Appendix B.1 provides these derivations, along with further details on these Monte Carlo simulations.

[21] Intuitively, serial correlation has two opposite effects on the statistical power of a DD estimator. It decreases power by reducing the effective number of observations for each cross-sectional unit, and it increases power by increasing the signal in estimating treatment effects off of a post−pre difference. In shorter panels, this second effect tends to dominate.

[22] Appendix A.2.4 mathematically demonstrates how power depends on ψ^X even in a two-period panel, despite the fact that the false rejection rate in a two-period panel is independent of ψ^X.

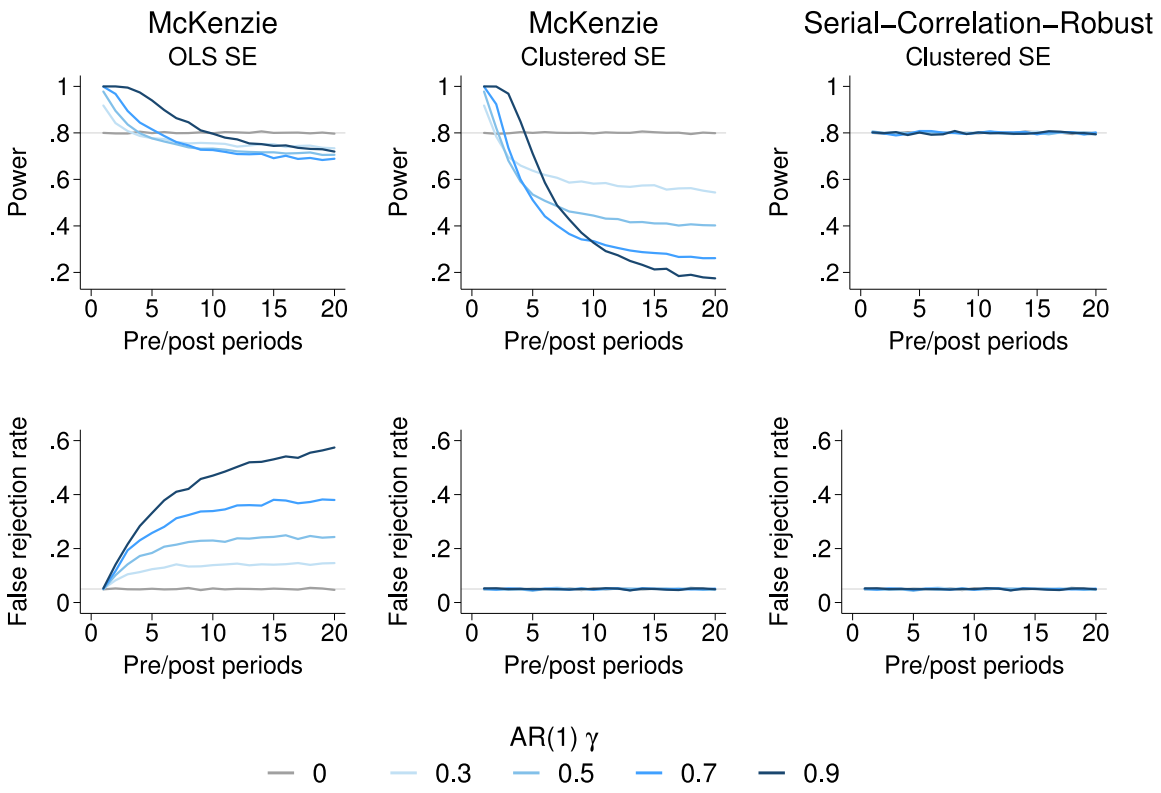


Fig. 1. Standard methods result in improperly powered experiments in AR(1) data. Notes: This figure displays power and false rejection rates from performing power calculations with three different sets of assumptions, using simulated data with AR(1) processes (with differing levels of serial correlation) and differing panel lengths (ranging from 2 (m = r = 1) to 40 (m = r = 20) periods). In the left column, we apply the standard McKenzie formula (Equation (3), which assumes away non-constant serial correlation), and use OLS standard errors ex post, in line with the assumptions of this formula. In the middle column, we again apply the McKenzie formula, but cluster standard errors ex post—which is inconsistent with the ex ante formula, but corrects for within-unit serial correlation following Bertrand et al. (2004). In the right column, we apply our serial-correlation-robust formula (Equation (2), which accounts for non-i.i.d. errors ex ante), and cluster standard errors ex post. As expected, this third set of simulations achieves the desired 80 percent power and 5 percent false rejection rate.

monotonically as panel length increases; the longer the panel, the more likely the McKenzie formula is to deliver underpowered experiments. While the McKenzie formula yields overpowered short panel experiments using these simulated data, it can yield *underpowered* experiments in real data, even for short panels.[23] By contrast, experiments calibrated using the SCR formula achieve the desired power regardless of panel length.

2.2.2. Alternative estimators

Our SCR power calculation formula assumes that researchers will employ a DD estimator with unit and time fixed effects. However, particularly in experiments with short panels, researchers often choose to use one of two alternative approaches: a simplified differences-in-differences estimator or an analysis of covariance (ANCOVA) estimator.

Simplified differences-in-differences Researchers often estimate DD models that replace a full set of unit and time fixed effects with a treatment group dummy and/or post-period dummy, respectively. The resulting estimating equation takes the following form:

$$Y_{it} = \beta + \tau\left[\text{Treat}_i \times \text{Post}_t\right] + \upsilon\left[\text{Treat}_i\right] + \delta\left[\text{Post}_t\right] + \varepsilon_{it} \tag{8}$$

where $\text{Treat}_i = 1$ if unit i is in the treatment group, and $\text{Post}_t = 1$ for all post-treatment periods. Fig. 3 explores how the McKenzie and SCR formulas perform under three alternative *ex post* DD estimating equations, which alter our original DD estimating equation by: (i) replacing unit fixed effects with a Treat_i dummy, (ii) replacing time fixed effects with a Post_t dummy, and (iii) replacing both sets of fixed effects with dummies (Equation (8)). In all cases, we use the same DGP as in Assumption 1, which includes idiosyncratic unit and time shocks; as before, we simulate data following Equation (5), with an idiosyncratic error term following an AR(1) process.

As in Fig. 1, the SCR formula achieves correctly powered experiments at all levels of serial correlation, for all three DD estimators in Fig. 3. This is because all three yield the same treatment effect estimator—and therefore have the same variance—as our original DD estimating equation with unit and time fixed effects. However, these different estimating equations have varying levels of serial correlation in the *error term*. Using OLS standard errors would produce different estimates of the variance, even though the true underlying variance is the same across these estimating equations. On the other hand, with randomized treatment, the CRVE (clustered at the unit level) correctly estimates the (same) variance in all cases, because it accounts for serial correlation within unit, regardless of whether that correlation enters through the underlying error structure or the omission of fixed effects.[24] Realized power under the McKenzie formula is like-

[23] See Appendix Fig. C1.

[24] Appendix A.4 mathematically shows that the DD estimator in Equation (8) has the same variance as the DD estimator with unit and time fixed effects.

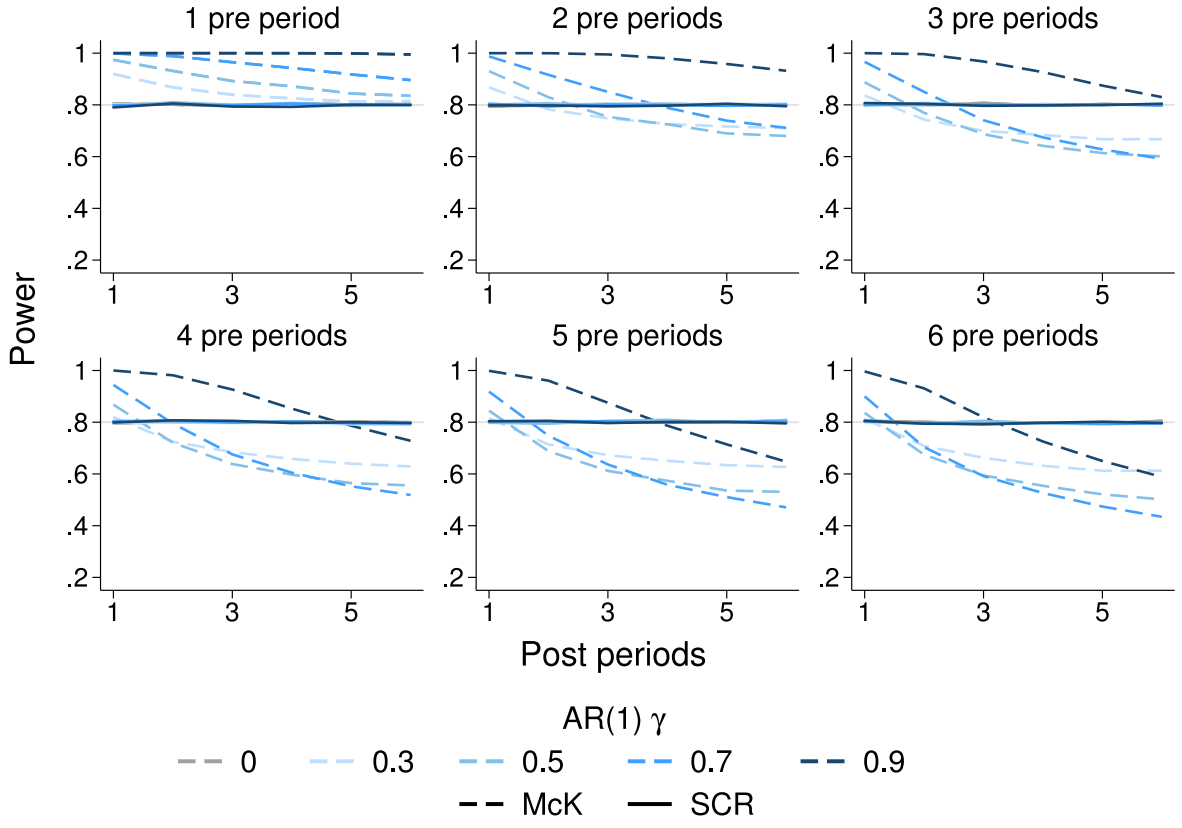


Fig. 2. Power in short panels – AR(1) data. Notes: This figure repeats the same simulation exercise as Fig. 1, except that we separately vary the number of pre-treatment and post-treatment periods. Each panel conducts simulations with a distinct number of pre-treatment periods ($1 \leq m \leq 6$), and each horizontal axis varies the number of post-treatment periods ($1 \leq r \leq 6$). Dotted lines report realized power for experiments calibrated using the McKenzie formula (i.e. the top-middle panel of Fig. 1), while solid lines report realized power using the SCR formula (i.e. the top-right panel of Fig. 1). For all cases where $m = 1$ or $r = 1$, the McKenzie formula yields over-powered experiments across the full range of positive AR(1) parameters. This shows that traditional "one baseline, one follow-up" experiments will calibrate to excessively large sample sizes if they ignore non-constant serial correlation ex ante. The SCR formula is properly powered in all cases.

wise equivalent across all three alternative estimators, yielding over-powered experiments in short panels and underpowered experiments in long panels. In Fig. 5 below, we extend this analysis to also consider alternative DGPs.

ANCOVA Another common strategy, particularly in short panels, is to employ the analysis of covariance (ANCOVA) estimator.[25] To do this, the econometrician estimates the following specification using post-treatment data only:

$$Y_{it} = \alpha + \tau D_i + \theta \overline{Y}_i^B + \varepsilon_{it} \tag{9}$$

where $\overline{Y}_i^B = \sum_{t=-m+1}^{0} Y_{it}$ is the pre-treatment average value of the dependent variable for unit i. This estimator has become popular in economics, as it is more efficient than the DD model with the same number of periods (McKenzie (2012)).

Frison and Pocock (1992) also derive a power calculation formula for the ANCOVA estimator, based on the same assumptions as their DD formula. As with the DD estimator, McKenzie draws on a more restrictive FP formula, which assumes constant serial correlation within each panel unit. McKenzie's formula (henceforth McKenzie ANCOVA) has become the standard for ANCOVA power calculations, which we adapt to our notation:

$$MDE \approx (t_{1-\kappa}^J + t_{\alpha/2}^J)\sqrt{\left(\frac{1}{P(1-P)J}\right)\left[(1-\theta)^2\sigma_v^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2\right]} \tag{10}$$

where $\theta = \frac{m\sigma_v^2}{m\sigma_v^2+\sigma_\omega^2}$.[26]

Importantly, deriving this formula under non-constant within-unit serial correlation necessitates an additional simplifying assumption for

[25] In a randomized setting where unit fixed effects are not needed for identification, this method may be preferred to DD because it more efficiently estimates $\hat{\tau}$ (Frison and Pocock (1992)). McKenzie (2012) notes that under constant serial correlation (i.i.d. ω_{it} in our SCR framework), ANCOVA is always more efficient than the DD model with the same number of time periods, but that these gains are eroded as the intracluster correlation coefficient increases. These gains are also eroded as the number of pre-treatment periods increases. Neither Frison and Pocock (1992) nor McKenzie (2012) handles the fully general case of arbitrary serial correlation. Teerenstra et al. (2012) begins with a similar setup for the ANCOVA framework, but considers the $m = r = 1$ case only, obviating the need to address the CRVE-related issues raised here.

[26] Our McKenzie ANCOVA formula differs slightly from that in McKenzie (2012) and Frison and Pocock (1992, page 1693). These previous derivations have assumed that the true data generating process follows Equation (9), where post-treatment outcomes are determined in part by pre-treatment outcomes. We instead assume unit-specific random effects, as in Assumption 1. FP and McKenzie assumed deterministic time shocks with no variance, or $\sigma_\delta^2 = 0$. We instead must assume that there are no time shocks for analytical tractability. While both data generating processes yield identical treatment effect estimators, they imply different variances of this estimator. As in Frison and Pocock (1992) and McKenzie (2012), our McKenzie ANCOVA formula is approximate because we ignore sampling error in the estimation of θ, which approaches zero as the number of units increases.

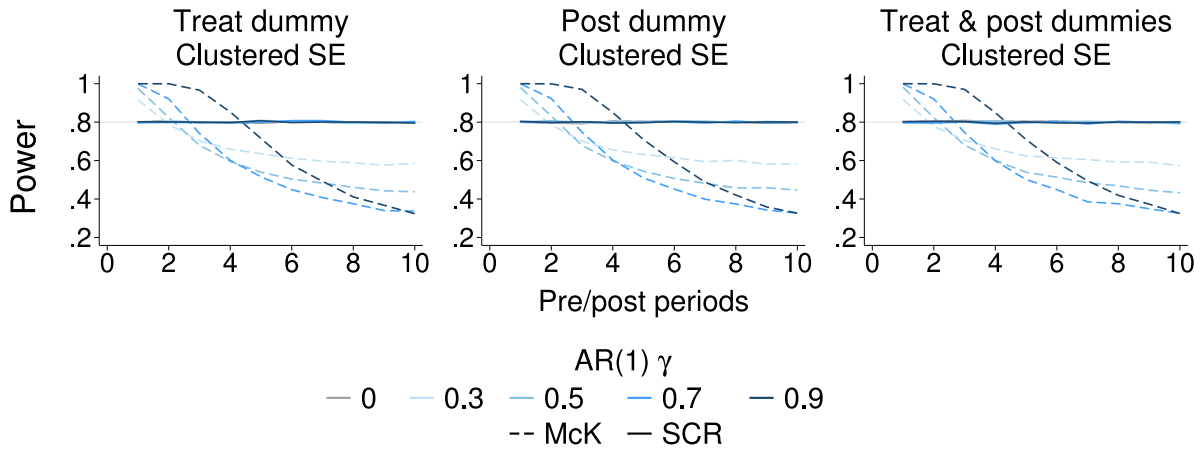


Fig. 3. SCR outperforms traditional methods with alternative estimators. Notes: This figure repeats the same simulation exercise as Fig. 1, except that each panel alters the ex post DD estimating equation. The left panel replaces unit fixed effects with a Treati dummy. The middle panel replaces time fixed effects with a Postt dummy. The right panel replaces both sets of fixed effects with Treati and Postt dummies. Dotted lines report realized power for experiments calibrated using the McKenzie formula (i.e. the top-middle panel of Fig. 1), while solid lines report realized power using the SCR formula (i.e. the top-right panel of Fig. 1). All three DD estimating equations yield power that is identical to Fig. 1 (estimated with unit and time fixed effects). This is because the variance of these estimators is a function of the underlying error structure, and the CRVE correctly accounts for unmodeled serial correlation in each case. Hence, the performance gap between McKenzie vs. SCR formulas remains for all DD estimators, provided that researchers account for serial correlation when conducting ex post inference.

analytical tractability: we must assume away time shocks.[27] Under this assumption, we derive the variance of the ANCOVA estimator allowing for arbitrary serial correlation, along with the corresponding serial-correlation-robust power calculation formula (henceforth SCR ANCOVA):

$$MDE \approx (t^{J}_{1-\kappa} + t^{J}_{\alpha/2})\times \sqrt{\frac{1}{P(1-P)J}\left[(1-\theta)^2\sigma^2_{\upsilon} + \left(\frac{\theta^2}{m} + \frac{1}{T}\right)\sigma^2_{\omega} + \frac{\theta^2(m-1)}{m}\psi^B + \frac{T-1}{T}\psi^A - 2\theta\psi^X\right]} \tag{11}$$

where $\theta = \frac{m\sigma^2_{\upsilon} + m\psi^X}{m\sigma^2_{\upsilon} + \sigma^2_{\omega} + (m-1)\psi^B}$.[28]

Fig. 4 compares the McKenzie vs. SCR ANCOVA formulas, using simulations analogous to Fig. 1. For each level of the AR(1) parameter and panel length, we compute the treatment effect size τ implied by each ANCOVA formula. Unlike Figs. 1–3, these simulations use a data-generating process *without* random time shocks (i.e. $\sigma^2_{\delta} = 0$), for consistency with the assumptions underlying Equations (10)–(11). The McKenzie ANCOVA formula produces properly-powered experiments only when idiosyncratic errors are i.i.d., while the SCR ANCOVA is robust to all levels of AR(1) serial correlation.

Even though the SCR ANCOVA formula outperforms the McKenzie ANCOVA formula in the presence of AR(1) errors, we do not recommend that researchers use this formula in real-world applications (even in short panels). Time shocks with non-zero variance are a common feature of panel data, and assuming them away may result in improperly powered experiments. We discuss this further in Section 3 below, where we apply our SCR methods to real data. If researchers plan to use the ANCOVA estimator, we recommend that they perform power calculations by simulation, as discussed in Section 4.2.2 below.

2.2.3. *Alternative data generating processes*

In addition to considering alternative estimators, we also investigate the effectiveness of the SCR power calculation formula under alternative DGPs. We consider three cases: (i) deterministic unit effects ($\sigma^2_{\upsilon} = 0$), (ii) deterministic time effects ($\sigma^2_{\delta} = 0$), and (iii), deterministic unit *and* time effects ($\sigma^2_{\upsilon} = \sigma^2_{\delta} = 0$). Given that each is simply a special case of the DGP in Assumption 1, we expect the SCR to continue to achieve the desired power. However, these cases provide more favorable conditions to test the McKenzie formula: in the absence of random unit or time shocks, the variance of the *composite* error term is the same as the variance of the *idiosyncratic* error term (ω_{it}).

We test the relative performance of the SCR and McKenzie formulas in Fig. 5. The top row presents simulations using the three alternative DGPs, while holding the fixed effects estimator fixed. In the bottom row, we match each DGP with its "matched" estimator (e.g., for the DGP with deterministic unit effects, we replace unit fixed effects with a Treat_i dummy). The SCR formula yields correctly powered experiments, even under alternative DGPs with misspecified *ex post* estimators. In contrast, the McKenzie formula generates improperly powered experiments even in the absence of unit and time shocks. Next, we test the performance of the SCR formula when we do not control the DGP, using real-world panel data.

3. Applications to real-world data

3.1. *Bloom et al. (2015) data*

In this section, we conduct an analogous simulation exercise using a real dataset from an experiment in a developing-country setting. These data come from Bloom et al. (2015), in which Chinese call center employees were randomly assigned to work either from home or from

[27] Frison and Pocock (1992) and McKenzie (2012) both assume away random time shocks. A critical step in the derivation of the ANCOVA model with time shocks and arbitrary serial correlation requires us to calculate a conditional expectation that depends on the error term ϵ_{it} and the pre-period mean $\overline{Y}^B_i$ of every unit in the experiment, which becomes analytically intractable for any reasonable number of experimental units. See Appendix A.2.5 for more details. By contrast, the variance of the DD estimator depends on the distribution of errors conditional on only the treatment indicator, which is orthogonal to the error terms by randomization.

[28] We present the formal derivation of the SCR ANCOVA formula in Appendix A.2.5. For analytical tractability, we assume that the ψ parameters are uniform across all units. We also ignore sampling error in the estimation of θ, which approaches zero as the number of units increases; Frison and Pocock (1992) and McKenzie (2012) also make this simplification. Through additional Monte Carlo simulations, we confirm that neither of these assumptions is likely to affect statistical power.

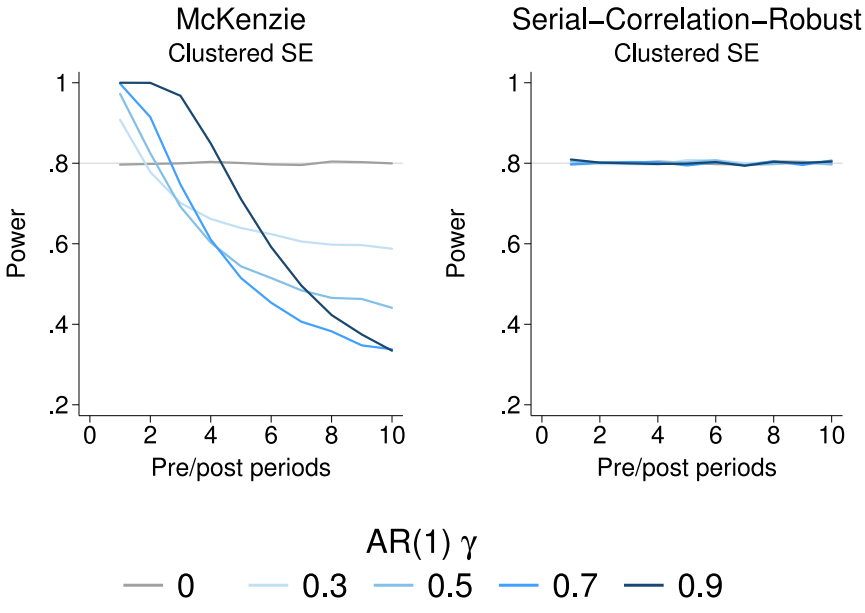


Fig. 4. Standard ANCOVA methods fail under serial correlation. Notes: This figure repeats the same simulation exercise as Fig. 1, using ANCOVA power calculation formulas ex ante and an ANCOVA estimating equation ex post. The left panel calibrates MDE using the McKenzie ANCOVA formula (Equation (10), and is analogous to the top-middle panel of Fig. 1. The right panel calibrates MDE using the SCR ANCOVA formula (Equation (11), and is analogous to the top-right panel of Fig. 1. Both set of simulations estimate Equation (9) ex post, cluster standard errors by unit, and simulate DGPs without random time shocks ($\sigma^2_\delta = 0$). As in the DD simulations, the McKenzie ANCOVA formula fails to generate correctly-powered experiments, whereas the SCR formula is properly powered across all panel lengths and degrees of AR(1) correlation.

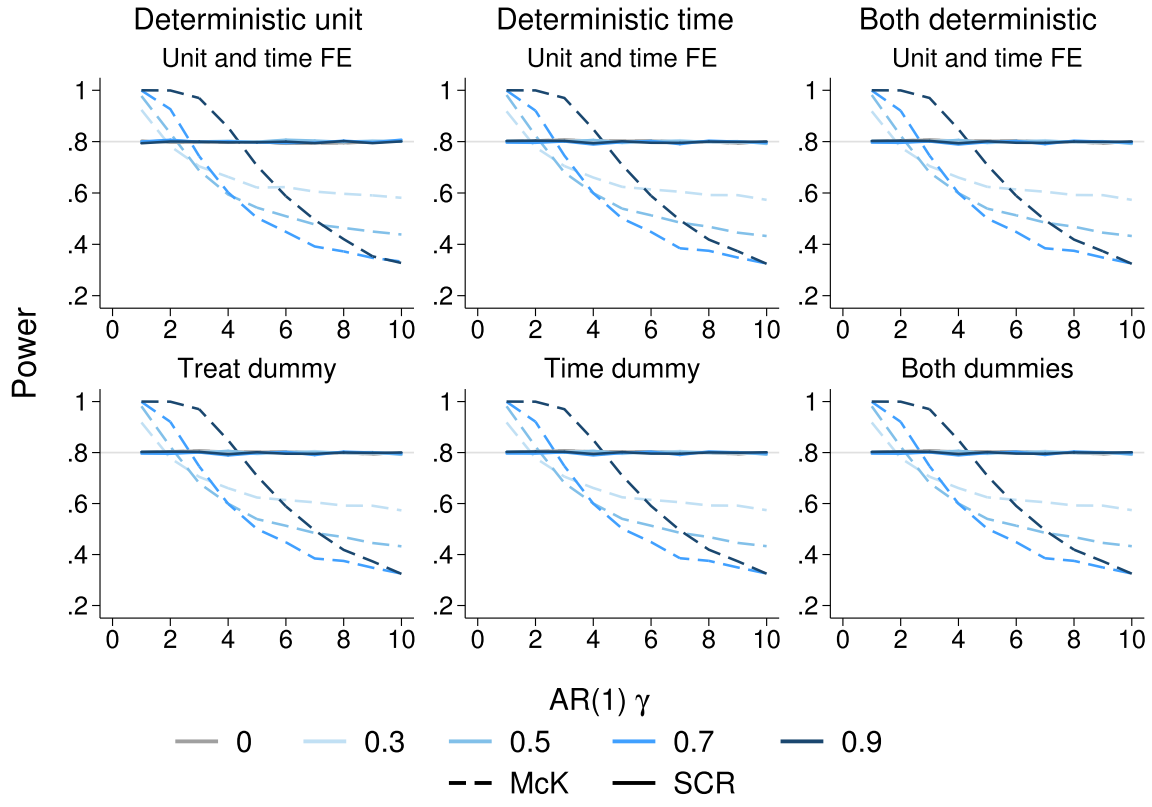


Fig. 5. SCR outperforms traditional methods with alternative DGPs. Notes: This figure repeats the same simulation exercise as Fig. 1, for alternative DGPs and DD estimating equations. The left column removes random unit effects from the DGP ($\sigma^2_\upsilon = 0$); the middle column removes random time effects from the DGP ($\sigma^2_\delta = 0$); and the right column removes both random unit effects and random time effects ($\sigma^2_\upsilon = \sigma^2_\delta = 0$). The top row uses an ex post estimator with unit and time fixed effects (as in Fig. 1, albeit now misspecified); the bottom rows replaces fixed effects with dummy variables (Treati and/or Postt, as in Fig. 3) to align with each DGP, respectively. Dotted lines report realized power for experiments calibrated using the McKenzie formula (i.e. the top-middle panel of Fig. 1), while solid lines report realized power using the SCR formula (i.e. the top-right panel of Fig. 1). In all cases, our results are identical to Fig. 1 (DGP with random unit and time effects; DD estimator with unit and time fixed effects). This show that our SCR formula is robust to alternative DGPs, which may not match the DD estimator. By contrast, the McKenzie formula continues to perform poorly in the presence of non-constant serial correlation—even after removing intracluster correlation, random time shocks, and fixed effects.

the office for a nine-month period.[29] The authors estimate the following equation to derive the central result, reported in Table 2 in the original paper:

$$\text{Performance}_{it} = \alpha \text{Treat}_i \times \text{Experiment}_t + \beta_t + \gamma_i + \epsilon_{it} \quad (12)$$

This is a standard DD estimating equation with fixed effects for individual i and week t. From this model's residuals, we estimate an AR(1) parameter of $\hat{\gamma} = 0.233$, which is highly statistically significant and indicates that these worker performance data exhibit weak serial correlation.

We perform Monte Carlo simulations on this dataset that are analogous to those presented above. We subset consecutive periods of the Bloom et al. (2015) dataset to create panels ranging in length from 2 periods ($m = r = 1$) to 20 periods ($m = r = 10$). For each simulation panel length, we randomly assign three treatment effect sizes, τ^{McK}, $\tau^{AR(1)}$, and τ^{SCR}, at the individual level and estimate Equation (12) separately for each treatment effect size. We calibrate τ^{McK} using the McKenzie formula that assumes no serial correlation[30]; $\tau^{AR(1)}$ using the SCR formula with ψ parameters consistent with an AR(1) error structure of $\gamma = 0.233$; and τ^{SCR} using the SCR formula with non-parametrically estimated $\psi_{\hat{\omega}}$ parameters. We define $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$ and $\psi^X_{\hat{\omega}}$ to be the estimated analogues of σ^2_{ω}, ψ^B, ψ^A, and ψ^X, where the subscript $\hat{\omega}$ denotes the variance/covariance of *residuals* rather than errors. Unlike Section 2.1.2 above, where the simulated DGP is known, real datasets require researchers to estimate these residual-based parameters to properly implement our SCR formula.[31]

Fig. 6 reports the results of this exercise, demonstrating that only the SCR formula achieves the desired statistical power in the Bloom et al. (2015) data. Failing to account for non-constant serial correlation leads to experiments that deviate dramatically from 80 percent power, even when that serial correlation is relatively weak. For an experiment with 10 pre/post-treatment periods, applying the McKenzie formula with $\kappa = 0.80$ yields an experiment with only 51 percent power. This is consistent with our results from simulated data, demonstrating that researchers can calibrate a panel RCT to 80 percent power if the *ex ante* formula properly accounts for the within-unit correlation structure of the data.[32]

ANCOVA in real data The ANCOVA estimator is more efficient than the DD estimator. As discussed in Section 2.2.2, for DGPs with no time shocks, our SCR ANCOVA formula can achieve properly powered experiments; however, an SCR ANCOVA formula that accommodates random time shocks is not analytically tractable. Here, we test McKenzie and SCR ANCOVA formulas using the Bloom et al. (2015) data, where the true DGP is unknown, and may contain time shocks. Fig. 7 displays the results of these power calculations in simulated experiments of varying length, with $m \in \{1, 5, 10\}$ and $1 \leq r \leq 10$. Neither formula consistently yields properly powered experiments. The McKenzie formula comes closest with one pre-treatment period and many post-treatment periods, but performs poorly for $m > 1$ or $r < 4$. On the other hand, the SCR ANCOVA performs reasonably well with 10 pre-treatment periods, but yields overpowered experiments with $m \leq 5$ or $r < 5$.

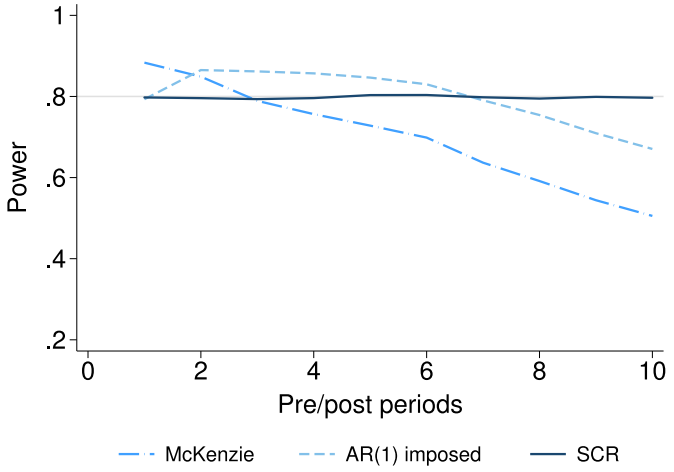


Fig. 6. Power simulations for Bloom et al. (2015) data. Notes: This figure shows results from Monte Carlo simulations using Bloom et al. (2015) data. Each curve plots realized power for a DD experiment with a certain number of pre/post-treatment periods (from m = r = 1 to m = r = 10), using different ex ante assumptions. The long-dashed line applies the McKenzie formula (Equation (3)), which assumes away non-constant serial correlation. The short-dashed line applies the SCR formula, under the assumption of AR(1) serial correlation (using Equation (6) to estimate an AR(1) parameter). The solid line applies the SCR formula (Equation (2)), where we non-parametrically estimate $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ terms from the (residualized) Bloom et al. (2015) dataset. All simulations apply the CRVE ex post, clustering at the individual level. Only the SCR formula achieves the desired 80 percent power, even though the Bloom et al. (2015) data exhibit relatively weak serial correlation.

Given that neither formula delivers properly-powered experiments with real data, we caution against using ANCOVA power calculation formulas in practice. When researchers intend to estimate an ANCOVA model *ex post*, we suggest they conduct *ex ante* power calculations using simulation-based methods (see Section 4.2.2 below).[33]

3.2. Pecan Street data

Having demonstrated the importance of properly accounting for serial correlation using data from an RCT in a developing country setting, we now turn to a much higher-frequency dataset with higher serial correlation: household electricity consumption in the United States (Pecan Street (2016)).[34] Electricity consumption data tend to exhibit high within-household autocorrelation, making them particularly well-suited for this analysis. Additionally, RCTs using energy consumption

[29] This dataset consists of weekly performance measures for the 249 workers enrolled in the experiment between January 2010 and August 2011. We keep only those individuals who have non-missing performance data for the entire pre-treatment period, leaving us with a balanced panel of 79 individuals over 48 pre-treatment weeks (a different sample from that in the paper). Our purpose with this exercise is not to comment on the statistical power of the original paper, but rather to investigate the importance of accounting for serial correlation *ex ante* in real experimental data. Appendix B.2 provides more information on this simulation dataset, including summary statistics.

[30] In Appendix C.4, we apply McKenzie's alternative empirical approach: calculating $\tau^{McK-Avg}$ by parameterizing his power calculation formula using the average autocorrelation for panels of length $(m + r)$. In both this dataset and the Pecan Street data described in Section 3.2, this alternative approach yields underpowered experiments, except in two-period panels where $m = r = 1$.

[31] Appendix D.1 explains how to estimate these residual-based parameters, which is not trivial and requires small-sample corrections. Appendix E shows how to correct for estimation bias in $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$ and $\psi^X_{\hat{\omega}}$, in order to calculate an unbiased *MDE*. In real-world settings, researchers will need to estimate parameters of an unobserved DGP, as we do here.

[32] Appendix C.1 replicates the sensitivity analyses from Section 2.2 using the Bloom et al. (2015) data (and the Pecan Street data discussed below). We again find that the SCR formula achieves 80 percent power for short panels and DD estimators without fixed effects, while the McKenzie formula fails in both cases.

[33] If researchers lack access to pre-existing data for performing simulations, one option is to apply the SCR DD power calculation formula to calibrate the sample size *ex ante*, but then estimate ANCOVA *ex post*. Since ANCOVA is (weakly) more efficient than DD, this will tend to yield overpowered experiments.

[34] Pecan Street is a research organization, based at the University of Texas at Austin, that makes high-resolution energy usage data available to academic researchers. The raw data, which are available with a login from https://dataport.pecanstreet.org/data/interactive, consist of hourly electricity consumption for 699 households over 26,888 hours. Appendix B.3 provides further detail on both these data and the ensuing simulations.

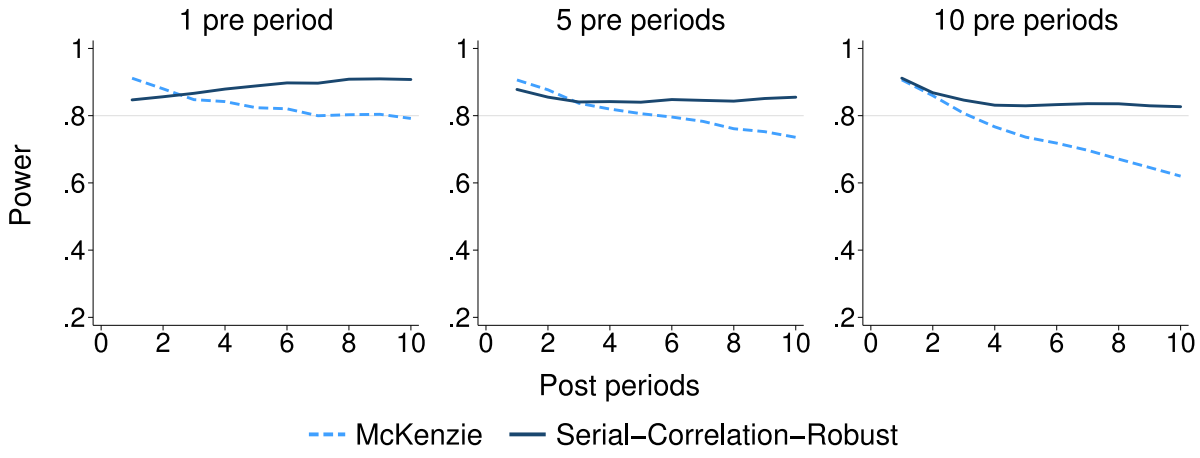


Fig. 7. ANCOVA power calculations in real data. Notes: This figure repeats the same exercise as Fig. 6, for ANCOVA experiments simulated on the Bloom et al. (2015) dataset. Each panel simulates experiments with a certain number of pre-treatment periods (m ∈{1, 5, 10}), and horizontal axes vary the number of post-treatment periods ($1 \leq r \leq 10$). Dotted lines apply the McKenzie ANCOVA formula ex ante (Equation (10)), which assumes away non-constant serial correlation. Solid lines apply the SCR ANCOVA formula ex ante (Equation (11)), where we non-parametrically estimate $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ terms from the (residualized) Bloom et al. (2015) dataset. All cases estimate ANCOVA ex post (Equation (9)), clustering standard errors by individual. Both formulas generate improperly powered experiments with real data that likely have time shocks, which neither formula can account for.

data are becoming increasingly common in economics, making our Pecan Street application relevant to this growing literature.[35]

We aggregate these data to four different temporal frequencies: hourly, daily, weekly, and monthly, each with a different correlation structures and amounts of idiosyncratic variation. This allows us to compare the McKenzie vs. SCR power calculation formula over a range of underlying error structures. We conduct Monte Carlo simulations on all four temporal frequencies, following the same procedure as the Bloom et al. (2015) simulations. Fig. 8 shows that in all four cases, realized power sharply deviates from the desired 80 percent under both the McKenzie assumption of i.i.d. idiosyncratic errors and an assumed AR(1) structure. We achieve correctly powered experimental designs only by applying the SCR method, which accounts for the full covariance structure of the Pecan Street data.[36]

3.3. Power calculations in real data

To operationalize our SCR power calculation formula in practice, researchers must assume values for σ^2_ω, ψ^B, ψ^A, and ψ^X that reflect the error structure likely to be present in (future) experimental datasets. In the best case scenario, researchers have access to data that are representative of what will be collected in the field, and they can estimate these variance and covariance terms from this pre-existing dataset.[37] Plugging these estimates into the SCR formula, researchers can evaluate the tradeoffs between desired power (κ), number of units (J), pre- and post-treatment observations per unit (m and r, respectively), proportion of the population treated (P), and expected effect size (*MDE*).

We perform this procedure on the daily Pecan Street dataset to imitate the design of an experiment that affects household electricity consumption. We do so both assuming i.i.d. idiosyncratic errors (using the McKenzie formula) and allowing for arbitrary serial correlation (using the SCR formula). For simplicity, we consider only balanced panels of households with the same number of observations before and after treatment (i.e. $m = r$). For each panel length, we re-estimate $\sigma^2_{\hat{\omega}}$ and $\psi_{\hat{\omega}}$ terms from the daily Pecan Street data.[38]

We plot the results of this exercise in Fig. 9. The left panel applies the McKenzie formula and assumes i.i.d. idiosyncratic errors. The right panel applies the SCR formula, using our non-parametric estimates of $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ to reflect the real error structure of the data. Each curve corresponds to experiments of a particular length (ranging from $m = r = 1$ to $m = r = 10$), and plots the number of households (J) required to achieve 80 percent power as a function of *MDE*. In all cases, longer panels lower J needed to achieve a given *MDE*. However, the McKenzie formula always calls for substantially fewer households than the SCR formula. For example, with $m = r = 10$ and an *MDE* of 5 percent, the McKenzie formula solves for 123 households, while the SCR formula solves for 374 households—over 3 times greater sample size. Hence, if a researcher in this setting applied the CRVE *ex post* but assumes i.i.d. idiosyncratic errors *ex ante*, she would likely include too few households to achieve the desired statistical power.

4. Power calculations in practice

4.1. Trading off units and time periods

Recruiting participants, administering treatment, and collecting data are all costly, and these implementation costs are often the limiting factor in study size. We can use the power calculation framework to conceptualize the optimal design of a panel RCT given a budget, by couching it in a simple constrained optimization problem of the follow-

[35] For example, see Allcott (2011); Jessoe and Rapson (2014); Ito et al. (2018); Fowlie et al. (2018); Fowlie et al. (2017); Allcott and Greenstone (2017); and Jack and Smith (2019). There is also a large quasi-experimental literature that uses energy consumption data.

[36] As with the Bloom et al. (2015) dataset, we estimate parameters of the unobserved DGP for each temporal frequency. Appendix C.4 shows again that the McKenzie (2012, p. 215) approach yields underpowered experiments for all four temporal frequencies for panels longer than 2 periods.

[37] Appendix D.1 provides details on how to estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ from pre-existing data, and Appendix E proves that power calculations using estimated parameters recover the same *MDE* in expectation as those using true parameters. The plausibility of estimating these parameters will vary across settings. Researchers with implementing partners that have access to large amounts of historical data may use these data to estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. On the other hand, this may not be possible for experiments in completely unstudied settings. See Appendix D.3 for more details on how to overcome a lack of pre-experimental data.

[38] We fix $P = 0.5$, $\kappa = 0.80$, and $\alpha = 0.05$. See Appendix B.4 for details.

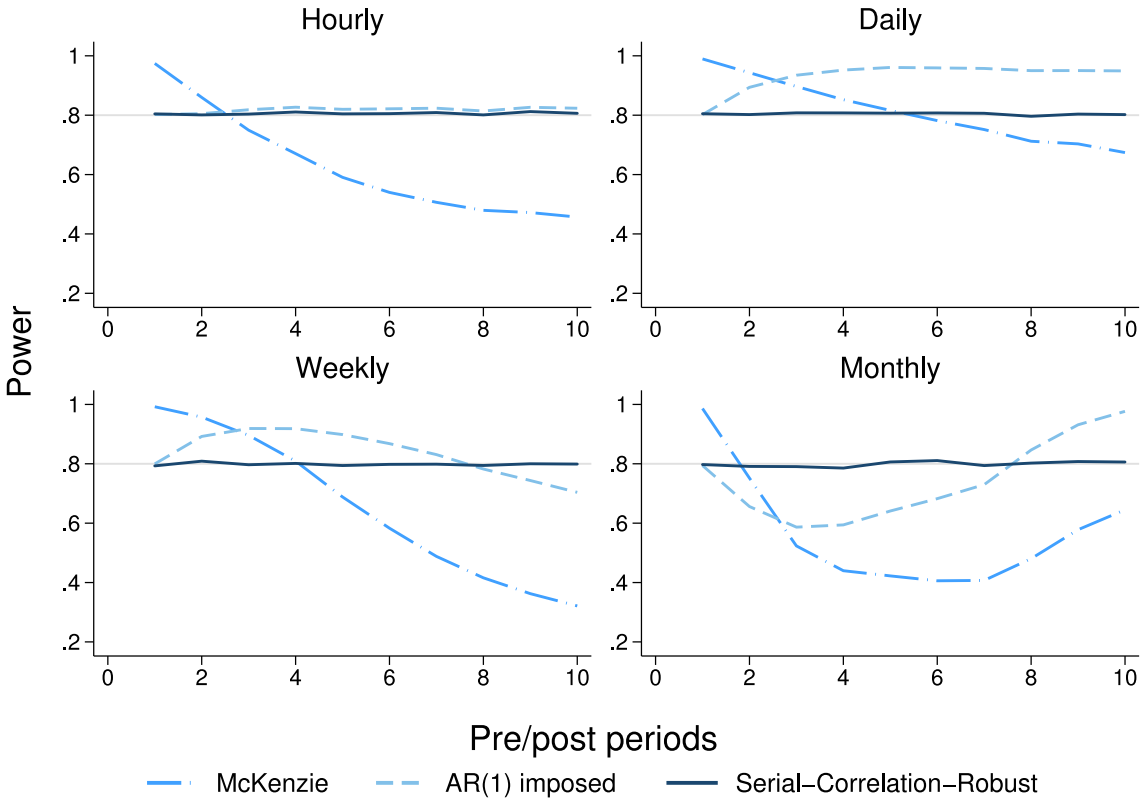


Fig. 8. Power simulations for Pecan Street data. Notes: This figure conducts simulations that are identical to Fig. 6, using four Pecan Street datasets collapsed to different levels of temporal frequency. Each curve plots realized power for a DD experiment with a certain number of pre/post-treatment periods (from m = r = 1 to m = r = 10), using different ex ante assumptions. The long-dashed lines apply the McKenzie formula (Equation (3)), which assumes away non-constant serial correlation. The short-dashed lines apply the SCR formula, under the assumption of AR(1) serial correlation (using Equation (6) to estimate an AR(1) parameter). The solid lines apply the SCR formula (Equation (2)), where we non-parametrically estimate $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ terms from the (residualized) Pecan Street data. All simulations apply the CRVE ex post, clustering at the individual level. While each temporal frequencies exhibits a unique correlation structure, only the SCR power calculation formula achieves the desired power in each case.

ing form:

$$\min_{P,J,m,r} MDE(P,J,m,r) \quad \text{s.t.} \quad C(P,J,m,r) \leq B \tag{13}$$

where $C(P,J,m,r)$ is the cost of conducting an experiment and B is the experiment's budget.

The budget constraint creates a fundamental tradeoff between including additional units and including additional time periods in the experiment, since each comes at a cost.[39] This tradeoff also arises from differences in the marginal effects of units and time periods on the *MDE*. Using our SCR formula, the "elasticities" of the *MDE* with respect to number of units and number of time periods are:

$$\frac{\partial MDE/MDE}{\partial J/J} = -\frac{1}{2}$$

$$\frac{\partial MDE/MDE}{\partial m/m} = -\frac{1}{2}\left[\frac{\frac{\sigma^2_\omega}{m} - \frac{\psi^B}{m} - (m-1)\frac{\partial \psi^B}{\partial m} + 2m\frac{\partial \psi^X}{\partial m}}{\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X}\right]$$

$$\frac{\partial MDE/MDE}{\partial r/r} = -\frac{1}{2}\left[\frac{\frac{\sigma^2_\omega}{r} - \frac{\psi^A}{r} - (r-1)\frac{\partial \psi^A}{\partial r} + 2r\frac{\partial \psi^X}{\partial r}}{\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X}\right]$$

There is a constant elasticity of *MDE* with respect to J of −0.5, meaning that a 1 percent increase in the number of units always yields a 0.5 percent reduction in the *MDE*. However, the elasticity of *MDE* with respect to m and r depends on the error structure and panel length.[40] For some parameter values, this elasticity can be positive, such that increasing the length of the experiment would actually *increase* the *MDE*. This may seem counter-intuitive, but adding time periods can reduce the average covariance between pre- and post-treatment observations (ψ^X), which introduces more noise in the estimation of pre-vs. post-treatment difference. For relatively short panels with errors that exhibit strong serial correlation, this effect can dominate the benefits of collecting more time periods.

Fig. 10 illustrates how adding time periods may either increase or decrease power. The left panel plots the *MDE* of an experiment as a

[39] Researchers may also adjust P to make an experimental design more cost effective. An RCT will have the lowest *MDE* at $P = 0.5$, but if control units are cheap compared to treatment units, the same power may be achieved at lower cost by decreasing P and increasing J. See Duflo et al. (2007) for more details. We also typically consider α and κ to be fixed "by convention." While α is the product of research norms, and therefore relatively inflexible, researchers may want to adjust κ. $1-\kappa$ is the probability of being unable to distinguish a true effect from 0. In lab experiments which are cheaply replicated, researchers may accept $\kappa < 0.80$, whereas in large, expensive field experiments that can only be conducted once, researchers may instead wish to set $\kappa > 0.80$. Researchers may also choose to size their experiments such that they achieve a power of 80 percent for the smallest economically meaningful effect, even if they expect the true *MDE* to be larger.

[40] Note that J, m, and r must all be integer-valued, hence these derivatives serve as continuous approximations of discrete changes in these parameters. Likewise, the partial derivatives of ψ^B, ψ^A, and ψ^X with respect to m and r are not technically defined, as these covariance terms are averaged over discrete numbers of periods (as shown in Assumption 5).

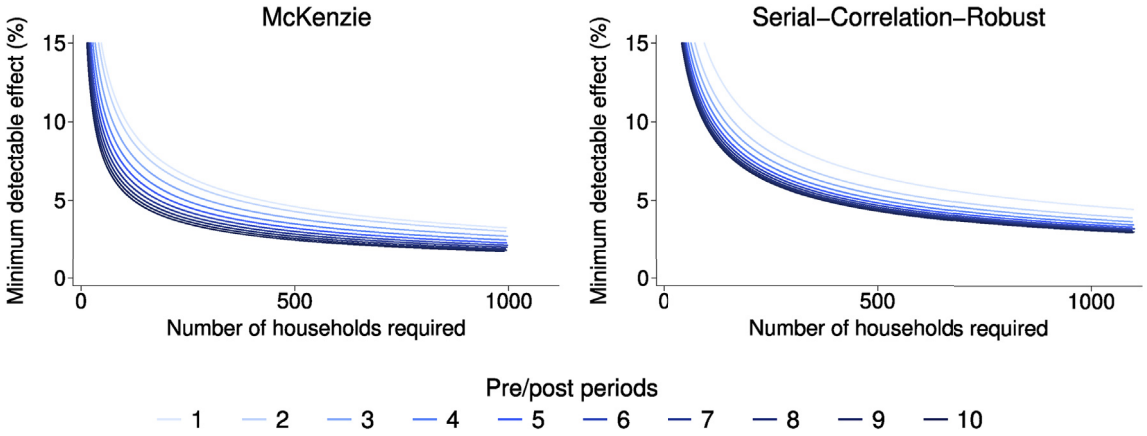


Fig. 9. Analytical power calculations – daily Pecan Street dataset. Notes: This figure shows the result of analytic power calculations on Pecan Street electricity data, collapsed to the daily level. Each curve displays the number of units required to detect a given minimum detectable effect with 80 percent power. Each iso-power curve corresponds to a particular panel length, with the shortest panel (1 pre-period, 1 post-period) in light blue, and the longest panel (10 pre-periods, 10 post-periods) in navy. The left panel shows a power calculation using the standard McKenzie (2012) formula, which does not accommodate non-constant serial correlation. The right panel applies the serial-correlation-robust formula, which accounts for the real error structure of the data. In this setting, failing to account for the full covariance structure will dramatically understate the sample size required to detect a given effect.

function of the number of pre- and post-treatment periods, holding the number of units J constant. The right panel depicts the tradeoff between adding units vs. time periods by plotting the combinations of J and $m = r$ that yield a given *MDE*. We use the SCR formula to analytically construct these curves, assuming AR(1) idiosyncratic errors with varying γ values.

At low to moderate levels of serial correlation, increasing the panel length always reduces *MDE* given J, and vice versa. However, at higher levels of serial correlation, this relationship is no longer monotonic. For $\gamma \geq 0.6$, marginally increasing m or r in a relatively short panel increases *MDE* for a given J, and likewise increases J required to achieve a given *MDE*. This suggests that in settings with strong non-constant serial correlation, adding periods of data might *decrease* statistical power if the panel is not sufficiently long.[41]

4.2. Power calculations in STATA

To facilitate implementation of *ex ante* power calculations in practice, we have developed the STATA package `pcpanel`.[42] This software implements panel data power calculations that properly account for serial correlation, a feature not present in STATA's built-in `power` command.[43] `pcpanel` operationalizes our SCR formula based on *either* user-input assumptions on the correlation structure *or* nonparametric estimates of variance/covariance parameters from a pre-existing dataset. Our package also executes power calculations by simulation, accommodating a range of experimental designs outside the scope of this paper's analytical framework.

4.2.1. Analytical DD power calculations

The program `pc_dd_analytic` conducts analytic power calculations for DD experiments using our SCR formula (Equation (2)). `pc_dd_analytic` takes (exactly two of) sample size (J), a desired MDE, and a desired power (κ), and returns the third. It behaves similarly to the legacy STATA function `sampsi`, except that `pc_dd_analytic` accepts the *idiosyncratic* residual variance σ^2_ω, equal to $\sigma^2_\varepsilon(1-\rho)$ in the notation of McKenzie (2012).[44]

Users have two options for incorporating non-constant serial correlation. First, they may allow the subprogram `pc_dd_covar` to nonparametrically estimate the average covariance structure of a pre-existing dataset, using the same procedure that generated the solid lines in Figs. 6 and 8. By characterizing the relevant components of the data's actual correlation structure, `pc_dd_covar` enables users with representative pre-existing data to accurately estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, in order to perform accurate *ex ante* power calculations (as in Section 3.1 above).[45]

Second, in the absence of pre-existing data, users may instead input assumed within-unit AR(1) correlations (γ), assumed average idiosyn-

[41] McKenzie (2012) argues that stronger unit-specific shocks (i.e. higher σ^2_v) can erode the benefits of collecting additional waves of data. Here, we extend that argument to within-unit serial correlation, demonstrating that higher autocorrelation in the idiosyncratic error term can similarly erode—and even reverse—the benefits of increased panel length. This result reflects the analytical properties of estimators that leverage both pre- and post-treatment data, and does not reflect the DD estimator over-controlling for pre-period data. Appendix Fig. C3 replicates this figure using the SCR ANCOVA formula and finds the same result.

[42] `pcpanel` is available for download from SSC. Both Appendix D.4 and the pcpanel help file describe the software package in further detail.

[43] While `power` does allow for repeated-measures ANOVA, it cannot accommodate standard panel research designs used in economics, including DD and ANCOVA. The previous STATA power calculations command, `sampsi`, did allow for this, but it was depreciated as of 2013 and is no longer supported.

[44] For example, `-sampsi 0 10, n1(150) n2(150) pre(3) post(5) sd(50) r1(0.3) m(change)-` becomes `-pc_dd_analytic, mde(10) n(300) p(0.5) pre(3) post(5) var(1750)-`, as $1750 = 50^2*(1-0.3)$. Both of these commands yield a power of 0.81. For very small samples, `sampsi` and `pc_dd_analytic` will yield slightly different results, as `pc_dd_analytic` uses a finite-sample t distribution to calculate critical values, whille `sampsi` uses a normal distribution.

[45] The option `-depvar(y)-` tells `pc_dd_covar` to nonparametrically estimate the average covariance structure of the variable y, given the number of pre- and post-treatment periods. `pc_dd_covar` implements the estimation approach detailed in Appendix D.1, and pc_dd_analytic then applies the bias-correction factors derived in Appendix E. Implementing this procedure correctly is not trivial, and we encourage users with pre-existing data to use our software to do so.

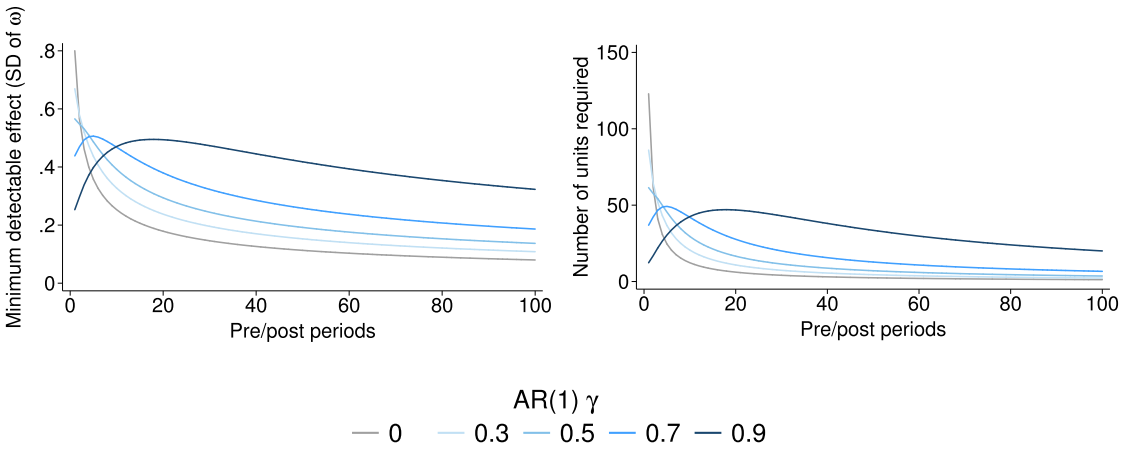


Fig. 10. Analytical power calculations with increasing panel length. Notes: This figure displays the results of analytical power calculations using the SCR formula for varying AR(1) parameters. The left panel shows the tradeoff between the minimum detectable effect (MDE) and the number of time periods (m = r) for varying levels of serial correlation, holding the number of units fixed at J = 100 and normalizing MDE by the standard deviation of ωit. At low levels of γ, MDE declines monotonically in m and r. However, for higher γ, increasing m and r actually increases MDE when m = r is relatively small, and decreases MDE when m = r is relatively large. The right panel shows the relationship between the number of units (J) and number of pre/post periods (m = r) required to detect an MDE equal to one standard deviation of ωit. Similarly, for low levels of serial correlation, the trade-off between J and m = r is monotonic. However, as γ increases, adding periods in short panels necessitates a greater number of units to achieve the same MDE, while adding periods in longer panels means that fewer units are required to achieve the same MDE. Appendix Fig. C3 replicates this result using the SCR ANCOVA formula.

cratic covariances (ψ^B, ψ^A, ψ^X), or assumed average idiosyncratic correlations (ψ^B/σ^2_ω, ψ^A/σ^2_ω, ψ^X/σ^2_ω).[46]

4.2.2. Power calculations by simulation

Simulation-based power calculations are the most robust, flexible strategy for designing experiments *ex ante*. Hence, `pcpanel` includes the program `pc_simulate`, which enables researchers to implement our simulation approach from Section 3.[47] `pc_simulate` recovers the *ex ante* power of a given experimental design and *MDE*—by simulating the *ex post* estimating equation on subsamples of a pre-existing dataset.[48] `pc_simulate` enables power calculations via simulation for four standard treatment effect estimators: cross-sectional ("one-shot"), repeated cross-sections (post-treatment periods only), DD, and ANCOVA. Users may flexibly condition on pre-determined covariates (e.g. household size) or more detailed fixed effects (e.g. unit interacted with month-of-year), both common strategies to increase an experiment's power. `pc_simulate` also accommodates datasets in which a longer panel has been collapsed to a single pre-treatment and post-treatment period, as recommended by Bertrand et al. (2004) as a way of controlling the false rejection rate.[49]

Finally, `pc_simulate` supports two additional randomization techniques: stratified randomization and cluster randomization. For stratified randomization, users identify one or multiple categorical covariates (e.g. gender), and `pc_simulate` selects PJ treated units and $(1-P)J$ control units within each stratification group. This randomization approach ensures balance across the treated and control groups. For cluster randomization, users input a group identifier (e.g. village) and `pc_simulate` randomizes *groups* of units into treatment (e.g. selecting treated villages, rather than treated households). Users may alter the number of units per group, and also vary the intensity of unit-level treatment within treated groups.[50]

5. Conclusion

Randomized experiments are costly, and researchers should avoid both underpowered experiments that are uninformative and overpowered experiments that waste resources. *Ex ante* power calculations enable researchers to design experiments with sample sizes that are sufficient, but not excessive. As multi-wave data collection becomes cheaper, panel RCTs are becoming increasingly common. Temporally disaggregated data allow researchers to ask new questions, applying a wider range of empirical methods (McKenzie (2012)).

In this paper, we develop a framework for panel data power calculations, generalizing the standard power calculation formula for difference-in-differences (originally derived by Frison and Pocock (1992)) to accommodate fully arbitrary correlations in the error struc-

[46] Alternatively, adding the option `-ar1(0.4)-` to `-pc_dd_analytic, mde(10) n(300) p(0.5) pre(3) post(5) var(1750)-` reduces power from 0.81 to 0.64. Figs. 6 and 8 illustrate how AR(1) imperfectly approximates the correlation structures present in real data. However, we provide this option for `pc_dd_analytic` to let users without pre-existing data examine the sensitivity of their power calculations to differing levels of serial correlation.

[47] To construct the solid lines in Figs. 6 and 8 we perform both analytical and simulation-based power calculations. For each panel length, we calculate the *MDE* that achieves 80 percent power, based on nonparametric estimates of the covariance structure (as in, run `pc_dd_analytic`, calling subprogram `pc_dd_covar`). Then, we simulate (as in, run `pc_simulate`) to confirm that realized power is indeed 80 percent.

[48] Each iteration re-randomizes *PJ* units into treatment and adds *MDE* to treated units' outcomes for all post-treatment periods. Appendix D.2 outlines this simulation algorithm in more detail. Users may also provide simulated data based on a plausible DGP. Appendix D.3 provides guidance on conducting simulation-based power calculations in the absence of a representative pre-existing dataset.

[49] Collapsing to cross-sectional unit-specific averages (or two-period pre/post averages for DD estimators) obviates the need to adjust the Type I error rate using the CRVE. However, collapsing data does not obviate the need to account for serial correlation in power calculations (see Appendix A.2.4).

[50] `pc_simulate` does not allow for treatment spillovers, which would require (essentially arbitrary) *ex ante* assumptions on the strength and direction of these spillovers, which are beyond the scope of this work. Researchers interested in randomized saturation designs should consult Baird et al. (2018), which considers these designs in the cross-section. Appendix C.3 uses `pc_simulate` to compare cluster randomized designs to unit-level randomization in simulated data.

ture. Unlike the power calculation formulas highlighted in McKenzie (2012), our "serial-correlation-robust" formula achieves the desired power in settings with arbitrary serial correlation. These results hold in Monte Carlo simulations, real data from a panel RCT in China, and household electricity consumption data similar to that used in panel RCTs in the energy economics literature.

Our new method is robust to alternative difference-in-differences estimators, to ANCOVA with deterministic time shocks, and to real-world data generating processes. We also provide a framework for trading off minimum detectable effects vs. sample sizes, while discussing practical issues in designing panel RCTs. Our accompanying STATA package `pcpanel` executes both analytical and simulation-based power calculations, and we recommend simulation-based methods when researchers have access to representative pre-existing data *ex ante*. A productive avenue for future work would be extending the treatment interference framework in Baird et al. (2018) to panel data experiments.

Supplementary Appendix

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2020.102458.

References

Abadie, Alberto, Athey, Susan, Imbens, Guido W., Wooldridge, Jeffrey, 2017. When should you adjust standard errors for clustering? ArXivWorking Paper No. 1710.02926.

Allcott, Hunt, 2011. Social norms and energy Conservation. J. Publ. Econ. 95 (9), 1082–1095.

Allcott, Hunt, Greenstone, Michael, 2017. Measuring the welfare effects of residential energy efficiency programs. National Bureau of Economic Research Working. Paper No. 23386.

Arellano, Manuel, 1987. Computing robust standard errors for within-group estimators. Oxf. Bull. Econ. Stat. 49 (4), 431–434.

Athey, Susan, Imbens, Guido W., 2017. The econometrics of randomized experiments. Handbook of Economic Field Experiments, 1, pp. 73–140.

Atkin, Azam, Chaudhry, David aand, Chaudry, Shamyla, Khandelwal, Amit K., Verhoogen, Eric, 2017a. Organization barriers to technology adoption: evidence from soccer-ball producers in Pakistan. Q. J. Econ. 132 (3), 1101–1164.

Atkin, David, Khandelwal, Amit K., Adam, Osman, 2017b. Exporting and firm performance:evidence from a randomized experiment. Q. J. Econ. 132 (2), 551–615.

Baird, Sarah, Bohren, J. Aislinn, McIntosh, Craig, Özler, Berk, 2018. Optimal design of experiments in the presence of interference. Rev. Econ. Stat. 100 (5), 844–860.

Bertrand, Marianne, Duflo, Esther, Mullainathan, Sendhil, 2004. How much should we trust differences-in-differences estimates? Q. J. Econ. 119 (1), 249–275.

Blattman, Christopher, Nathan, Fiala, Martinez, Sebastian, 2014. Generating skilled self-employment in developing Countries: experimental evidence from Uganda. Q. J. Econ. 129 (2), 697–752.

Bloom, Howard S., 1995. Minimum detectable effects: a simple way to report the statistical power of experimental designs. Eval. Rev. 19 (5), 547–556.

Bloom, Nicholas, Benn, Eifert, Mahajan, Aprajit, McKenzie, David, Roberts, John, 2013. Does management matter? Evidence from India. Q. J. Econ. 128 (1), 1–51.

Bloom, Nicholas, Liang, James, Roberts, John, Jenny Ying, Zhichun, 2015. Does working from home work? Evidence from a Chinese experiment. Q. J. Econ. 130 (1), 165–218.

Cameron, A. Colin, Miller, Douglas L., 2015. A practitioner's guide to cluster-robust inference. J. Hum. Resour. 50 (2), 317–372.

Card, David, DellaVigna, Stefano, Malmendier, Ulrike, 2011. The role of theory in field experiments. J. Econ. Perspect. 25 (3), 39–62.

Cohen, Jacob, 1977. Statistical Power Analysis for the Behavioral Sciences. Academic Press, New York, NY.

Duflo, Esther, Glennerster, Rachel, Kremer, Michael, 2007. "Using randomization in development economics research: a toolkit." Chap. 61. In: Schultz, Paul T., Strauss, John A. (Eds.), Handbook of Development Economics, vol. 4. Elsevier, Oxford, UK, pp. 3895–3962.

Fowlie, Meredith, Greenstone, Michael, Wolfram, Catherine, 2018. Do energy efficiency investments deliver? Evidence from the weatherization assistance program. Q. J. Econ. 133 (3), 1597–1664.

Fowlie, Meredith, Wolfram, Catherine, Spurlock, C. Anna, Todd, Annika, Baylis, Patrick, Cappers, Peter, 2017. Default effects and follow-on behavior: evidence from an electricity pricing program. National Bureau of Economic Research Working. Paper No. 23553.

Frison, L., Pocock, S.J., 1992. Repeated measures in clinical trials: analysis using mean summary statistics and its implications for design. Stat. Med. 11 (13), 1685–1704.

Glennerster, Rachel, Takavarasha, Kudzai, 2013. Running Randomized Evaluations: A Practical Guide. Princeton University Press.

Ito, Koichiro, Ida, Takanori, Tanaka, Makoto, 2018. Moral suasion and economic incentives: field experimental evidence from energy demand. Am. Econ. J. Econ. Pol. 10 (1), 240–267.

Jack, B. Kelsey, Smith, Grant, 2019. Charging ahead: prepaid electricity metering in South Africa. Am. Econ. J. Appl. Econ. (Forthcoming).

Jessoe, Katrina, Rapson, David, 2014. Knowledge is (less) power: experimental evidence from residential energy use. Am. Econ. Rev. 104 (4), 1417–1438.

McKenzie, David, 2012. Beyond baseline and follow-up: the case for more T in experiments. J. Dev. Econ. 99 (2), 210–221.

McKenzie, David, 2017. Identifying and spurring high-growth entrepreneurship: experimental evidence from a business plan Competition. Am. Econ. Rev. 107 (8), 2278–2307.

Murphy, Kevin, Myors, Brett, Allen, Wolach, 2014. Statistical Power Analysis: A Simple and General Model for Traditional and Modern Hypothesis Tests, fourth ed. Routledge, New York NY.

Pecan Street, 2016. Dataport. https://dataport.pecanstreet.org/.

Teerenstra, Steven, Eldridge, Sandra, Graff, Maud, de Hoop, Esther, Borm, George F., 2012. A simple sample size formula for analysis of covariance in cluster randomized trials. Stat. Med. 31 (20), 2169–2178.

White, Halbert, 1984. Asymptotic Theory for Econometricians, first ed. Academic Press, San Diego, CA.

Panel Data and Experimental Design

Fiona Burlig, Louis Preonas, and Matt Woerman

February 4, 2019

Appendix: For online publication

Contents

A Derivations and proofs **2**
A.1 Cross sectional experiments . . . 2
A.2 Panel experiments . . . 4
A.2.1 Independent error structure . . . 4
A.2.2 Serially correlated error structure . . . 7
A.2.3 Serially and cross-sectionally correlated error structure . . . 10
A.2.4 Collapsed dataset . . . 13
A.2.5 Analysis of covariance (ANCOVA) . . . 16
A.3 Arbitrary cross-sectional correlations . . . 22
A.4 Equivalence of alternative difference-in-difference estimators . . . 28

B Figures in main text **30**
B.1 Simulated AR(1) data . . . 30
B.2 Bloom et al. (2015) data . . . 33
B.3 Pecan Street data . . . 36
B.4 Analytic power calculations . . . 39

C Additional results **40**
C.1 Sensitivities with real data . . . 40
C.2 Tradeoffs between MDE and time periods: ANCOVA . . . 41
C.3 Cluster randomization in panel RCTs . . . 42

D A practical guide to power calculations **45**
D.1 Analytical power calculations . . . 45
D.2 Simulation-based power calculations . . . 52
D.3 Lack of (representative) pre-existing data . . . 53
D.4 STATA package `pcpanel` . . . 55

E Estimation-related proofs **58**
E.1 Recovering estimated parameters . . . 58
E.2 Estimating MDE from residual-based parameters . . . 60
E.3 Estimation with ANCOVA . . . 70

A Derivations and proofs

We are the first to derive analytic power calculation formulas for panel data models under non-i.i.d. error structures, accounting for (i) heterogeneous error structures across panel units, and (ii) random time shocks. In this section, we derive power calculation analytics for cross-sectional, difference-in-differences (DD), collapsed data, and analysis of covariance (ANCOVA) estimators. We present the resulting equations in Section 2 of the main text.

We first re-derive well-known power calculation formulas, for both a cross-sectional experiment and a panel experiment that applies the difference-in-differences estimator, under the assumption that error terms are uncorrelated. We then relax this assumption to consider the difference-in-differences estimator applied to a panel experiment in which the error structure of the data exhibits an arbitrary form of serial correlation. This resembles the most general model derived by Frison and Pocock (1992), while incorporating two features characteristic of panel data in economics: (i) error structures that vary across individual panel units; and (ii) random time series shocks that are common to all panel units. We then consider a collapsed data research design, and demonstrate that collapsing to a single pre/post observation for each panel unit *ex post* does not obviate the need to account for non-constant within-unit serial correlation *ex ante*. We also conduct a parallel series of derivations using the ANCOVA estimator, which underscores the strong assumptions required to achieve analytical tractability in this regression model.

Next, we prove Lemma 1 presented in the main text. We also prove an analogous Lemma A1 for a cross-sectional model. These lemmas show that the above power calculation formulas can be applied even in the presence of cross-sectional correlations, so long as treatment is randomly assigned at the unit level. In particular, Lemma 1 states that the variance estimator we derive, which accounts for non-constant serial correlation, gives an unbiased estimate of the *ex ante* expected variance under unit-level randomization, even when cross-sectional correlations exist. In other words, our newly derived power calculation formula can be applied to any panel experiment setting, regardless of the true error structure of the data, so long as treatment is randomly assigned to units.

Finally, we demonstrate that replacing unit and/or time fixed effects with a Treat_i and/or Post_t dummy, respectively, does not alter the statistical power of the DD estimator, as long as researchers implement the CRVE *ex post*. This analysis complements our simulation results presented in Section 2.2.2 of the main text.

A.1 Cross sectional experiments

Model There are J units randomly assigned a treatment status D_i, with proportion P in treatment ($D_i = 1$) and proportion $(1 - P)$ in control ($D_i = 0$). The units are indexed so $i \in [1, PJ]$ is treated and $j \in [PJ + 1, J]$ is a control. We make standard assumptions for randomized trials:

Assumption A1 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_i = \beta + \tau D_i + \varepsilon_i \tag{A1}$$

where Y_i is the outcome of interest for unit i; β is the expected outcome of non-treated units; τ is the treatment effect which is homogeneous across all units; D_i is a treatment indicator; and ε_i is an idiosyncratic error term distributed i.i.d. $\mathcal{N}(0, \sigma_\varepsilon^2)$.

Assumption A2 (Strict exogeneity)**.** $\mathrm{E}[\varepsilon_i \mid \mathbf{X}] = 0$*, where* $\mathbf{X} = [\mathbf{1}\ \mathbf{D}]$*. In practice, this follows from random assignment of* D_i*.*

Coefficient estimate The coefficient estimates from an OLS regression are

$$\begin{aligned}
\begin{pmatrix} \widehat{\beta} \\ \widehat{\tau} \end{pmatrix} &= (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{Y} \\
&= \begin{pmatrix} J & PJ \\ PJ & PJ \end{pmatrix}^{-1} \begin{pmatrix} \sum_{i=1}^{J} Y_i \\ \sum_{i=1}^{J} D_i Y_i \end{pmatrix} \\
&= \frac{1}{P(1-P)J^2} \begin{pmatrix} PJ\left(\sum_{i=1}^{J} Y_i - \sum_{i=1}^{J} D_i Y_i\right) \\ J\left(\sum_{i=1}^{J} D_i Y_i - P\sum_{i=1}^{J} Y_i\right) \end{pmatrix} \\
&= \begin{pmatrix} \frac{1}{(1-P)J}\sum_{i=PJ+1}^{J} Y_i \\ \frac{1}{PJ}\sum_{i=1}^{PJ} Y_i - \frac{1}{(1-P)J}\sum_{i=PJ+1}^{J} Y_i \end{pmatrix}
\end{aligned}$$

Defining the mean outcome in the treatment and control groups, respectively, as

$$\begin{aligned}
\overline{Y}_T &= \frac{1}{PJ}\sum_{i=1}^{PJ} Y_i \\
\overline{Y}_C &= \frac{1}{(1-P)J}\sum_{i=PJ+1}^{J} Y_i
\end{aligned}$$

gives coefficient estimates of

$$\begin{aligned}
\widehat{\beta} &= \overline{Y}_C \\
\widehat{\tau} &= \overline{Y}_T - \overline{Y}_C
\end{aligned}$$

Variance of coefficient estimate The variance of the estimate of the treatment effect, $\widehat{\tau}$, is

$$\begin{aligned}
\operatorname{Var}(\widehat{\tau} \mid \mathbf{X}) &= \operatorname{Var}\left(\overline{Y}_T \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_C \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\overline{Y}_T, \overline{Y}_C \mid \mathbf{X}\right) \\
&= \operatorname{Var}\left(\overline{Y}_T \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_C \mid \mathbf{X}\right) \qquad \text{(A2)}
\end{aligned}$$

The first term of Equation (A2) is

$$\begin{aligned}
\operatorname{Var}\left(\overline{Y}_T \mid \mathbf{X}\right) &= \operatorname{Var}\left(\frac{1}{PJ}\sum_{i=1}^{PJ} Y_i \mid \mathbf{X}\right) \\
&= \frac{\sigma_\varepsilon^2}{PJ} \qquad \text{(A3)}
\end{aligned}$$

Similarly, the second term of Equation (A2) is

$$\operatorname{Var}\left(\overline{Y}_C \mid \mathbf{X}\right) = \frac{\sigma_\varepsilon^2}{(1-P)J} \qquad \text{(A4)}$$

Substituting Equations (A3) and (A4) into Equation (A2) gives

$$\begin{aligned}\text{Var}(\widehat{\tau} \mid \mathbf{X}) &= \frac{\sigma_\varepsilon^2}{PJ} + \frac{\sigma_\varepsilon^2}{(1-P)J} \\ &= \frac{\sigma_\varepsilon^2}{P(1-P)J}\end{aligned} \tag{A5}$$

The variance estimator produced by an OLS regression of Equation (A1) is an unbiased estimator of this variance.

Minimum detectable effect The minimum detectable effect (MDE), or the smallest treatment effect we have the power to detect, is

$$\begin{aligned}MDE &= \left(t_{1-\kappa}^{J-2} + t_{\alpha/2}^{J-2}\right)\sqrt{\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)} \\ &= \left(t_{1-\kappa}^{J-2} + t_{\alpha/2}^{J-2}\right)\sqrt{\frac{\sigma_\varepsilon^2}{P(1-P)J}}\end{aligned} \tag{A6}$$

where κ is the power of the hypothesis test, α is the significance level, and the critical values are drawn from t-distributions with $J-2$ degrees of freedom.

A.2 Panel experiments

A.2.1 Independent error structure

Model In this model, P proportion of the J units are again randomized into treatment. The researcher collects the outcome Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For units in the treatment group, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for units in the control group, $D_{it} = 0$ in all $(m+r)$ periods.

Assumption A3 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \delta_t + \omega_{it} \tag{A7}$$

where Y_{it} is the outcome of interest for unit i at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{it} is a time-varying treatment indicator; υ_i is a unit-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma_\upsilon^2)$; δ_t is a time-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma_\delta^2)$; and ω_{it} is an idiosyncratic error term distributed i.i.d. $\mathcal{N}(0, \sigma_\omega^2)$.

Assumption A4 (Strict exogeneity)**.** $\text{E}[\omega_{it} \mid \mathbf{X}] = 0$*, where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$*,* $J-1$ *unit dummies, and* $(m+r)-1$ *time dummies. This again follows from random assignment of* D_{it}*.*

Assumption A5 (Balanced panel)**.** *The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.*

Coefficient estimate The treatment effect, τ, can be estimated by OLS with unit and time fixed effects. In a balanced panel, this is equivalent to de-meaning at both the unit and time levels. Define

$$\ddot{Y}_{it} = Y_{it} - \overline{Y}_i - \overline{Y}_t + \overline{\overline{Y}} \tag{A8}$$

$$\ddot{D}_{it} = D_{it} - \overline{D}_i - \overline{D}_t + \overline{\overline{D}} \tag{A9}$$

$$\ddot{\omega}_{it} = \omega_{it} - \overline{\omega}_i - \overline{\omega}_t + \overline{\overline{\omega}} \tag{A10}$$

where

$$\overline{Y}_i = \frac{1}{m+r} \sum_{t=-m+1}^{r} Y_{it}$$

$$\overline{Y}_t = \frac{1}{J} \sum_{i=1}^{J} Y_{it}$$

$$\overline{\overline{Y}} = \frac{1}{J(m+r)} \sum_{t=-m+1}^{r} \sum_{i=1}^{J} Y_{it}$$

with $\overline{D}_i$, $\overline{D}_t$, $\overline{\overline{D}}$, $\overline{\omega}_i$, $\overline{\omega}_t$, and $\overline{\overline{\omega}}$ defined analogously. Substituting Equations (A8)–(A10) into Equation (A7) and simplifying gives the de-meaned DGP

$$\ddot{Y}_{it} = \tau \ddot{D}_{it} + \ddot{\omega}_{it}$$

The estimate of the treatment effect is

$$\begin{aligned}
\widehat{\tau} &= (\ddot{\mathbf{D}}'\ddot{\mathbf{D}})^{-1}\ddot{\mathbf{D}}'\ddot{\mathbf{Y}} \\
&= \left(\sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it}^2 \right)^{-1} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it}\ddot{Y}_{it} \\
&= \frac{m+r}{P(1-P)Jmr} \left[\sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} Y_{it} - \sum_{i=1}^{J} \overline{Y}_i \sum_{t=-m+1}^{r} \ddot{D}_{it} - \sum_{t=-m+1}^{r} \overline{Y}_t \sum_{i=1}^{J} \ddot{D}_{it} + \overline{Y} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} \right] \\
&= \frac{m+r}{P(1-P)Jmr} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} Y_{it} \\
&= \frac{m+r}{P(1-P)Jmr} \left[\sum_{i=1}^{PJ} \left(\frac{(1-P)m}{m+r} \sum_{t=1}^{r} Y_{it} - \frac{(1-P)r}{m+r} \sum_{t=-m+1}^{0} Y_{it} \right) \right. \\
&\qquad \left. + \sum_{i=PJ+1}^{J} \left(\frac{Pr}{m+r} \sum_{t=-m+1}^{0} Y_{it} - \frac{Pm}{m+r} \sum_{t=1}^{r} Y_{it} \right) \right] \\
&= \left(\overline{Y}_T^A - \overline{Y}_T^B \right) - \left(\overline{Y}_C^A - \overline{Y}_C^B \right)
\end{aligned} \tag{A11}$$

where

$$\overline{Y}_T^A = \frac{1}{PJr} \sum_{i=1}^{PJ} \sum_{t=1}^{r} Y_{it}$$

$$\overline{Y}_T^B = \frac{1}{PJm} \sum_{i=1}^{PJ} \sum_{t=-m+1}^{0} Y_{it}$$

$$\overline{Y}_C^A = \frac{1}{(1-P)Jr} \sum_{i=PJ+1}^{J} \sum_{t=1}^{r} Y_{it}$$

$$\overline{Y}_C^B = \frac{1}{(1-P)Jm} \sum_{i=PJ+1}^{J} \sum_{t=-m+1}^{0} Y_{it}$$

Variance of coefficient estimate The variance of the estimate of the treatment effect, $\widehat{\tau}$, is

$$\begin{aligned}
\operatorname{Var}(\widehat{\tau} \mid \mathbf{X}) &= \operatorname{Var}\left(\overline{Y}_T^A \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_T^B \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_C^A \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_C^B \mid \mathbf{X}\right) \\
&\quad - 2\operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_T^B \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_C^A \mid \mathbf{X}\right) + 2\operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_C^B \mid \mathbf{X}\right) \\
&\quad + 2\operatorname{Cov}\left(\overline{Y}_T^B, \overline{Y}_C^A \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\overline{Y}_T^B, \overline{Y}_C^B \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\overline{Y}_C^A, \overline{Y}_C^B \mid \mathbf{X}\right) \\
&= \operatorname{Var}\left(\overline{Y}_T^A \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_T^B \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_C^A \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{Y}_C^B \mid \mathbf{X}\right) \\
&\quad - 2\Bigg[\operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_T^B \mid \mathbf{X}\right) + \operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_C^A \mid \mathbf{X}\right) \\
&\qquad + \operatorname{Cov}\left(\overline{Y}_T^B, \overline{Y}_C^B \mid \mathbf{X}\right) + \operatorname{Cov}\left(\overline{Y}_C^A, \overline{Y}_C^B \mid \mathbf{X}\right)\Bigg]
\end{aligned} \tag{A12}$$

The first term of Equation (A12) is

$$\begin{aligned}
\operatorname{Var}\left(\overline{Y}_T^A \mid \mathbf{X}\right) &= \operatorname{Var}\left(\frac{1}{PJr} \sum_{i=1}^{PJ} \sum_{t=1}^{r} Y_{it} \mid \mathbf{X}\right) \\
&= \operatorname{Var}\left(\frac{1}{PJ} \sum_{i=1}^{PJ} \upsilon_i \mid \mathbf{X}\right) + \operatorname{Var}\left(\frac{1}{r} \sum_{t=1}^{r} \delta_t \mid \mathbf{X}\right) + \operatorname{Var}\left(\frac{1}{PJr} \sum_{i=1}^{PJ} \sum_{t=1}^{r} \omega_{it} \mid \mathbf{X}\right) \\
&= \frac{1}{PJr}\left(r\sigma_\upsilon^2 + PJ\sigma_\delta^2 + \sigma_\omega^2\right)
\end{aligned} \tag{A13}$$

Similarly, the remaining variance terms of Equation (A12) are

$$\operatorname{Var}\left(\overline{Y}_T^B \mid \mathbf{X}\right) = \frac{1}{PJm}\left(m\sigma_\upsilon^2 + PJ\sigma_\delta^2 + \sigma_\omega^2\right) \tag{A14}$$

$$\operatorname{Var}\left(\overline{Y}_C^A \mid \mathbf{X}\right) = \frac{1}{(1-P)Jr}\left(r\sigma_\upsilon^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2\right) \tag{A15}$$

$$\operatorname{Var}\left(\overline{Y}_C^B \mid \mathbf{X}\right) = \frac{1}{(1-P)Jm}\left(m\sigma_\upsilon^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2\right) \tag{A16}$$

The first covariance component of Equation (A12) is

$$\begin{aligned}\text{Cov}\left(\overline{Y}_T^A, \overline{Y}_T^B \mid \mathbf{X}\right) &= \text{E}\left[\overline{Y}_T^A \overline{Y}_T^B \mid \mathbf{X}\right] - \text{E}\left[\overline{Y}_T^A \mid \mathbf{X}\right] \text{E}\left[\overline{Y}_T^B \mid \mathbf{X}\right] \\ &= \beta(\beta+\tau) + \frac{1}{PJ}\text{E}\left[\upsilon_i^2 \mid \mathbf{X}\right] - \beta(\beta+\tau) \\ &= \frac{\sigma_\upsilon^2}{PJ} \end{aligned} \tag{A17}$$

Similarly, the remaining covariance terms of Equation (A12) are

$$\text{Cov}\left(\overline{Y}_T^A, \overline{Y}_C^A \mid \mathbf{X}\right) = \frac{\sigma_\delta^2}{r} \tag{A18}$$

$$\text{Cov}\left(\overline{Y}_T^B, \overline{Y}_C^B \mid \mathbf{X}\right) = \frac{\sigma_\delta^2}{m} \tag{A19}$$

$$\text{Cov}\left(\overline{Y}_C^A, \overline{Y}_C^B \mid \mathbf{X}\right) = \frac{\sigma_\upsilon^2}{(1-P)J} \tag{A20}$$

Substituting Equations (A13)–(A20) into Equation (A12) gives

$$\begin{aligned}\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right) &= 2\frac{\sigma_\upsilon^2}{P(1-P)J} + 2\frac{(m+r)\sigma_\delta^2}{mr} + \frac{(m+r)\sigma_\omega^2}{P(1-P)Jmr} - 2\left[\frac{\sigma_\upsilon^2}{P(1-P)J} + \frac{(m+r)\sigma_\delta^2}{mr}\right] \\ &= \left(\frac{\sigma_\omega^2}{P(1-P)J}\right)\left(\frac{m+r}{mr}\right) \end{aligned} \tag{A21}$$

The variance estimator produced by an OLS regression of Equation (A7) is an unbiased estimator of this variance.

Minimum detectable effect The MDE is

$$\begin{aligned} MDE &= \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)} \\ &= \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\left(\frac{\sigma_\omega^2}{P(1-P)J}\right)\left(\frac{m+r}{mr}\right)} \end{aligned}$$

This is the standard Frison and Pocock (1992) result (page 1693), also referenced by McKenzie (2012), and is shown as Equation (3) in the main text. We assume J degrees of freedom to be consistent with the assumptions of the CRVE; alternatively, $J(m+r) - (J+m+r)$ degrees of freedom would be consistent with the assumptions of OLS standard errors. Note that this model differs slightly from Frison and Pocock (1992) and McKenzie (2012), which both assume $\sigma_\delta^2 = 0$.

A.2.2 Serially correlated error structure

Model There are J units, P proportion of which are randomized into treatment. The researcher again collects outcome data Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For treated units, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for control units, $D_{it} = 0$ in all periods.

Assumption A6 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \delta_t + \omega_{it} \tag{A22}$$

where Y_{it} is the outcome of interest for unit i at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{it} is a time-varying treatment indicator; υ_i is a unit-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma_\upsilon^2)$; δ_t is a time-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma_\delta^2)$; and ω_{it} is an idiosyncratic error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma_\omega^2)$.

Assumption A7 (Strict exogeneity)**.** $\mathrm{E}[\omega_{it} \mid \mathbf{X}] = 0$*, where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$*, $J-1$ unit dummies, and $(m+r)-1$ time dummies. This again follows from random assignment of D_{it}.*

Assumption A8 (Balanced panel)**.** *The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.*

Assumption A9 (Independence across units)**.** $\mathrm{E}[\omega_{it}\omega_{js} \mid \mathbf{X}] = 0, \ \forall \ i \neq j, \ \forall \ t, s.$

Assumption A10 (Symmetric covariance structures)**.** *Define:*

$$\psi^B \equiv \frac{2}{Jm(m-1)} \sum_{i=1}^{J} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

$$\psi^A \equiv \frac{2}{Jr(r-1)} \sum_{i=1}^{J} \sum_{t=1}^{r-1} \sum_{s=t+1}^{r} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

$$\psi^X \equiv \frac{1}{Jmr} \sum_{i=1}^{J} \sum_{t=-m+1}^{0} \sum_{s=1}^{r} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

to be the average pre-treatment, post-treatment, and across-period covariance between different error terms of the same unit, respectively. Define ψ_T^B, ψ_T^A, and ψ_C^X analogously, where we consider only the PJ treated units; also define ψ_C^B, ψ_C^A, and ψ_C^X analogously, where we consider only the $(1-P)J$ control units. Using these definitions, assume that $\psi^B = \psi_T^B = \psi_C^B$; $\psi^A = \psi_T^A = \psi_C^A$; and $\psi^X = \psi_T^X = \psi_C^X$.[1]

Coefficient estimate The treatment effect estimator is the same as Equation (A11):

$$\widehat{\tau} = \left(\overline{Y}_T^A - \overline{Y}_T^B\right) - \left(\overline{Y}_C^A - \overline{Y}_C^B\right)$$

1. We choose the letters "B" to indicate the Before-treatment period, and "A" to indicate the After-treatment period. We index the m pre-treatment periods $\{-m+1, \ldots, 0\}$, and the r post-treatment periods $\{1, \ldots, r\}$. In a randomized setting, $\mathrm{E}\left[\psi^B\right] = \mathrm{E}\left[\psi_T^B\right] = \mathrm{E}\left[\psi_C^B\right]$, $\mathrm{E}\left[\psi^A\right] = \mathrm{E}\left[\psi_T^A\right] = \mathrm{E}\left[\psi_C^A\right]$, and $\mathrm{E}\left[\psi^X\right] = \mathrm{E}\left[\psi_T^X\right] = \mathrm{E}\left[\psi_C^X\right]$, making this a reasonable assumption *ex ante*. However, it is possible for treatment to alter the covariance structure of treated units only.

Variance of coefficient estimate With a serially correlated error structure, Equation (A12) is still correct by Assumption A9, but Equations (A13)–(A20) no longer hold. With serially correlated errors, the first term of Equation (A12) is

$$\begin{aligned}
\operatorname{Var}\left(\overline{Y}_T^A \mid \mathbf{X}\right) &= \operatorname{Var}\left(\frac{1}{PJr}\sum_{i=1}^{PJ}\sum_{t=1}^{r} Y_{it} \mid \mathbf{X}\right) \\
&= \operatorname{Var}\left(\frac{1}{PJ}\sum_{i=1}^{PJ} \upsilon_i \mid \mathbf{X}\right) + \operatorname{Var}\left(\frac{1}{r}\sum_{t=1}^{r} \delta_t \mid \mathbf{X}\right) + \operatorname{Var}\left(\frac{1}{PJr}\sum_{i=1}^{PJ}\sum_{t=1}^{r} \omega_{it} \mid \mathbf{X}\right) \\
&= \frac{1}{PJr}\left(r\sigma_\upsilon^2 + PJ\sigma_\delta^2 + \sigma_\omega^2\right) + \frac{2}{(PJr)^2}\sum_{i=1}^{PJ}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r} \operatorname{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) \\
&= \frac{1}{PJr}\left(r\sigma_\upsilon^2 + PJ\sigma_\delta^2 + \sigma_\omega^2 + (r-1)\psi^A\right) \qquad \text{(A23)}
\end{aligned}$$

Similarly, with serial correlation, the remaining variance terms of Equation (A12) are

$$\operatorname{Var}\left(\overline{Y}_T^B \mid \mathbf{X}\right) = \frac{1}{PJm}\left(m\sigma_\upsilon^2 + PJ\sigma_\delta^2 + \sigma_\omega^2 + (m-1)\psi^B\right) \qquad \text{(A24)}$$

$$\operatorname{Var}\left(\overline{Y}_C^A \mid \mathbf{X}\right) = \frac{1}{(1-P)Jr}\left(r\sigma_\upsilon^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2 + (r-1)\psi^A\right) \qquad \text{(A25)}$$

$$\operatorname{Var}\left(\overline{Y}_C^B \mid \mathbf{X}\right) = \frac{1}{(1-P)Jm}\left(m\sigma_\upsilon^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2 + (m-1)\psi^B\right) \qquad \text{(A26)}$$

With serial correlation, the first covariance component of Equation (A12) is

$$\begin{aligned}
\operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_T^B \mid \mathbf{X}\right) &= \operatorname{E}\left[\overline{Y}_T^A \overline{Y}_T^B \mid \mathbf{X}\right] - \operatorname{E}\left[\overline{Y}_T^A \mid \mathbf{X}\right]\operatorname{E}\left[\overline{Y}_T^B \mid \mathbf{X}\right] \\
&= \beta(\beta+\tau) + \frac{1}{PJ}\operatorname{E}\left[\upsilon_i^2 \mid \mathbf{X}\right] + \frac{1}{(PJ)^2 mr}\sum_{i=1}^{PJ}\sum_{t=-m+1}^{0}\sum_{s=1}^{r} \operatorname{E}[\omega_{it}\omega_{is} \mid \mathbf{X}] - \beta(\beta+\tau) \\
&= \frac{\sigma_\upsilon^2}{PJ} + \frac{1}{(PJ)^2 mr}\sum_{i=1}^{PJ}\sum_{t=-m+1}^{0}\sum_{s=1}^{r} \operatorname{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) \\
&= \frac{1}{PJ}\left(\sigma_\upsilon^2 + \psi^X\right) \qquad \text{(A27)}
\end{aligned}$$

Similarly, with serial correlation, the remaining covariance terms of Equation (A12) are

$$\operatorname{Cov}\left(\overline{Y}_T^A, \overline{Y}_C^A \mid \mathbf{X}\right) = \frac{\sigma_\delta^2}{r} \qquad \text{(A28)}$$

$$\operatorname{Cov}\left(\overline{Y}_T^B, \overline{Y}_C^B \mid \mathbf{X}\right) = \frac{\sigma_\delta^2}{m} \qquad \text{(A29)}$$

$$\operatorname{Cov}\left(\overline{Y}_C^A, \overline{Y}_C^B \mid \mathbf{X}\right) = \frac{1}{(1-P)J}\left(\sigma_\upsilon^2 + \psi^X\right) \qquad \text{(A30)}$$

Substituting Equations (A23)–(A30) into Equation (A12) and simplifying gives

$$\begin{aligned}
\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right) &= \frac{1}{PJr}\left(r\sigma_{\upsilon}^2 + PJ\sigma_{\delta}^2 + \sigma_{\omega}^2 + (r-1)\psi^A\right) \\
&\quad + \frac{1}{PJm}\left(m\sigma_{\upsilon}^2 + PJ\sigma_{\delta}^2 + \sigma_{\omega}^2 + (m-1)\psi^B\right) \\
&\quad + \frac{1}{(1-P)Jr}\left(r\sigma_{\upsilon}^2 + (1-P)J\sigma_{\delta}^2 + \sigma_{\omega}^2 + (r-1)\psi^A\right) \\
&\quad + \frac{1}{(1-P)Jm}\left(m\sigma_{\upsilon}^2 + (1-P)J\sigma_{\delta}^2 + \sigma_{\omega}^2 + (m-1)\psi^B\right) \\
&\quad - 2\left[\frac{1}{PJ}\left(\sigma_{\upsilon}^2 + \psi^X\right) + \frac{\sigma_{\delta}^2}{r} + \frac{\sigma_{\delta}^2}{m} + \frac{1}{(1-P)J}\left(\sigma_{\upsilon}^2 + \psi^X\right)\right] \\
&= \left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma_{\omega}^2 + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right] \quad \text{(A31)}
\end{aligned}$$

Equation (A31) differs from Equation (A21), the variance of $\widehat{\tau}$ that is estimated by an OLS regression, due to the presence of the ψ terms that describe the serially correlated error structure. This error structure alters the true variance of the treatment effect estimator such that the OLS estimator of the variance is not correct. The cluster-robust variance estimator must be used for correct inference.

Minimum detectable effect With serial correlation, the MDE is

$$\begin{aligned}
MDE &= \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)} \\
&= \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma_{\omega}^2 + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]}
\end{aligned}$$

This is the serial-correlation-robust (SCR) power calculation formula, found in Equation (2) in the main text. Note that ψ^B, ψ^A, and ψ^X are likely to depend on the length of the pre- and post-treatment periods, m and r; serial correlation often diminishes as time periods become further apart, so larger values of m and r will result in less correlation on average and smaller parameter values. These parameters do not depend in a systematic way on the number of experimental units, J, however.

A.2.3 Serially and cross-sectionally correlated error structure

Randomization at the unit level justifies Assumptions A9 and A10, but we can also characterize the full variance without this random assignment assumption. This highlights the type of correlation structures that a researcher might face when wanting to perform a power calculation for a quasi-experimental design.

Model There are J units, P proportion of which are randomized into treatment. The researcher again collects outcome data Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For treated units, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for control units, $D_{it} = 0$ in all periods.

Assumption A11 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \delta_t + \omega_{it}$$

where Y_{it} is the outcome of interest for unit i at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{it} is a time-varying treatment indicator; υ_i is a unit-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma_\upsilon^2)$; δ_t is a time-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma_\delta^2)$; and ω_{it} is an idiosyncratic error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma_\omega^2)$.

Assumption A12 (Strict exogeneity)**.** $\mathrm{E}[\omega_{it} \mid \mathbf{X}] = 0$*, where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$*,* $J-1$ *unit dummies, and* $(m+r)-1$ *time dummies. This again follows from random assignment of* D_{it}*.*

Assumption A13 (Balanced panel)**.** *The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.*

Assumption A14 (Independence across units at different times)**.** $\mathrm{E}[\omega_{it}\omega_{js} \mid \mathbf{X}] = 0,\ \forall\ i \neq j,\ t \neq s.$

Define ψ_i to be the average serial correlation parameters previously defined in Appendix A.2.2, with the subscript i denoting the correlation is within-unit serial correlation. Also define ψ_t to be the comparable parameters characterizing cross-sectional correlations, with the subscript t denoting the correlation is cross-sectional within a time period. For example, the average cross-sectional covariance among the treated group post-treatment is

$$\psi_{t,T}^A = \frac{2}{PJ(PJ-1)r} \sum_{i=1}^{PJ-1} \sum_{j=i+1}^{PJ} \sum_{t=1}^{r} \mathrm{Cov}(\omega_{it}, \omega_{jt} \mid \mathbf{X})$$

Coefficient estimate The treatment effect estimator is the same as Equation (A11):

$$\widehat{\tau} = \left(\overline{Y}_T^A - \overline{Y}_T^B\right) - \left(\overline{Y}_C^A - \overline{Y}_C^B\right)$$

Variance of coefficient estimate As with the serially correlated error structure in Appendix A.2.2, Equation (A12) is still correct, in this case by Assumption A14. However, neither Equations (A13)–(A20) nor Equations (A23)–(A30) hold. With these arbitrary correlations, the first term of Equation (A12) is

$$\begin{aligned}
\mathrm{Var}\left(\overline{Y}_T^A \mid \mathbf{X}\right) &= \mathrm{Var}\left(\frac{1}{PJr} \sum_{i=1}^{PJ} \sum_{t=1}^{r} Y_{it} \mid \mathbf{X}\right) \\
&= \mathrm{Var}\left(\frac{1}{PJ} \sum_{i=1}^{PJ} \upsilon_i \mid \mathbf{X}\right) + \mathrm{Var}\left(\frac{1}{r} \sum_{t=1}^{r} \delta_t \mid \mathbf{X}\right) + \mathrm{Var}\left(\frac{1}{PJr} \sum_{i=1}^{PJ} \sum_{t=1}^{r} \omega_{it} \mid \mathbf{X}\right) \\
&= \frac{1}{PJr}\left(r\sigma_\upsilon^2 + PJ\sigma_\delta^2 + \sigma_\omega^2\right) + \frac{2}{(PJr)^2} \sum_{i=1}^{PJ} \sum_{t=1}^{r-1} \sum_{s=t+1}^{r} \mathrm{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) \\
&\quad + \frac{2}{(PJr)^2} \sum_{t=1}^{r} \sum_{i=1}^{PJ-1} \sum_{j=i+1}^{PJ} \mathrm{Cov}(\omega_{it}, \omega_{jt} \mid \mathbf{X})
\end{aligned}$$

$$= \frac{1}{PJr}\left(r\sigma_v^2 + PJ\sigma_\delta^2 + \sigma_\omega^2 + (r-1)\psi_{i,T}^A + (PJ-1)\psi_{t,T}^A\right) \tag{A32}$$

Similarly, with arbitrary correlations, the remaining variance terms of Equation (A12) are

$$\text{Var}\left(\overline{Y}_T^B \mid \mathbf{X}\right) = \tfrac{1}{PJm}\left(m\sigma_v^2 + PJ\sigma_\delta^2 + \sigma_\omega^2 + (m-1)\psi_{i,T}^B + (PJ-1)\psi_{t,T}^B\right) \tag{A33}$$

$$\text{Var}\left(\overline{Y}_C^A \mid \mathbf{X}\right) = \tfrac{1}{(1-P)Jr}\left(r\sigma_v^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2 + (r-1)\psi_{i,C}^A + ((1-P)J-1)\psi_{t,C}^A\right) \tag{A34}$$

$$\text{Var}\left(\overline{Y}_C^A \mid \mathbf{X}\right) = \tfrac{1}{(1-P)Jm}\left(m\sigma_v^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2 + (m-1)\psi_{i,C}^B + ((1-P)J-1)\psi_{t,C}^B\right) \tag{A35}$$

With arbitrary correlations, the first covariance component of Equation (A12) is

$$\begin{aligned}
\text{Cov}\left(\overline{Y}_T^A, \overline{Y}_T^B \mid \mathbf{X}\right) &= \text{E}\left[\overline{Y}_T^A\overline{Y}_T^B \mid \mathbf{X}\right] - \text{E}\left[\overline{Y}_T^A \mid \mathbf{X}\right]\text{E}\left[\overline{Y}_T^B \mid \mathbf{X}\right] \\
&= \beta(\beta+\tau) + \frac{1}{PJ}\text{E}\left[\upsilon_i^2 \mid \mathbf{X}\right] + \frac{1}{(PJ)^2mr}\sum_{i=1}^{PJ}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{E}[\omega_{it}\omega_{is} \mid \mathbf{X}] - \beta(\beta+\tau) \\
&= \frac{\sigma_v^2}{PJ} + \frac{1}{(PJ)^2mr}\sum_{i=1}^{PJ}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) \\
&= \frac{1}{PJ}\left(\sigma_v^2 + \psi_{i,T}^X\right)
\end{aligned} \tag{A36}$$

Similarly, with arbitrary correlations, the remaining covariance terms of Equation (A12) are

$$\text{Cov}\left(\overline{Y}_T^A, \overline{Y}_C^A \mid \mathbf{X}\right) = \frac{1}{r}\left(\sigma_\delta^2 + \psi_{t,X}^A\right) \tag{A37}$$

$$\text{Cov}\left(\overline{Y}_T^B, \overline{Y}_C^B \mid \mathbf{X}\right) = \frac{1}{m}\left(\sigma_\delta^2 + \psi_{t,X}^B\right) \tag{A38}$$

$$\text{Cov}\left(\overline{Y}_C^A, \overline{Y}_C^B \mid \mathbf{X}\right) = \frac{1}{(1-P)J}\left(\sigma_v^2 + \psi_{i,C}^X\right) \tag{A39}$$

Substituting Equations (A32)–(A39) into Equation (A12) and simplifying gives

$$\begin{aligned}
\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right) = &\frac{1}{PJr}\left(r\sigma_v^2 + PJ\sigma_\delta^2 + \sigma_\omega^2 + (r-1)\psi_{i,T}^A + (PJ-1)\psi_{t,T}^A\right) \\
&+ \frac{1}{PJm}\left(m\sigma_v^2 + PJ\sigma_\delta^2 + \sigma_\omega^2 + (m-1)\psi_{i,T}^B + (PJ-1)\psi_{t,T}^B\right) \\
&+ \frac{1}{(1-P)Jr}\left(r\sigma_v^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2 + (r-1)\psi_{i,C}^A + ((1-P)J-1)\psi_{t,C}^A\right) \\
&+ \frac{1}{(1-P)Jm}\left(m\sigma_v^2 + (1-P)J\sigma_\delta^2 + \sigma_\omega^2 + (m-1)\psi_{i,C}^B + ((1-P)J-1)\psi_{t,C}^B\right) \\
&- 2\left[\frac{1}{PJ}\left(\sigma_v^2 + \psi_{i,T}^X\right) + \frac{1}{r}\left(\sigma_\delta^2 + \psi_{t,X}^A\right) + \frac{1}{m}\left(\sigma_\delta^2 + \psi_{t,X}^B\right) + \frac{1}{(1-P)J}\left(\sigma_v^2 + \psi_{i,C}^X\right)\right] \\
= &\left(\frac{m+r}{P(1-P)Jmr}\right)\sigma_\omega^2 + \left(\frac{m-1}{PJm}\right)\psi_{i,T}^B + \left(\frac{PJ-1}{PJm}\right)\psi_{t,T}^B \\
&+ \left(\frac{r-1}{PJr}\right)\psi_{i,T}^A + \left(\frac{PJ-1}{PJr}\right)\psi_{t,T}^A + \left(\frac{m-1}{(1-P)Jm}\right)\psi_{i,C}^B
\end{aligned}$$

$$+\left(\frac{(1-P)J-1}{(1-P)Jm}\right)\psi^B_{t,C}+\left(\frac{r-1}{(1-P)Jr}\right)\psi^A_{i,C}+\left(\frac{(1-P)J-1}{(1-P)Jr}\right)\psi^A_{t,C}$$
$$-\frac{2}{PJ}\psi^X_{i,T}-\frac{2}{m}\psi^B_{t,X}-\frac{2}{r}\psi^A_{t,X}-\frac{2}{(1-P)J}\psi^X_{i,C}$$

Minimum detectable effect With arbitrary correlations, the MDE is

$$\begin{aligned}MDE &= \left(t^J_{1-\kappa}+t^J_{\alpha/2}\right)\sqrt{\text{Var}\left(\widehat{\tau}\mid\mathbf{X}\right)}\\ &= \left(t^J_{1-\kappa}+t^J_{\alpha/2}\right)\left[\left(\frac{m+r}{P(1-P)Jmr}\right)\sigma^2_\omega+\left(\frac{m-1}{PJm}\right)\psi^B_{i,T}+\left(\frac{PJ-1}{PJm}\right)\psi^B_{t,T}\right.\\ &\quad+\left(\frac{r-1}{PJr}\right)\psi^A_{i,T}+\left(\frac{PJ-1}{PJr}\right)\psi^A_{t,T}+\left(\frac{m-1}{(1-P)Jm}\right)\psi^B_{i,C}\\ &\quad+\left(\frac{(1-P)J-1}{(1-P)Jm}\right)\psi^B_{t,C}+\left(\frac{r-1}{(1-P)Jr}\right)\psi^A_{i,C}+\left(\frac{(1-P)J-1}{(1-P)Jr}\right)\psi^A_{t,C}\\ &\quad\left.-\frac{2}{PJ}\psi^X_{i,T}-\frac{2}{m}\psi^B_{t,X}-\frac{2}{r}\psi^A_{t,X}-\frac{2}{(1-P)J}\psi^X_{i,C}\right]^{1/2}\end{aligned}\tag{A40}$$

Lemma 1 in Appendix A.3 shows that Equation (A40) is equal in expectation to the SCR power calculation formula when treatment is randomly assigned.

A.2.4 Collapsed dataset

Bertrand, Duflo, and Mullainathan (2004, henceforth BDM) suggest an alternative to the CRVE in order to achieve the correct false rejection rates in the presence of serial correlation: ignore the time-series structure of the data by averaging the pre-treatment data and the post-treatment data for each unit, then estimate a panel DD regression on this two-period collapsed dataset and apply the OLS variance estimator. While this does yield the desired false rejection rate, simply applying the McKenzie formula to a collapsed dataset will *not* yield the desired power.

Using the model from Appendix A.2.2 (under Assumptions A6–A10), suppose that prior to estimation, the data are collapsed to 1 pre- and 1 post-treatment observation per unit (to eliminate serial correlation, as suggested in BDM). The resulting DGP for the collapsed dataset is

$$Y^C_{ip}=\beta+\tau D^C_{ip}+\upsilon_i+\delta^C_p+\omega^C_{ip}\tag{A41}$$

where Y^C_{ip} is the average outcome for unit i for collapsed period p and the other variables are as defined in Appendix A.2.2 or are the collapsed analogs. Note that the τ in Equation (A41) is equivalent to that in Equation (A7). These models will yield the same estimate of the treatment effect, $\widehat{\tau}$, but different estimates of its variance in the presence of a (pre-collapsed) serially correlated error structure.

Equivalence of first-difference model BDM show that applying the OLS variance estimator to Equation (A41) achieves the correct false rejection rate. To see why this is the case, note that this collapsed model can alternatively be expressed as a first-difference model by subtracting each unit's collapsed pre-treatment data from its collapsed post-treatment data. Let ΔY^C_i be this difference

for the outcome of interest, which gives

$$\begin{aligned}\Delta Y_i^C &= Y_{iA}^C - Y_{iB}^C \\ &= \left(\beta + \tau D_{iA}^C + \upsilon_i + \delta_A^C + \omega_{iA}^C\right) - \left(\beta + \tau D_{iB}^C + \upsilon_i + \delta_B^C + \omega_{iB}^C\right) \\ &= \tau\left(D_{iA}^C - D_{iB}^C\right) + \left(\delta_A^C - \delta_B^C\right) + \left(\omega_{iA}^C - \omega_{iB}^C\right)\end{aligned}$$

Defining the other differences variables similarly gives the first-difference DGP of

$$\Delta Y_i^C = \tau \Delta D_i^C + \Delta\delta^C + \Delta\omega_i^C \tag{A42}$$

Equations (A41) and (A42) are isomorphic, so estimating these models yields not only the same estimate of the treatment effect, $\widehat{\tau}$, but also the same estimate of its variance. Note that the first-difference model, Equation (A42), is cross-sectional, so the error terms are i.i.d. and the model meets the assumptions of OLS.[2] As a result, the OLS variance estimator is unbiased for the first-difference model, as well as the isomorphic collapsed model of Equation (A41).

Power Although using a collapsed dataset yields the correct false rejection (or Type I error) rate, experiments will not be correctly powered if the McKenzie formula is applied to a collapsed dataset. To see this, first consider the MDE of an experiment based on the first-difference model of Equation (A42). This is a cross-sectional model, so applying Equations (A5) and (A6) yields:

$$\begin{aligned}\text{Var}\left(\hat{\tau} \mid \mathbf{X}\right) &= \frac{\sigma^2_{\Delta\omega^C}}{P(1-P)J} \\ MDE &= \left(t_{1-\kappa}^{J-2} + t_{\alpha/2}^{J-2}\right)\sqrt{\frac{\sigma^2_{\Delta\omega^C}}{P(1-P)J}}\end{aligned} \tag{A43}$$

where $\sigma^2_{\Delta\omega^C}$ is the variance of the error term in the collapsed, first-difference model. This variance can be expressed as a function of the parameters that define the error structure of the *collapsed* data

$$\begin{aligned}\sigma^2_{\Delta\omega^C} &= \text{Var}\left(\omega_{iA}^C - \omega_{iB}^C \mid \mathbf{X}\right) \\ &= \text{Var}\left(\omega_{iA}^C \mid \mathbf{X}\right) + \text{Var}\left(\omega_{iB}^C \mid \mathbf{X}\right) - 2\,\text{Cov}\left(\omega_{iA}^C, \omega_{iB}^C \mid \mathbf{X}\right) \\ &= 2\left(\sigma^2_{\omega^C} - \psi^X_{\omega^C}\right)\end{aligned} \tag{A44}$$

where $\sigma^2_{\omega^C}$ is the variance of the error term of the collapsed model, and $\psi^X_{\omega^C}$ is the average covariance between error terms for the same unit in the collapsed model. Substituting Equation (A44) into Equation (A43) gives the variance of $\hat{\tau}$ in terms of parameters of the collapsed data:

$$\text{Var}\left(\hat{\tau} \mid \mathbf{X}\right) = \left(\frac{2}{P(1-P)J}\right)\left(\sigma^2_{\omega^C} - \psi^X_{\omega^C}\right) \tag{A45}$$

2. There are no cross-sectional error correlations due to Assumption A9, because randomization obviates the need to account for this kind of correlation (see Lemma 1).

This formula is equal to the variance of $\hat{\tau}$ from the SCR formula applied to collapsed data, where $m = r = 1$. Applying the McKenzie formula to collapsed data, however, gives the incorrect variance:

$$\text{Var}\left(\hat{\tau} \mid \mathbf{X}\right) = \left(\frac{2}{P(1-P)J}\right)\sigma^2_{\omega^C} \tag{A46}$$

Equations (A45) and (A46) differ by the $\psi^X_{\omega^C}$ term that characterizes the covariance between the pre- and post-treatment error terms in the collapsed data. This term is omitted from the McKenzie formula that (incorrectly) assumes no serial correlation in the error structure. The error structure parameters of the collapsed data in Equation (A45) can also be expressed as functions of the parameters that define the error structure of the original, uncollapsed data

$$\begin{aligned}
\sigma^2_{\omega^C} &= \frac{1}{2}\text{Var}\left(\omega^C_{iA} \mid \mathbf{X}\right) + \frac{1}{2}\text{Var}\left(\omega^C_{iB} \mid \mathbf{X}\right) \\
&= \frac{1}{2}\text{Var}\left(\frac{1}{r}\sum_{t=1}^{r}\omega_{it} \mid \mathbf{X}\right) + \frac{1}{2}\text{Var}\left(\frac{1}{m}\sum_{s=-m+1}^{0}\omega_{is} \mid \mathbf{X}\right) \\
&= \frac{1}{2r}\left[\sigma^2_\omega + (r-1)\psi^A\right] + \frac{1}{2m}\left[\sigma^2_\omega + (m-1)\psi^B\right] \\
&= \frac{1}{2}\left[\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{r-1}{r}\right)\psi^A + \left(\frac{m-1}{m}\right)\psi^B\right]
\end{aligned} \tag{A47}$$

$$\begin{aligned}
\psi^X_{\omega^C} &= \frac{1}{J}\sum_{i=1}^{J}\text{Cov}\left(\omega^C_{iA}, \omega^C_{iB} \mid \mathbf{X}\right) \\
&= \frac{1}{J}\sum_{i=1}^{J}\text{Cov}\left(\frac{1}{r}\sum_{t=1}^{r}\omega_{it}, \frac{1}{m}\sum_{s=-m+1}^{0}\omega_{is} \mid \mathbf{X}\right) \\
&= \frac{1}{Jmr}\sum_{i=1}^{J}\sum_{t=1}^{r}\sum_{s=-m+1}^{0}\text{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) \\
&= \psi^X
\end{aligned} \tag{A48}$$

Substituting Equations (A47) and (A48) into Equation (A45) gives

$$\text{Var}\left(\hat{\tau} \mid \mathbf{X}\right) = \left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]$$

which is equivalent to the variance of $\hat{\tau}$ given in Equation (A31) that is a component of the SCR formula. Hence, the uncollapsed, collapsed, and first-difference models yield (virtually) equivalent MDEs when using the appropriate power calculation formula.[3] By contrast, the McKenzie formula ignores the between-period serial correlation that remains after collapsing serially correlated data.

3. The only difference between these three MDE calculations is the critical values $t^d_{1-\kappa}$ and $t^d_{\alpha/2}$. The uncollapsed model will apply the CRVE *ex post*, which implies $d = J$ degrees of freedom *ex ante*. The collapsed model will apply the OLS variance estimator *ex post* to a panel with $2J$ observations and $J + 2$ regressors, which implies $d = J - 2$ degrees of freedom *ex ante*. The first-difference model will apply the OLS variance estimator *ex post* to a cross-sectional specification with J observations and 2 regressors, which implies $d = J - 2$ degrees of freedom *ex ante*.

A.2.5 Analysis of covariance (ANCOVA)

Model There are J units, P proportion of which are randomized into treatment. The researcher again collects outcome data Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For treated units, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for control units, $D_{it} = 0$ in all periods.

Assumption A15 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \omega_{it}$$

where Y_{it} is the outcome of interest for unit i at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{it} is a time-varying treatment indicator; υ_i is a time-invariant unit effect distributed i.i.d. $\mathcal{N}(0, \sigma_\upsilon^2)$; and ω_{it} is an idiosyncratic error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma_\omega^2)$.

Assumption A16 (Strict exogeneity)**.** $\mathrm{E}[\omega_{it} \mid \mathbf{X}] = 0$*, where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$*, and* $J - 1$ *unit dummies. This again follows from random assignment of* D_{it}*.*

Assumption A17 (Balanced panel)**.** *The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.*

Assumption A18 (Independence across units)**.** $\mathrm{E}[\omega_{it}\omega_{js} \mid \mathbf{X}] = 0,\ \forall\ i \neq j,\ \forall\ t, s.$

Assumption A19 (Uniform covariance structures)**.** *Define:*

$$\psi_i^B \equiv \frac{2}{m(m-1)} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

$$\psi_i^A \equiv \frac{2}{r(r-1)} \sum_{t=1}^{r-1} \sum_{s=t+1}^{r} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

$$\psi_i^X \equiv \frac{1}{mr} \sum_{t=-m+1}^{0} \sum_{s=1}^{r} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

to be the average pre-treatment, post-treatment, and across-period covariance between different error terms of unit i, respectively. Using these definitions, assume that $\psi^B = \psi_i^B$, $\psi^A = \psi_i^A$, and $\psi^X = \psi_i^X\ \forall i$.

In this section, we consider an analysis of covariance (ANCOVA) regression model:

$$Y_{it} = \alpha + \tau D_i + \theta \overline{Y}_i^B + \varepsilon_{it} \tag{A49}$$

where Y_{it}, τ, and D_i are defined as above; α is an intercept term, $\overline{Y}_i^B = \frac{1}{m} \sum_{t=-m+1}^{0} Y_{it}$ is the mean pre-period value of the outcome variable for unit i, and ε_{it} is an idiosyncratic error term. Note that this regression is estimated using only post-treatment outcomes as the dependent variable, with a unit's pre-treatment mean outcome entering as an independent variable.

Note that this following derivations necessitate two relatively strong assumptions, as we discuss in Section 2.2.2 of the main text. First, we assume zero time shocks in the data generating process, thereby omitting δ_t from Assumption A15. If we included time shocks as in Assumption A3, then the conditional expectations in Equations (A56) and (A57) would each depend on the error terms and pre-period means of all units in the experiment, making the analytic solution intractable.[4] Second, Assumption A19 forces the ψ parameters to be constant across all units. Without this simplifying assumption, Equations (A58)–(A60) would contain many additional terms describing the heterogeneity of ψ parameters, which has a negligible effect in experiments of resaonable size[5] and cannot be estimated from a pre-existing dataset.[6]

Coefficient estimate The coefficient estimates from an OLS regression are

$$\begin{aligned}
\begin{pmatrix} \hat{\alpha} \\ \hat{\tau} \\ \hat{\theta} \end{pmatrix} &= (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{Y} \\
&= \left(\begin{pmatrix} \mathbf{1}' \\ \mathbf{D}' \\ \overline{\mathbf{Y}}^{\mathbf{B}\prime} \end{pmatrix} \begin{pmatrix} \mathbf{1} & \mathbf{D} & \overline{\mathbf{Y}}^{\mathbf{B}} \end{pmatrix} \right)^{-1} \begin{pmatrix} \mathbf{1}' \\ \mathbf{D}' \\ \overline{\mathbf{Y}}^{\mathbf{B}\prime} \end{pmatrix} \mathbf{Y} \\
&= \underbrace{\begin{pmatrix} Jr & PJr & Jr\overline{Y}^{B} \\ PJr & PJr & PJr\overline{Y}^{B}_{T} \\ Jr\overline{Y}^{B} & PJr\overline{Y}^{B}_{T} & r\sum_{i=1}^{J}(\overline{Y}^{B}_{i})^2 \end{pmatrix}^{-1}}_{\mathbf{M}} \begin{pmatrix} Jr\overline{Y}^{A} \\ PJr\overline{Y}^{A}_{T} \\ r\sum_{i=1}^{J}\overline{Y}^{A}_{i}\overline{Y}^{B}_{i} \end{pmatrix}
\end{aligned} \tag{A50}$$

The inverse of the $\mathbf{M}$ matrix is

$$\mathbf{M}^{-1} = \frac{PJ^2r^2}{\det(\mathbf{M})} \times \begin{pmatrix} \frac{1}{J}\sum_{i=1}^{J}(\overline{Y}^{B}_{i})^2 - P(\overline{Y}^{B}_{T})^2 & \overline{Y}^{B}\overline{Y}^{B}_{T} - \frac{1}{J}\sum_{i=1}^{J}(\overline{Y}^{B}_{i})^2 & P\overline{Y}^{B}_{T} - \overline{Y}^{B} \\ \overline{Y}^{B}\overline{Y}^{B}_{T} - \frac{1}{J}\sum_{i=1}^{J}(\overline{Y}^{B}_{i})^2 & \frac{1}{PJ}\sum_{i=1}^{J}(\overline{Y}^{B}_{i})^2 - \frac{1}{P}(\overline{Y}^{B})^2 & \overline{Y}^{B} - \overline{Y}^{B}_{T} \\ P\overline{Y}^{B}_{T} - \overline{Y}^{B} & \overline{Y}^{B} - \overline{Y}^{B}_{T} & 1 - P \end{pmatrix}$$

4. If we include time shocks and time fixed effects in our data generating process and regression model, respectively, the simplest analytic solution includes de-meaning by time period to remove these time shocks, similar to Appendix A.2.1. Then Equations (A56) and (A57) would instead describe the expectation of the product of the de-meaned error terms conditional on the de-meaned pre-period averages, which are a function of the error terms and pre-period means of all units in the experiment. These expressions increase in complexity in proportion to the number of units, making them analytically intractable for any reasonable number of experimental units while allowing for arbitrary serial correlation. An alternate solution method does not de-mean by time period to remove time shocks, but instead solves directly for all parameters in the model. This method requires inverting a $(m + r + 2) \times (m + r + 2)$ matrix, which is analytically intractable for an experiment of arbitrary length.

5. We test this second assumption by simulating datasets with uniform ψ parameters or heterogeneous ψ parameters and then performing Monte Carlo simulations as described in Appendix B.1. We find no appreciable difference in the simulated power of these different datasets.

6. A dataset contains only one realiztion of each error draw, so it is impossible to estimate a covariance between any two of these draws. We can, however, estimate an average covariance over all units by treating each unit's error as a draw from the same distribution, as described in more detail in Appendices D.1 and E.1.

and the determinant of $\mathbf{M}$ is

$$\begin{aligned}
\det(\mathbf{M}) &= PJ^3r^3\left[\frac{1}{J}\sum_{i=1}^{J}(\overline{Y}_i^B)^2 - P(\overline{Y}_T^B)^2\right] + P^2J^3r^3\left[\overline{Y}^B\overline{Y}_T^B - \frac{1}{J}\sum_{i=1}^{J}(\overline{Y}_i^B)^2\right] \\
&\quad + PJ^3r^3\overline{Y}^B\left[P\overline{Y}_T^B - \overline{Y}^B\right] \\
&= P(1-P)J^2r^3\sum_{i=1}^{J}(\overline{Y}_i^B)^2 - P^2(1-P)(\overline{Y}_T^B)^2 - P(1-P)^2(\overline{Y}_C^B)^2 \\
&= P(1-P)J^2r^3\left[\sum_{i=1}^{PJ}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2\right]
\end{aligned}$$

Therefore, the coefficient estimates are

$$\begin{aligned}
\hat{\alpha} &= \frac{\overline{Y}_C^A\left[\sum_{i=1}^{J}(\overline{Y}_i^B)^2 - PJ(\overline{Y}_T^B)^2 - (1-P)J(\overline{Y}_C^B)^2\right]}{\sum_{i=1}^{J}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2} \\
&\quad - \frac{\overline{Y}_C^B\left[\sum_{i=1}^{J}\overline{Y}_i^B\overline{Y}_i^A - PJ\overline{Y}_T^B\overline{Y}_T^A - (1-P)J\overline{Y}_C^B\overline{Y}_T^A\right]}{\sum_{i=1}^{J}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2} \\
\hat{\tau} &= \frac{(\overline{Y}_T^A - \overline{Y}_C^A)\left[\sum_{i=1}^{J}(\overline{Y}_i^B)^2 - PJ(\overline{Y}_T^B)^2 - (1-P)J(\overline{Y}_C^B)^2\right]}{\sum_{i=1}^{J}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2} \\
&\quad - \frac{(\overline{Y}_T^B - \overline{Y}_C^B)\left[\sum_{i=1}^{J}\overline{Y}_i^B\overline{Y}_i^A - PJ\overline{Y}_T^B\overline{Y}_T^A - (1-P)J\overline{Y}_C^B\overline{Y}_T^A\right]}{\sum_{i=1}^{J}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2} \\
\hat{\theta} &= \frac{\sum_{i=1}^{J}\overline{Y}_i^B\overline{Y}_i^A - PJ\overline{Y}_T^B\overline{Y}_T^A - (1-P)J\overline{Y}_C^B\overline{Y}_C^A}{\sum_{i=1}^{J}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2}
\end{aligned}$$

We rewrite these terms as

$$\begin{aligned}
\hat{\alpha} &= \overline{Y}_C^A - \hat{\theta}\overline{Y}_C \\
\hat{\tau} &= (\overline{Y}_T^A - \overline{Y}_C^A) - \hat{\theta}(\overline{Y}_T^B - \overline{Y}_C^B) \\
\hat{\theta} &= \frac{\sum_{i=1}^{PJ}(\overline{Y}_i^B - \overline{Y}_T^B)(\overline{Y}_i^A - \overline{Y}_T^A) + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)(\overline{Y}_i^A - \overline{Y}_C^A)}{\sum_{i=1}^{PJ}(\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J}(\overline{Y}_i^B - \overline{Y}_C^B)^2}
\end{aligned}$$

Variance of coefficient estimates The variance of each coefficient estimate is:

$$\text{Var}\left[\begin{pmatrix}\hat{\alpha}\\ \hat{\tau}\\ \hat{\theta}\end{pmatrix} \mid \mathbf{X}\right] = \underbrace{(\mathbf{X}'\mathbf{X})}_{\mathbf{M}}^{-1}\underbrace{\mathbf{X}'\,\mathrm{E}[\varepsilon\varepsilon' \mid \mathbf{X}]\mathbf{X}}_{\mathbf{N}}\underbrace{(\mathbf{X}'\mathbf{X})}_{\mathbf{M}}^{-1}$$

where $\mathbf{M}$ is the same as in Equation (A50) and $\mathbf{N}$ is

$$\begin{aligned}\mathbf{N} &= \begin{pmatrix} \mathbf{1}' \\ \mathbf{D}' \\ \overline{\mathbf{Y}}^{\mathbf{B}\prime} \end{pmatrix} \begin{pmatrix} \mathrm{E}[\varepsilon_{11}\varepsilon_{11} \mid \mathbf{X}] & \dots & \mathrm{E}[\varepsilon_{11}\varepsilon_{Jr} \mid \mathbf{X}] \\ \vdots & \ddots & \vdots \\ \mathrm{E}[\varepsilon_{Jr}\varepsilon_{11} \mid \mathbf{X}] & \dots & \mathrm{E}[\varepsilon_{Jr}\varepsilon_{Jr} \mid \mathbf{X}] \end{pmatrix} \begin{pmatrix} \mathbf{1} & \mathbf{D} & \overline{\mathbf{Y}}^{\mathbf{B}} \end{pmatrix} \\ &= \sum_{t=1}^{r}\sum_{s=1}^{r} \begin{pmatrix} \sum_{i=1}^{J}\sum_{j=1}^{J} \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] & \sum_{i=1}^{PJ}\sum_{j=1}^{J} \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] & \sum_{i=1}^{J}\sum_{j=1}^{J} \overline{Y}_i^B \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] \\ \sum_{i=1}^{PJ}\sum_{j=1}^{J} \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] & \sum_{i=1}^{PJ}\sum_{j=1}^{PJ} \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] & \sum_{i=1}^{PJ}\sum_{j=1}^{J} \overline{Y}_j^B \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] \\ \sum_{i=1}^{J}\sum_{j=1}^{J} \overline{Y}_i^B \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] & \sum_{i=1}^{PJ}\sum_{j=1}^{PJ} \overline{Y}_j^B \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] & \sum_{i=1}^{J}\sum_{j=1}^{J} \overline{Y}_i^B\overline{Y}_j^B \mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}] \end{pmatrix}\end{aligned}$$

We now focus on the variance of the treatment effect estimator, $\hat{\tau}$. Combining this expression for $\mathbf{N}$ with the expression for $\mathbf{M}^{-1}$ above, this variance is given by

$$\begin{aligned}\mathrm{Var}(\hat{\tau} \mid \mathbf{X}) = \frac{1}{J^2r^2Z^2}\sum_{t=1}^{r}\sum_{s=1}^{r}\Bigg\{ &\frac{1}{P^2}\sum_{i=1}^{PJ}\sum_{j=1}^{PJ}\Bigg[\Big(Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_T^B)\Big) \\ &\times \Big(Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_j^B - \overline{Y}_T^B)\Big)\mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}]\Bigg] \\ &+ \frac{2}{P(1-P)}\sum_{i=1}^{PJ}\sum_{j=PJ+1}^{J}\Bigg[\Big(Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_T^B)\Big) \\ &\times \Big(-Z - (1-P)J(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_j^B - \overline{Y}_C^B)\Big)\mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}]\Bigg] \\ &+ \frac{1}{(1-P)^2}\sum_{i=PJ+1}^{J}\sum_{j=PJ+1}^{J}\Bigg[\Big(-Z - (1-P)J(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_C^B)\Big) \\ &\times \Big(-Z - (1-P)J(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_j^B - \overline{Y}_C^B)\Big)\mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}]\Bigg]\Bigg\} \qquad \text{(A51)}\end{aligned}$$

where

$$Z = \sum_{k=1}^{PJ}(\overline{Y}_k^B - \overline{Y}_T^B)^2 + \sum_{k=PJ+1}^{J}(\overline{Y}_k^B - \overline{Y}_C^B)^2$$

We consider each pair of summations in Equation (A51) by defining

$$\begin{aligned}V_T \equiv \frac{1}{P^2}\sum_{t=1}^{r}\sum_{s=1}^{r}\sum_{i=1}^{PJ}\sum_{j=1}^{PJ}\Bigg[&\Big(Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_T^B)\Big) \\ &\times \Big(Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_j^B - \overline{Y}_T^B)\Big)\mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}]\Bigg] \qquad \text{(A52)} \\ V_X \equiv \frac{2}{P(1-P)}\sum_{t=1}^{r}\sum_{s=1}^{r}\sum_{i=1}^{PJ}\sum_{j=PJ+1}^{J}\Bigg[&\Big(Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_T^B)\Big) \\ &\times \Big(-Z - (1-P)J(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_j^B - \overline{Y}_C^B)\Big)\mathrm{E}[\varepsilon_{it}\varepsilon_{js} \mid \mathbf{X}]\Bigg] \qquad \text{(A53)}\end{aligned}$$

$$V_C \equiv \frac{1}{(1-P)^2}\sum_{t=1}^{r}\sum_{s=1}^{r}\sum_{i=PJ+1}^{J}\sum_{j=PJ+1}^{J}\Bigg[\Big(-Z-(1-P)J(\overline{Y}_T^B-\overline{Y}_C^B)(\overline{Y}_i^B-\overline{Y}_C^B)\Big) \times\Big(-Z-(1-P)J(\overline{Y}_T^B-\overline{Y}_C^B)(\overline{Y}_j^B-\overline{Y}_C^B)\Big)\,\mathrm{E}[\varepsilon_{it}\varepsilon_{js}\mid\mathbf{X}]\Bigg] \quad \text{(A54)}$$

so Equation (A51) can be rewritten as

$$\mathrm{Var}(\hat{\tau}\mid\mathbf{X}) = \frac{1}{J^2r^2Z^2}\left(V_T+V_X+V_C\right) \quad \text{(A55)}$$

Each of these expressions contains an expectation term, which we rewrite, using the Law of Iterated Expectations, as

$$\mathrm{E}[\varepsilon_{it}\varepsilon_{js}\mid\mathbf{X}] = \mathrm{E}[\varepsilon_{it}\varepsilon_{js}\mid\overline{Y}_i^B,\overline{Y}_j^B] = \mathrm{E}\left[\varepsilon_{js}\,\mathrm{E}[\varepsilon_{it}\mid\varepsilon_{js},\overline{Y}_i^B,\overline{Y}_j^B]\mid\overline{Y}_i^B,\overline{Y}_j^B\right]$$

Note that ε_{it}, ε_{js}, $\overline{Y}_i^B$, and $\overline{Y}_j^B$ are random variables drawn from a multivariate normal distribution. Consider two cases. First, if $i \neq j$, then the inner expectation term is

$$\mathrm{E}[\varepsilon_{it}\mid\varepsilon_{js},\overline{Y}_i^B,\overline{Y}_j^B] = \mathrm{E}[\varepsilon_{it}\mid\overline{Y}_i^B] = \frac{\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B)}{\mathrm{Var}(\overline{Y}_i^B)}\overline{Y}_i^B$$

and the full expectation term is

$$\mathrm{E}[\varepsilon_{it}\varepsilon_{js}\mid\mathbf{X}] = \frac{\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B)}{\mathrm{Var}(\overline{Y}_i^B)}\overline{Y}_i^B\,\mathrm{E}[\varepsilon_{js}\mid\overline{Y}_j^B] = \frac{\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B)}{\mathrm{Var}(\overline{Y}_i^B)}\overline{Y}_i^B\frac{\mathrm{Cov}(\varepsilon_{js},\overline{Y}_j^B)}{\mathrm{Var}(\overline{Y}_j^B)}\overline{Y}_j^B \quad \text{(A56)}$$

However, note that

$$\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B) = \mathrm{Cov}(\omega_{it},\overline{\omega}_i^B) - r\psi^X$$

so summing over all r post-treatent periods gives

$$\sum_{t=1}^{r}\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B) = \frac{1}{m}\sum_{t=1}^{r}\sum_{s=-m+1}^{0}\mathrm{Cov}(\omega_{it},\omega_{is}) - r\psi^X = 0$$

As a result, the case of $i \neq j$ will not contribute to V_T, V_X, and V_C. Consider instead the case of $i = j$, in which case the inner expectation term is

$$\begin{aligned}\mathrm{E}[\varepsilon_{it}\mid\varepsilon_{js},\overline{Y}_i^B,\overline{Y}_j^B] &= \mathrm{E}[\varepsilon_{it}\mid\varepsilon_{is},\overline{Y}_i^B] \\ &= \tfrac{\mathrm{Var}(\overline{Y}_i^B)\,\mathrm{Cov}(\varepsilon_{it},\varepsilon_{is})-\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B)\,\mathrm{Cov}(\varepsilon_{is},\overline{Y}_i^B)}{\mathrm{Var}(\varepsilon_{is})\,\mathrm{Var}(\overline{Y}_i^B)-\mathrm{Cov}(\varepsilon_{is},\overline{Y}_i^B)^2}\varepsilon_{is} + \tfrac{\mathrm{Var}(\varepsilon_{is})\,\mathrm{Cov}(\varepsilon_{it},\overline{Y}_i^B)-\mathrm{Cov}(\varepsilon_{it},\varepsilon_{is})\,\mathrm{Cov}(\varepsilon_{is},\overline{Y}_i^B)}{\mathrm{Var}(\varepsilon_{is})\,\mathrm{Var}(\overline{Y}_i^B)-\mathrm{Cov}(\varepsilon_{is},\overline{Y}_i^B)^2}\overline{Y}_i^B\end{aligned}$$

and the full expectation term is

$$\mathrm{E}[\varepsilon_{it}\varepsilon_{js}\mid\mathbf{X}] = \mathrm{E}\left[\varepsilon_{is}\,\mathrm{E}[\varepsilon_{it}\mid\varepsilon_{is},\overline{Y}_i^B]\mid\overline{Y}_i^B\right]$$

$$= \text{Cov}(\varepsilon_{it}, \varepsilon_{is}) - \frac{\text{Cov}(\varepsilon_{it}, \overline{Y}_i^B)\,\text{Cov}(\varepsilon_{is}, \overline{Y}_i^B)}{\text{Var}(\overline{Y}_i^B)} + \frac{\text{Cov}(\varepsilon_{it}, \overline{Y}_i^B)\,\text{Cov}(\varepsilon_{is}, \overline{Y}_i^B)}{\text{Var}(\overline{Y}_i^B)^2}(\overline{Y}_i^B)^2 \tag{A57}$$

Substituting Equations (A56) and (A57) into Equations (A52)–(A54) and simplifying gives

$$V_T = \frac{r^2}{P^2}\left[(1-\theta)^2\sigma_\upsilon^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right] \times \sum_{i=1}^{PJ}\left[Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_T^B))\right]^2 \tag{A58}$$

$$V_X = 0 \tag{A59}$$

$$V_C = \frac{r^2}{(1-P)^2}\left[(1-\theta)^2\sigma_\upsilon^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right] \times \sum_{i=PJ+1}^{J}\left[-Z - (1-P)J(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_C^B))\right]^2 \tag{A60}$$

where θ is the same as in Equation (A49) and can be expressed as

$$\theta = \frac{m\sigma_\upsilon^2 + m\psi^X}{m\sigma_\upsilon^2 + \sigma_\omega^2 + (m-1)\psi^B}$$

Then substituting Equations (A58)–(A60) into Equation (A55) gives the variance of the treatment effect estimator:

$$\begin{aligned}
\text{Var}(\hat{\tau} \mid \mathbf{X}) &= \frac{1}{J^2r^2Z^2}\left[(1-\theta)^2\sigma_\upsilon^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right] \\
&\quad \times \left[\frac{r^2}{P^2}\sum_{i=1}^{PJ}\left[Z - PJ(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_T^B))\right]^2\right. \\
&\quad \left. + \frac{r^2}{(1-P)^2}\sum_{i=PJ+1}^{J}\left[-Z - (1-P)J(\overline{Y}_T^B - \overline{Y}_C^B)(\overline{Y}_i^B - \overline{Y}_C^B))\right]^2\right] \\
&= \frac{1}{J^2Z^2}\left[(1-\theta)^2\sigma_\upsilon^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right] \\
&\quad \times \left[\frac{JZ^2}{P(1-P)} + J^2(\overline{Y}_T^B - \overline{Y}_C^B)^2Z\right] \\
&= \left[\frac{1}{P(1-P)J} + \frac{(\overline{Y}_T^B - \overline{Y}_C^B)^2}{Z}\right] \\
&\quad \times \left[(1-\theta)^2\sigma_\upsilon^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right]
\end{aligned} \tag{A61}$$

The second component of the first term in Equation (A61), $\frac{(\overline{Y}_T^B - \overline{Y}_C^B)^2}{Z}$, is the only component of this formula that includes data, rather than parameters describing the experimental design or error structure. Additionally, this term decreases in expectation to zero as the number of units in the experiment increases, so it is negligible when J is sufficiently large. Accordingly, we drop this term to yield an approximate expression for the variance of the treatment effect estimator:

$$\text{Var}(\hat{\tau} \mid \mathbf{X}) \approx \frac{1}{P(1-P)J}\left[(1-\theta)^2\sigma_v^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right]$$

Minimum detectable effect When using an ANCOVA regression model, the MDE is

$$\begin{aligned} MDE &= \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\text{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)} \\ &= \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\left[\tfrac{1}{P(1-P)J} + \tfrac{(\overline{Y}_T^B - \overline{Y}_C^B)^2}{Z}\right]^{1/2} \\ &\qquad \times \left[(1-\theta)^2\sigma_v^2 + \left(\tfrac{\theta^2}{m} + \tfrac{1}{r}\right)\sigma_\omega^2 + \tfrac{\theta^2(m-1)}{m}\psi^B + \tfrac{r-1}{r}\psi^A - 2\theta\psi^X\right]^{1/2} \qquad \text{(A62)} \\ &\approx \left(t_{1-\kappa}^J + t_{\alpha/2}^J\right)\sqrt{\tfrac{1}{P(1-P)J}\left[(1-\theta)^2\sigma_v^2 + \left(\tfrac{\theta^2}{m} + \tfrac{1}{r}\right)\sigma_\omega^2 + \tfrac{\theta^2(m-1)}{m}\psi^B + \tfrac{r-1}{r}\psi^A - 2\theta\psi^X\right]} \end{aligned}$$

This is the serial-correlation-robust ANCOVA (SCR ANCOVA) power calculation formula, found in Equation (11) in the main text.

A.3 Arbitrary cross-sectional correlations

In this section, we provide proofs of Lemma 1 from the main text. We begin by proving the analogous cross-sectional case, in Lemma A1:[7]

Lemma A1. *In a cross-sectional model with random assignment to treatment,* $\frac{\sigma_\varepsilon^2}{P(1-P)J}$ *is an unbiased estimator of the expectation of* $\text{Var}(\hat{\tau} \mid \mathbf{X})$ *even if* $\text{E}[\varepsilon_i\varepsilon_j \mid \mathbf{X}] \neq 0$ *for some* $i \neq j$.

Proof We wish to demonstrate that the OLS variance estimator, which assumes that errors are independent across units, is an unbiased estimator of the *ex ante* expected variance under non-i.i.d. errors when units are randomly assigned to treatment. To do this, consider a model similar to that of Appendix A.1 but allowing for correlation in error terms. That is:

Model There are J units randomly assigned a treatment status D_i, with proportion P in treatment ($D_i = 1$) and proportion $(1-P)$ in control ($D_i = 0$). The units are indexed so $i \in [1, PJ]$ is treated and $j \in [PJ+1, J]$ is a control. We make standard assumptions for randomized trials:

Assumption A20 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_i = \beta + \tau D_i + \varepsilon_i$$

7. Campbell (1977) provides the first version of this proof, which is cited by Moulton (1986), and which imposes a grouped error structure. Our proof allows for arbitrary cross-sectional error dependence. Athey and Imbens (2017b, 2017a) still recommend using Eicker-Huber-White standard errors in this case, to allow for heteroskedasticity. To our knowledge, no paper discusses power calculations in the presence of heteroskedastic disturbances.

where Y_i is the outcome of interest for unit i; β is the expected outcome of non-treated units; τ is the treatment effect which is homogeneous across all units; D_i is a treatment indicator; and ε_i is an idiosyncratic error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma_\varepsilon^2)$.

Assumption A21 (Strict exogeneity)**.** $\mathrm{E}[\varepsilon_i \mid \mathbf{X}] = 0$*, where* $\mathbf{X} = [\mathbf{1}\ \mathbf{D}]$*. In practice, this follows from random assignment of* D_i*.*

Coefficient estimate The coefficient estimates from an OLS regression are the same as in Appendix A.1:

$$\begin{aligned}\widehat{\beta} &= \overline{Y}_C \\ \widehat{\tau} &= \overline{Y}_T - \overline{Y}_C\end{aligned}$$

Variance of coefficient estimate The variance of the estimate of the treatment effect, $\hat{\tau}$, is

$$\mathrm{Var}\left(\widehat{\tau} \mid \mathbf{X}\right) = \mathrm{Var}\left(\overline{Y}_T \mid \mathbf{X}\right) + \mathrm{Var}\left(\overline{Y}_C \mid \mathbf{X}\right) - 2\,\mathrm{Cov}\left(\overline{Y}_T, \overline{Y}_C \mid \mathbf{X}\right) \tag{A63}$$

where the first term of Equation (A63) is

$$\begin{aligned}\mathrm{Var}\left(\overline{Y}_T \mid \mathbf{X}\right) &= \mathrm{Var}\left(\frac{1}{PJ}\sum_{i=1}^{PJ} Y_i \mid \mathbf{X}\right) \\ &= \mathrm{Var}\left(\frac{1}{PJ}\sum_{i=1}^{PJ} \varepsilon_i \mid \mathbf{X}\right) \\ &= \frac{\sigma_\varepsilon^2}{PJ} + \frac{2}{(PJ)^2}\sum_{i=1}^{PJ-1}\sum_{j=i+1}^{PJ} \mathrm{Cov}(\varepsilon_i, \varepsilon_j \mid \mathbf{X}) \\ &= \frac{\sigma_\varepsilon^2}{PJ} + \frac{PJ-1}{PJ}\psi_T \end{aligned} \tag{A64}$$

where

$$\psi_T \equiv \frac{2}{PJ(PJ-1)}\sum_{i=1}^{PJ-1}\sum_{j=i+1}^{PJ} \mathrm{Cov}\left(\varepsilon_i, \varepsilon_j \mid \mathbf{X}\right)$$

is the average covariance between treated units. Similarly, the second term of Equation (A63) is

$$\mathrm{Var}\left(\overline{Y}_C \mid \mathbf{X}\right) = \frac{\sigma_\varepsilon^2}{(1-P)J} + \frac{(1-P)J-1}{(1-P)J}\psi_C \tag{A65}$$

where

$$\psi_C \equiv \frac{2}{(1-P)J((1-P)J-1)}\sum_{i=PJ+1}^{J-1}\sum_{j=i+1}^{J} \mathrm{Cov}\left(\varepsilon_i, \varepsilon_j \mid \mathbf{X}\right)$$

is the average covariance between control units. The third term of Equation (A63) is

$$\begin{aligned}
-2\operatorname{Cov}\left(\overline{Y}_T, \overline{Y}_C \mid \mathbf{X}\right) &= -2\operatorname{Cov}\left(\frac{1}{PJ}\sum_{i=1}^{PJ} Y_i, \frac{1}{(1-P)J}\sum_{j=PJ+1}^{J} Y_j \mid \mathbf{X}\right) \\
&= \frac{-2}{P(1-P)J^2}\sum_{i=1}^{PJ}\sum_{j=PJ+1}^{J}\operatorname{Cov}(\varepsilon_i, \varepsilon_j \mid \mathbf{X}) \\
&= -2\psi_{TC} \qquad \text{(A66)}
\end{aligned}$$

where

$$\psi_{TC} \equiv \frac{1}{P(1-P)J^2}\sum_{i=1}^{PJ}\sum_{j=PJ+1}^{J}\operatorname{Cov}\left(\varepsilon_i, \varepsilon_j \mid \mathbf{X}\right)$$

is the average covariance between treatment and control units. Substituting Equations (A64)–(A66) into Equation (A63) yields

$$\begin{aligned}
\operatorname{Var}\left(\widehat{\tau} \mid \mathbf{X}\right) &= \left[\frac{\sigma_\varepsilon^2}{PJ} + \frac{PJ-1}{PJ}\psi_T\right] + \left[\frac{\sigma_\varepsilon^2}{(1-P)J} + \frac{(1-P)J-1}{(1-P)J}\psi_C\right] - 2\psi_{TC} \\
&= \frac{\sigma_\varepsilon^2}{P(1-P)J} + \frac{PJ-1}{PJ}\psi_T + \frac{(1-P)J-1}{(1-P)J}\psi_C - 2\psi_{TC}
\end{aligned}$$

Note that σ_ε^2, P, and J are constant population or design parameters. With this in mind, taking expectations yields

$$\operatorname{E}\left[\operatorname{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)\right] = \frac{\sigma_\varepsilon^2}{P(1-P)J} + \frac{PJ-1}{PJ}\operatorname{E}\left[\psi_T\right] + \frac{(1-P)J-1}{(1-P)J}\operatorname{E}\left[\psi_C\right] - 2\operatorname{E}\left[\psi_{TC}\right] \qquad \text{(A67)}$$

By random assignment to treatment, the expectation terms are

$$\begin{aligned}
\operatorname{E}\left[\psi_T\right] &= \frac{2}{PJ(PJ-1)}\operatorname{E}\left[\sum_{i=1}^{PJ-1}\sum_{j=i+1}^{PJ}\operatorname{Cov}\left(\varepsilon_i, \varepsilon_j \mid \mathbf{X}\right)\right] \\
&= \frac{2}{PJ(PJ-1)}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\operatorname{Cov}\left(\varepsilon_i, \varepsilon_j\right)\operatorname{E}\left[\mathbf{1}\{i \in T, j \in T\}\right] \\
&= \frac{2P}{J(PJ-1)}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\operatorname{Cov}\left(\varepsilon_i, \varepsilon_j\right) \qquad \text{(A68)}
\end{aligned}$$

$$\operatorname{E}\left[\psi_C\right] = \frac{2(1-P)}{J((1-P)J-1)}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\operatorname{Cov}\left(\varepsilon_i, \varepsilon_j\right) \qquad \text{(A69)}$$

$$\operatorname{E}\left[\psi_{TC}\right] = \frac{2}{J^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\operatorname{Cov}\left(\varepsilon_i, \varepsilon_j\right) \qquad \text{(A70)}$$

Substituting Equations (A68)–(A70) into Equation (A67) yields

$$\begin{aligned}\mathrm{E}\left[\mathrm{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)\right] &= \frac{\sigma_{\varepsilon}^2}{P(1-P)J} + \frac{2}{J^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\mathrm{Cov}\left(\varepsilon_i, \varepsilon_j\right) + \frac{2}{J^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\mathrm{Cov}\left(\varepsilon_i, \varepsilon_j\right) \\ &\quad - \frac{4}{J^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\mathrm{Cov}\left(\varepsilon_i, \varepsilon_j\right) \\ &= \frac{\sigma_{\varepsilon}^2}{P(1-P)J} \end{aligned} \tag{A71}$$

By Assumptions A20 and A21, the OLS variance estimator is an unbiased estimator of $\frac{\sigma_{\varepsilon}^2}{P(1-P)J}$. That is:

$$\mathrm{E}\left[\widehat{\mathrm{Var}}_{OLS}\left(\widehat{\tau} \mid \mathbf{X}\right)\right] = \frac{\sigma_{\varepsilon}^2}{P(1-P)J} \tag{A72}$$

Combining Equations (A71) and (A72) gives

$$\mathrm{E}\left[\mathrm{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)\right] = \mathrm{E}\left[\widehat{\mathrm{Var}}_{OLS}\left(\widehat{\tau} \mid \mathbf{X}\right)\right] \tag{A73}$$

Therefore, the OLS variance estimator is an unbiased estimator of the *ex ante* expected variance under random assignment to treatment, even under non-i.i.d. errors. □

Lemma 1. *In a panel difference-in-differences model with treatment randomly assigned at the unit level,* $\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma_{\omega}^2 + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]$ *is an unbiased estimator of the expectation of* $\mathrm{Var}(\hat{\tau} \mid \mathbf{X})$*, even in the presence of arbitrary within-period cross-sectional correlations.*

Proof We wish to demonstrate that the serial-correlation-robust variance, which assumes that errors are independent across units, is an unbiased estimator of the *ex ante* expected variance under arbitrary within-period correlations, when units are randomly assigned to treatment. To do this, again consider the model of Appendix A.2.3.

Coefficient estimate The coefficient estimate is the same as in Appendix A.2.3:

$$\widehat{\tau} = \left(\overline{Y}_T^A - \overline{Y}_T^B\right) - \left(\overline{Y}_C^A - \overline{Y}_C^B\right)$$

Variance of coefficient estimate The variance of the treatment effect estimator, $\widehat{\tau}$, is also the same as in Appendix A.2.3:

$$\begin{aligned}\mathrm{Var}\left(\widehat{\tau} \mid \mathbf{X}\right) &= \left(\frac{m+r}{P(1-P)Jmr}\right)\sigma_{\omega}^2 + \left(\frac{m-1}{PJm}\right)\psi_{i,T}^B + \left(\frac{PJ-1}{PJm}\right)\psi_{t,T}^B \\ &\quad + \left(\frac{r-1}{PJr}\right)\psi_{i,T}^A + \left(\frac{PJ-1}{PJr}\right)\psi_{t,T}^A + \left(\frac{m-1}{(1-P)Jm}\right)\psi_{i,C}^B \\ &\quad + \left(\frac{(1-P)J-1}{(1-P)Jm}\right)\psi_{t,C}^B + \left(\frac{r-1}{(1-P)Jr}\right)\psi_{i,C}^A + \left(\frac{(1-P)J-1}{(1-P)Jr}\right)\psi_{t,C}^A\end{aligned}$$

$$- \frac{2}{PJ}\psi^X_{i,T} - \frac{2}{m}\psi^B_{t,X} - \frac{2}{r}\psi^A_{t,X} - \frac{2}{(1-P)J}\psi^X_{i,C}$$

Next, we show that, in expectation, this is equal to the variance component of the SCR formula. We begin by taking expectations:

$$\begin{aligned}
\mathrm{E}\left[\mathrm{Var}\left(\widehat{\tau} \mid \mathbf{X}\right)\right] = &\left(\frac{m+r}{P(1-P)Jmr}\right)\sigma^2_\omega + \left(\frac{m-1}{PJm}\right)\mathrm{E}\left[\psi^B_{i,T}\right] + \left(\frac{PJ-1}{PJm}\right)\mathrm{E}\left[\psi^B_{t,T}\right] \\
&+ \left(\frac{r-1}{PJr}\right)\mathrm{E}\left[\psi^A_{i,T}\right] + \left(\frac{PJ-1}{PJr}\right)\mathrm{E}\left[\psi^A_{t,T}\right] + \left(\frac{m-1}{(1-P)Jm}\right)\mathrm{E}\left[\psi^B_{i,C}\right] \\
&+ \left(\frac{(1-P)J-1}{(1-P)Jm}\right)\mathrm{E}\left[\psi^B_{t,C}\right] + \left(\frac{r-1}{(1-P)Jr}\right)\mathrm{E}\left[\psi^A_{i,C}\right] + \\
&+ \left(\frac{(1-P)J-1}{(1-P)Jr}\right)\mathrm{E}\left[\psi^A_{t,C}\right] - \frac{2}{PJ}\mathrm{E}\left[\psi^X_{i,T}\right] - \frac{2}{m}\mathrm{E}\left[\psi^B_{t,X}\right] \\
&- \frac{2}{r}\mathrm{E}\left[\psi^A_{t,X}\right] - \frac{2}{(1-P)J}\mathrm{E}\left[\psi^X_{i,C}\right] \qquad \text{(A74)}
\end{aligned}$$

where

$$\mathrm{E}\left[\psi^B_{i,T}\right] = \frac{2}{Jm(m-1)}\sum_{i=1}^{J}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{is}\right)$$

$$\mathrm{E}\left[\psi^B_{t,T}\right] = \frac{2P}{J(PJ-1)m}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right)$$

$$\mathrm{E}\left[\psi^A_{i,T}\right] = \frac{2}{Jr(r-1)}\sum_{i=1}^{J}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{is}\right)$$

$$\mathrm{E}\left[\psi^A_{t,T}\right] = \frac{2P}{J(PJ-1)r}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right)$$

$$\mathrm{E}\left[\psi^B_{i,C}\right] = \frac{2}{Jm(m-1)}\sum_{i=1}^{J}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{is}\right)$$

$$\mathrm{E}\left[\psi^B_{t,C}\right] = \frac{2(1-P)}{J((1-P)J-1)m}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right)$$

$$\mathrm{E}\left[\psi^A_{i,C}\right] = \frac{2}{Jr(r-1)}\sum_{i=1}^{J}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{is}\right)$$

$$\mathrm{E}\left[\psi^A_{t,C}\right] = \frac{2(1-P)}{J((1-P)J-1)r}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right)$$

$$\mathrm{E}\left[\psi^X_{i,T}\right] = \frac{1}{Jmr}\sum_{i=1}^{J}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{is}\right)$$

$$\mathrm{E}\left[\psi^B_{t,TC}\right] = \frac{2}{J^2m}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right)$$

$$\mathrm{E}\left[\psi^A_{t,TC}\right] = \frac{2}{J^2 r}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right)$$

$$\mathrm{E}\left[\psi^X_{i,C}\right] = \frac{1}{Jmr}\sum_{i=1}^{J}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{is}\right)$$

Using these terms, we rewrite Equation (A74) as

$$\begin{aligned}
\mathrm{E}\left[\mathrm{Var}\left(\widehat{\tau}\mid\mathbf{X}\right)\right] &= \left(\frac{m+r}{P(1-P)Jmr}\right)\sigma^2_\omega + \left(\frac{m-1}{PJm}\right)\psi^B + \frac{2}{J^2m^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) \\
&\quad + \left(\frac{r-1}{PJr}\right)\psi^A + \frac{2}{J^2r^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) + \left(\frac{m-1}{(1-P)Jm}\right)\psi^B \\
&\quad + \frac{2}{J^2m^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) + \left(\frac{r-1}{(1-P)Jr}\right)\psi^A \\
&\quad + \frac{2}{J^2r^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) - \left(\frac{2}{PJ}\right)\psi^X \\
&\quad - \frac{4}{J^2m^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) - \left(\frac{2}{(1-P)J}\right)\psi^X \\
&\quad - \frac{4}{J^2r^2}\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) \\
&= \left(\frac{m+r}{P(1-P)Jmr}\right)\sigma^2_\omega + \left[\frac{m-1}{PJm} + \frac{m-1}{(1-P)Jm}\right]\psi^B \\
&\quad + \left[\frac{r-1}{PJr} + \frac{r-1}{(1-P)Jr}\right]\psi^A - \left[\frac{2}{PJ} + \frac{2}{(1-P)J}\right]\psi^X \\
&\quad + \left[\frac{2}{J^2m^2} + \frac{2}{J^2m^2} - \frac{4}{J^2m^2}\right]\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=-m+1}^{0}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) \\
&\quad + \left[\frac{2}{J^2r^2} + \frac{2}{J^2r^2} - \frac{4}{J^2r^2}\right]\sum_{i=1}^{J-1}\sum_{j=i+1}^{J}\sum_{t=1}^{r}\mathrm{Cov}\left(\omega_{it},\omega_{jt}\right) \\
&= \frac{1}{P(1-P)J}\left[\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right] \qquad \text{(A75)}
\end{aligned}$$

Recall from Equation (A31) that the variance component of the SCR formula is

$$\mathrm{Var}_{SCR}\left(\widehat{\tau}\mid\mathbf{X}\right) = \frac{1}{P(1-P)J}\left[\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right] \qquad \text{(A76)}$$

Combining Equations (A75) and (A76) gives

$$\mathrm{E}\left[\mathrm{Var}\left(\widehat{\tau}\mid\mathbf{X}\right)\right] = \mathrm{Var}_{SCR}\left(\widehat{\tau}\mid\mathbf{X}\right)$$

Therefore, the SCR variance estimator is an unbiased estimator of the *ex ante* expected variance under random assignment to treatment, even under non-i.i.d. errors. □

A.4 Equivalence of alternative difference-in-difference estimators

In this section, we demonstrate that the DD treatment effect estimator—and therefore, the variance of this estimator—with a full set of unit and time fixed effects is the same as the "simplified" DD estimator where unit fixed effects are replaced with a treatment group dummy and time fixed effects are replaced with a post-period dummy. We begin with the same model, data generating process, and assumptions as Section A.2.1, suppressed here in the interest of parsimony.

Coefficient estimate In Section A.2.1 above, we estimate the treatment effect, τ, using OLS with unit and time fixed effects. Here, we instead estimate the τ using OLS with a treatment group dummy and a post-treatment period dummy (i.e. Equation (8), with new notation):

$$Y_{it} = \beta + \tau D_{it} + \alpha_g + \gamma_p + \varepsilon_{it} \tag{A77}$$

where α_g is a treatment group dummy variable and γ_p is a post-treatment-period dummy variable. In a balanced panel, this is equivalent to de-meaning by the group and post-treatment period. Define:

$$\ddot{Y}_{it} = Y_{it} - \overline{Y}_g - \overline{Y}_p + \overline{\overline{Y}} \tag{A78}$$

$$\ddot{D}_{it} = D_{it} - \overline{D}_g - \overline{D}_p + \overline{\overline{D}} \tag{A79}$$

$$\ddot{\varepsilon}_{it} = \varepsilon_{it} - \overline{\varepsilon}_g - \overline{\varepsilon}_p + \overline{\overline{\varepsilon}} \tag{A80}$$

where:

$$\overline{Y}_T = \frac{1}{PJ(m+r)} \sum_{i=1}^{PJ} \sum_{t=-m+1}^{r} Y_{it}$$

$$\overline{Y}_C = \frac{1}{(1-P)J(m+r)} \sum_{i=PJ+1}^{J} \sum_{t=-m+1}^{r} Y_{it}$$

$$\overline{Y}_A = \frac{1}{Jr} \sum_{i=1}^{J} \sum_{t=1}^{r} Y_{it}$$

$$\overline{Y}_B = \frac{1}{Jm} \sum_{i=1}^{J} \sum_{t=-m+1}^{0} Y_{it}$$

$$\overline{\overline{Y}} = \frac{1}{J(m+r)} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} Y_{it}$$

with $\overline{D}_g$, $\overline{D}_p$, $\overline{\overline{D}}$, $\overline{\varepsilon}_g$, $\overline{\varepsilon}_p$, and $\overline{\overline{\varepsilon}}$ defined analogously. Substituting Equations (A78)–(A80) into Equation (A77) and simplifying gives the de-meaned DGP:

$$
\begin{aligned}
\ddot{Y}_{it} &= (\tau D_{it} + \alpha_g + \gamma_p + \varepsilon_{it}) - (\tau \overline{D}_g + \alpha_g + \overline{\gamma} + \overline{\varepsilon}_g) - (\tau \overline{D}_p + \overline{\alpha} + \gamma_p + \overline{\varepsilon}_p) + (\tau \overline{\overline{D}} + \overline{\alpha} + \overline{\gamma} + \overline{\overline{\varepsilon}}) \\
&= \tau(D_{it} - \overline{D}_g - \overline{D}_p + \overline{\overline{D}}) + (\varepsilon_{it} - \overline{\varepsilon}_g - \overline{\varepsilon}_p + \overline{\overline{\varepsilon}}) \\
&= \tau \ddot{D}_{it} + \ddot{\varepsilon}_{it}
\end{aligned}
$$

The estimate of the treatment effect is

$$
\begin{aligned}
\widehat{\tau} &= (\ddot{\mathbf{D}}'\ddot{\mathbf{D}})^{-1}\ddot{\mathbf{D}}'\ddot{\mathbf{Y}} \\
&= \left(\sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it}^2 \right)^{-1} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} \ddot{Y}_{it} \\
&= \frac{m+r}{P(1-P)Jmr} \left[\sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} Y_{it} - \sum_{i=1}^{J} \overline{Y}_i \sum_{t=-m+1}^{r} \ddot{D}_{it} - \sum_{t=-m+1}^{r} \overline{Y}_t \sum_{i=1}^{J} \ddot{D}_{it} + \overline{Y} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} \right] \\
&= \frac{m+r}{P(1-P)Jmr} \sum_{i=1}^{J} \sum_{t=-m+1}^{r} \ddot{D}_{it} Y_{it} \\
&= \frac{m+r}{P(1-P)Jmr} \left[\sum_{i=1}^{PJ} \left(\frac{(1-P)m}{m+r} \sum_{t=1}^{r} Y_{it} - \frac{(1-P)r}{m+r} \sum_{t=-m+1}^{0} Y_{it} \right) \right. \\
&\qquad \left. + \sum_{i=PJ+1}^{J} \left(\frac{Pr}{m+r} \sum_{t=-m+1}^{0} Y_{it} - \frac{Pm}{m+r} \sum_{t=1}^{r} Y_{it} \right) \right] \\
&= \left(\overline{Y}_T^A - \overline{Y}_T^B \right) - \left(\overline{Y}_C^A - \overline{Y}_C^B \right)
\end{aligned}
$$

where

$$
\begin{aligned}
\overline{Y}_T^A &= \frac{1}{PJr} \sum_{i=1}^{PJ} \sum_{t=1}^{r} Y_{it} \\
\overline{Y}_T^B &= \frac{1}{PJm} \sum_{i=1}^{PJ} \sum_{t=-m+1}^{0} Y_{it} \\
\overline{Y}_C^A &= \frac{1}{(1-P)Jr} \sum_{i=PJ+1}^{J} \sum_{t=1}^{r} Y_{it} \\
\overline{Y}_C^B &= \frac{1}{(1-P)Jm} \sum_{i=PJ+1}^{J} \sum_{t=-m+1}^{0} Y_{it}
\end{aligned}
$$

which is identical to Equation (A11), meaning that the variance is also identical. □

It is trivial to extend the same logic to variants on the DD estimator in which *either* unit fixed effects are replaced with a treatment group dummy or time fixed effects are replaced with a post-treatment dummy.

B Figures in main text

This section provides further detail on the simulations and power calculations referenced in the main text. We discuss the algorithms and assumptions behind each of the simulation plots, as well as the two analytical power calculation figures.

B.1 Simulated AR(1) data

In Figure 1, we run Monte Carlo simulations where each iteration generates a new simulated dataset with an idiosyncratic error term (ω_{it}) that evolves via an AR(1) process. We vary the following three parameters across sets of 10,000 simulations: the number of pre-treatment periods (m), the number of post-treatment periods (r), and the AR(1) dependence parameter (γ). The remaining parameters (J, P, α, κ, β, μ_υ, σ^2_υ, μ_δ, σ^2_δ, σ^2_ω) are fixed across all simulations.

Step 1: We calculate τ^{McK} and τ^{SCR} for each set of simulations, given a set of parameters values for m, r, and γ. These values are functions of the number of pre-treatment periods m, the number of post-treatment periods r, the number of units J, the proportion of units randomized into treatment P, the desired Type-I error rate α, the desired power κ, the idiosyncratic variance σ^2_ω, and (for τ^{SCR}) the error structure as defined by ψ^B, ψ^A, and ψ^X:

$$\tau^{McK} = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right)\sqrt{\left(\frac{\sigma^2_\omega}{P(1-P)J}\right)\left(\frac{m+r}{mr}\right)}$$

$$\tau^{SCR} = (t^J_{1-\kappa} + t^J_{\alpha/2})\sqrt{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]}$$

Note that for both τ^{McK} and τ^{SCR}, we calculate the critical values $t^J_{1-\kappa}$ and $t^J_{\alpha/2}$ assuming J degrees of freedom, which is consistent with applying the CRVE *ex post*, clustering at the unit level with J units. Note also that ψ^B, ψ^A, and ψ^X depend on the correlation structure of the errors, and the AR(1) process enables us to derive closed form expressions for these covariances in terms of γ, σ^2_ω, m, and r. Because we set $m = r$ across all simulations, we can write ψ^B, ψ^A, and ψ^X as:

$$\psi^B = \frac{2\sigma^2_\omega}{(m-1)m}\sum_{z=1}^{m-1}(m-z)\gamma^z \tag{B1}$$

$$\psi^A = \psi^B \tag{B2}$$

$$\psi^X = \frac{\sigma^2_\omega}{m^2}\left[\sum_{z=1}^{m} z\gamma^z + \sum_{z=m+1}^{2m-1}(2m-z)\gamma^z\right] \tag{B3}$$

Step 2: For each simulation, we generate a dataset as specified by the data generating process:

$$Y_{it} = \beta + \upsilon_i + \delta_t + \omega_{it}$$

To do this, we draw J independent values of υ_i from the distribution $N(\mu_\upsilon, \sigma^2_\upsilon)$, and draw $m + r$ independent values of δ_t from the distribution $N(\mu_\delta, \sigma^2_\delta)$. We create the idiosyncratic error $\omega_{it} = \gamma\omega_{i(t-1)} + \xi_{it}$ by simulating an AR(1) process with serial correlation γ and a white noise term ξ_{it}

drawn from the distribution $N(0, \sigma_\xi^2)$, where $\sigma_\xi^2 = \sigma_\omega^2(1-\gamma^2)$.[8]

Step 3: We randomly assign treatment to PJ units. This involves randomly scrambling a vector of PJ ones and $(1-P)J$ zeros and assigning each unit i either a 1 indicating treatment or a 0 indicating control.[9] This allows us to construct a time-varying treatment indicator D_{it}, where $D_{it} = 1$ for all treated units in post-treatment periods only and $D_{it} = 0$ otherwise. We then create three outcome variables by adding treatment effects to the data generated in the previous step:

$$\begin{aligned} Y_{it}^{McK} &\equiv Y_{it} + \tau^{McK} D_{it} \\ Y_{it}^{SCR} &\equiv Y_{it} + \tau^{SCR} D_{it} \\ Y_{it}^{0} &\equiv Y_{it} + \tau^{0} D_{it}, \end{aligned}$$

where $\tau^0 = 0$ is a placebo treatment effect.

Step 4: We separately estimate the following three OLS-fixed effects regressions:

$$\begin{aligned} Y_{it}^{McK} &= \beta + \tau^{McK} D_{it} + \upsilon_i + \delta_t + \omega_{it} \\ Y_{it}^{SCR} &= \beta + \tau^{SCR} D_{it} + \upsilon_i + \delta_t + \omega_{it} \\ Y_{it}^{0} &= \beta + \tau^{0} D_{it} + \upsilon_i + \delta_t + \omega_{it} \end{aligned}$$

Step 5: For each estimated $\hat{\tau}^{McK}$, $\hat{\tau}^{SCR}$, and $\hat{\tau}^0$, we compute both OLS standard errors and CRVE standard errors, clustered at the unit level.

We repeat Steps 2–5 10,000 times, for values of $m = r \in \{1, \ldots, 20\}$ and for values of $\gamma \in \{0, 0.3, 0.5, 0.7, 0.9\}$.[10] After each set of 10,000 simulations, we calculate the percent of simulations where $\hat{\tau}^{McK}$, $\hat{\tau}^{SCR}$, and $\hat{\tau}^0$ reject the null hypothesis of $\tau = 0$ at significance level α, under both OLS and CRVE standard errors.

Figure 1 reports these rejection rates on the vertical axes, with the number of pre- and post-treatment periods ($m = r$) on the horizontal axes, for each value of γ. Reading the top row left to right, we report the rejection rates for τ^{McK} under OLS standard errors, for τ^{McK} under CRVE standard errors, and for τ^{SCR} under CRVE standard errors, respectively. Because these are rejection rates of true effects, we interpret these curves as realized statistical power. Reading the bottom row left to right, we report the rejection rates for τ^0 under OLS standard errors, for τ^0 under CRVE standard errors, and for τ^0 under CRVE standard errors, respectively. Because these are rejection rates of placebo effects, we report these curves as realized false rejection rates. (The bottom-center and bottom-right panels report identical rejection rates, because the center and right columns have the same test for false rejection rates.)

We fix $\alpha = 0.05$ and $\kappa = 0.80$ across all simulations, as these are the critical values commonly used in practice. However, they are essentially arbitrary, and our simulation results would look identical if we had chosen alternative tolerances for Type I vs. Type II errors. (The only difference

8. We allow for a sufficiently long "burn-in period" in this AR(1) process, so that the process starts to evolve many periods before the first period of simulated data.

9. Our code rounds PJ to the nearest integer value, even though PJ is already an integer in our main parameterization. Note that for the τ^{McK}, $\tau^{AR(1)}$, and τ^{SCR} to be precisely calibrated, the effective $\tilde{P}$ (where $\tilde{P} = \text{round}(PJ)/J$) needs to equal the actual parameter value P.

10. We set $m = r$ only for simplicity. However, the results are very similar if we fix $m = 3$ and vary $r \in \{1, \ldots, 20\}$, or vice versa. In Appendix C.1, we present results that vary m and r separately.

would be that the vertical axes would change to reflect these alternative values.) All other fixed parameter values are arbitrary. We have set $J = 500$, $P = 0.5$, $\beta = 1$, $\mu_\upsilon = 100$, $\sigma^2_\upsilon = 80$, $\mu_\delta = 20$, $\sigma^2_\delta = 10$, and $\sigma^2_\omega = 10$. These values of σ^2_υ, σ^2_δ, and σ^2_ω imply an intracluster correlation coefficient of $\rho_\upsilon = 0.8$ and within-period correlation coefficient of $\rho_\delta = 0.1$. Importantly, our simulation results do not depend on any particular combination of these parameters values, because they rely on the internal consistency of the *ex ante* treatment effect calibration and the *ex post* estimation, conditional on a *given* set of parameter values. The only exceptions are for J and P: J must be larger enough to allow us to use the CRVE estimator (i.e., at least 40 clusters), and P must be within a reasonable range (i.e., between 0.1 and 0.9) such that there are a sufficient number of both treated clusters and control clusters.

We report additional simulations for DD power calculations with AR(1) data, each of which slightly tweaks the above algorithm. Figure 2 uses $m \in \{1, 2, 3, 4, 5, 6\}$ and $r \in \{1, 2, 3, 4, 5, 6\}$, varying the number of pre-treatment and post-treatment period separately. Figure 3 alters Step 4 above, by either (i) replacing υ_i fixed effects with a Treat_i dummy, (ii) replacing δ_t fixed effects with a Post_t dummy, or (iii) both. Figure 5 combines this tweak to Step 4 with alternate parameterization of the DGP in Step 2: (i) $\sigma^2_\upsilon = 0$, (ii) $\sigma^2_\delta = 0$, or (iii) $\sigma^2_\upsilon = \sigma^2_\delta = 0$.

Importantly, removing unit fixed effects alters the asymptotic properties of the CRVE, as the number of clusters no longer increases one-for-one with the number of regressors. For all simulations in this paper and appendix, we apply the CRVE estimator that is appropriate to each estimating equation (following Cameron and Miller (2015), pp. 14–15): if the regression includes unit fixed effects and we are clustering by unit, we scale standard errors by $\sqrt{\frac{N-1}{N-J-1}}$, where N the number of observations in the regression and J is the number of units (clusters); if the regression does not include unit fixed effects, we do not apply this correction factor.

The ANCOVA simulations in Figure 4 follow the a very similar algorithm as those in Figure 1. Each of the above steps is identical, except for the following changes:

Step 1: Instead of using the DD power calculation formulas, we apply the analogous ANCOVA formulas to calculate τ^{McK} and τ^{SCR}.[11]

$$\tau^{McK} = (t^J_{1-k} + t^J_{\alpha/2}) \sqrt{\tfrac{1}{P(1-P)J}\left[(1-\breve{\theta})^2\sigma^2_\upsilon + \left(\tfrac{\breve{\theta}^2}{m} + \tfrac{1}{r}\right)\sigma^2_\omega\right]}$$

where $\breve{\theta} = \dfrac{m\sigma^2_\upsilon}{m\sigma^2_\upsilon + \sigma^2_\omega}$, and

$$\tau^{SCR} = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right)\left[\frac{1}{P(1-P)J} + \frac{(\overline{Y}^B_T - \overline{Y}^B_C)^2}{Z}\right]^{1/2} \times \left[(1-\theta)^2\sigma^2_\upsilon + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma^2_\omega + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X\right]^{1/2}$$

11. To create Figure 4, we use the exact SCR ANCOVA formula of Equation (A62) rather than the approximate formula of Equation (11).

where

$$\theta = \frac{m\sigma_\upsilon^2 + m\psi^X}{m\sigma_\upsilon^2 + \sigma_\omega^2 + (m-1)\psi^B}$$
$$Z = \sum_{i=1}^{PJ} (\overline{Y}_i^B - \overline{Y}_T^B)^2 + \sum_{i=PJ+1}^{J} (\overline{Y}_i^B - \overline{Y}_C^B)^2$$

Step 2: We set $\sigma_\delta^2 = 0$, consistent with the assumptions of our analytic ANCOVA derivations. This assumption would be unrealistic for real-world data, however this Monte Carlo exercise focuses on the internal (in)consistencies of the ANCOVA power calculation formulas.

Step 3: We also collapse all pre-treatment observations into unit-specific unweighted averages:

$$\overline{Y}_i^B \equiv \sum_{t=-m+1}^{0} Y_{it}$$

Step 4: We separately estimate the following ANCOVA regressions:

$$Y_{it}^{McK} = \beta + \tau^{McK} D_i + \theta \overline{Y}_i^B + \varepsilon_{it}$$
$$Y_{it}^{SCR} = \beta + \tau^{SCR} D_i + \theta \overline{Y}_i^B + \varepsilon_{it}$$
$$Y_{it}^{0} = \beta + \tau^{0} D_i + \theta \overline{Y}_i^B + \varepsilon_{it}$$

Note that we do not include time fixed effects δ_t, as we have assumed away time shocks for these simulations.

Step 5: We compute only CRVE standard errors, clustered at the unit level.

Figure 4 reports the resulting rejection rates of the ANCOVA estimator. We do not report the false rejection rates from the τ^0 regressions for the sake of brevity, and they do achieve the desired $\alpha = 0.05$ rejection rate in all cases.

An additional nuance with the ANCOVA simulations is that our simulation results are now sensitive to the intracluster correlation coefficient ρ_υ. This is because the proportion of variance that is unit-specific now affects the precision of the $\hat{\tau}$ estimator, because we have replaced the unit fixed effect (which directly controlled for this variance) with a linear control in the average pre-treatment level of the outcome variable. Figure 4 sets $\rho_\upsilon = 0.8$, and the results are similar for alternative values of ρ_υ.

B.2 Bloom et al. (2015) data

In Figure 6, we run Monte Carlo simulations using data from Bloom et al. (2015). These simulations are analogous to those described above, except that rather than simulating data, we use an actual dataset from a published panel RCT. We downloaded the paper's dataset from the *Quarterly Journal of Economics* website, and focused on the data used to estimate the paper's main results, reported in Column (1) of Table II of the paper. Consistent with the regression model that produced this

result, we base our Monte Carlo analysis on the following DD specification:

$$\text{Performance}_{it} = \alpha \text{Treat}_i \times \text{Experiment}_t + \beta_t + \gamma_i + \varepsilon_{it}$$

Converting the original paper's notation to our notation, and substituting the outcome variable and fixed effects with the names of the variables in the Bloom et al. (2015) dataset, we have:

$$\underbrace{\texttt{perform1}_{it}}_{Y_{it}} = \tau D_{it} + \underbrace{\texttt{year\_week}_t}_{\delta_t} + \underbrace{\texttt{personid}_i}_{\upsilon_i} + \omega_{it}$$

We keep only units in the main sample (i.e. $\texttt{expgroup} \in \{0, 1\}$), only pre-treatment weeks of data (i.e. $\texttt{year\_week} < 201049$), and only individuals with non-missing $\texttt{perform1}_{it}$ values for all weeks of pre-treatment data. This leaves us with a balanced panel of $J = 79$ individuals across 48 weeks. Table B1 provides summary statistics of the resulting dataset.

Table B1: Summary statistics – Bloom et al. (2015)

Mean	Std. Dev.	Min	Max	AR(1) $\hat{\gamma}$	Individuals	Periods	Observations
0.153	0.943	−2.766	3.665	0.233	79	48	3,792

Notes: This table shows summary statistics for worker productivity in the Bloom et al. (2015) data. The data are weekly job performance z-scores, constructed by taking the average of normalized performance measures, where each measure is standardized to have a mean of 0 and standard deviation of 1 across the sample. Our sample consists only of individuals that had no missing observations throughout the entire pre-treatment period, January 1, 2010 through November 28, 2010. We compute $\hat{\gamma}$ by estimating Equation (6) on residuals from this dataset. In doing so, we cluster standard errors at the individual level. The 95% confidence interval is [0.165, 0.300]. For more details on the standardized job performance measures and the actual experimental design, see Bloom et al. (2015).

We conduct simulations on this dataset by varying the number of pre-treatment periods (m) and the number of post-treatment periods (r). As with the simulations described above, we vary the panel length for values $m = r \in \{1, \ldots, 20\}$, iterating 10,000 simulations for each value of $m = r$. We set the parameter values $\sigma^2_\omega = 0.507$ and $\gamma = 0.233$ by regressing Y_{it} on person and week fixed effects, calculating the variance of the resulting residuals $\hat{\omega}_{it}$, and then estimating $\hat{\gamma}$ using

$$\hat{\omega}_{it} = \gamma \hat{\omega}_{i(t-1)} + \xi_{it}$$

Step 1: We calculate τ^{McK} and $\tau^{AR(1)}$ for each set of simulations, given $m = r$:

$$\tau^{McK} = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \sqrt{\left(\frac{\sigma^2_\omega}{P(1-P)J}\right)\left(\frac{m+r}{mr}\right)}$$

$$\tau^{AR(1)} = (t^J_{1-\kappa} + t^J_{\alpha/2}) \sqrt{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma^2_\omega + \left(\frac{m-1}{m}\right)\breve{\psi}^B + \left(\frac{r-1}{r}\right)\breve{\psi}^A - 2\breve{\psi}^X\right]}$$

where

$$\breve{\psi}^B = \frac{2\sigma^2_\omega}{(m-1)m} \sum_{z=1}^{m-1} (m - z)\gamma^z$$

$$\breve{\psi}^A = \breve{\psi}^B$$
$$\breve{\psi}^X = \frac{\sigma_\omega^2}{m^2}\left[\sum_{z=1}^{m} z\gamma^z + \sum_{z=m+1}^{2m-1}(2m-z)\gamma^z\right]$$

We denote the covariance terms as $\breve{\psi}^B$, $\breve{\psi}^A$, and $\breve{\psi}^X$ to indicate that the AR(1) error assumption is a (poor) representation of the more complex covariance structure of this dataset. For both τ^{McK} and $\tau^{AR(1)}$, we calculate the critical values $t^J_{1-\kappa}$ and $t^J_{\alpha/2}$ assuming J degrees of freedom, which is consistent with applying the CRVE *ex post*, clustering at the individual level with J individuals.

Step 2: We calculate τ^{SCR} given $m = r$, by non-parametrically estimating $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ from residuals. Appendix D.1 provides step-by-step details of this estimation algorithm. Rather than impose an AR(1) structure on the serial correlation, this method enables us to flexibly characterize the covariance structure of the Bloom et al. (2015) dataset with just three averaged parameters. This allows us to calculate τ^{SCR} as:

$$\tau^{SCR} = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right)\sqrt{\left(\tfrac{1}{P(1-P)J}\right)\left[\left(\tfrac{m+r}{mr}\right)k_\sigma\sigma^2_{\hat{\omega}} + \left(\tfrac{m-1}{m}\right)k_B\psi^B_{\hat{\omega}} + \left(\tfrac{r-1}{r}\right)k_A\psi^A_{\hat{\omega}} - 2k_X\psi^X_{\hat{\omega}}\right]}$$

where

$$k_\sigma = \frac{I(m+r)^2}{2(I-1)mr}$$
$$k_B = \frac{I(m+r)^2}{2(I-1)r^2}$$
$$k_A = \frac{I(m+r)^2}{2(I-1)m^2}$$
$$k_X = 0$$

Appendix E provides a derivation of the coefficients k_σ, k_B, k_A, and k_X, and it proves that this expression for τ^{SCR} as a function of *estimated* variance-covariance parameters is equal (in expectation) to the MDE as a function of the *true* variance-covariance parameters.[12]

Step 3: For each simulation, we randomly select a range of $m+r$ consecutive weeks in the dataset. This subset of weeks will become the $(m+r)$-period panel dataset used in this particular simulation. We randomly assign treatment to PJ individuals. This involves randomly scrambling a vector of PJ ones and $(1-P)J$ zeros and assigning each individual i either a 1 indicating treatment or a 0 indicating control.[13] This allows us to construct a time-varying treatment indicator D_{it}, where $D_{it} = 1$ for all treated units in post-treatment periods only and $D_{it} = 0$ otherwise. We then create three outcome variables by adding treatment effects to the data generated in the previous step:

$$Y^{McK}_{it} \equiv Y_{it} + \tau^{McK}D_{it}$$

12. I denotes the number of units used to estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$ and $\psi^X_{\hat{\omega}}$. This is distinct from the sample size of the experiment J, however these simulations set $I = J = 79$ to include all units in the Bloom et al. (2015) dataset.

13. Our code rounds PJ to the nearest integer value. Note that for the τ^{McK}, $\tau^{AR(1)}$, and τ^{SCR} to be precisely calibrated, the effective $\tilde{P}$ (where $\tilde{P} = \text{round}(PJ)/J$) needs to equal the actual parameter value P.

$$Y_{it}^{AR(1)} \equiv Y_{it} + \tau^{AR(1)} D_{it}$$
$$Y_{it}^{SCR} \equiv Y_{it} + \tau^{SCR} D_{it}$$

Step 4: We separately estimate the following three OLS-fixed effects regressions:

$$Y_{it}^{McK} = \beta + \tau^{McK} D_{it} + \upsilon_i + \delta_t + \omega_{it}$$
$$Y_{it}^{AR(1)} = \beta + \tau^{AR(1)} D_{it} + \upsilon_i + \delta_t + \omega_{it}$$
$$Y_{it}^{SCR} = \beta + \tau^{SCR} D_{it} + \upsilon_i + \delta_t + \omega_{it}$$

Step 5: For each estimated $\hat{\tau}^{McK}$, $\hat{\tau}^{AR(1)}$, and $\hat{\tau}^{SCR}$, we compute CRVE standard errors, clustered at the individual level.

As with the AR(1) simulations above, we repeat Steps 3–5 10,000 times, for values of $m = r \in \{1, \ldots, 12\}$.[14] After each set of 10,000 simulations, we calculate the percent of simulations where $\hat{\tau}^{McK}$, $\hat{\tau}^{AR(1)}$, and $\hat{\tau}^{SCR}$ reject the null hypothesis of $\tau = 0$ at significance level α, under CRVE standard errors. Figure 6 reports these three rejection rates on the vertical axes, with the number of pre- and post-treatment periods ($m = r$) on the horizontal axes. We can interpret these curves as realized statistical power, just as in the top row of Figure 1. We fix $\alpha = 0.05$ and $\kappa = 0.80$ across all simulations, for the reasons discussed above. Besides our arbitrary choices of $P = 0.5$, all other parameters are determined by the Bloom et al. (2015) dataset: $J = I = 79$, $\sigma_\upsilon^2 = 0.243$, $\sigma_\delta^2 = 0.146$, and $\sigma_\omega^2 = 0.507$, implying $\rho_\upsilon = 0.271$ and $\rho_\delta = 0.163$. We do not estimate β, μ_υ, or μ_δ, as these parameters are no longer relevant when simulating on top of an existing dataset.

B.3 Pecan Street data

In Figure 8, we present analogous Monte Carlo results for simulations using the Pecan Street dataset of household electricity consumption (Pecan Street (2016)). These data are publicly available (with a researcher login) at `https://dataport.pecanstreet.org/data/interactive`, and they include 699 households over 26,888 hours. As with the Bloom et al. (2015) simulations, we construct a balanced panel of households and hours, by restricting the full Pecan Street dataset to a sample of households that report non-missing, non-zero electricity consumption for every hour between January 1, 2013 and December 31, 2014. This results in a balanced panel of $J = 97$ households over 17,520 hours, which we collapse to create daily, weekly, and monthly datasets. Table B2 presents basic summary statistics for all four datasets, and Figure B1 displays the time series of data for one randomly selected household in our sample at varying levels of aggregation. Unsurprisingly, while mean electricity use is consistent across different collapses of the data, the standard deviation decreases as we move from higher- to lower-frequency datasets.

These Pecan Street simulations follow an algorithm identical to the Bloom et al. (2015) simulations, and we describe this algorithm in detail above. We repeat the same set of simulations four times, estimating separate rejection rates for τ^{McK}, $\tau^{AR(1)}$, and τ^{SCR}, for each of the hourly, daily, weekly, and monthly datasets. We again set $\alpha = 0.05$, $\kappa = 0.80$, and $P = 0.5$. The other relevant parameters for each dataset are:

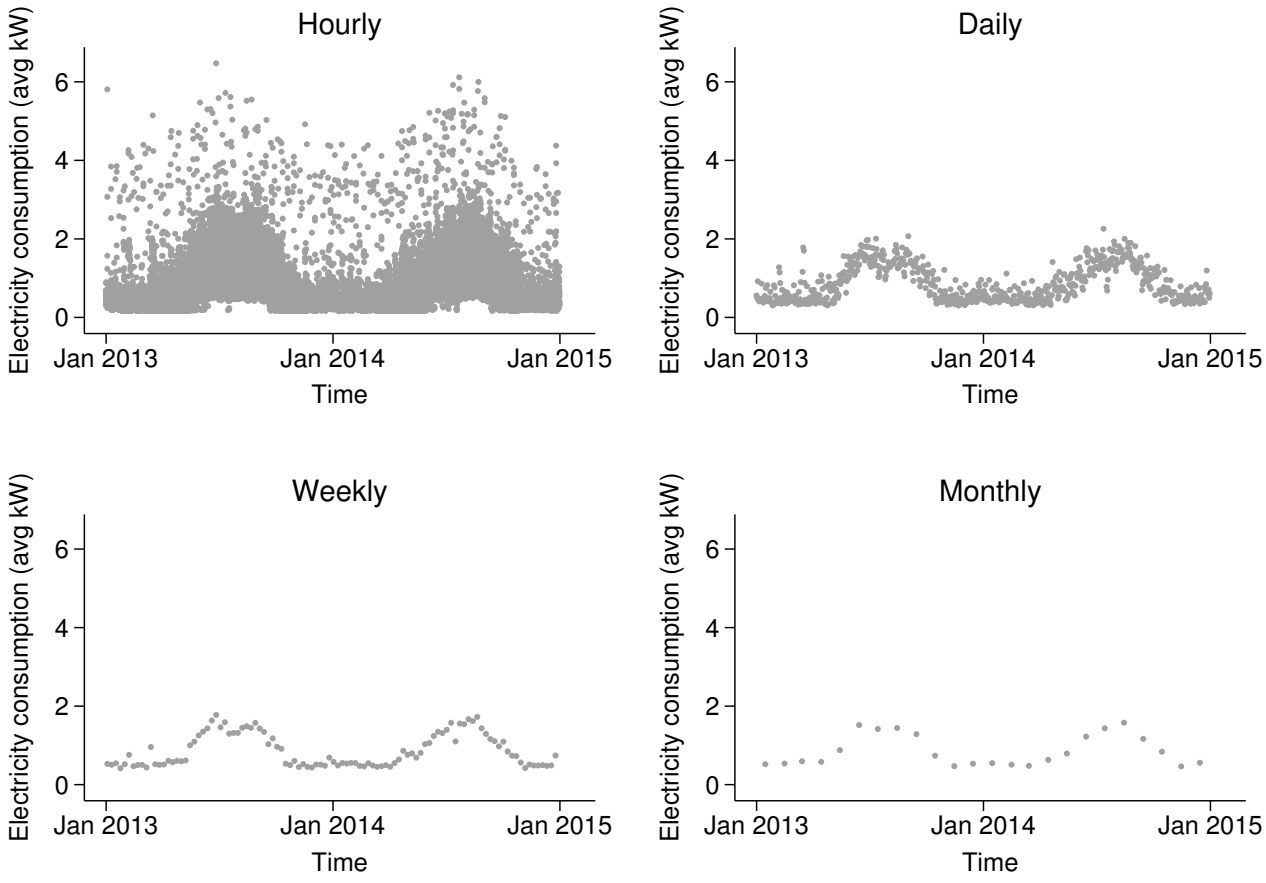
14. As with the AR(1) simulations, we set $m = r$ only for simplicity. However, the results are very similar if we fix $m = 3$ and vary $r \in \{1, \ldots, 20\}$, or vice versa.

Table B2: Summary statistics – Pecan Street

Dataset	Mean	Std. Dev.	Min	Max	AR(1) $\hat{\gamma}$	Households	Periods	Observations
Hourly	1.200	1.164	0.019	13.501	0.628	97	17,520	1,699,440
Daily	1.200	0.789	0.082	6.013	0.651	97	730	70,810
Weekly	1.198	0.739	0.105	5.175	0.713	97	106	10,282
Monthly	1.197	0.712	0.169	4.296	0.654	97	24	2,328

Notes: This table shows summary statistics for electricity consumption in the Pecan Street data. All values are in average kW of electricity consumed. The raw data are at the hourly level, in kWh. To construct the daily, weekly, and monthly dataset, we average hourly kWh consumption data across the relevant time period. We compute $\hat{\gamma}$ by estimating Equation (6) on residuals from this dataset. In doing so, we cluster standard errors at the household level. All are statistically significant at less than the 1% level.

Figure B1: Pecan Street data – Varying levels of aggregation



Notes: This figure shows the time series of Pecan Street electricity consumption data for one randomly selected household in our sample. Each panel displays the data at a different level of aggregation. The data are in units of average kW. These data are highly serially correlated: when we estimate an AR(1) model, Equation (6), on residuals from these datasets, we recover the AR(1) parameters of 0.628, 0.651, 0.713, and 0.654 for the hourly, daily, weekly, and monthly data, respectively.

Table B3: Pecan Street Simulation Parameters

DATASET	J	σ_υ^2	σ_δ^2	σ_ω^2	γ	ρ_υ	ρ_δ
Hourly	97	0.257	0.458	0.642	0.623	0.189	0.337
Daily	97	0.257	0.234	0.135	0.651	0.411	0.373
Weekly	97	0.256	0.211	0.083	0.713	0.465	0.384
Monthly	97	0.256	0.203	0.058	0.654	0.495	0.392

These values are estimated separately from each dataset used in the simulations.

In Appendix C.1, we present simulations analogous to Figures 2 and 3 in the main text, but using the Bloom et al. (2015) and Pecan Street datasets. The former vary m and r separately; the latter replace fixed effects with dummies in the DD estimating equations in Step 4 of Appendix B.2.

B.4 Analytic power calculations

Figure 9 displays the results of analytic power calculations performed using the daily Pecan Street dataset. In other words, we calculate the number of units needed by applying the McKenzie and SCR power calculation formulas, using the data to estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. For each experiment of length $m = r \in \{1, \ldots, 12\}$, we estimate the average values of $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ over all possible panels of that length.[15] We assign half of the households to treatment ($P = 0.5$), allow for a 5 percent Type I error rate ($\alpha = 0.05$), and calibrate to 80 percent power ($\kappa = 0.80$). Finally, we rearrange Equations (3) and (D2) to calculate the number of households required to detect MDEs that range from 0 to 15 percent of baseline electricity consumption.

Figure 10 also shows the results of analytic power calculations using the SCR formula. However, instead of parameterizing Equation (2) using estimates from a dataset, we now normalize $\sigma^2_{\omega} = 1$ and assume an AR(1) correlation structure with $\gamma \in \{0, 0.3, 0.5, 0.7, 0.9\}$. For panel lengths $m = r \in \{1, \ldots, 100\}$, we analytically derive ψ^B, ψ^A, and ψ^X using the formulas from Equations (B1)–(B3). In the left panel, we fix $P = 0.5$, $\alpha = 0.05$, $\kappa = 0.80$, and $J = 100$, and use Equation (2) to solve for MDE as a function of $m = r$ and γ. In the right panel, we fix $P = 0.5$, $\alpha = 0.05$, $\kappa = 0.80$, and $MDE = 1$, and rearrange Equation (2) to solve for J as a function of $m = r$ and γ.

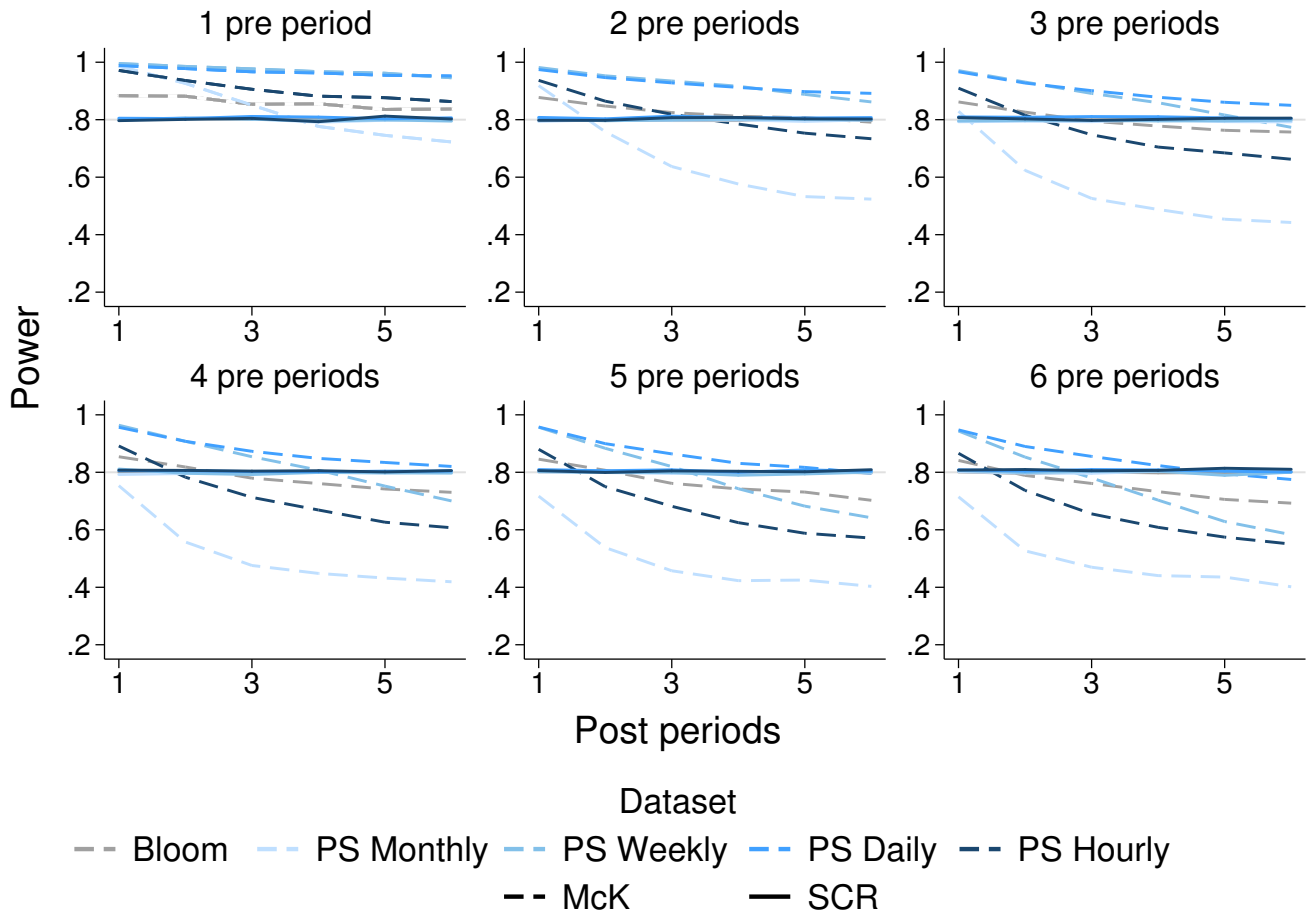
15. We follow the algorithm outlined in Appendix D.1 below. For a given value of $m = r$, we consider each (consecutive) subset S of the daily Pecan Street data with length $2r$. We first residualize this subset of the data with household and day fixed effects, and we calculate $\sigma^2_{\hat{\omega},S}$ from these residuals. We then assign the first m residuals for each household to the pre-treatment period and the remaining r residuals to the post-treatment period, thereby estimating $\psi^B_{\hat{\omega},S}$, $\psi^A_{\hat{\omega},S}$, and $\psi^X_{\hat{\omega},S}$ (by averaging all pairwise covariances for subset S). Averaging $\sigma^2_{\hat{\omega},S}$, $\psi^B_{\hat{\omega},S}$, $\psi^A_{\hat{\omega},S}$, and $\psi^X_{\hat{\omega},S}$ over all subsets S, we arrive at estimates for $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$.

C Additional results

C.1 Sensitivities with real data

In this section, we present extensions of our simulation results from the main text. In Figure C1, we present an analogous version of Figure 2 using real data from Bloom et al. (2015) and Pecan Street. Each panel conducts simulations that are identical to Figures 6 and 8, varying m and r separately (for $m \in \{1, \ldots, 6\}$ and $r \in \{1, \ldots, 6\}$). As with the simulated data, we find that the McKenzie formula typically yields over-powered experiments with either one pre-treatment or one post-treatment period, while the SCR formula yields the desired 80 percent power in all cases.

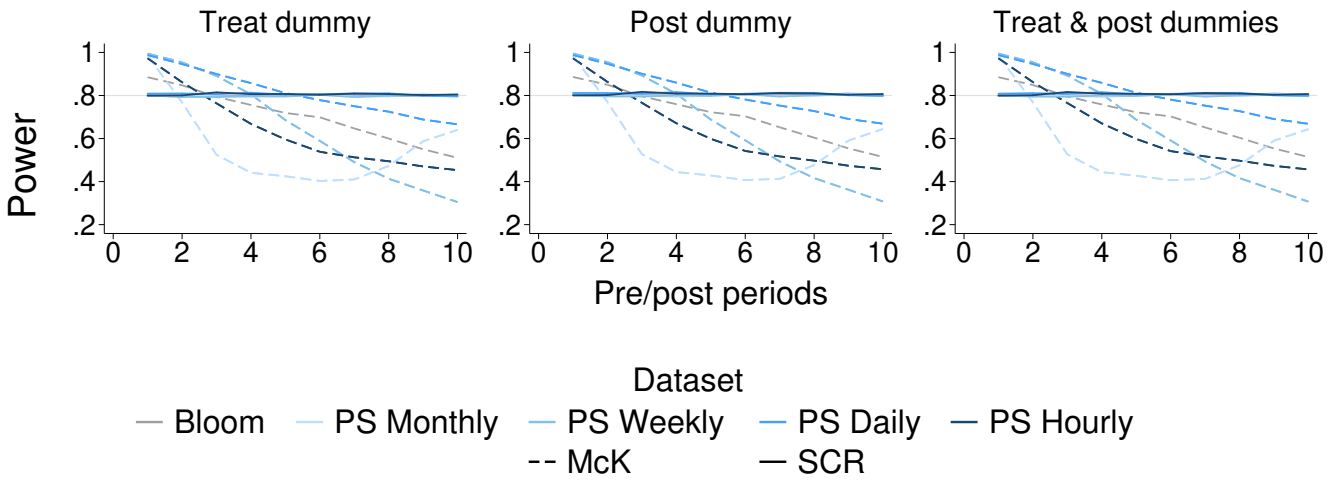
Figure C1: Power in short panels – Real data



Notes: This figure combines Figures 6 and 8 from the main text with the short panels of Figure 2. Simulations using the Bloom et al. (2015) and Pecan Street datasets follow the same algorithm describe in Appendix B.2, but separately varying the number of pre-treatment and post-treatment periods (for $m \in \{1, \ldots, 6\}$ and $r \in \{1, \ldots, 6\}$). For nearly all cases with either one pre-treatment period or one post-treatment period, the McKenzie formula yields over-powered experiments. Hence, experiments that follow the traditional "one baseline, one follow-up" structure will likely be overpowered, having calibrated an excessively large sample size. As the number of pre-treatment periods increases, power decreases monotonically for each dataset. By contrast, the SCR formula is properly powered in all cases.

Next, Figure C2 presents an analogous version of Figure 3 from the main text, using Bloom et al. (2015) and Pecan Street datasets. In the left panel, we replace unit fixed effects in the DD estimating equation with a Treat_i dummy. In the middle panel, we replace time fixed effects in the DD estimating equation with a Post_t dummy. In the right panel, we replace both unit and time fixed effects with $\text{Treat}_i + \text{Post}_t$ dummies. These resulting rejection rates are virtually identical to Figures 6 and 8, demonstrating that these alternative DD estimating equations all yield identical *ex post* power in real data (just as they do with simulated data in Figure 3).

Figure C2: Sensitivities to estimating equation – Real data



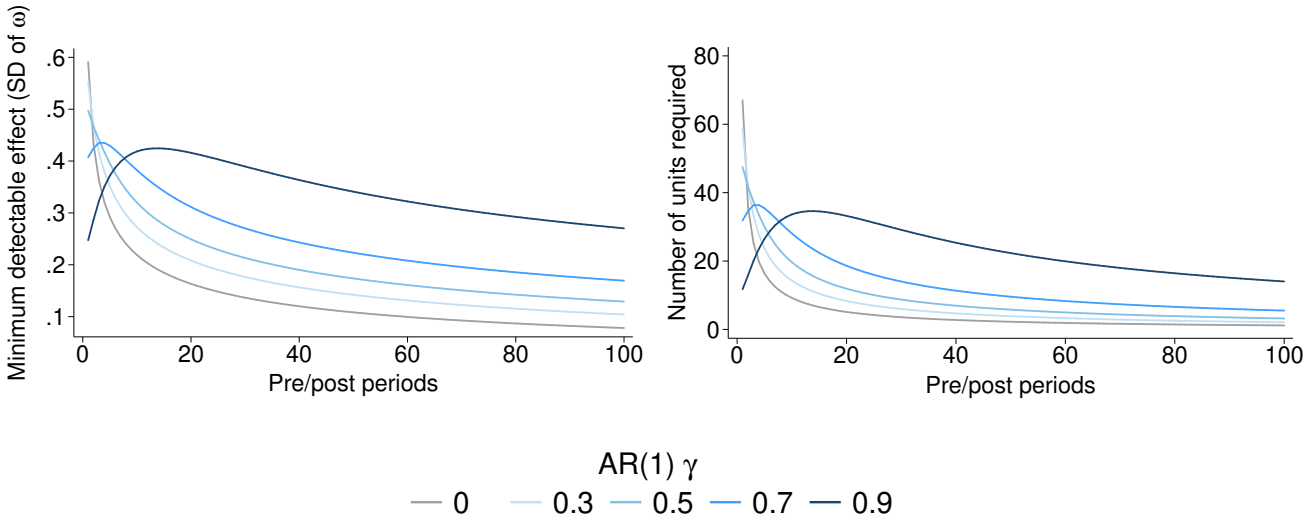
Notes: This figure replicates Figure 3 from the main text, using the Bloom et al. (2015) and Pecan Street datasets. The left panel replaces unit fixed effects in the DD estimating equation with a Treat_i dummy. The middle panel replaces time fixed effects in the DD estimating equation with a Post_t dummy. The right replaces both unit and time fixed effects with $\text{Treat}_i + \text{Post}_t$ dummies. As with simulated data, DD power calculations using real data are incorrectly powered using the McKenzie formula but correctly powered using our SCR formula.

C.2 Tradeoffs between MDE and time periods: ANCOVA

Figure 10 demonstrates that for DD experiments with strong serial correlation, it is possible to increase the MDE (or necessary sample size) by adding pre/post-treatment periods. This follows from Equation (4), which shows that serial correlation can either increase or decrease the variance of the DD estimator. Because DD identifies the treatment effect using differences between pre- and post-treatment outcomes, stronger serial correlation makes differences caused by treatment easier to detect. At the same time, stronger serial correlation means each additional time period provides less information. This intuition also holds for the ANCOVA estimator, as shown by Figure C3. This replicates Figure 10 using the SCR ANCOVA power calculation formula (Equation (11)). As

with DD, we find that adding pre/post periods to an ANCOVA experiment can decrease power, for short panels with strong serial correlation.[16]

Figure C3: Analytical power calculations with increasing panel length – ANCOVA



Notes: This figure replicates Figure 10 from the main text, using the SCR ANCOVA formula (Equation (11)) instead of the SCR DD formula (Equation (2)). The left panel shows the tradeoff between the minimum detectable effect (MDE) and the number of time periods ($m = r$) for varying levels of AR(1) serial correlation, holding the number of units fixed at $J = 100$ and normalizing MDE by the standard deviation of ω_{it}. At low levels of γ, MDE declines monotonically in m and r. However, for higher γ, increasing m and r actually *increases* MDE when $m = r$ is relatively small, and *decreases* MDE when $m = r$ is relatively large. The right panel shows the relationship between the number of units (J) and number of pre/post periods ($m = r$) required to detect an MDE equal to one standard deviation of ω_{it}. Similarly, for low levels of serial correlation, the trade-off between J and $m = r$ is monotonic. However, as γ increases, adding periods in short panels necessitates a greater number of units to achieve the same MDE, while adding periods in longer panels means that fewer units are required to achieve the same MDE.

C.3 Cluster randomization in panel RCTs

Finally, we use simulation-based power calculations to compare panel RCTs with unit-level randomization and cluster-level randomization. While cluster randomization remains outside the scope of this paper's analytical framework, it is a common approach for RCTs in development economics. Randomizing at the cluster (e.g. village) level, rather than the unit (e.g. household) level can be less expensive, simplify the logistics of administering treatment, and eliminate concerns about treatment interference (e.g. spillovers from treated to control households within a village). Here, we investigate how cluster-level randomization affects statistical power (relative to unit-level randomization), using the program `pc_simulate` from our STATA package `pcpanel`.[17]

We compare unit-level vs. cluster-level randomization, using simulated panel datasets based on the following DGP:

16. The SCR ANCOVA formula has an additional degree of freedom: we can vary σ_v^2 and σ_ω^2 separately. Here, we set a relatively small σ_v^2 (equal to $0.1\sigma_\omega^2$), which causes ANCOVA to be relatively dissimilar to DD (the two estimators converge as σ_v^2 approaches infinity).

17. We do not incorporate treatment spillovers, since doing so in a simulation context would necessitate (essentially) arbitrary assumptions on the strength and direction of treatment spillovers. Instead, we simulate RCTs with a 100 percent treatment intensity within treated clusters.

Assumption C1 (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{igt} = \beta + \tau D_{igt} + \zeta_g + \upsilon_i + \delta_t + \omega_{igt} \tag{C1}$$

where Y_{igt} is the outcome of interest for unit i in group g at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{igt} is a time-varying treatment indicator; ζ_g is a group-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\zeta)$; υ_i is a unit-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\upsilon)$; δ_t is a time-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\delta)$; and ω_{igt} is an idiosyncratic error term with variance σ^2_ω.

We simulate 9 datasets, each containing 500 groups g, where each group is defined by a separate realization of ζ_g. We vary the number of units i nested within each group g, such that all groups within a given dataset comprise either 10, 25, or 50 units, but hold the total number of units fixed. We also vary the relative strength of group-specific shocks while holding constant total cross-sectional variation—that is, we vary $\sigma^2_\zeta/(\sigma^2_\zeta + \sigma^2_\upsilon)$ such that groups contribute 25, 50, of 75 percent of the total cross-sectional variation, while holding $\sigma^2_\zeta + \sigma^2_\upsilon$ fixed. For all datasets, we incorporate non-constant serial correlation where ω_{igt} follows an AR(1) process with $\gamma = 0.5$.[18]

Using `pc_simulate`, we conduct 18 sets of simulation-based power calculations, for varying panel lengths ($m = r \in \{1, 2, 3, 4, 5, 6\}$). While all 18 sets of power calculations hold the sample size fixed at 2000 units i, 9 assign treatment at the unit level (ignoring cross-sectional group correlations) and 9 assign treatment at the group level (with 100 percent treatment intensity within treated groups). Hence, for group sizes of 10, 25, and 50, this means that cluster randomization selects sample sizes of 200, 80, and 40 groups, respectively. We also hold the proportion of units in treatment and minimum detectable effect constant across all simulations ($P = 0.5$, $MDE = 1.4$). All simulations cluster standard errors at the level of treatment (either unit or group), and calculate average rejection rates over 10,000 iterations to compute power.[19]

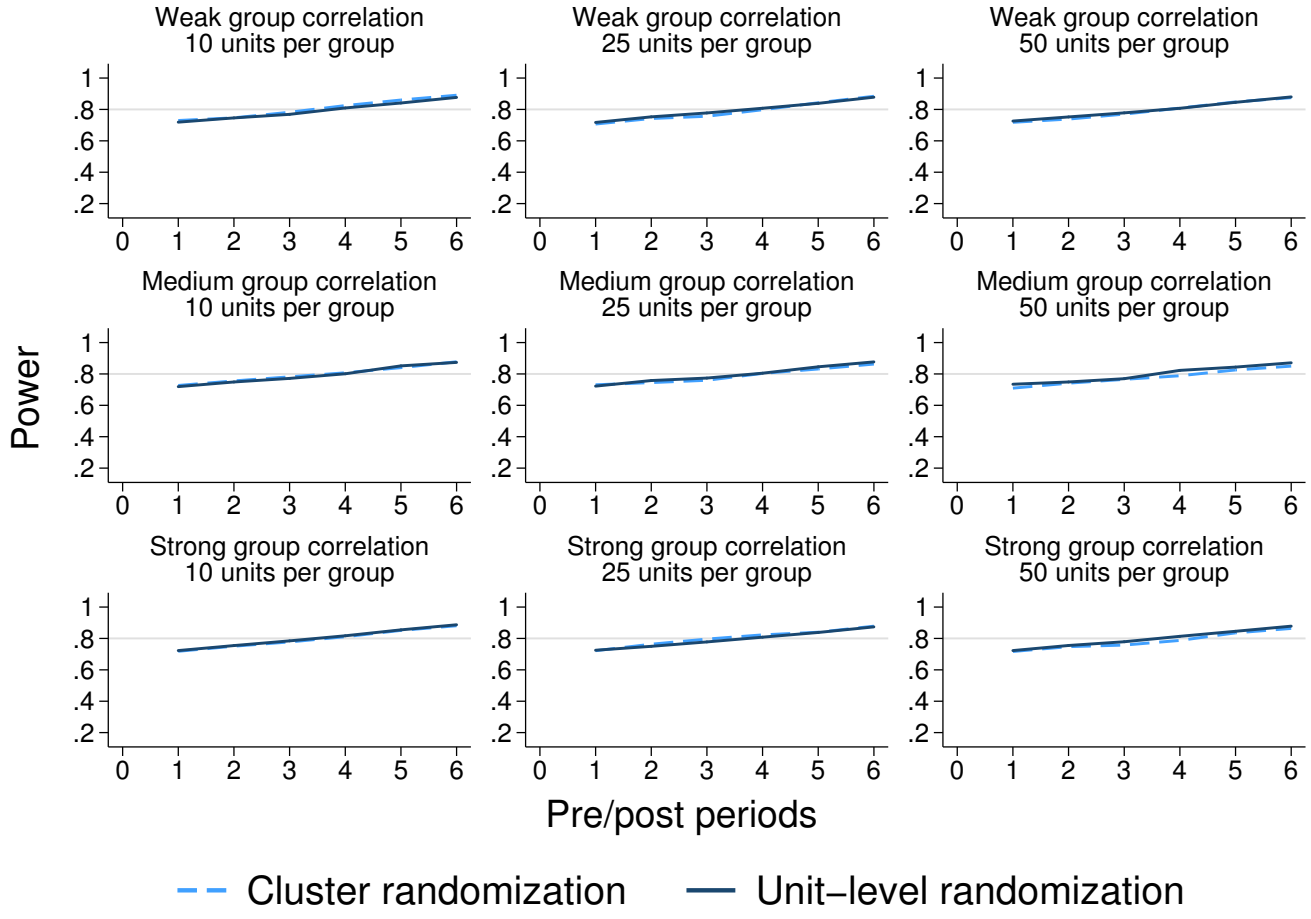
Figure C4 presents the results of this exercise. Each plot reports an "apples-to-apples" comparison for power calculations on the same dataset, randomized at the unit vs. group level. Moving left to right, the number of units per group increases from 10 to 25 to 50. Moving top to bottom, the relative strength of group-specific shocks increases from 25 percent ("weak") to 50 percent ("medium") to 75 percent ("strong"). For all 9 datasets, randomizing at the group level yields statistical power that is indistinguishable from the same experiment randomized at the unit level. The intuition behind this equivalence lies in how panel estimators control for unit-specific baselines: DD identifies off of (post − pre) differences at the *unit* level, which controls for the remaining within-group variation not exploited when randomizing at the group level. By contrast, when we repeat the same exercise for cross-sectional RCTs, we finds substantially lower power for group-level randomization (relative to unit-level randomization). This is because the cross-sectional treatment effect estimator does not control for within-group variation, meaning that the treatment indicator leaves more unexplained variation when randomized at the group level. Because these results are on simulations in fake data with a relatively simple DGP, we encourage researchers interested in performing cluster randomization to do so using simulation-based power calculations, in case these

18. We set the following specific parameters: $\sigma^2_\zeta \in \{20, 40, 60\}$, $\sigma^2_\upsilon \in \{60, 40, 20\}$, $\sigma^2_\zeta + \sigma^2_\upsilon = 80$, $\sigma^2_\delta = 10$, $\sigma^2_\omega = 150$. All parameter values are arbitrary, besides imposing constant $\sigma^2_\zeta + \sigma^2_\upsilon$ (which facilitates comparisons across datasets).

19. We use a large sample size of 2000 units to ensure that for datasets with 50-unit groups, we still have a sufficient number of clusters (40) to apply the CRVE.

results do not fully translate to real-world datasets with more complex DGPs.

Figure C4: Power calculations by simulation - Cluster randomization



Notes: This figure reports results from simulation-based power calculations, comparing unit-level randomization vs. cluster randomization. Each panel reports power for DD experiments using a single simulated dataset, varying the number of pre-treatment and post-treatment periods ($m = r = \{1, 2, 3, 4, 5, 6\}$). Datasets have either 10, 25, or 50 units within each group (moving left to right); and either weak, medium, or strong within-group correlations (either 25, 50, or 75 percent of total cross-sectional variation, respectively). All experiments include 2000 units, treating either half of units or half of groups. When cluster randomizing, all treated groups have a treatment intensity of 100 percent. All simulations assume an MDE of 1.4, and calculate average rejection rates over 10,000 iterations. See text for further details.

D A practical guide to power calculations

In this section, we address several practical considerations when conducting power calculations. Most of these challenges involve variance and covariance parameters that must be either estimated or assumed in order to operationalize a power calculation formula. We also outline steps for estimating power calculations via simulation, which is our preferred method.

D.1 Analytical power calculations

The most challenging aspect of analytical power calculations is parameterizing the variance and (if applicable) covariance terms that characterize the data's error structure. In the absence of a representative pre-existing dataset, researchers may struggle to even guess the order of magnitude of the error variance, let alone generate a precise estimate of this key parameter. Our theoretical results demonstrate that power for panel RCTs also hinges on the error *covariance* structure.

As in the paper, we denote the true parameters governing the data generating process as σ^2_ω, ψ^B, ψ^A, and ψ^X. We define $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ to be the parameters that characterize the *residuals* (rather than real errors). If researchers have access to a representative dataset *ex ante*, they can directly estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, and use these values to parameterize power calculations. Appendix E.2 proves that researchers can recover the true MDE using these residual-based parameters. Nevertheless, this process is not trivial, for several reasons.

First, while the idiosyncratic variance σ^2_ω is a population parameter, the three ψ parameters are functions of both the full covariance structure of the population and the specific values of m and r.[20] For a given population and serially correlated outcome variable, experiments with small m and r are likely to exhibit larger ψ^B, ψ^A, and ψ^X parameters than experiments with large m and r. This is because as the number of pre-treatment (post-treatment) periods increases, ψ^B (ψ^A) averages across covariances of time periods that are farther apart. For example, compare ψ^B with $m = 3$ vs. $m = 30$, for an outcome with a covariance structure where adjacent periods are more positively correlated than distant periods. For $m = 3$, ψ^B averages $m(m-1)/2 = 3$ pairwise covariances, 2 of which are for adjacent periods; for $m = 30$, ψ^B averages $m(m-1)/2 = 435$ pairwise covariances, only 29 of which are for adjacent periods. Because ψ^X expands with both m and r, it attenuates relatively faster than ψ^B and ψ^A as panel length grows.

Second, estimating $\text{Cov}(\hat{\omega}_{it}, \hat{\omega}_{is})$ using residuals from an existing dataset is fundamentally impossible, given that each dataset contains only one realization of (Y_{it}, Y_{is}). However, researchers may treat the $(I \times 1)$ vectors of residuals $(\hat{\boldsymbol{\omega}}_t, \hat{\boldsymbol{\omega}}_s)$ as I draws from the distributions of residuals for periods (t,s) and estimate these distributions' covariance. The resulting estimates, which we denote $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$, are unbiased estimators of $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$.[21]

Third, if the representative dataset contains a long time series, the residual variance and covariance structure may change throughout the time series. This means if researchers estimate $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ by averaging across the full time series, these estimated parameters may be less representative than if they were estimated from just the end of the time series.[22] Since the residual

20. Deriving the residual-based parameters $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ introduces an additional complexity, as these residual-based parameters are defined by the number of pre-treatment periods (m), post-treatment periods (r) and cross-sectional units (I) used to produce these residuals.

21. Appendix E.1 proves that $\text{E}[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}] = \sigma^2_{\hat{\omega}}$, $\text{E}[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}] = \psi^B_{\hat{\omega}}$, $\text{E}[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}] = \psi^A_{\hat{\omega}}$, and $\text{E}[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}] = \psi^X_{\hat{\omega}}$.

22. Of course, if the researcher expects a certain subset of her data is likely more representative, it would be wise to perform power calculations on this subset alone.

variance is not a function of panel length, it may be tempting to estimate $\tilde{\sigma}^2_{\hat{\omega}}$ using a long vector of residuals, while estimating $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ using only residuals within an $(m+r)$-period range. However, in a time series where the variance-covariance structure is changing, this would produce $\tilde{\psi}_{\hat{\omega}}$ estimates that are inconsistent with $\tilde{\sigma}^2_{\hat{\omega}}$.

Fourth, while $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ are unbiased estimators of $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, they are **not** unbiased estimators of σ^2_{ω}, ψ^B, ψ^A, and ψ^X. This is because the residuals from the regression $Y_{it} = \upsilon_i + \delta_t + \omega_{it}$ will have a variance less than the parameter σ^2_{ω} from the data generating process, by the properties of linear projection. In addition, when they are estimated using residuals from shorter panels, $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ have a more severe bias, but these estimates converge to their true values (i.e., σ^2_{ω}, ψ^B, ψ^A, and ψ^X) as the panel length used to estimate these residuals increases.[23] Importantly, for the purposes of power calculations using the SCR formula, we *can* recover an unbiased estimate of the minimum detectable effect with the true parameters using our parameter estimates. That is, $MDE^{est}(\sigma^2_{\hat{\omega}}, \psi^B_{\hat{\omega}}, \psi^A_{\hat{\omega}}, \psi^X_{\hat{\omega}}) = MDE(\sigma^2_{\omega}, \psi^B, \psi^A, \psi^X)$. As Appendix E.1 shows, $\mathrm{E}[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}] = \sigma^2_{\hat{\omega}}$, $\mathrm{E}[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}] = \psi^B_{\hat{\omega}}$, $\mathrm{E}[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}] = \psi^A_{\hat{\omega}}$, and $\mathrm{E}[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}] = \psi^X_{\hat{\omega}}$. Combining these two proofs suggests that $MDE^{est}(\mathrm{E}[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}], \mathrm{E}[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}], \mathrm{E}[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}], \mathrm{E}[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}]) = MDE(\sigma^2_{\omega}, \psi^B, \psi^A, \psi^X)$. Therefore, for values of $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ estimated from a pre-existing dataset with I cross-sectional units:

$$\begin{aligned} MDE^{est} &= \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \left\{ \left(\frac{1}{P(1-P)J} \right) \left[\left(\frac{m+r}{mr} \right) k_\sigma \, \mathrm{E}\left[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}\right] + \left(\frac{m-1}{m} \right) k_B \, \mathrm{E}\left[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}\right] \right. \right. \\ &\qquad \left. \left. + \left(\frac{r-1}{r} \right) k_A \, \mathrm{E}\left[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}\right] - 2 k_X \, \mathrm{E}\left[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}\right] \right] \right\}^{1/2} \qquad \text{(D1)} \\ &= \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \left\{ \left(\frac{1}{P(1-P)J} \right) \left[\left(\frac{m+r}{mr} \right) k_\sigma \sigma^2_{\hat{\omega}} + \left(\frac{m-1}{m} \right) k_B \psi^B_{\hat{\omega}} \right. \right. \\ &\qquad \left. \left. + \left(\frac{r-1}{r} \right) k_A \psi^A_{\hat{\omega}} - 2 k_X \psi^X_{\hat{\omega}} \right] \right\}^{1/2} \qquad \text{(D2)} \end{aligned}$$

where

$$\begin{aligned} k_\sigma &= \frac{I(m+r)^2}{2(I-1)mr} \\ k_B &= \frac{I(m+r)^2}{2(I-1)r^2} \\ k_A &= \frac{I(m+r)^2}{2(I-1)m^2} \\ k_X &= 0 \end{aligned}$$

and the expectation of parameters are taken over subsets of the dataset, as described in the next point. Appendix E.2 proves that Equations (D1) and (D2) are equivalent, and derives the above expressions for the coefficients k_σ, k_B, k_A, and k_X.

23. The estimated residuals include both the true idiosyncratic error, ω_{it}, and (attenuating) fixed-effect estimation error. Although both sets of fixed effects, υ_i and δ_t, are unbiased and consistent in T and I, respectively, error in estimating these parameters will always yield residuals that are smaller on average, biasing the estimation of these parameters. The estimation error and resulting biases decrease in T and I.

Fifth, because the estimated variance-covariance terms enter the power calculation under a radical, researchers must be conscious of Jensen's Inequality. If the researcher is estimating $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ by taking the expectation of $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ across a range of $(m+r)$-period subsets, then the correct calculation is:

$$MDE^{est}\left(\mathrm{E}\left[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}\right], \mathrm{E}\left[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}\right], \mathrm{E}\left[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}\right], \mathrm{E}\left[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}\right]\right), \text{ not } \mathrm{E}\left[MDE^{est}\left(\tilde{\sigma}^2_{\hat{\omega}}, \tilde{\psi}^B_{\hat{\omega}}, \tilde{\psi}^A_{\hat{\omega}}, \tilde{\psi}^X_{\hat{\omega}}\right) \mid \mathbf{X}\right].$$

Similarly, if Equation (2) is rearranged as a function of κ, it becomes convex in the variance-covariance parameters, and the correct calculation is:

$$\kappa\left(\mathrm{E}\left[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}\right], \mathrm{E}\left[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}\right], \mathrm{E}\left[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}\right], \mathrm{E}\left[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}\right]\right), \text{ not } \mathrm{E}\left[\kappa\left(\tilde{\sigma}^2_{\hat{\omega}}, \tilde{\psi}^B_{\hat{\omega}}, \tilde{\psi}^A_{\hat{\omega}}, \tilde{\psi}^X_{\hat{\omega}}\right) \mid \mathbf{X}\right].$$

When solving for sample size J, Equation (2) becomes linear in variance-covariance parameters, meaning that Jensen's Inequality does not affect the estimate of $J\left(\sigma^2_{\hat{\omega}}, \psi^B_{\hat{\omega}}, \psi^A_{\hat{\omega}}, \psi^X_{\hat{\omega}}\right)$.

In light of each of these issues, we recommend the following algorithm for estimating the MDE using a pre-existing panel dataset (implemented by our STATA program `pc_dd_covar`):

1. Determine all feasible ranges of experiments with $(m+r)$ periods, given the number of time periods in the pre-existing dataset. For example, if this dataset contains 100 time periods indexed $t = \{1, \ldots, 100\}$, and $m = 5$ and $r = 6$, then there are 90 feasible ranges for an experiment with $(m+r) = 11$ periods (i.e., beginning in periods $t = \{1, \ldots, 90\}$).

2. For each feasible range S:

   (a) Regress the outcome variable on unit and time-period fixed effects, $Y_{it} = \upsilon_i + \delta_t + \omega_{it}$, and store the residuals. (This regression includes all I available cross-sectional units, but only time periods with the specific range S.[24])

   (b) Calculate the variance of the stored residuals, and save as $\tilde{\sigma}^2_{\hat{\omega},S}$.

   (c) For each pair of pre-treatment periods (i.e., the first m periods in range S), calculate the covariance between these periods' residuals. Take an unweighted average of these $m(m-1)/2$ covariances, and save as $\tilde{\psi}^B_{\hat{\omega},S}$.

   For example, if $m = 4$, $r = 2$, and range S begins in period $t = 1$, sum $\mathrm{Cov}(\boldsymbol{\omega}_1, \boldsymbol{\omega}_2)$, $\mathrm{Cov}(\boldsymbol{\omega}_1, \boldsymbol{\omega}_3)$, $\mathrm{Cov}(\boldsymbol{\omega}_1, \boldsymbol{\omega}_4)$, $\mathrm{Cov}(\boldsymbol{\omega}_2, \boldsymbol{\omega}_3)$, $\mathrm{Cov}(\boldsymbol{\omega}_2, \boldsymbol{\omega}_4)$, and $\mathrm{Cov}(\boldsymbol{\omega}_3, \boldsymbol{\omega}_4)$, and divide by $m(m-1)/2 = 6$.

   (d) For each pair of post-treatment periods (i.e., the last r periods in range S), calculate the covariance between these periods' residuals. Take an unweighted average of these $r(r-1)/2$ covariances, and save as $\tilde{\psi}^A_{\hat{\omega},S}$.

   For example, if $m = 4$, $r = 2$, and range S begins in period $t = 1$, $\tilde{\psi}^A_{\hat{\omega},S}$ is the average of a single post-period covariance, $\mathrm{Cov}(\boldsymbol{\omega}_5, \boldsymbol{\omega}_6)$.

   (e) For each pair of pre- and post-treatment periods (i.e. the first m and the last r periods in range S), calculate the covariance between these periods' residuals. Take an unweighted average of these mr covariances, and save as $\tilde{\psi}^X_{\hat{\omega},S}$.

24. This bears no relationship to the sample size J units to be included in the power calculation. Assuming that all I units in the pre-existing dataset represent the population to be included in the randomization, estimating the variance and covariances using all available units will provide the best estimates of $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$ (by the Weak Law of Large Numbers).

For example, if $m = 4$, $r = 2$, and range S begins in period $t = 1$, sum $\text{Cov}(\boldsymbol{\omega}_1, \boldsymbol{\omega}_5)$, $\text{Cov}(\boldsymbol{\omega}_1, \boldsymbol{\omega}_6)$, $\text{Cov}(\boldsymbol{\omega}_2, \boldsymbol{\omega}_5)$, $\text{Cov}(\boldsymbol{\omega}_2, \boldsymbol{\omega}_6)$, $\text{Cov}(\boldsymbol{\omega}_3, \boldsymbol{\omega}_5)$, $\text{Cov}(\boldsymbol{\omega}_3, \boldsymbol{\omega}_6)$, $\text{Cov}(\boldsymbol{\omega}_4, \boldsymbol{\omega}_5)$, and $\text{Cov}(\boldsymbol{\omega}_4, \boldsymbol{\omega}_6)$, and divide by $mr = 8$.

3. Calculate the average of $\tilde{\sigma}^2_{\hat{\omega},S}$, $\tilde{\psi}^B_{\hat{\omega},S}$, $\tilde{\psi}^A_{\hat{\omega},S}$, and $\tilde{\psi}^X_{\hat{\omega},S}$ across all ranges S, deflating $\tilde{\sigma}^2_{\hat{\omega},S}$ by $\frac{IT-1}{IT}$, and $\tilde{\psi}^B_{\hat{\omega},S}$, $\tilde{\psi}^A_{\hat{\omega},S}$, and $\tilde{\psi}^X_{\hat{\omega},S}$ by $\frac{I-1}{I}$. These averages are equal in expectation to $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$.

4. Plug these values into Equation (D2) to produce MDE^{est}.

Figure D1 highlgihts the difference between true and estimated variance-covariance parameters in AR(1) data. In particular, we show true parameter values of σ^2_{ω}, ψ^B, ψ^A, and ψ^X alongside estimated values of these same parameters, calculated according to the procedure outlined above. As expected, $\sigma^2_{\hat{\omega}}$ is biased downwards relative to σ^2_{ω}, but converges towards this value as the panel length increases. This convergence is slower for larger AR(1) parameters, as highly serially correlated errors make it harder to identify the unit fixed effects. Similarly, while the true ψ^X is positive across all panel lengths, $\psi^X_{\hat{\omega}}$ is negative everywhere, and ψ^B and ψ^A also differ from their estimated counterparts. Despite the differences between the true parameters and their estimated values, Appendix E.2 proves that we can recover the MDE based on true underlying parameters using residual-based parameters. In conjunction with the fact that we can estimate the residual-based parameters from real data, this confirms that researchers can use estimated parameters to calibrate power calculations.

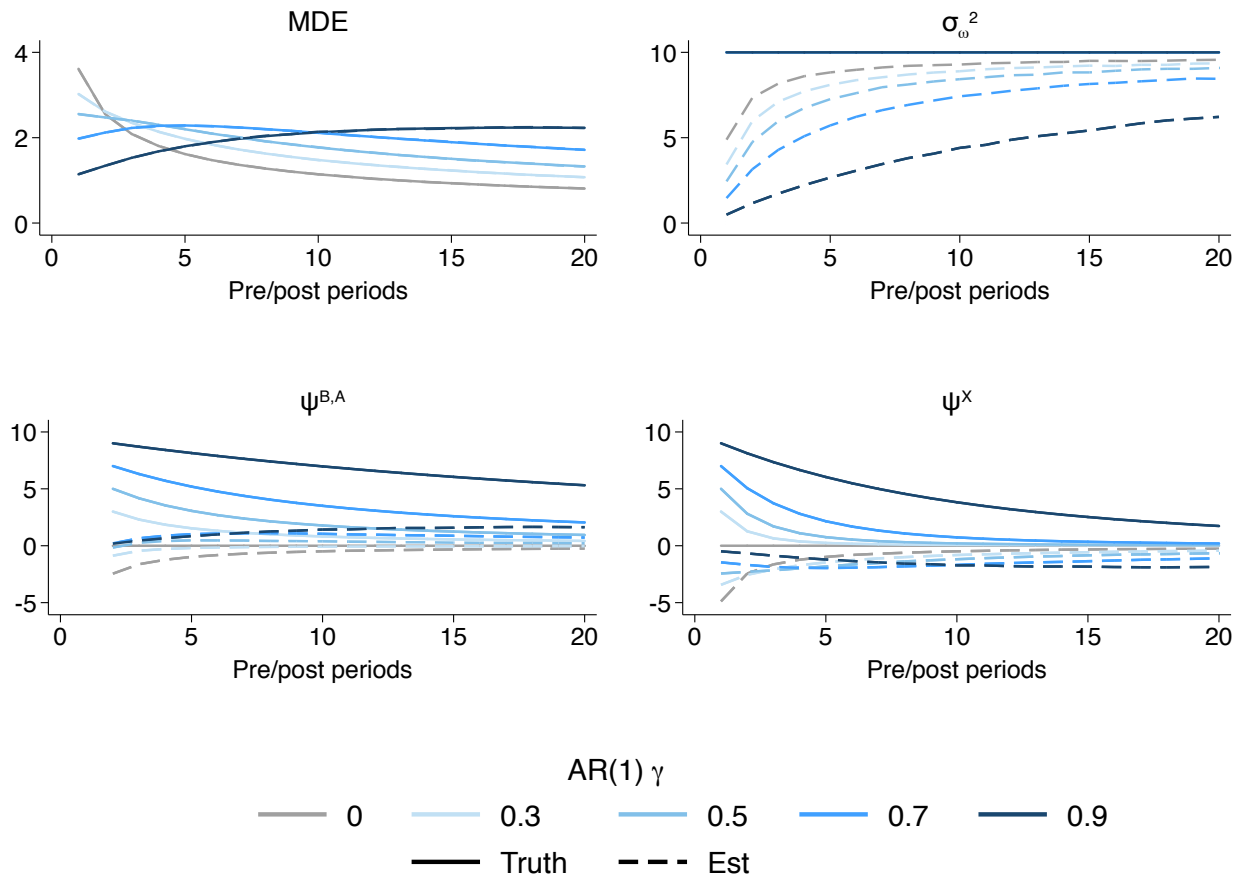
Figure D2 uses the Bloom et al. (2015) dataset to present an analogous comparison between actual vs. estimated σ^2_{ω}, ψ^B, ψ^A, and ψ^X parameters. Here, as in Figure D1, the dotted lines estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ using the above algorithm. However, unlike with simulated AR(1) datasets, the "true" parameters of the Bloom et al. (2015) data generating process are unknown. We estimate these "true" values using residuals from the full 48-period time series, which minimizes the fixed effect estimation error that biases $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ in short panels.[25] This reveals a very similar pattern: "subsetted" $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ estimates are systematically biased downward, but converge to their "full time-series" (i.e. closer to "true") values as panel length increases. As in Figure D1, we show that both sets of estimated variance-covariance parameters yield (virtually) identical MDEs, as long as Equation (2) uses estimated parameters that are internally consistent.

Figure D3 replicates Figure D2 for all four Pecan Street datasets. We see that while the estimated values $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ differ across different levels of aggregation, they follow the same pattern. The subsetted estimates are biased downward, but appear to converge to the full time series estimates (i.e. closer to truth) as panel length increases. In all four cases, the MDE is (virtually) identical when calculated using either all full time series estimates or all subsetted estimates.

Two additional nuances that arise during analytical power calculations are worth noting. First, the critical values $t^d_{1-\kappa}$ and $t^d_{\alpha/2}$ should be drawn from an inverse t-distribution with the same degrees of freedom as the *ex post* regression model. This means that if researchers plan to use CRVE standard errors clustered by experimental unit, they should draw these critical values from

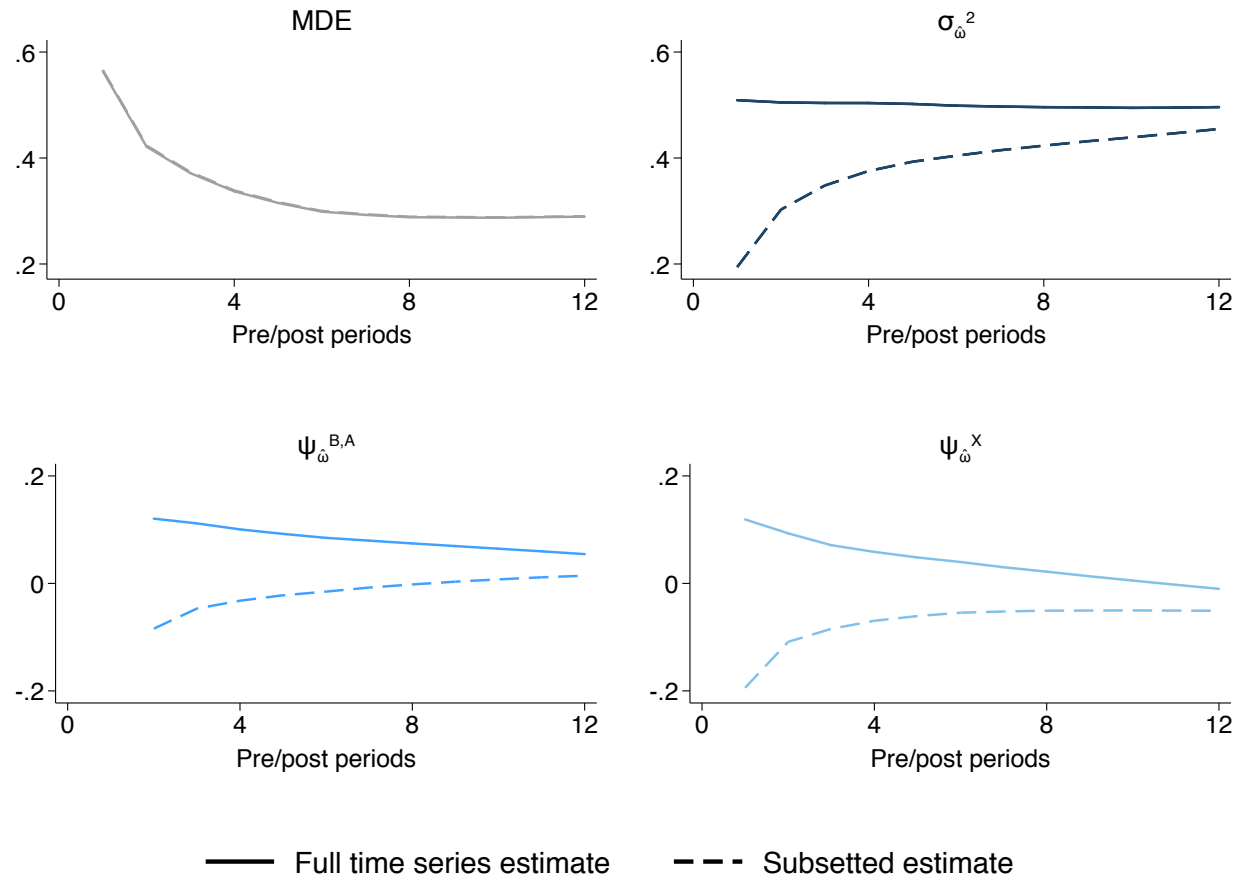
25. These $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ estimates (represented by solid lines in Figure D2) result from the same algorithm as detailed above, except omitting Step 2(a) and estimating a single 48-period set of residuals in Step 1. This provides the closest possible approximation to the "true" variance-covariance structure of these data, and hence the most apples-to-apples comparison to Figure D1.

Figure D1: Actual vs. estimated parameters – AR(1) data



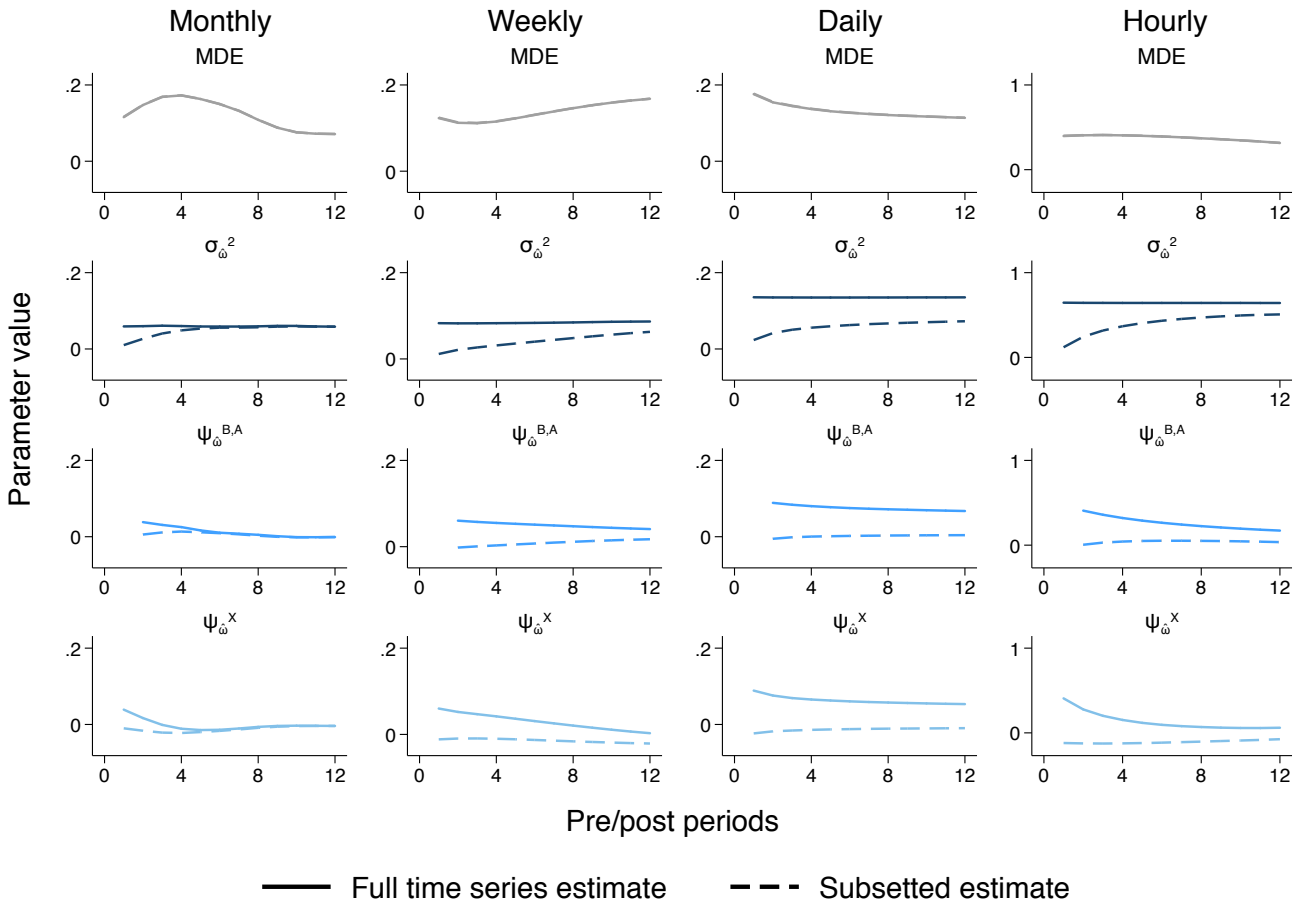
Notes: This figure displays the difference between the true residual variance (σ^2_ω), average pre (post)-period covariance ($\psi^{B,A}$), and average cross-period covariance (ψ^X), and their estimated counterparts over varying panel lengths. It also shows the resulting minimum detectable effect (MDE), calculated using the SCR formula (Equation (2)). These parameters and estimates come from a simulated datasets with AR(1) errors, generated identically to those presented in Figure 1. True parameters are displayed with solid lines (with ψ terms derived analytical using Equations (B1)–(B3)), and estimates are displayed with dashed lines (estimated according to the algorithm described above). As expected, the true σ^2_ω is constant across panel lengths, while $\psi^{B,A}$ declines with the number of pre (post) periods, and ψ^X declines more quickly than $\psi^{B,A}$ as the panel length increases. The higher the AR(1) parameter, the larger are the ψ terms. The estimated parameters behave quite differently from their true counterparts. In short panels, $\sigma^2_{\hat{\omega}}$ is biased downwards, because the regression model's individual fixed effects are inconsistently estimated, and capture more of the true error variance than they should explain in expectation. This has the effect of reducing the estimated covariances $\psi^{B,A}_{\hat{\omega}}$, which scale with $\sigma^2_{\hat{\omega}}$. At the same time, $\psi^X_{\hat{\omega}}$ is mechanically negative, as the estimated fixed effects yield residuals that are negatively correlated within individuals across pre/post-treatment time periods (and some of the variation that should be captured by $\sigma^2_{\hat{\omega}}$ is instead loaded on to the $\psi_{\hat{\omega}}$ terms). As panel length increases, the $\psi_{\hat{\omega}}$ terms converge to the true ψ values. Estimated parameters result in the same MDEs as real parameters after applying the correction factors in Equation (D2), as demonstrated by the top left panel.

Figure D2: Estimated parameters – Bloom et al. (2015) data



Notes: This figure shows two different methods for estimating variance parameters, applied to the Bloom et al. (2015) data, and also depicts the resulting minimum detectable effect (MDE) resulting from both methods. The solid lines plot parameters calculated by running a regression of the outcome variable on unit and time period fixed effects, estimated on the entire time series of data, and then using the residuals from this regression to calculate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ for the average panel of length $m+r$. We then plug these estimates into Equation (2) to calculate the minimum detectable effect. The dashed lines show parameters estimated using the procedure described above, where rather than use residuals from the full time series, we subset the dataset into shorter panels of length $m+r$, calculate the parameters using residuals only from this subset, and average across all possible subsets to arrive at $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. We calculate the MDE by plugging these estimates into Equation (D2). Note that these variance-covariance estimates converge as the panel length increases. Both procedures yield (virtually) identical MDEs, even though the underlying parameter estimates differ substantially.

Figure D3: Estimated parameters – Pecan Street data



Notes: This figure shows two different methods for estimating variance parameters, applied to the Pecan Street data at the four levels of aggregation presented in the main text, and also depicts the resulting minimum detectable effect (MDE) resulting from both methods. Note that the y axis scale differs between the hourly data and the other three datasets; this is because the degree of residual variation left in the hourly data after removing time and individual fixed effects is much greater than in the other datasets. The solid lines show parameters calculated by running a regression of the outcome variable on unit and time period fixed effects, estimated on the entire time series of data, and then using the residuals from this regression to calculate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ for the average panel of length $m + r$. We then plug these estimates into Equation (2) to calculate the minimum detectable effect. By contrast, the dashed lines show parameters estimated using the procedure described above, where rather than use residuals from the full time series, we subset the dataset into shorter panels of length $m + r$, calculate the parameters using residuals only from this subset, and average across all possible subsets to arrive at $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. We calculate the MDE by plugging these estimates into Equation (D2). Note that these variance-covariance estimates converge as the panel length increases. Both procedures yield (virtually) identical MDEs, even though the two procedures' method for estimating the underlying parameters differ substantially.

an inverse t-distribution with J degrees of freedom. To be precise, these critical values should be sensitive to changes in the number of unit/clusters J, although the t degrees of freedom has a very small effect on MDE, relative to other parameters in Equation (2).[26]

Finally, in panel RCTs with CRVE standard errors clustered by unit, the proportion of units treated P cannot be too large or too small. Our simulations have demonstrated that the Equation (2) performs poorly if $P < 0.1$ or $P > 0.9$, because the CRVE requires a sufficient number of clusters that are both treated and control.

D.2 Simulation-based power calculations

In cases where researchers have access to a representative pre-existing dataset, we recommend that they perform power calculations via simulation. This obviates the need to estimate *ex ante* variance-covariance parameters, and it ensures that *ex ante* power calculations assume the same experimental design, regression model, and variance estimator expected to be used in *ex post* analysis. Our accompanying STATA package facilitates simulation-based power calculations using the program `pc_simulate`. This program implements the following algorithm:

1. Choose the following candidate parameters: sample size J, pre-treatment periods m, and post-treatment periods r, treatment ratio P, minimum detectable effect MDE, and significance level α. Let X_{it} denote the outcome variable of interest in the pre-existing dataset.

2. Randomly select J units from the representative dataset, and randomly select a range of $(m + r)$ consecutive time periods. This will serve as a simulated experimental dataset, with sample size J, m pre-treatment periods, and r post-treatment periods.

3. Randomly scramble a $[J \times 1]$ vector of PJ ones and $(1 - P)J$ zeros, rounding PJ to the nearest integer. Assign each of the J units to either treatment ($D = 1$) or control ($D = 0$), based on the order of this scrambled vector.

4. Construct an experimental outcome variable Y_{it}, where $Y_{it} = X_{it} + MDE$ for treated units in post-treatment periods, and $Y_{it} = X_{it}$ otherwise.

5. Using this simulated experimental dataset and the simulated outcome variable Y_{it}, implement the exact regression specification and variance estimator to be used in *ex post* analysis. Record whether this model rejects the null hypotheses of zero treatment effects with significance level α (i.e. $H_0 : \tau = 0$).

6. Repeat Steps 2–5 many times, and calculate the rejection rate across all simulations. This is the experiment's statistical power as a function of J, m, r, P, MDE, and α.

7. Repeat Steps 1–6 for a range of MDEs and design parameters, increasing the number of simulations after narrowing down this range of parameters to more precisely calibrate power.

This algorithm allows users to test alternative regression specifications and alternative standard error assumptions, without needing to formally derive a power calculation expression for each model. If the pre-existing dataset contains fewer cross-sectional units than the desired sample size

26. The STATA program `sampsi` assumes a normal distribution, which is not appropriate for small samples.

J, `pc_simulate` lets users simulate additional units by bootstrapping units with replacement from the existing dataset. Unfortunately, if the pre-existing dataset contains fewer time periods than the desired panel length ($m+r$), an analogous bootstrapping procedure would be much less straightforward (because unlike cross-sectional units, time periods are ordered and have a ordered covariance structure that is not orthogonal to the treatment vector D).

This simulation-based algorithm can only calibrate statistical power κ. Rather than rely on the critical value $t^d_{1-\kappa}$, the algorithm simply estimates realized power as the proportion of simulations where the treatment effect is statistically distinguishable from zero. (By contrast, users may algebraically rearrange (or invert) an analytical power calculation formula to solve for any one of its parameters.) Calibrating simulation-based power calculations for a parameter other than κ necessitates a grid search over candidate parameter values, as described in Step 7 above. For example, to calibrate sample size J by simulation, users may repeat Step 1–6 over a range of candidate J values, narrowing this range (while simultaneously increasing the number of simulations) to calibrate to the desired power.

D.3 Lack of (representative) pre-existing data

To perform accurate *ex ante* power calculations, researchers must either have access to data that is representative (in expectation) of their future experimental data, or be able to parameterize an analytical formula with accurate estimates of the variance and covariance of the error structure. We recommend that simulation-based power calculations (as described above) in cases where researchers have a representative pre-existing dataset with (i) data for the desired outcome (and relevant control variables); (ii) at least as many unique cross-sectional units as the desired experimental sample size; and (iii) a time series at least as long as the desired experimental panel length. Many candidate experiments likely satisfy these criteria, such as when researchers partner with organizations that maintain historical databases on the desired population of experimental subjects.

At the same time, researchers may lack access to representative data *ex ante*. This problem is not unique to panel data, as even the simple cross-sectional power calculation formula (Equation (A6)) hinges on (an estimate of) the variance σ^2_ε. However, power calculations for panel RCT designs require four variance-covariance parameters: σ^2_ω, ψ^B, ψ^A, and ψ^X. While σ^2_ω is fixed in the population, the ψ (and $\psi_{\hat{\omega}}$) terms are endogenous to the panel length of the experiment, which underscores the importance of estimating $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ from a representative time series.

In the absence of representative data, researchers may using analytical formulas in conjunction with appropriate sensitivity analyses.[27] Depending on the type of data that *is* available, approximating the parameters σ^2_ω, ψ^B, ψ^A, and ψ^X may be possible. We consider four cases:

1. *Too few units:* If researchers have access to a representative pre-existing dataset with too few cross-sectional units, they may still estimate $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, and apply these values to the (estimation-specific) analytic formula.[28] These variance-covariance parameters do *not* depend on sample size J in the SCR power calculation formula, and estimates of $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ derived from residuals are *not* sensitive in expectation to the number of panel units

27. An alternative is to either impose assumptions on some existing dataset or construct a simulated dataset, either of which could be used to conduct power calculations by simulation. However, this process will be much more computationally intensive than simply applying an analytical formula with appropriate parameter sensitivities.

28. Researchers using *estimated* parameters should apply Equation (D2) rather than Equation (2).

J to be used in the experiment.[29] Alternatively, we recommend that researchers bootstrap units by sampling existing units with replacement, and use this expanded dataset (including simulated units) to conduct power calculations by simulation, as described above.[30]

2. *Too few time periods:* If the pre-existing dataset contains too few time periods, researchers may still estimate $\tilde{\sigma}^2_{\hat{\omega}}$ using residuals from a regression with fewer than $m+r$ periods (because σ^2_ω does not depend on panel length). However, the ψ terms do depend on panel length, and they cannot be estimated directly from a dataset with fewer than $m+r$ periods. One strategy is to simply estimate $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ using the longest possible panel (i.e., all available time periods in the pre-existing dataset), even if it is shorter than $m+r$ periods. The resulting $\psi_{\hat{\omega}}$ estimates are likely to be upper bounds (in absolute value) on the $\psi_{\hat{\omega}}$ estimates for longer panels, because as the panel length increases, the ψ terms incorporate more covariances between time periods that are further apart (which tend to become less correlated in distance). Another strategy is to attempt to extend the time series for each unit, analogous to the approach of bootstrapping units. As a rule of thumb, researchers often approximate time series data as an AR(k) process with $k \geq \sqrt[3]{T}$, where T is the full time series length. To extend short panels, researchers may estimate this AR(k) process using (residuals from) the existing dataset, and then simulate forward for each unit's outcome realization. Neither of these strategies is perfect, and we recommend conducting appropriate sensitivity analysis in either case.

3. *No data, standard cross-sectional or DD model:* In the complete absence of data, power calculations will be challenging. At the very least, we recommend that researchers search for estimates of the residual variance in the existing literature, noting that panel fixed effects models are likely to yield lower residual variances than cross-sectional models with similar outcome data. If this is not possible, researchers may iterate analytical power calculations over a range of parameter choices. If researchers are able to guess a reasonable value of σ^2_ω, they my test a range of AR(1) parameters for plausible values of $\psi^{B,A,X}$. As a rule of thumb, $\psi^{B,A,X}$ are likely to be positive in the absence of a strong prior of negative serial correlation. In absolute value, $\psi^{B,A,X}$ should not exceed σ^2_ω, and they should decrease monotonically in panel length. To provide a sense of what plausible values of ψ^B, ψ^A, and ψ^X (and their residual-based counterparts) may be, we plot estimates from a range of panel lengths using simulated AR(1) data, the Bloom et al. (2015) data, and Pecan Street data, in Figures D1, D2, and D3, respectively.

4. *No data, other models:* For more complicated RCT designs (or ANCOVA in the presence of time shocks), analytical power calculation formulas might not align with the desired *ex post* estimating equation. This would render our above Case 3 recommendation inactionable. An alternative strategy (in the absence of data) is to simulate data based on a simple DGP, incorporating AR(1) serial correlation into the idiosyncratic error term. Researchers can perform simulation-based power calculations using these simulated data, iterating over multiple simulated datasets to test for sensitivity to DGP (and AR(1)) assumptions.

29. Estimates of $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ are sensitive to the number of cross-sectional units I used to estimate $\hat{\omega}_{it}$, but this is not related to the sample size parameter J. In Equation (D2), the comparative static $d\psi_{\hat{\omega}}/dJ = 0$.

30. The `bootstrap` option in `pc_simulate` does exactly this, drawing units with replacement until reaching the desire sample size.

D.4 STATA package `pcpanel`

To facilitate user implementation of the methods described above, we have built an accompanying STATA package `pcpanel`. This package contains three programs, which we introduce iin Section 4.2 of the main text. We describe each function in detail below:

`pc_dd_analytic`

This program performs analytical power calculations using our SCR formula for difference-in-differences (Equation (2)). Users may input any two of the options {`mde` (effect size MDE), `n` (sample size J), `power` (κ)}, and the program will solve Equation (2) for the third option. Users may also adjust four additional experimental design options: treatment ratio `p` (P), number of pre-treatment periods `pre` (m), number of post-treatment periods `post` (r), and false rejection rate `alpha` (α). `pc_dd_analytic` parameterizes the variance (σ^2_ω) and covariance (ψ^B, ψ^A, ψ^X) in Equaton (2) in one of two ways.

First, users may allow the subprogram `pc_dd_covar` to nonparametrically estimate the idiosyncratic residual variance/covariance papers. To do this, they input the outcome variable of interest from the dataset in memory (using option `depvar`), as well as variables that identify units and time periods (using options `i` and `t`, respectively). This is our preferred means of parameterizing Equation (2) in practice, using pre-existing data.

Second, users may manually input *either* an idiosyncratic residual variance (option `variance` (σ^2_ω)) *or* an idiosyncratic residual standard deviation (option `sd` (σ_ω)). They may also incorporate non-i.i.d. idiosyncratic errors in 1 of 3 ways: (1) input assumed AR(1) parameter(s) (option `ar1` (γ)) which `pc_dd_analytic` uses to calculate average covariances (ψ^B, ψ^A, ψ^X); (2) input assumed average covariances themselves (option `avgcov`); or (3) input assumed average correlations (option `avgcor`), which the program multiplies by the variance to convert to covariances. Specifying `var` (or `sd`) without specifying `ar1`, `avgcov`, or `avgcor` will cause `pc_dd_analytic` to default to assuming $\psi^B = \psi^A = \psi^X = 0$.[31] For users accustomed to `sampsi`, which accepts the *composite* residual standard deviation and intracluster correlation (σ_ε and ρ, in the notation of McKenzie (2012)), the `pc_dd_analytic` option `var` combines these two terms into a single input (σ^2_ω, in our notation) where $\sigma^2_\omega = \sigma^2_\varepsilon(1-\rho)$.[32]

`pc_dd_covar`

This program nonparametrically estimates $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ from an existing dataset (i.e. the dataset in memory), for a given number of pre-treatment and post-treatment periods. Users must input the outcome variable in question, as well as the number or pre-treatment periods (option `pre` (m)), post-treatment periods (opton `post` (r)), unit identifier (option `i`) and time period identifier (option `t`). Given these inputs, `pc_dd_covar` operationalizes the procedure outlined in Appendix D.1. Most users will not need to directly apply this program, as it is designed to operate "under the hood" of `pc_dd_analytic`.

31. Given that estimating ψ^B, ψ^A, ψ^X is nontrivial (see Appendix D.1), users should apply the options `avgcov` and `avgcor` with caution.

32. In the absence of time shocks (i.e. $\sigma^2_\delta = 0$, as in McKenzie (2012)), this equality holds. However, if the data generating process includes time shocks, then $\sigma^2_\omega = \sigma^2_\varepsilon(1-\rho_\upsilon-\rho_\delta)$, where ρ_υ is the intracluster correlation within units and ρ_δ is the intracluster correlation within time periods.

`pc_simulate`

This program performs power calculations by simulation, following the algorhtim in Appendix D.2. it supports four types of RCTs:

- `ONESHOT` (one wave of post-treatment data)
- `POST` (multiple waves of post-treatment data)
- `DD` (pre-treatment and post-treatment data)
- `ANCOVA` (post-treatment data, conditioning on pre-treatment data)

Users may input six basic experimental design options: effect size `mde` (MDE), sample size `n` (J), treatment ratio `p` (P), number of pre-treatment periods `pre` (m), number of post-treatment periods `post` (r), and false rejection rate `alpha` (α). Given these options, `pc_simulate` calculates the average rejection rate (i.e. power (κ)) of the treatment effect estimator, across all simulations.

The program is sufficiently flexible to allow for linear controls (option `controls`), fixed effects (options `absorb`, `absorbfactor`), regression weights (opton `weight`) and standard errors (option `vce`). Each of these options, when specified, is passed through the regression function `reghdfe` in each simulation.[33] Toggling the option `bootstrap` causes the program to sample J units with replacement for each simulation, which facilitates power calculations with a greater number of units than are present in the dataset in memory. If `bootstrap` is not toggled, each simulation samples J units without replacement. Users may also restrict the range of time periods over which simulations occur (option `tstart`), and the number of simulations for each set of parameters (option `nsim`).[34]

Instead of applying the CRVE, Bertrand, Duflo, and Mullainathan (2004) also recommend collapsing panel datasets to a single pre-treatment and post-treatment observation. `pc_simulate` lets users conduct power calculations on collapsed panel estimators, by toggling the option `collapse`. For `POST` and `ANCOVA` models, each simulation collapses the estimating equation to a cross-sectional model with one observation per unit (where the dependent variable is averaged within units across all r time periods). For the `DD` model, each simulation collapses the estimating equation to a two-period panel, with one pre-treatment and one post-treatment observation per unit (where the dependent variable is averaged within units across all m or r time periods).

`pc_simulate` supports stratified randomization via the option `stratify`. When users specify one or more categorical variables (e.g. `stratify(gender race)`), the program separately randomizes P units within each `gender-race` cell. For stratified randomization, the option `n` governs the number of units within each *cell* (as opposed to the total number of units in the full sample). The program continues to estimate a single *pooled* average treatment effect. If researchers are interested in heterogeneous treatment effects by cell, they can iterate `pc_simulate` separately for each stratification cell—leaving `stratify` unspecified, and conditioning on stratification cells using the option `if` (e.g. `pc_simulate y if gender=="male" & race=="white", ...`).

`pc_simulate` also supports cluster randomization via the option `idcluster`. When users specify a group identifier (e.g. `idcluster(village_id)`), the program randomizes at the group level, where whole groups are randomly assigned to treatment/control. For cluster randomization, the

33. `pc_simulate` simulates regressions using the function `reghdfe` (`http://scorreia.com/software/reghdfe/`).

34. The program defaults users to 500 simulations. Increasing the number of simulations will improve accuracy at the expense of runtime.

option `n` governs the number of *groups* (e.g. villages), as opposed to the total number of individual units (e.g. households). The program continues to estimate a single *pooled* average treatment effect at the unit level. `pc_simulate` also supports two cluster randomization suboptions. The suboption `sizecluster` governs the number of units sampled within each cluster; if not specified, the program defaults to the size of each cluster in the existing dataset. The suboption `pcluster` governs the intensity of treatment within treated clusters: (i) if `pcluster` is not specified, the program defaults to a treatment intensity of 1 in all treated clusters (i.e. all units in treated clusters receive treatment); (ii) if `pcluster` is specified with a single value (e.g. `pcluster(0.5)`), the program randomly assigns treatment to this proportion (i.e. 50 percent) of units within each treated cluster (all units in control clusters remain untreated); (iii) if `pcluster` is specified with multiple values (e.g. `pcluster(0.33 0.67 1)`), the program varies the intensity of treatment in equal proportions across treated clusters (i.e. one third of treated clusters receive `pcluster(0.33)`, etc.).

E Estimation-related proofs

In this section, we prove that researchers may calculate unbiased power calculations by estimating the variance-covariance parameters from a real dataset, where the parameters governing the data generating process is unknown.

E.1 Recovering estimated parameters

Here, we demonstrate that the procedure described in Appendix D.1 recovers unbiased estimates of the variance and covariance parameters governing the residuals $\hat{\omega}_{it}$ from a regression of Y_{it} on unit and time fixed effects, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ (though these do *not* represent unbiased estimates of the *true* parameters σ^2_{ω}, ψ^B, ψ^A, and ψ^X). We denote our procedure for computing these parameters with a $\sim$. Note that throughout this section, we are considering I units in the sample used to estimate $\hat{\omega}_{it}$, which may be distinct from the sample size J units used in the ensuing power calculations. Note also that because we are estimating the variance and covariance of a *population* of residuals, we use the population variance/covariance estimators as opposed to the (unbiased) sample variance/covariance estimators.[35]

In order to estimate the variance of the residuals, $\sigma^2_{\hat{\omega}}$, we define:

$$\tilde{\sigma}^2_{\hat{\omega}} \equiv \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} \left(\hat{\omega}_{it} - \overline{\overline{\hat{\omega}}} \right)^2$$

where $\overline{\overline{\hat{\omega}}} = \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} \hat{\omega}_{it} = 0$. Taking expectations of both sides:

$$\begin{aligned} \mathrm{E}\left[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X} \right] &= \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} \mathrm{E}\left[\hat{\omega}^2_{it} \mid \mathbf{X} \right] \\ &= \sigma^2_{\hat{\omega}} \end{aligned}$$

To estimate $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, we define the $[I \times 1]$ vector of residuals for period t as $\hat{\boldsymbol{\omega}}_t$ and calculate the average covariance between any two vectors in the relevant range of time periods. For the pre-period, we define:

$$\begin{aligned} \tilde{\psi}^B_{\hat{\omega}} &\equiv \frac{2}{Im(m-1)} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} \sum_{i=1}^{I} \left(\hat{\omega}_{it} - \overline{\hat{\omega}}_t \right) \left(\hat{\omega}_{is} - \overline{\hat{\omega}}_s \right) \\ &= \frac{2}{Im(m-1)} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} \sum_{i=1}^{I} \left(\hat{\omega}_{it}\hat{\omega}_{is} - \hat{\omega}_{it}\overline{\hat{\omega}}_s - \hat{\omega}_{is}\overline{\hat{\omega}}_t + \overline{\hat{\omega}}_t\overline{\hat{\omega}}_s \right) \end{aligned}$$

35. This means that to calculate $\tilde{\sigma}^2_{\hat{\omega}}$, we deflate the sample variance estimate by $\frac{IT-1}{IT}$, and to calculate the $\tilde{\psi}_{\hat{\omega}}$ terms, we deflate the sample covariance estimates by $\frac{I-1}{I}$. This distinction is ultimately innocuous, and the following derivations simply rely on a consistent decision to use either the population or sample variance/covariance estimators.

where $\overline{\hat{\omega}}_t = \frac{1}{I}\sum_{i=1}^{I}\hat{\omega}_{it} = 0$. Taking expectations yields:

$$\begin{aligned}
\mathrm{E}\left[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}\right] &= \frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\mathrm{E}\left[\hat{\omega}_{it}\hat{\omega}_{is} \mid \mathbf{X}\right] \\
&= \frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\mathrm{Cov}(\hat{\omega}_{it},\hat{\omega}_{is} \mid \mathbf{X}) \\
&= \psi^B_{\hat{\omega}}
\end{aligned}$$

Similarly:

$$\begin{aligned}
\tilde{\psi}^A_{\hat{\omega}} &\equiv \frac{2}{Ir(r-1)}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\sum_{i=1}^{I}\left(\hat{\omega}_{it}-\overline{\hat{\omega}}_t\right)\left(\hat{\omega}_{is}-\overline{\hat{\omega}}_s\right) \\
&= \frac{2}{Ir(r-1)}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\sum_{i=1}^{I}\left(\hat{\omega}_{it}\hat{\omega}_{is}-\hat{\omega}_{it}\overline{\hat{\omega}}_s-\hat{\omega}_{is}\overline{\hat{\omega}}_t+\overline{\hat{\omega}}_t\overline{\hat{\omega}}_s\right)
\end{aligned}$$

and therefore:

$$\mathrm{E}\left[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}\right] = \psi^A_{\hat{\omega}}$$

Applying the same steps to $\psi^X_{\hat{\omega}}$:

$$\begin{aligned}
\tilde{\psi}^X_{\hat{\omega}} &\equiv \frac{1}{Imr}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\sum_{i=1}^{I}\left(\hat{\omega}_{it}-\overline{\hat{\omega}}_t\right)\left(\hat{\omega}_{is}-\overline{\hat{\omega}}_s\right) \\
&= \frac{1}{Imr}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\sum_{i=1}^{I}\left(\hat{\omega}_{it}\hat{\omega}_{is}-\hat{\omega}_{it}\overline{\hat{\omega}}_s-\hat{\omega}_{is}\overline{\hat{\omega}}_t+\overline{\hat{\omega}}_t\overline{\hat{\omega}}_s\right)
\end{aligned}$$

Taking expectations of both sides:

$$\begin{aligned}
\mathrm{E}\left[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}\right] &= \frac{1}{Imr}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\sum_{i=1}^{I}\mathrm{E}\left[\hat{\omega}_{it}\hat{\omega}_{is} \mid \mathbf{X}\right] \\
&= \frac{1}{Imr}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\sum_{i=1}^{I}\mathrm{Cov}\left(\hat{\omega}_{it},\hat{\omega}_{is} \mid \mathbf{X}\right) \\
&= \psi^X_{\hat{\omega}}
\end{aligned}$$

Hence, we can recover unbiased estimates of the parameters $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ (defined over residuals $\hat{\omega}_{it}$, rather than errors ω_{it}) by calculating the averages of the estimated $\tilde{\sigma}^2_{\hat{\omega}}$, $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$, respectively.

E.2 Estimating MDE from residual-based parameters

To calculate the MDE using the SCR formula, we must know the true parameters that characterize the variance and covariance of the error structure, σ^2_ω, ψ^B, ψ^A, and ψ^X. We cannot calculate these parameters directly from a real dataset, however, because we do not observe the true error structure or data generating process. Instead, we estimate a residual for each observation and calculate the residual-based analogs of these parameters, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. In this section, we derive an expression for MDE^{est} in terms of these residual-based parameters that is equivalent to MDE from the SCR formula as defined in terms of true variance-covariance parameters:

$$MDE^{est}\left(\sigma^2_{\hat{\omega}}, \psi^B_{\hat{\omega}}, \psi^A_{\hat{\omega}}, \psi^X_{\hat{\omega}}\right) = MDE\left(\sigma^2_\omega, \psi^B, \psi^A, \psi^X\right)$$

Model While estimating the variance and covariance parameters of a dataset does not require a treatment, we assume that all other features of this model are identical to the model that generates the serial-correlation-robust power calculation formula, Equation (2).

That is, there are J units, P proportion of which are randomized into treatment. The researcher again collects outcome data Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For treated units, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for control units, $D_{it} = 0$ in all periods. We restate the remaining assumptions from Appendix A.2.2 here for convenience:

Assumption (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \delta_t + \omega_{it}$$

where Y_{it} is the outcome of interest for unit i at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{it} is a time-varying treatment indicator; υ_i is a unit-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\upsilon)$; δ_t is a time-specific disturbance distributed i.i.d. $\mathcal{N}(0, \sigma^2_\delta)$; and ω_{it} is an idiosyncratic error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma^2_\omega)$.

Assumption (Strict exogeneity)**.** $\mathrm{E}[\omega_{it} \mid \mathbf{X}] = 0$*, where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$*,* $J-1$ *unit dummies, and* $(m+r)-1$ *time dummies. This again follows from random assignment of* D_{it}*.*

Assumption (Balanced panel)**.** *The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.*

Assumption (Independence across units)**.** $\mathrm{E}[\omega_{it}\omega_{js} \mid \mathbf{X}] = 0,\ \forall\ i \neq j,\ \forall\ t, s.$

Assumption (Symmetric covariance structures)**.** *Define:*

$$\psi^B \equiv \frac{2}{Jm(m-1)} \sum_{i=1}^{J} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

$$\psi^A \equiv \frac{2}{Jr(r-1)} \sum_{i=1}^{J} \sum_{t=1}^{r-1} \sum_{s=t+1}^{r} \mathrm{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

$$\psi^X \equiv \frac{1}{Jmr} \sum_{i=1}^{J} \sum_{t=-m+1}^{0} \sum_{s=1}^{r} \text{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

to be the average pre-treatment, post-treatment, and across-period covariance between different error terms of the same unit, respectively. Define ψ_T^B, ψ_T^A, *and* ψ_C^X *analogously, where we consider only the* PJ *treated units; also define* ψ_C^B, ψ_C^A, *and* ψ_C^X *analogously, where we consider only the* $(1-P)J$ *control units. Using these definitions, assume that* $\psi^B = \psi_T^B = \psi_C^B$; $\psi^A = \psi_T^A = \psi_C^A$; *and* $\psi^X = \psi_T^X = \psi_C^X$.

Parameters We first need to estimate the residuals of this model. To do this, we regress Y_{it} on a constant and fixed effects at the unit and time levels. For a balanced panel, the estimated coefficients are

$$\begin{aligned}
\hat{\beta} &= \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} Y_{it} \\
\hat{\upsilon}_i &= \frac{1}{T} \sum_{t=-m+1}^{r} Y_{it} - \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} Y_{it} \\
\hat{\delta}_t &= \frac{1}{I} \sum_{i=1}^{I} Y_{it} - \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} Y_{it}
\end{aligned}$$

Then the residual is defined as

$$\begin{aligned}
\hat{\omega}_{it} &= Y_{it} - \hat{Y}_{it} \\
&= (\beta + \upsilon_i + \delta_t + \omega_{it}) - \left(\hat{\beta} + \hat{\upsilon}_i + \hat{\delta}_t\right) \\
&= \omega_{it} - \overline{\omega}_i - \overline{\omega}_t + \overline{\overline{\omega}}
\end{aligned}$$

where

$$\begin{aligned}
\overline{\omega}_i &= \frac{1}{T} \sum_{t=-m+1}^{r} \omega_{it} \\
\overline{\omega}_t &= \frac{1}{I} \sum_{i=1}^{I} \omega_{it} \\
\overline{\overline{\omega}} &= \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} \omega_{it}
\end{aligned}$$

We can now use this definition of residuals to derive expressions for $\sigma_{\hat{\omega}}^2$, $\psi_{\hat{\omega}}^B$, $\psi_{\hat{\omega}}^A$, and $\psi_{\hat{\omega}}^X$. We first derive an expression for $\sigma_{\hat{\omega}}^2$, the average variance of a residual:

$$\sigma_{\hat{\omega}}^2 = \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} \text{Var}(\hat{\omega}_{it} \mid \mathbf{X})$$

$$
\begin{aligned}
&= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\omega_{it} - \overline{\omega}_i - \overline{\omega}_t + \overline{\overline{\omega}} \mid \mathbf{X}\right) \\
&= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \Big[\operatorname{Var}(\omega_{it} \mid \mathbf{X}) + \operatorname{Var}\left(\overline{\omega}_i \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{\omega}_t \mid \mathbf{X}\right) + \operatorname{Var}\left(\overline{\overline{\omega}} \mid \mathbf{X}\right) \\
&\qquad - 2\operatorname{Cov}\left(\omega_{it}, \overline{\omega}_i \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\omega_{it}, \overline{\omega}_t \mid \mathbf{X}\right) + 2\operatorname{Cov}\left(\omega_{it}, \overline{\overline{\omega}} \mid \mathbf{X}\right) \\
&\qquad + 2\operatorname{Cov}\left(\overline{\omega}_i, \overline{\omega}_t \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\overline{\omega}_i, \overline{\overline{\omega}} \mid \mathbf{X}\right) - 2\operatorname{Cov}\left(\overline{\omega}_t, \overline{\overline{\omega}} \mid \mathbf{X}\right) \Big]
\end{aligned}
$$

Calculating each of these terms, in turn, gives

$$
\begin{aligned}
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}(\omega_{it} \mid \mathbf{X}) &= \sigma_\omega^2 \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\overline{\omega}_i \mid \mathbf{X}\right) &= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\frac{1}{T}\sum_{s=-m+1}^{r}\omega_{is} \mid \mathbf{X}\right) \\
&= \frac{1}{T}\sigma_\omega^2 + \frac{m(m-1)}{T^2}\psi^B + \frac{r(r-1)}{T^2}\psi^A + \frac{2mr}{T^2}\psi^X \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\overline{\omega}_t \mid \mathbf{X}\right) &= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt} \mid \mathbf{X}\right) \\
&= \frac{1}{I}\sigma_\omega^2 \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\overline{\overline{\omega}} \mid \mathbf{X}\right) &= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Var}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{s=-m+1}^{r}\omega_{js} \mid \mathbf{X}\right) \\
&= \frac{1}{IT}\sigma_\omega^2 + \frac{m(m-1)}{IT^2}\psi^B + \frac{r(r-1)}{IT^2}\psi^A + \frac{2mr}{IT^2}\psi^X \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} 2\operatorname{Cov}\left(\omega_{it}, \overline{\omega}_i \mid \mathbf{X}\right) &= \frac{2}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Cov}\left(\omega_{it}, \frac{1}{T}\sum_{s=-m+1}^{r}\omega_{is} \mid \mathbf{X}\right) \\
&= \frac{2}{T}\sigma_\omega^2 + \frac{2m(m-1)}{T^2}\psi^B + \frac{2r(r-1)}{T^2}\psi^A + \frac{4mr}{T^2}\psi^X \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} 2\operatorname{Cov}\left(\omega_{it}, \overline{\omega}_t \mid \mathbf{X}\right) &= \frac{2}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Cov}\left(\omega_{it}, \frac{1}{I}\sum_{j=1}^{I}\omega_{jt} \mid \mathbf{X}\right) \\
&= \frac{2}{I}\sigma_\omega^2 \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} 2\operatorname{Cov}\left(\omega_{it}, \overline{\overline{\omega}} \mid \mathbf{X}\right) &= \frac{2}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Cov}\left(\omega_{it}, \frac{1}{IT}\sum_{j=1}^{I}\sum_{s=-m+1}^{r}\omega_{js} \mid \mathbf{X}\right) \\
&= \frac{2}{IT}\sigma_\omega^2 + \frac{2m(m-1)}{IT^2}\psi^B + \frac{2r(r-1)}{IT^2}\psi^A + \frac{4mr}{IT^2}\psi^X \\
\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} 2\operatorname{Cov}\left(\overline{\omega}_i, \overline{\omega}_t \mid \mathbf{X}\right) &= \frac{2}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Cov}\left(\frac{1}{T}\sum_{s=-m+1}^{r}\omega_{is}, \frac{1}{I}\sum_{j=1}^{I}\omega_{jt} \mid \mathbf{X}\right) \\
&= \frac{2}{IT}\sigma_\omega^2 + \frac{2m(m-1)}{IT^2}\psi^B + \frac{2r(r-1)}{IT^2}\psi^A + \frac{4mr}{IT^2}\psi^X
\end{aligned}
$$

$$\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} 2\operatorname{Cov}\left(\overline{\omega}_i, \overline{\overline{\omega}} \mid \mathbf{X}\right) = \frac{2}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Cov}\left(\frac{1}{T}\sum_{s=-m+1}^{r}\omega_{is}, \frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp} \mid \mathbf{X}\right)$$
$$= \frac{2}{IT}\sigma_\omega^2 + \frac{2m(m-1)}{IT^2}\psi^B + \frac{2r(r-1)}{IT^2}\psi^A + \frac{4mr}{IT^2}\psi^X$$
$$\frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} 2\operatorname{Cov}\left(\overline{\omega}_t, \overline{\overline{\omega}} \mid \mathbf{X}\right) = \frac{2}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \operatorname{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt}, \frac{1}{IT}\sum_{k=1}^{I}\sum_{s=-m+1}^{r}\omega_{ks} \mid \mathbf{X}\right)$$
$$= \frac{2}{IT}\sigma_\omega^2 + \frac{2m(m-1)}{IT^2}\psi^B + \frac{2r(r-1)}{IT^2}\psi^A + \frac{4mr}{IT^2}\psi^X$$

Combining these terms and simplifying yields

$$\sigma_{\hat{\omega}}^2 = \left(\tfrac{(I-1)(T-1)}{IT}\right)\sigma_\omega^2 - \left(\tfrac{(I-1)m(m-1)}{IT^2}\right)\psi^B - \left(\tfrac{(I-1)r(r-1)}{IT^2}\right)\psi^A - \left(\tfrac{2(I-1)mr}{IT^2}\right)\psi^X \tag{E1}$$

We next derive an expression for $\psi_{\hat{\omega}}^A$:

$$\begin{aligned}
\psi_{\hat{\omega}}^A &= \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\hat{\omega}_{it}, \hat{\omega}_{is} \mid \mathbf{X}) \\
&= \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\omega_{it} - \overline{\omega}_i - \overline{\omega}_t + \overline{\overline{\omega}}, \omega_{is} - \overline{\omega}_i - \overline{\omega}_s + \overline{\overline{\omega}} \mid \mathbf{X}) \\
&= \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\Big[\operatorname{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) - \operatorname{Cov}(\omega_{it}, \overline{\omega}_i \mid \mathbf{X}) - \operatorname{Cov}(\omega_{it}, \overline{\omega}_s \mid \mathbf{X}) + \operatorname{Cov}(\omega_{it}, \overline{\overline{\omega}} \mid \mathbf{X}) \\
&\qquad - \operatorname{Cov}(\overline{\omega}_i, \omega_{is} \mid \mathbf{X}) + \operatorname{Cov}(\overline{\omega}_i, \overline{\omega}_i \mid \mathbf{X}) + \operatorname{Cov}(\overline{\omega}_i, \overline{\omega}_s \mid \mathbf{X}) - \operatorname{Cov}(\overline{\omega}_i, \overline{\overline{\omega}} \mid \mathbf{X}) \\
&\qquad - \operatorname{Cov}(\overline{\omega}_t, \omega_{is} \mid \mathbf{X}) + \operatorname{Cov}(\overline{\omega}_t, \overline{\omega}_i \mid \mathbf{X}) + \operatorname{Cov}(\overline{\omega}_t, \overline{\omega}_s \mid \mathbf{X}) - \operatorname{Cov}(\overline{\omega}_t, \overline{\overline{\omega}} \mid \mathbf{X}) \\
&\qquad + \operatorname{Cov}(\overline{\overline{\omega}}, \omega_{is} \mid \mathbf{X}) - \operatorname{Cov}(\overline{\overline{\omega}}, \overline{\omega}_i \mid \mathbf{X}) - \operatorname{Cov}(\overline{\overline{\omega}}, \overline{\omega}_s \mid \mathbf{X}) + \operatorname{Cov}(\overline{\overline{\omega}}, \overline{\overline{\omega}} \mid \mathbf{X})\Big]
\end{aligned}$$

We again calculate each term:

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) = \psi^A$$
$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\omega_{it}, \overline{\omega}_i \mid \mathbf{X}) = \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\omega_{it}, \frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip} \mid \mathbf{X}\right)$$
$$= \frac{2}{r(r-1)IT}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(r-t)\operatorname{Cov}(\omega_{it}, \omega_{ip} \mid \mathbf{X})$$
$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\omega_{it}, \overline{\omega}_s \mid \mathbf{X}) = \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\omega_{it}, \frac{1}{I}\sum_{j=1}^{I}\omega_{js} \mid \mathbf{X}\right)$$
$$= \frac{1}{I}\psi^A$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\omega_{it},\overline{\overline{\omega}}\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\omega_{it},\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp}\mid\mathbf{X}\right)$$
$$=\frac{2}{r(r-1)I^2T}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(r-t)\operatorname{Cov}(\omega_{it},\omega_{ip}\mid\mathbf{X})$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_i,\omega_{is}\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\omega_{is}\mid\mathbf{X}\right)$$
$$=\frac{2}{r(r-1)IT}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(t-1)\operatorname{Cov}\left(\omega_{it},\omega_{ip}\mid\mathbf{X}\right)$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_i,\overline{\omega}_i\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\frac{1}{T}\sum_{q=-m+1}^{r}\omega_{iq}\mid\mathbf{X}\right)$$
$$=\frac{1}{T}\sigma_\omega^2+\frac{m(m-1)}{T^2}\psi^B+\frac{r(r-1)}{T^2}\psi^A+\frac{2mr}{T^2}\psi^X$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_i,\overline{\omega}_s\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\frac{1}{I}\sum_{j=1}^{I}\omega_{js}\mid\mathbf{X}\right)$$
$$=\frac{2}{r(r-1)I^2T}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(t-1)\operatorname{Cov}(\omega_{it},\omega_{ip}\mid\mathbf{X})$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_i,\overline{\overline{\omega}}\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\frac{1}{IT}\sum_{j=1}^{I}\sum_{q=-m+1}^{r}\omega_{jq}\mid\mathbf{X}\right)$$
$$=\frac{1}{IT}\sigma_\omega^2+\frac{m(m-1)}{IT^2}\psi^B+\frac{r(r-1)}{IT^2}\psi^A+\frac{2mr}{IT^2}\psi^X$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_t,\omega_{is}\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\omega_{is}\mid\mathbf{X}\right)$$
$$=\frac{1}{I}\psi^A$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_t,\overline{\omega}_i\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip}\mid\mathbf{X}\right)$$
$$=\frac{2}{r(r-1)I^2T}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(r-t)\operatorname{Cov}(\omega_{it},\omega_{ip}\mid\mathbf{X})$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_t,\overline{\omega}_s\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\frac{1}{I}\sum_{k=1}^{I}\omega_{ks}\mid\mathbf{X}\right)$$
$$=\frac{1}{I}\psi^A$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\omega}_t,\overline{\overline{\omega}}\mid\mathbf{X})=\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\frac{1}{IT}\sum_{k=1}^{I}\sum_{p=-m+1}^{r}\omega_{kp}\mid\mathbf{X}\right)$$
$$=\frac{2}{r(r-1)I^2T}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(r-t)\operatorname{Cov}(\omega_{it},\omega_{ip}\mid\mathbf{X})$$

$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\overline{\omega}},\omega_{is}\mid\mathbf{X}) = \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\omega_{is}\mid\mathbf{X}\right)$$
$$= \frac{2}{r(r-1)I^2T}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(t-1)\operatorname{Cov}(\omega_{it},\omega_{ip}\mid\mathbf{X})$$
$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\overline{\omega}},\overline{\omega}_{i}\mid\mathbf{X}) = \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\frac{1}{T}\sum_{q=-m+1}^{r}\omega_{iq}\mid\mathbf{X}\right)$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{m(m-1)}{IT^2}\psi^B + \frac{r(r-1)}{IT^2}\psi^A + \frac{2mr}{IT^2}\psi^X$$
$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\overline{\omega}},\overline{\omega}_{s}\mid\mathbf{X}) = \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\frac{1}{I}\sum_{k=1}^{I}\omega_{ks}\mid\mathbf{X}\right)$$
$$= \frac{2}{r(r-1)I^2T}\sum_{i=1}^{I}\sum_{t=1}^{r}\sum_{p=-m+1}^{r}(t-1)\operatorname{Cov}(\omega_{it},\omega_{ip}\mid\mathbf{X})$$
$$\frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}(\overline{\overline{\omega}},\overline{\overline{\omega}}\mid\mathbf{X}) = \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r}\operatorname{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\right.$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{m(m-1)}{IT^2}\psi^B + \frac{r(r-1)}{IT^2}\psi^A + \frac{2mr}{IT^2}\psi^X$$

Combining these terms and simplifying yields

$$\psi_{\hat{\omega}}^A = -\left(\tfrac{I-1}{IT}\right)\sigma_\omega^2 + \left(\tfrac{(I-1)m(m-1)}{IT^2}\right)\psi^B + \left(\tfrac{(I-1)(m^2+2m+r)}{IT^2}\right)\psi^A - \left(\tfrac{2(I-1)m^2}{IT^2}\right)\psi^X \tag{E2}$$

By symmetry, the expression for $\psi_{\hat{\omega}}^B$ is

$$\psi_{\hat{\omega}}^B = -\left(\tfrac{I-1}{IT}\right)\sigma_\omega^2 + \left(\tfrac{(I-1)(r^2+2r+m)}{IT^2}\right)\psi^B + \left(\tfrac{(I-1)r(r-1)}{IT^2}\right)\psi^A - \left(\tfrac{2(I-1)r^2}{IT^2}\right)\psi^X \tag{E3}$$

We finally derive an expression for $\psi_{\hat{\omega}}^X$:

$$\psi_{\hat{\omega}}^X = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\operatorname{Cov}(\hat{\omega}_{it},\hat{\omega}_{is}\mid\mathbf{X})$$
$$= \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\operatorname{Cov}(\omega_{it}-\overline{\omega}_i-\overline{\omega}_t+\overline{\overline{\omega}},\omega_{is}-\overline{\omega}_i-\overline{\omega}_s+\overline{\overline{\omega}}\mid\mathbf{X})$$
$$= \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\Big[\operatorname{Cov}(\omega_{it},\omega_{is}\mid\mathbf{X}) - \operatorname{Cov}(\omega_{it},\overline{\omega}_i\mid\mathbf{X}) - \operatorname{Cov}(\omega_{it},\overline{\omega}_s\mid\mathbf{X}) + \operatorname{Cov}(\omega_{it},\overline{\overline{\omega}}\mid\mathbf{X})$$
$$- \operatorname{Cov}(\overline{\omega}_i,\omega_{is}\mid\mathbf{X}) + \operatorname{Cov}(\overline{\omega}_i,\overline{\omega}_i\mid\mathbf{X}) + \operatorname{Cov}(\overline{\omega}_i,\overline{\omega}_s\mid\mathbf{X}) - \operatorname{Cov}(\overline{\omega}_i,\overline{\overline{\omega}}\mid\mathbf{X})$$
$$- \operatorname{Cov}(\overline{\omega}_t,\omega_{is}\mid\mathbf{X}) + \operatorname{Cov}(\overline{\omega}_t,\overline{\omega}_i\mid\mathbf{X}) + \operatorname{Cov}(\overline{\omega}_t,\overline{\omega}_s\mid\mathbf{X}) - \operatorname{Cov}(\overline{\omega}_t,\overline{\overline{\omega}}\mid\mathbf{X})$$
$$+ \operatorname{Cov}(\overline{\overline{\omega}},\omega_{is}\mid\mathbf{X}) - \operatorname{Cov}(\overline{\overline{\omega}},\overline{\omega}_i\mid\mathbf{X}) - \operatorname{Cov}(\overline{\overline{\omega}},\overline{\omega}_s\mid\mathbf{X}) + \operatorname{Cov}(\overline{\overline{\omega}},\overline{\overline{\omega}}\mid\mathbf{X})\Big]$$

We again calculate each term:

$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\omega_{is},\omega_{it}\mid\mathbf{X})=\psi^X$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\omega_{it},\overline{\omega}_i\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\omega_{it},\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip}\mid\mathbf{X}\right)\\&=\frac{1}{T}\sigma_\omega^2+\frac{m-1}{T}\psi^B+\frac{r}{T}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\omega_{it},\overline{\omega}_s\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\omega_{it},\frac{1}{I}\sum_{j=1}^{I}\omega_{js}\mid\mathbf{X}\right)\\&=\frac{1}{I}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\omega_{it},\overline{\overline{\omega}}\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\omega_{it},\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp}\mid\mathbf{X}\right)\\&=\frac{1}{IT}\sigma_\omega^2+\frac{m-1}{IT}\psi^B+\frac{r}{IT}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\overline{\omega}_i,\omega_{is}\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\omega_{is}\mid\mathbf{X}\right)\\&=\frac{1}{T}\sigma_\omega^2+\frac{r-1}{T}\psi^A+\frac{m}{T}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\overline{\omega}_i,\overline{\omega}_i\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\frac{1}{T}\sum_{q=-m+1}^{r}\omega_{iq}\mid\mathbf{X}\right)\\&=\frac{1}{T}\sigma_\omega^2+\frac{m(m-1)}{T^2}\psi^B+\frac{r(r-1)}{T^2}\psi^A+\frac{2mr}{T^2}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\overline{\omega}_i,\overline{\omega}_s\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\frac{1}{I}\sum_{j=1}^{I}\omega_{js}\mid\mathbf{X}\right)\\&=\frac{1}{IT}\sigma_\omega^2+\frac{r-1}{IT}\psi^A+\frac{m}{IT}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\overline{\omega}_i,\overline{\overline{\omega}}\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip},\frac{1}{IT}\sum_{j=1}^{I}\sum_{q=-m+1}^{r}\omega_{jq}\mid\mathbf{X}\right)\\&=\frac{1}{IT}\sigma_\omega^2+\frac{m(m-1)}{IT^2}\psi^B+\frac{r(r-1)}{IT^2}\psi^A+\frac{2mr}{IT^2}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\overline{\omega}_t,\omega_{is}\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\omega_{is}\mid\mathbf{X}\right)\\&=\frac{1}{I}\psi^X\end{aligned}$$

$$\begin{aligned}\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}(\overline{\omega}_t,\overline{\omega}_i\mid\mathbf{X})&=\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\mathrm{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\frac{1}{T}\sum_{p=-m+1}^{r}\omega_{ip}\mid\mathbf{X}\right)\\&=\frac{1}{IT}\sigma_\omega^2+\frac{m-1}{IT}\psi^B+\frac{r}{IT}\psi^X\end{aligned}$$

$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\overline{\omega}_t,\overline{\omega}_s \mid \mathbf{X}) = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\frac{1}{I}\sum_{k=1}^{I}\omega_{ks} \mid \mathbf{X}\right)$$
$$= \frac{1}{I}\psi^X$$
$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\overline{\omega}_t,\overline{\overline{\omega}} \mid \mathbf{X}) = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}\left(\frac{1}{I}\sum_{j=1}^{I}\omega_{jt},\frac{1}{IT}\sum_{k=1}^{I}\sum_{p=-m+1}\omega_{kp} \mid \mathbf{X}\right)$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{m-1}{IT}\psi^B + \frac{r}{IT}\psi^X$$
$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\overline{\overline{\omega}},\omega_{is} \mid \mathbf{X}) = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\omega_{is} \mid \mathbf{X}\right)$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{r-1}{IT}\psi^A + \frac{m}{IT}\psi^X$$
$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\overline{\overline{\omega}},\overline{\omega}_i \mid \mathbf{X}) = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\frac{1}{T}\sum_{q=-m+1}^{r}\omega_{iq} \mid \mathbf{X}\right)$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{m(m-1)}{IT^2}\psi^B + \frac{r(r-1)}{IT^2}\psi^A + \frac{2mr}{IT^2}\psi^X$$
$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\overline{\overline{\omega}},\overline{\omega}_s \mid \mathbf{X}) = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\frac{1}{I}\sum_{k=1}^{I}\omega_{ks} \mid \mathbf{X}\right)$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{r-1}{IT}\psi^A + \frac{m}{IT}\psi^X$$
$$\frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}(\overline{\overline{\omega}},\overline{\overline{\omega}} \mid \mathbf{X}) = \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r}\text{Cov}\left(\frac{1}{IT}\sum_{j=1}^{I}\sum_{p=-m+1}^{r}\omega_{jp},\frac{1}{IT}\sum_{k=1}^{I}\sum_{q=-m+1}^{r}\omega_{kq} \mid \mathbf{X}\right)$$
$$= \frac{1}{IT}\sigma_\omega^2 + \frac{m(m-1)}{IT^2}\psi^B + \frac{r(r-1)}{IT^2}\psi^A + \frac{2mr}{IT^2}\psi^X$$

Combining these terms and simplifying yields

$$\psi_{\hat{\omega}}^X = -\left(\tfrac{I-1}{IT}\right)\sigma_\omega^2 - \left(\tfrac{(I-1)r(m-1)}{IT^2}\right)\psi^B - \left(\tfrac{(I-1)m(r-1)}{IT^2}\right)\psi^A + \left(\tfrac{2(I-1)mr}{IT^2}\right)\psi^X \tag{E4}$$

To summarize, Equations (E1), (E2), (E3), and (E4) express the residual-based parameters as functions of the true parameters that define the error structure. Rearranging these four equations:

$$\sigma_{\hat{\omega}}^2 = \left(\frac{I-1}{IT^2}\right)\left(T(T-1)\sigma_\omega^2 - m(m-1)\psi^B - r(r-1)\psi^A - 2mr\psi^X\right)$$
$$\psi_{\hat{\omega}}^B = \left(\frac{I-1}{IT^2}\right)\left(-T\sigma_\omega^2 + (r^2+2r+m)\psi^B + r(r-1)\psi^A - 2r^2\psi^X\right)$$
$$\psi_{\hat{\omega}}^A = \left(\frac{I-1}{IT^2}\right)\left(-T\sigma_\omega^2 + m(m-1)\psi^B + (m^2+2m+r)\psi^A - 2m^2\psi^X\right)$$
$$\psi_{\hat{\omega}}^X = \left(\frac{I-1}{IT^2}\right)\left(-T\sigma_\omega^2 - r(m-1)\psi^B - m(r-1)\psi^A + 2mr\psi^X\right)$$

In matrix notation, these four equations become:

$$\begin{bmatrix} \sigma^2_{\hat{\omega}} \\ \psi^B_{\hat{\omega}} \\ \psi^A_{\hat{\omega}} \\ \psi^X_{\hat{\omega}} \end{bmatrix} = \mathbf{\Gamma} \begin{bmatrix} \sigma^2_{\omega} \\ \psi^B \\ \psi^A \\ \psi^X \end{bmatrix} \tag{E5}$$

where

$$\mathbf{\Gamma} = \frac{I-1}{IT^2} \begin{bmatrix} T(T-1) & -m(m-1) & -r(r-1) & -2mr \\ -T & r^2+2r+m & r(r-1) & -2r^2 \\ -T & m(m-1) & m^2+2m+r & -2m^2 \\ -T & -r(m-1) & -m(r-1) & 2mr \end{bmatrix}$$

Minimum detectable effect We are ultimately interested in deriving an expression for the MDE of an experiment as a function of the residual-based parameters, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, rather than the true parameters, σ^2_{ω}, ψ^B, ψ^A, and ψ^X. Recall that:

$$MDE = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \sqrt{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma^2_{\omega} + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]}$$

Having solved for $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ as linear functions of the true parameters σ^2_{ω}, ψ^B, ψ^A, and ψ^X, we can define k_σ, k_B, k_A, and k_X as coefficients on the residual-based parameters $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. These coefficients will allow us to use residual-based parameters in the SCR formula in place of the true parameters. In other words, k_σ, k_B, k_A, and k_X must satisfy the following equation:[36]

$$\begin{aligned} &\left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \sqrt{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)k_\sigma\sigma^2_{\hat{\omega}} + \left(\frac{m-1}{m}\right)k_B\psi^B_{\hat{\omega}} + \left(\frac{r-1}{r}\right)k_A\psi^A_{\hat{\omega}} - 2k_X\psi^X_{\hat{\omega}}\right]} \\ &\quad = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \sqrt{\left(\frac{1}{P(1-P)J}\right)\left[\left(\frac{m+r}{mr}\right)\sigma^2_{\omega} + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X\right]} \end{aligned}$$

or more simply:

$$\begin{aligned} &\left(\frac{m+r}{mr}\right)k_\sigma\sigma^2_{\hat{\omega}} + \left(\frac{m-1}{m}\right)k_B\psi^B_{\hat{\omega}} + \left(\frac{r-1}{r}\right)k_A\psi^A_{\hat{\omega}} - 2k_X\psi^X_{\hat{\omega}} \\ &\quad = \left(\frac{m+r}{mr}\right)\sigma^2_{\omega} + \left(\frac{m-1}{m}\right)\psi^B + \left(\frac{r-1}{r}\right)\psi^A - 2\psi^X \end{aligned}$$

36. This assumes k_σ, k_B, k_A, and k_X are functions of m, r, and I only, and do not themselves depend on σ^2_{ω}, ψ^B, ψ^A, or ψ^X. We show this to be true below.

We can express this equality in matrix form as:[37]

$$\begin{bmatrix} \frac{m+r}{mr} & \frac{m-1}{m} & \frac{r-1}{r} & -2 \end{bmatrix} \begin{bmatrix} \sigma^2_\omega \\ \psi^B \\ \psi^A \\ \psi^X \end{bmatrix} = \begin{bmatrix} \left(\frac{m+r}{mr}\right) k_\sigma & \left(\frac{m-1}{m}\right) k_B & \left(\frac{r-1}{r}\right) k_A & -2k_X \end{bmatrix} \begin{bmatrix} \sigma^2_{\hat{\omega}} \\ \psi^B_{\hat{\omega}} \\ \psi^A_{\hat{\omega}} \\ \psi^X_{\hat{\omega}} \end{bmatrix}$$

Substituting Equation (E5):

$$\Rightarrow \quad \begin{bmatrix} \frac{m+r}{mr} & \frac{m-1}{m} & \frac{r-1}{r} & -2 \end{bmatrix} \begin{bmatrix} \sigma^2_\omega \\ \psi^B \\ \psi^A \\ \psi^X \end{bmatrix} = \begin{bmatrix} \left(\frac{m+r}{mr}\right) k_\sigma & \left(\frac{m-1}{m}\right) k_B & \left(\frac{r-1}{r}\right) k_A & -2k_X \end{bmatrix} \mathbf{\Gamma} \begin{bmatrix} \sigma^2_\omega \\ \psi^B \\ \psi^A \\ \psi^X \end{bmatrix} \tag{E6}$$

where $\mathbf{\Gamma}$ is defined as above:

$$\mathbf{\Gamma} = \frac{I-1}{IT^2} \begin{bmatrix} T(T-1) & -m(m-1) & -r(r-1) & -2mr \\ -T & r^2+2r+m & r(r-1) & -2r^2 \\ -T & m(m-1) & m^2+2m+r & -2m^2 \\ -T & -r(m-1) & -m(r-1) & 2mr \end{bmatrix}$$

$\mathbf{\Gamma}$ is a singular matrix and cannot be inverted. However, we can show that, rather than having no solutions, Equation (E6) is instead overdetermined and there are infinite solutions. We are simply interested in one such solution. To find one set of k_σ, k_B, k_A, and k_X coefficients that solve this system, we iteratively solve for each k coefficient as a function of the remaining k coefficients and then substitute into the subsequent equations. That is, we use the first equation of this system to solve for k_σ as a function of k_B, k_A, and k_X and substitute this expression in place of k_σ in the subsequent equations in the system, and we repeat for the remaining coefficients and equations. This iterative procedure yields:

$$k_\sigma = \frac{I(m+r)^2 + (I-1)[r(m-1)k_B + m(r-1)k_A - 2mrk_X]}{(I-1)(m+r)(m+r-1)}$$

$$k_B = \frac{I(m+r)^2(m-r+mr+r^2) + (I-1)[m^2(r-1)(2-m-r)k_A + 2mr(r-m-mr-r^2)k_X]}{(I-1)r^2(3m+r+mr+r^2-2)}$$

$$k_A = \frac{I(m+r)^2 - 2(I-1)mrk_X}{2(I-1)m^2}$$

$$k_X = 0$$

We iteratively substitute each coefficient into the expressions for the remaining coefficients. That is, we first substitute this value of k_X into the expressions for the other three coefficients, then substitute k_A, and so on. This yields expressions for k_σ, k_B, k_A, and k_X in terms of m, r, and I.

$$k_\sigma = \frac{I(m+r)^2}{2(I-1)mr}$$

37. Note that if $m = 1$ (or $r = 1$), the corresponding ψ^B and $\psi^B_{\hat{\omega}}$ (or ψ^A and $\psi^A_{\hat{\omega}}$) parameters are undefined and no longer enter the system. Similarly, the corresponding row(s) and column(s) are removed from $\mathbf{\Gamma}$.

$$k_B = \frac{I(m+r)^2}{2(I-1)r^2}$$
$$k_A = \frac{I(m+r)^2}{2(I-1)m^2}$$
$$k_X = 0$$

We can now express the MDE of an experiment as a function of the residual-based parameters:

$$MDE^{est} = \left(t^J_{1-\kappa} + t^J_{\alpha/2}\right) \left\{ \left(\frac{1}{P(1-P)J}\right) \left[\left(\frac{m+r}{mr}\right) \left(\frac{I(m+r)^2}{2(I-1)mr}\right) \sigma^2_{\hat{\omega}} + \left(\frac{m-1}{m}\right) \left(\frac{I(m+r)^2}{2(I-1)r^2}\right) \psi^B_{\hat{\omega}} + \left(\frac{r-1}{r}\right) \left(\frac{I(m+r)^2}{2(I-1)m^2}\right) \psi^A_{\hat{\omega}} \right] \right\}^{1/2}$$

E.3 Estimation with ANCOVA

Here, we extend Appendices E.1 and E.2, which discuss estimating the *MDE* from residual-based parameters, for the ANCOVA model. As above, we do not observe the true parameters characterizing the error structure (σ^2_υ, σ^2_ω, ψ^B, ψ^A, and ψ^X). We cannot calculate these from a dataset, so we turn to residual-based versions of these parameters ($\sigma^2_{\hat{\upsilon}}$, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$), which we can use to define $MDE^{est}_{ANCOVA}(\sigma^2_{\hat{\upsilon}}, \sigma^2_{\hat{\omega}}, \psi^B_{\hat{\omega}}, \psi^A_{\hat{\omega}}, \psi^X_{\hat{\omega}}) = MDE_{ANCOVA}(\sigma^2_\upsilon, \sigma^2_\omega, \psi^B, \psi^A, \psi^X)$ such that the MDE from estimated parameters is equivalent to the SCR formula's MDE derived from true parameters.

Model As in Appendix E.2, we assume this model is identical to the model that generates the relevant power calculation formula; in this case, SCR ANCOVA given in Equation (11).

That is, there are J units, P proportion of which are randomized into treatment. The researcher again collects outcome data Y_{it} for each unit i, across m pre-treatment time periods and r post-treatment time periods. For treated units, $D_{it} = 0$ in pre-treatment periods and $D_{it} = 1$ in post-treatment periods; for control units, $D_{it} = 0$ in all periods.

Assumption (Data generating process)**.** *The data are generated according to the following model:*

$$Y_{it} = \beta + \tau D_{it} + \upsilon_i + \omega_{it}$$

where Y_{it} is the outcome of interest for unit i at time t; β is the expected outcome of non-treated observations; τ is the treatment effect that is homogeneous across all units and all time periods; D_{it} is a time-varying treatment indicator; υ_i is a time-invariant unit effect distributed i.i.d. $\mathcal{N}(0, \sigma^2_\upsilon)$; and ω_{it} is an idiosyncratic error term distributed (not necessarily i.i.d.) $\mathcal{N}(0, \sigma^2_\omega)$.

Assumption (Strict exogeneity)**.** $\mathrm{E}[\omega_{it} \mid \mathbf{X}] = 0$*, where* $\mathbf{X}$ *is a full rank matrix of regressors, including a constant, the treatment indicator* $\mathbf{D}$*, and* $J-1$ *unit dummies. This again follows from random assignment of* D_{it}*.*

Assumption (Balanced panel)**.** *The number of pre-treatment observations, m, and post-treatment observations, r, is the same for each unit, and all units are observed in every time period.*

Assumption (Independence across units)**.** $\mathrm{E}[\omega_{it}\omega_{js} \mid \mathbf{X}] = 0, \ \forall\ i \neq j, \ \forall\ t, s.$

Assumption (Uniform covariance structures)**.** *Define:*

$$\psi_i^B \equiv \frac{2}{m(m-1)} \sum_{t=-m+1}^{-1} \sum_{s=t+1}^{0} \text{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$
$$\psi_i^A \equiv \frac{2}{r(r-1)} \sum_{t=1}^{r-1} \sum_{s=t+1}^{r} \text{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$
$$\psi_i^X \equiv \frac{1}{mr} \sum_{t=-m+1}^{0} \sum_{s=1}^{r} \text{Cov}\left(\omega_{it}, \omega_{is} \mid \mathbf{X}\right)$$

to be the average pre-treatment, post-treatment, and across-period covariance between different error terms of unit i, respectively. Using these definitions, assume that $\psi^B = \psi_i^B$, $\psi^A = \psi_i^A$, and $\psi^X = \psi_i^X$ $\forall i$.

Parameters We begin by estimating the fixed effects and residuals. Note that even though we are ultimately interested in performing a power calculation for the ANCOVA estimator, we generate residuals for this data generating process by regressing Y_{it} on a constant and unit fixed effects:

$$Y_{it} = \beta + \upsilon_i + \omega_{it}$$

For a balanced panel, the estimated coefficients are:

$$\hat{\beta} = \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} Y_{it}$$
$$\hat{\upsilon}_i = \frac{1}{T} \sum_{t=-m+1}^{r} Y_{it} - \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} Y_{it}$$

Then the estimated fixed effects and the residuals are

$$\hat{\upsilon}_i = (\upsilon_i - \overline{\upsilon}) + (\overline{\omega}_i - \overline{\overline{\omega}})$$
$$\hat{\omega}_{it} = \omega_{it} - \overline{\omega}_i$$

where

$$\overline{\upsilon} = \frac{1}{I} \sum_{i=1}^{I} \upsilon_i$$
$$\overline{\omega}_i = \frac{1}{T} \sum_{t=-m+1}^{r} \omega_{it}$$
$$\overline{\overline{\omega}} = \frac{1}{IT} \sum_{i=1}^{I} \sum_{t=-m+1}^{r} \omega_{it}$$

We first use the estimated fixed effects to derive an expression for $\sigma^2_{\hat{\upsilon}}$:[38]

$$\begin{aligned}
\sigma^2_{\hat{\upsilon}} &= \frac{1}{I}\sum_{i=1}^{I} \text{Var}(\hat{\upsilon}_i \mid \mathbf{X}) \\
&= \frac{1}{I}\sum_{i=1}^{I} \left[\text{Var}(\upsilon_i \mid \mathbf{X}) + \text{Var}(\overline{\upsilon} \mid \mathbf{X}) - 2\,\text{Cov}(\upsilon_i, \overline{\upsilon} \mid \mathbf{X}) + \text{Var}(\overline{\omega}_i \mid \mathbf{X}) + \text{Var}(\overline{\overline{\omega}} \mid \mathbf{X}) - 2\,\text{Cov}(\overline{\omega}_i, \overline{\overline{\omega}} \mid \mathbf{X})\right] \\
&= \left(\frac{I-1}{IT^2}\right)\left[T^2\sigma^2_{\upsilon} + T\sigma^2_{\omega} + (m(m-1))\psi^B + (r(r-1))\psi^A + 2mr\psi^X\right]
\end{aligned}$$

We also use the definition of residuals to derive expressions for $\sigma^2_{\hat{\upsilon}}$, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$:

$$\begin{aligned}
\sigma^2_{\hat{\omega}} &= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \text{Var}(\hat{\omega}_{it} \mid \mathbf{X}) \\
&= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r} \left[\text{Var}(\omega_{it} \mid \mathbf{X}) + \text{Var}(\overline{\omega}_i \mid \mathbf{X}) - 2\,\text{Cov}(\omega_{it}, \overline{\omega}_i \mid \mathbf{X})\right] \\
&= \left(\frac{T-1}{T}\right)\sigma^2_{\omega} - \left(\frac{m(m-1)}{T^2}\right)\psi^B - \left(\frac{r(r-1)}{T^2}\right)\psi^A - \left(\frac{2mr}{T^2}\right)\psi^X \\
\psi^A_{\hat{\omega}} &= \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r} \text{Cov}(\hat{\omega}_{it}, \hat{\omega}_{is} \mid \mathbf{X}) \\
&= \frac{2}{r(r-1)I}\sum_{i=1}^{I}\sum_{t=1}^{r-1}\sum_{s=t+1}^{r} \left[\text{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) - \text{Cov}(\omega_{it}, \overline{\omega}_i \mid \mathbf{X}) - \text{Cov}(\omega_{is}, \overline{\omega}_i \mid \mathbf{X}) + \text{Var}(\overline{\omega}_i \mid \mathbf{X})\right] \\
&= \frac{-1}{T}\sigma^2_{\omega} + \left(\frac{m(m-1)}{T^2}\right)\psi^B + \left(\frac{m^2+2m+r}{T^2}\right)\psi^A - \left(\frac{2m^2}{T^2}\right)\psi^X \\
\psi^B_{\hat{\omega}} &= \frac{-1}{T}\sigma^2_{\omega} + \left(\frac{r^2+2r+m}{T^2}\right)\psi^B + \left(\frac{r(r-1)}{T^2}\right)\psi^A - \left(\frac{2r^2}{T^2}\right)\psi^X \\
\psi^X_{\hat{\omega}} &= \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r} \text{Cov}(\hat{\omega}_{it}, \hat{\omega}_{is} \mid \mathbf{X}) \\
&= \frac{1}{Imr}\sum_{i=1}^{I}\sum_{t=-m+1}^{0}\sum_{s=1}^{r} \left[\text{Cov}(\omega_{it}, \omega_{is} \mid \mathbf{X}) - \text{Cov}(\omega_{it}, \overline{\omega}_i \mid \mathbf{X}) - \text{Cov}(\omega_{is}, \overline{\omega}_i \mid \mathbf{X}) + \text{Var}(\overline{\omega}_i \mid \mathbf{X})\right] \\
&= \frac{-1}{T}\sigma^2_{\omega} - \left(\frac{r(m-1)}{T^2}\right)\psi^B - \left(\frac{m(r-1)}{T^2}\right)\psi^A + \left(\frac{2mr}{T^2}\right)\psi^X
\end{aligned}$$

Minimum detectable effect Having solved for $\sigma^2_{\hat{\upsilon}}$, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$ as linear functions of the true parameters σ^2_{υ}, σ^2_{ω}, ψ^B, ψ^A, and ψ^X, we can define k_{υ}, k_{ω}, k_B, k_A, and k_X as coefficients on the residual-based parameters $\sigma^2_{\hat{\upsilon}}$, $\sigma^2_{\hat{\omega}}$, $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$. These coefficients will allow us to use residual-based parameters in the SCR formula in place of the true parameters. In other words, k_{υ},

38. Note that, as stated in the strict exogeneity assumption, $\mathbf{X}$ is a full rank matrix of regressors, including a constant, the treatment indicator $\mathbf{D}$, and $J-1$ unit dummies. That is, $\mathbf{X}$ includes the covariates of the data generating process and residualizing regression, not of the ANCOVA model.

k_ω, k_B, k_A, and k_X must satisfy the following equation:

$$\begin{aligned}(1-\theta)^2\sigma_\upsilon^2 + \left(\frac{\theta^2}{m} + \frac{1}{r}\right)\sigma_\omega^2 + \frac{\theta^2(m-1)}{m}\psi^B + \frac{r-1}{r}\psi^A - 2\theta\psi^X \\ = k_\upsilon\sigma_{\hat{\upsilon}}^2 + k_\omega\sigma_{\hat{\omega}}^2 + k_B\psi_{\hat{\omega}}^B + k_A\psi_{\hat{\omega}}^A + k_X\psi_{\hat{\omega}}^X\end{aligned}$$

Following the solution procedure described in Appendix E.2, we solve for a set of k coefficients that satisfy this equality:

$$\begin{aligned}k_\upsilon &= \left(\frac{I}{I-1}\right)(1-\theta)^2 \\ k_\omega &= \left(\frac{m+\theta r}{2m^2r^2}\right)((m+r)(m+\theta r) + (1-\theta)(mr^2 - m^2r)) \\ k_B &= \left(\frac{m+\theta r}{2mr^2}\right)(m-1)(m+\theta r - (1-\theta)mr) \\ k_A &= \left(\frac{m+\theta r}{2m^2r}\right)(r-1)(m+\theta r + (1-\theta)mr) \\ k_X &= 0\end{aligned}$$

Note, however, that these coefficients are functions of not only m, r, and I, as in Appendix E.2. These coefficients are also a function of θ, which is itself a function of true parameters:

$$\theta = \frac{m\sigma_\upsilon^2 + m\psi^X}{m\sigma_\upsilon^2 + \sigma_\omega^2 + (m-1)\psi^B}$$

As a result, in order to use these coefficients in a residual-based SCR ANCOVA formula, we must also derive a residual-based expression for θ. We define a set of coefficients to correspond to the numerator of θ: k_υ^N, k_ω^N, k_B^N, k_A^N, and k_X^N; we also define a set of coefficients to correspond to the denominator of θ: k_υ^D, k_ω^D, k_B^D, k_A^D, and k_X^D. We seek to find the sets of k^N and k^D coefficients that satisfy the following equality:

$$\frac{m\sigma_\upsilon^2 + m\psi^X}{m\sigma_\upsilon^2 + \sigma_\omega^2 + (m-1)\psi^B} = \frac{k_\upsilon^N\sigma_{\hat{\upsilon}}^2 + k_\omega^N\sigma_{\hat{\omega}}^2 + k_B^N\psi_{\hat{\omega}}^B + k_A^N\psi_{\hat{\omega}}^A + k_X^N\psi_{\hat{\omega}}^X}{k_\upsilon^D\sigma_{\hat{\upsilon}}^2 + k_\omega^D\sigma_{\hat{\omega}}^2 + k_B^D\psi_{\hat{\omega}}^B + k_A^D\psi_{\hat{\omega}}^A + k_X^D\psi_{\hat{\omega}}^X}$$

We again follow the solution procedure described in Appendix E.2 as we separately solve for a set of k^N coefficients and k^D coefficients. One such solution for the numerator coefficients is:

$$\begin{aligned}k_\upsilon^N &= \left(\frac{I}{I-1}\right)m \\ k_\omega^N &= \left(\frac{-1}{4r}\right)(m(m-r+2) + r(r-m+2)) \\ k_B^N &= \left(\frac{-m}{4r}\right)(m-1)(m-r+2) \\ k_A^N &= \left(\frac{-1}{4}\right)(r-1)(r-m+2) \\ k_X^N &= 0\end{aligned}$$

and one such solution for the denominator coefficients is:

$$
\begin{aligned}
k_{\upsilon}^{D} &= \left(\frac{I}{I-1}\right) m \\
k_{\omega}^{D} &= \left(\frac{1}{2m}\right) (m(m+1) - r(m-1)) \\
k_{B}^{D} &= \left(\frac{1}{2}\right) (m+1)(m-1) \\
k_{A}^{D} &= \left(\frac{-r}{2m}\right) (m-1)(r-1) \\
k_{X}^{D} &= 0
\end{aligned}
$$

Combining these two sets of coefficients, we express θ as a function of residual-based parameters:

$$
\begin{aligned}
\theta = & \frac{m\left[\left(\frac{I}{I-1}\right) 4mr\sigma_{\hat{\upsilon}}^{2} - (m(m-r+2) + r(r-m+2))\sigma_{\hat{\omega}}^{2}\right]}{2r\left[\left(\frac{I}{I-1}\right) 2m^{2}\sigma_{\upsilon}^{2} + (m(m+1) - r(m-1))\sigma_{\hat{\omega}}^{2} + m(m-1)(m+1)\psi_{\hat{\omega}}^{B} - r(m-1)(r-1)\psi_{\hat{\omega}}^{A}\right]} \\
& + \frac{m\left[-m(m-1)(m-r+2)\psi_{\hat{\omega}}^{B} - r(r-1)(r-m+2)\psi_{\hat{\omega}}^{A}\right]}{2r\left[\left(\frac{I}{I-1}\right) 2m^{2}\sigma_{\upsilon}^{2} + (m(m+1) - r(m-1))\sigma_{\hat{\omega}}^{2} + m(m-1)(m+1)\psi_{\hat{\omega}}^{B} - r(m-1)(r-1)\psi_{\hat{\omega}}^{A}\right]}
\end{aligned}
$$

We can now express the SCR ANCOVA formula using only residual-based parameters:

$$
\begin{aligned}
MDE_{ANCOVA}^{est} \approx (t_{1-\kappa}^{J} + t_{\alpha/2}^{J}) \times \Bigg\{ & \left(\frac{1}{P(1-P)J}\right) \\
& \times \Bigg[\left(\frac{I}{I-1}\right) (1-\theta)^{2}\sigma_{\hat{\upsilon}}^{2} \\
& + \left(\frac{m+\theta r}{2m^{2}r^{2}}\right) ((m+r)(m+\theta r) + (1-\theta)(mr^{2} - m^{2}r))\sigma_{\hat{\omega}}^{2} \\
& + \left(\frac{m+\theta r}{2mr^{2}}\right) (m-1)(m+\theta r - (1-\theta)mr)\psi_{\hat{\omega}}^{B} \\
& + \left(\frac{m+\theta r}{2m^{2}r}\right) (r-1)(m+\theta r + (1-\theta)mr)\psi_{\hat{\omega}}^{A} \Bigg] \Bigg\}^{1/2}
\end{aligned}
$$

where θ is as given above.

Estimated parameters We finally show that we can recover unbiased estimates of the residual-based parameters, as in Appendix E.1. We begin with the variance of the fixed effects, $\sigma_{\hat{\upsilon}}^{2}$, and define:

$$
\tilde{\sigma}_{\hat{\upsilon}}^{2} \equiv \frac{1}{I}\sum_{i=1}^{I}\left(\hat{\upsilon}_{i} - \overline{\hat{\upsilon}}\right)^{2}
$$

where $\overline{\hat{v}} = \frac{1}{I}\sum_{i=1}^{I} \hat{v} = 0$. Taking expectations of both sides:

$$\begin{aligned} \mathrm{E}\left[\tilde{\sigma}^2_{\hat{v}} \mid \mathbf{X}\right] &= \frac{1}{I}\sum_{i=1}^{I} \mathrm{E}\left[\hat{v}^2 \mid \mathbf{X}\right] \\ &= \sigma^2_{\hat{v}} \end{aligned}$$

Next, to estimate the variance of the residuals, $\sigma^2_{\hat{\omega}}$, we define:

$$\tilde{\sigma}^2_{\hat{\omega}} \equiv \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r}\left(\hat{\omega}_{it} - \overline{\overline{\hat{\omega}}}\right)^2$$

where $\overline{\overline{\hat{\omega}}} = \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r}\hat{\omega}_{it} = 0$. Taking expectations of both sides:

$$\begin{aligned} \mathrm{E}\left[\tilde{\sigma}^2_{\hat{\omega}} \mid \mathbf{X}\right] &= \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=-m+1}^{r}\mathrm{E}\left[\hat{\omega}^2_{it} \mid \mathbf{X}\right] \\ &= \sigma^2_{\hat{\omega}} \end{aligned}$$

To estimate $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, we define the $[I \times 1]$ vector of residuals for period t as $\hat{\boldsymbol{\omega}}_t$ and calculate the average covariance between any two vectors in the relevant range of time periods. For the pre-period, we define:

$$\begin{aligned} \tilde{\psi}^B_{\hat{\omega}} &\equiv \frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\left(\hat{\omega}_{it} - \overline{\hat{\omega}}_t\right)\left(\hat{\omega}_{is} - \overline{\hat{\omega}}_s\right) \\ &= \frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\left(\hat{\omega}_{it}\hat{\omega}_{is} - \hat{\omega}_{it}\overline{\hat{\omega}}_s - \hat{\omega}_{is}\overline{\hat{\omega}}_t + \overline{\hat{\omega}}_t\overline{\hat{\omega}}_s\right) \\ &= \frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\left(\frac{I-1}{I}\right)\hat{\omega}_{it}\hat{\omega}_{is} \end{aligned}$$

Taking expectations yields:

$$\begin{aligned} \mathrm{E}\left[\tilde{\psi}^B_{\hat{\omega}} \mid \mathbf{X}\right] &= \left(\frac{I-1}{I}\right)\frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\mathrm{E}\left[\hat{\omega}_{it}\hat{\omega}_{is} \mid \mathbf{X}\right] \\ &= \left(\frac{I-1}{I}\right)\frac{2}{Im(m-1)}\sum_{t=-m+1}^{-1}\sum_{s=t+1}^{0}\sum_{i=1}^{I}\mathrm{Cov}(\hat{\omega}_{it}, \hat{\omega}_{is} \mid \mathbf{X}) \\ &= \left(\frac{I-1}{I}\right)\psi^B_{\hat{\omega}} \end{aligned}$$

Similarly:

$$\mathrm{E}\left[\tilde{\psi}^A_{\hat{\omega}} \mid \mathbf{X}\right] = \left(\frac{I-1}{I}\right)\psi^A_{\hat{\omega}}$$
$$\mathrm{E}\left[\tilde{\psi}^X_{\hat{\omega}} \mid \mathbf{X}\right] = \left(\frac{I-1}{I}\right)\psi^X_{\hat{\omega}}$$

Hence, we can recover unbiased estimates of the parameters $\sigma^2_{\hat{\upsilon}}$ and $\sigma^2_{\hat{\omega}}$ by calculating the averages of the estimated $\tilde{\sigma}^2_{\hat{\upsilon}}$ and $\tilde{\sigma}^2_{\hat{\omega}}$, respectively. However, to recover unbiased estimates of the parameters $\psi^B_{\hat{\omega}}$, $\psi^A_{\hat{\omega}}$, and $\psi^X_{\hat{\omega}}$, we must calculate $\tilde{\psi}^B_{\hat{\omega}}$, $\tilde{\psi}^A_{\hat{\omega}}$, and $\tilde{\psi}^X_{\hat{\omega}}$, respectively, and then inflate these averages by $\frac{I}{I-1}$.

Appendix References

Athey, Susan, and Guido Imbens. 2017a. "The State of Applied Econometrics: Causality and Policy Evaluation." *Journal of Economic Perspectives* 31 (2): 3–32.

Athey, Susan, and Guido W. Imbens. 2017b. "The Econometrics of Randomized Experiments." *Handbook of Economic Field Experiments* 1:73–140.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates?" *The Quarterly Journal of Economics* 119 (1): 249–275.

Bloom, Nicholas, James Liang, John Roberts, and Zhichun Jenny Ying. 2015. "Does Working from Home Work? Evidence from a Chinese Experiment." *The Quarterly Journal of Economics* 130 (1): 165–218.

Cameron, A. Colin, and Douglas L. Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50 (2): 317–372.

Campbell, Cathy. 1977. "Properties of Ordinary and Weighted Least Square Estimators of Regression Coefficients for Two-Stage Samples." *Proceedings of the Social Statistics Section, American Statistical Association:* 800–805.

Frison, L., and S. J. Pocock. 1992. "Repeated Measures in Clinical Trials: Analysis Using Mean Summary Statistics and its Implications for Design." *Statistics in Medicine* 11 (13): 1685–1704.

McKenzie, David. 2012. "Beyond Baseline and Follow-up: The Case for More T in Experiments." *Journal of Development Economics* 99 (2): 210–221.

Moulton, Brent. 1986. "Random group effects and the precision of regression estimates." *Journal of Econometrics* 32 (3): 385–397.

Pecan Street. 2016. "Dataport." `https://dataport.pecanstreet.org/`.